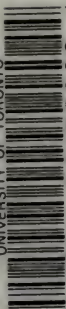


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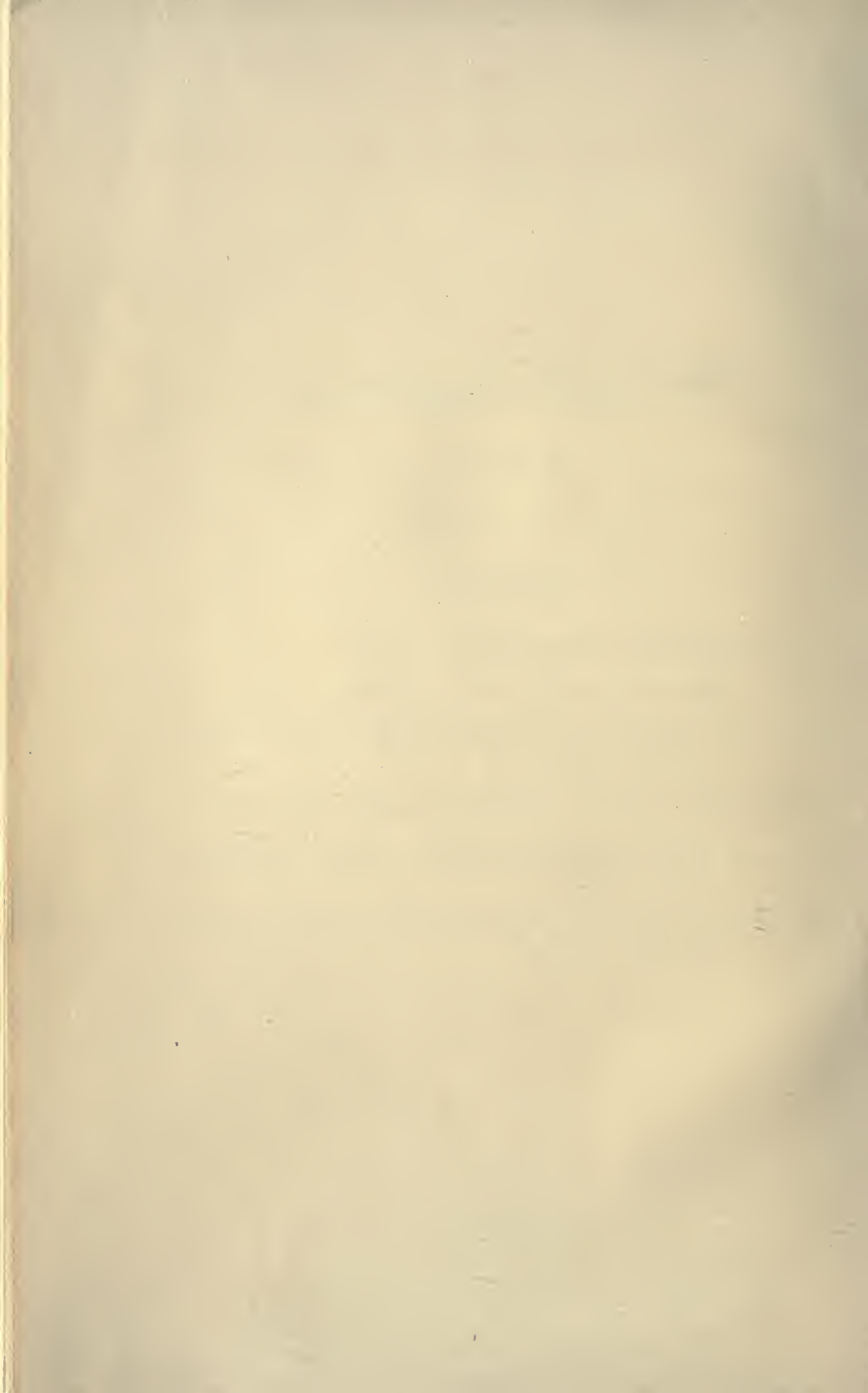
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## CONTENTS.

- XI. *On Inhibition:* B. B. BREESE. Pp. 65.
- XII. *On After-images:* SHEPHERD IVORY FRANZ. Pp. 61.
- XIII. *The Accuracy of Voluntary Movement:* R. S. WOOD-  
WORTH. Pp. 114.
- XIV. *A Study of Lapses:* H. HEATH BAWDEN. Pp. 122.
- XV. *The Mental Life of the Monkeys:* EDWARD L. THORN-  
DIKE. Pp. 57.
- XVI. *The Correlation of Mental and Physical Tests:* CLARK  
WISSLER. Pp. 62.



109.

# THE Psychological Review

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## On Inhibition

BY

B. B. BREESE, A.M.

[Submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy in the Faculty of Philosophy, Columbia University, and being Vol. V., No. 2, of Columbia University Contributions to Philosophy, Psychology and Education.]

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## CONTENTS.

INTRODUCTION.....	I
<i>Physiological Inhibition</i> .....	I
<i>Psychological Inhibition</i> .....	6
BINOCULAR RIVALRY.....	18
<i>Apparatus</i> .....	21
<i>Experiments</i> .....	23
Will Power.....	23
Eye Movements.....	27
Figures .....	28
Moving Field .....	30
Muscle Contraction.....	31
Colored Borders.....	33
Fields of Different Area.....	34
Rivalry Under Normal Conditions.....	35
Light Intensities .....	37
After-Images .....	39
Fields of Like Color.....	41
Monocular Rivalry.....	42
<i>Summary</i> .....	44
INHIBITION OF MOTOR REACTIONS.....	48
<i>Experiments</i> .....	50
<i>Conclusions</i> .....	58
APPLICATIONS TO EDUCATION.....	62



## INTRODUCTION.

### PHYSIOLOGICAL INHIBITION.

Inhibition, from the point of view of the physiologist, is a definite, positive process that takes place either in nerve fibers and centers or in the tissue itself. Since the discovery by Edward Weber<sup>1</sup> of the fact that nerve stimulation can produce restraint as well as excitement of an organ, many investigations have been made and many theories proposed to explain it. The general conception of inhibition for the physiologist may be expressed somewhat as follows: Inhibition is arrest of the function of an organ by the action upon it of another organ, or by another function of the same organ, while its power to execute the arrested function is retained, and can manifest itself as soon as the restraining power is removed.<sup>2</sup>

Two processes then are necessary in any phenomenon of inhibition, the process *inhibited* and the process *inhibiting*.

The phenomenon of inhibition may be considered as due to a special process, or as the result arising from the conflict of the different active processes of the body. In the first case there may be a special set of brain centers and nerve fibers whose function is merely that of arresting activity. Or, the brain centers may be arranged in such a way that each center is constantly under the inhibitive influences of higher centers. Or, some characteristic of the tissue itself brings about a condition in which it ceases to respond to stimulation. In the second case there is no process of inhibition as such. If activity is arrested, it is due to the fact that one process gets in the way of another, or that under certain circumstances two processes are opposed and neutralized, neither process being primarily an inhibitive

<sup>1</sup> Wagner's Handwörterbuch, Bd. III., p. 31.

<sup>2</sup> Dr. Meltzer gives the following definition of inhibition: "Inhibition means a temporary diminution or abolition of a vital activity brought about by an external or internal stimulus." Paper read before the New York Academy of Medicine, February, 1899.

one—*i. e.*, having for its object the arrest of activity. Or the energy of the body may be considered as limited, and that a manifestation of energy in one direction may inhibit its expenditure in any other direction.

Between these conceptions of inhibition there are such vital differences that their indiscriminate use leads to confusion. In physiology, as we have already indicated, the tendency has been to accept the first hypothesis: Inhibition is a definite physiological process taking place for the sole purpose of arresting bodily functions. It has been urged that if it were not for a direct inhibitive influence any central discharge would continue to the point of exhaustion. To account for the fact that this does not take place Dr. Mercier<sup>1</sup> suggests that every nerve center must have its intrinsic tendency to discharge under the control of some extrinsic influence. He does not believe there is any evidence of a special inhibitory center; on the other hand, there is much evidence against it. Every center exerts an inhibitive influence upon lower centers concurrently with its other functions. Therefore every nerve center is subject to, and in turn exercises an inhibiting influence upon, lower centers, thus forming a hierarchy of centers. Blanchi<sup>2</sup> agrees with this explanation. He also thinks that any cortical area may become in turn an inhibiting center for other centers. Ferrier<sup>3</sup> concludes that his experiments point to the frontal lobes as centers of inhibition. But both Blanchi and Monk deny the assertions of Ferrier. They found no loss of control upon removal of the frontal lobes of animals.<sup>4</sup> A monkey whose frontal lobes had been removed by Monk was deterred from picking up a morsel of food by a mere look or gesture of menace. Lewes<sup>5</sup> comes to the same conclusions—*i. e.*, that there are no special centers of inhibition, but that every center may exert an inhibiting influence. Brunton<sup>6</sup> is also of the opinion that special inhibitory centers are unnecessary to explain the

<sup>1</sup> *Brain*, Vol. XI., 1889, p. 361.

<sup>2</sup> *Brain*, 1895, p. 503.

<sup>3</sup> *The Functions of the Brain*, London, 1876.

<sup>4</sup> *Brain*, 1895.

<sup>5</sup> *The Physical Basis of Mind*, p. 333.

<sup>6</sup> *Nature*, March 8, 1893.



phenomenon of inhibition. All the above writers, however, consider inhibition as a special process.

There seems to be evidence that the brain has an inhibiting influence upon the spinal cord. The classical experiment upon frogs and lizards is cited by nearly every writer upon the subject. If the brain is removed, the reflexes of the animal increase.<sup>1</sup> They increase proportionately as the layers are removed; and the spinal cord becomes more excitable as its upper part is shaved away. In man lesions in the motor zone seem to cause proportionately greater muscular disturbance than in animals. This would indicate that the subcortical centers are less inhibited by the higher centers as the scale of animal organization descends.<sup>2</sup>

The interference of nerve waves coming from different stimuli, and meeting in the same center, has been proposed as an explanation of inhibition.<sup>3</sup> This theory supposes that under certain conditions nerve impulses may neutralize each other, somewhat in the same manner as light waves. Brunton explains upon this theory the inhibition of the tickling sensation by pressure. If the sole of the foot is touched lightly, violent muscular contractions are caused; but if a firm pressure be applied, muscular activity is inhibited. The following diagram will illustrate.

In slight touches the stimulus is sufficient only to excite the path  $S-S-M-M$ , which produces the contractions at  $M$ . When the stimulus is increased by pressure it ascends to a higher center, or cell  $S'$ , and this cell discharges into  $M$ . Therefore  $M$  gets two impulses traversing paths of different lengths,  $S-S-M$  and  $S-S-S'-M$ , or  $S'-S'-M$ . These two wave impulses neutralize each other in  $M$  in the same way that light waves of different lengths interfere with each other. So the muscular contraction does not take place. If the will power is used to restrain the muscular contraction, the impulse may come

<sup>1</sup> Exner, Erklärung phy. Erscheinungen, p. 70. Baldwin, Handbook of Psy., Feeling and Will, p. 37. Brunton, *Nature*, March 8, 1883. Goltz, Beiträge z. lehre v. d. Functionen d. Nervencentren d. Frosches, Berlin, 1869.

<sup>2</sup> Baldwin, op. cit. .

<sup>3</sup> Brunton, *Nature*, March 1, 1883. Wundt, Mechanik der Nerven. Medem, Grundzüge einer exakten Physiologie. Kaiser, Eine Hemmungserscheinung am Nervmuskelpreparat. Zeitschrift f. Biologie, XXVIII., 1892.

down the path from the cortical cell  $MB$  in the brain, and cause interference in  $M$ .<sup>1</sup> Wundt admits the possibility of the interference explanation of inhibition by saying that the effect of inhibition takes place when the stimulus waves are so directed

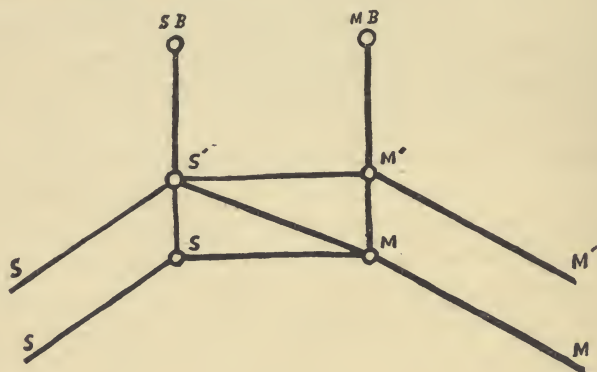


FIG. I.

$S$ . and  $S'$ . Sensory cells in spinal cord.  
 $M$ . and  $M'$ . Motor " " "  
 $S. B.$  Sensory cells in brain.  
 $M. B.$  Motor " " "

that they come together in one sensory center, after having traveled paths of different lengths and with different degrees of speed.<sup>2</sup>

Stimuli applied to a very small part of the skin produce a larger reflex than when a greater area is stimulated. Frogs react less when held in the hand, or tied with their bodies against a board, than when suspended by a string from a single point.

If the sciatic nerve on one side of a frog is irritated by a Faradic current, the reaction to a stimulus on the other side is much less; sometimes it is entirely arrested. These facts have been proposed as evidence for the interference theory.

In favor of the belief that there are cortical inhibitive centers there has been some convincing experimental work done. Set-

<sup>1</sup> Brunton's conclusion is that inhibition is not due to special inhibiting centers, but is a relative condition depending upon the length of the path along which the stimulus has to travel and the rate of transmission.

<sup>2</sup> *Physiol. Psy.*, Vol. I., p. 272.

schenow,<sup>1</sup> as early as 1863, asserted that stimulation of the thalami optici and the corpora quadrigemina of the frog increased the reflex time. This statement met with much opposition. Later, however, Sherrington<sup>2</sup> and Hering<sup>3</sup> produced inhibition of the activity of certain muscles by cortical stimulation. Topolanski<sup>4</sup> extended the work of Sherrington, and proved these results. Sherrington asserts that the cortical area for the inhibition of a set of muscles coincides with the area for the contraction of the antagonistic muscles.

Langley<sup>5</sup> has found special inhibitory nerve fibers in the vagus nerve. Pawlow<sup>6</sup> demonstrated that the closing muscles of the anodonta are supplied with two kinds of nerves, nerves that produce contraction and nerves that produce relaxation. Hering proposes the principle involved in his color theory, assimilation and dissimulation, as a theory of inhibition. Dr. Meltzer<sup>7</sup> believes that the process of inhibition is present in every part of the nervous system. Every stimulus calls forth not only activity, but also an inhibition of this activity. All the phenomena of life, then, are manifestations of the resultant of two antagonistic forces. Inhibition therefore is an important and ever-present function of the nervous organism.

The theory that inhibition takes place in the tissue itself considers all nerve stimuli excitatory, and that the condition of the tissue determines whether activity or arrest of activity is to take place. According to Gaskell,<sup>8</sup> nerve impulses bring about two processes in the tissue: anabolism and catabolism. The so-called inhibitory nerves are trophic in their function. The building up of any tissue corresponds with the arrest of the function of that tissue.

Dr. Lee<sup>9</sup> thinks that the processes of anabolism and catabol-

<sup>1</sup> *Physiologische Studien über Hemmungsnerven*, Berlin.

<sup>2</sup> *Jour. of Physiol.*, Vol. XXII., p. 319.

<sup>3</sup> *Pflüger's Archiv*, Bd. 68, p. 222.

<sup>4</sup> *Centralblatt f. Physiol.*, Bd. XII., p. 592.

<sup>5</sup> *Journal of Physiol.*, Vol. XXIII., p. 407.

<sup>6</sup> *Wie die Muschel ihre Schale öffnet*, *Pflüger's Archiv*, XXXVII.

<sup>7</sup> Inhibition—paper read before the New York Academy of Medicine, Feb., 1899.

<sup>8</sup> *Journal of Physiol.*, Vol. VII.

<sup>9</sup> In a discussion of Dr. Meltzer's paper.



ism take place at the same time and are excited by the same nerve stimulation. If the catabolic process exceeds the anabolic process, there is activity of the tissue; but if the anabolic process exceeds, then there is inhibition, or arrest of activity.

Heidenhain<sup>1</sup> believes that if a nerve cell is in action, an excitation may inhibit it; but if it were at rest, a like excitation would put it in action.

Piotrowski says that there are many physiological facts which support the theory that inhibition depends upon muscular phenomena, and that it takes place in the muscles themselves.<sup>2</sup>

#### PSYCHOLOGICAL INHIBITION.

From the foregoing, it is plain that in physiology the term inhibition stands for a definite physiological function taking place either in the tissue itself or in specific nerve centers and fibers. The question now arises as to whether there is any purely psychological process to which we can apply anything like the same conception of inhibition. What does inhibition in psychology mean? Most psychologists have presupposed that its meaning in psychology is practically the same as in physiology. They begin with illustrations of neural inhibition and end with illustrations of inhibition among ideas. The first is a physiological process confined to certain nerve fibers and tissues. The second is a mere logical concept, or else an account of hypothetical powers and entities. Inhibition of ideas often means the mere logical opposition of ideas. The fact that only one of two contradictory statements can be accepted as true does not result from any process of inhibition. It is not even a psychological fact. Yet the concept of inhibition has been applied to it. In fact, inhibition seems to be a general term for the psychologist. He has used it to signify all kinds of opposition. A brief survey of some of the theories of inhibition will show how broad and indefinite the conception is. The opposition of sensations, feelings and ideas was discussed by psychologists long before the term *inhibition* was imported into psychology. The physician Hippokrates said: "duobus doloribus,

<sup>1</sup> *Archives de Physiol.*, Vol. I., p. 333.

<sup>2</sup> *Journal of Physiol.*, Vol. XIV., 1893.



simul abortis, non in eodem loco, vehementer obscurat alterum." Aristotle held that we cannot perceive two distinct sensations at the same time. The sentient soul is indivisible and can exert only a single energy at once. If two stimuli of diverse intensities operate at the same time, either they will fuse<sup>1</sup> or the stronger will render us insensible to the other. When we are occupied with a loud sound, or in deep reflection, or with intense fright, visual objects are often unnoticed.<sup>2</sup> Concerning the suppression of one pleasure by another, he says that any pleasure which is not akin to the operation which it accompanies tends to weaken and obstruct it. If a person is agreeably employed in reading or study, he cannot, if he is a lover of music, persevere in his activity, if he happens to hear a pleasing melody. For the two pleasures not being akin, the stronger overpowers the weaker.<sup>3</sup>

Spinoza: "Whenever anything increases or diminishes, aids or restrains, the ability of the body to act, the idea of that thing increases or diminishes, aids or restrains the power of the mind to think."<sup>4</sup>

From Spinoza's time down to the time of Herbart there seems to have been nothing added to the conception of the inhibition of ideas.

For Herbart, concepts and ideas are expressions of force. They have an existence independent of presentation. They may attract or repel each other. They may enter into combination either in consciousness or on the threshold of consciousness, which combination may expel from consciousness, or prevent from coming into consciousness, opposing ideas and concepts. A suppressed idea is not destroyed. It continues its existence, waiting for an opportunity to present itself.<sup>5</sup>

This repression and conflict of ideas for which Herbart stands is purely a spiritual process in which ideas exert a direct power upon each other.

<sup>1</sup> Aristotle thought that even when they fuse there is a suppression or opposition, for each stimulus appears with a less intensity in the fusion than when alone.

<sup>2</sup> *De Sensu et Sensili*, VII., p. 447.

<sup>3</sup> *Eth. Nic.*, X., iv., 4.

<sup>4</sup> *Ethics*, III., ii.

<sup>5</sup> *Text-book of Psychology*, 1834.

Volkman follows Herbart very closely. He emphasizes the distinction between the ontological and the psychological aspect of ideas. Inhibition relates only to the latter—*i. e.*, the representation of ideas to consciousness. Inhibition does not imply the non-existence of a once existing idea, but rather its lessened degree of clearness. Ontologically there is no such thing as an inhibited idea. It still strives to get re-presented, but owing to the inhibiting power of ideas already in consciousness it is held at the threshold.<sup>1</sup>

Lotze says: We are wont to regard consciousness as a limited space within which ideas struggle for their places. By a comparison to material forms from whose impenetrability it arises that each one withdraws from the other the space which it itself fills, we smuggle in the conception of the incompatibility of ideas, and, therefore, the pressure which they exert upon each other. But how can relations hold between ideas which make it impossible for certain ones to be known? The obscuration or displacement of ideas is wholly unaffected by the degree of contrast between them in content; nor are the energies by which we conceive opposite concepts opposed in themselves. Concerning the limitations of consciousness, it may be said that we may apprehend many things at the same time if the relations between them are conscious. Only for an unconnected throng has consciousness no room. In place of an abiding contrast between ideas which determines the force with which they repel or attract one another, we would put a degree of affinity which is determined anew at every instant of consciousness. This affinity has for its basis the interest which certain ideas have to the total state of consciousness.<sup>2</sup>

Bain says that if two ideas point in opposite directions they are liable to neutralize one another's efficiency, for since in association ideas assist each other, this implies the power of resisting. Ideas which are not in harmony with existing emotions do not come to consciousness. When several lines of suggestion are open the absence of an additional link in any one of

<sup>1</sup> *Lehrbuch der Psychologie*, Vol. I., pp. 341-375.

<sup>2</sup> *Microcosmus*, Book II., pp. 196-219.

them obstructs its progress. On the other hand, the presence of many existing concepts which have found expression in language forms obstructs original thought. The more modern idea of apperception is expressed in a negative way by Bain's theory of obstructive association: The different ways of looking at the world conflict and obstruct each other. The scientific man cannot see the world as the poet does, because his own particular associations stand in the way. If he thinks at all, he must follow them. On the other hand, the poet is never so brilliant as when the trammels of truth are set aside—when the path is cleared for fancy.<sup>1</sup>

Spencer thinks that certain states of consciousness offer great resistance to each other when an attempt is made to put them together. Ex.: The ideas 'hot' and 'ice' will not be united.<sup>2</sup>

Sully: Doubt arrests action; but a strong impulse to act may overcome the doubt. Action is also arrested when it leads to pain, when the action itself is disagreeable, or when it anticipates a disagreeable result. Rivalry of impulses takes place when one is stimulated to take two positive lines of action which are different. Will enters as a determining factor; but it shows an inhibitive as well as an active side. Inhibition is not inactivity, but rather a positive and definite form of activity. Self-control has an inhibitive aspect when we decide not to do a thing.<sup>3</sup> Sully assumes that the higher cortical centers exert an inhibitory influence on the lower centers.

James Ward: The flow of ideas may be interrupted by intrusion of new presentations, by voluntary interference and by conflict of presentations already in the mind. Presentations of opposites with the same local sign is impossible. Since the attention is limited, new presentations draw it off at the expense of old presentations.<sup>4</sup>

Stout: The process of attention shows an inhibitive aspect in that it is a unified process that excludes everything which does

<sup>1</sup> *The Senses and the Intellect*, p. 597.

<sup>2</sup> *Principles of Psy.*, Vol. 2, *Dynamics of Consciousness*, p. 444.

<sup>3</sup> *The Human Mind*, Vol. II., pp. 248-270.

<sup>4</sup> *Encyc. Brit.*



not belong to the particular system of ideas which holds the attention at any one time. Each mode of the mental process tends to arrest and suppress all others, and is successful in proportion to its intensity and systematic complexity. There is a very compact resistance offered by a highly complex group of processes in systematic union and effective coöperation. When conditions operate to bring about a situation where attention takes disparate directions with equal force the result is mental stupefaction.<sup>1</sup>

Wundt<sup>2</sup> considers inhibition in its psychological aspect as the negative side of the associational process. The direction which association takes necessarily inhibits certain ideas from entering consciousness. Some are free to enter, others are shut out. The strength of inhibition depends upon the intensity of the subject of discourse. When an idea overcomes this inhibitory influence the struggle shows itself in the lengthened reaction time.

Höfdding: The cerebrum stands not only in a positive, but also in a negative relation to the lower centers—*i. e.*, it may inhibit as well as excite activity. It subjects the external impulse to a thorough elaboration, and initiates activities independently and in opposition to the excitation of the moment. The will, therefore, has a negative as well as a positive influence.<sup>3</sup> On the other hand, a motive to action may be suppressed by sensations, ideas and feelings which do not fuse with it.<sup>4</sup>

Ribot: Inhibition shows itself in the attention process. Ideas and impressions are in a state of perpetual progression. When the attention fixes itself upon a single idea it does so by a momentary inhibition of all other ideas and impressions. Inhibition then is the negative side of the conscious process, manifesting itself in suppressing the useless conscious states.<sup>5</sup>

Binet: Negation of one representation by another involves two

<sup>1</sup> Analytic Psychology, Vol. I., p. 194. For Stout's theory of Negative Apperception, Destructive Apperception, Conflict of Mental Systems, see Vol. II., pp. 3, 14c.

<sup>2</sup> *Physiol. Psy.*, Vol. II., pp. 481, 506.

<sup>3</sup> *Outlines of Psy.*, Eng. tr., p. 43.

<sup>4</sup> *Ibid.*, p. 335.

<sup>5</sup> *Psy. of Attention*, p. 64.

positive representations which are contradictory; *c. g.*, "There are no books upon the table" means that first one thinks of the table *with* books upon it, and then of the table *with no* books upon it. The one representation is inhibited by the other. Fixed ideas inhibit other mental imagery. In illusions the false idea inhibits the true. In general, mental states which do not fall into the same system inhibit each other.<sup>1</sup>

Baldwin: The clearest and most important kind of mental inhibition is that exercised by the will. It is possible by direct force of will to prevent a nervous reaction or a train of thoughts. Inhibition is at its maximum in reactions which involve centers of most complex activity; or, in other terms, it is possible in proportion as the system grows away from a single line of action. The more possibility for diverse action the more inhibition is manifest. Conscious life is a complex of conflicts, repressions and reinforcements among images almost as great as the systems of tensions found in the nervous system. The image of a winged horse is inhibited by that of a real horse, or a fixed idea may exert an inhibitive force over the whole field of consciousness.<sup>2</sup>

James: Any mental process once begun would continue to the point of exhaustion if not inhibited by other processes. This inhibition is to be explained upon psychophysical grounds, and consists in a drainage of the sensory impulses away from the active centers corresponding to the mental process. This drainage brings about a lessened or suspended activity of that special process. One mental process then can inhibit another by appropriating its nervous energy. On the other hand, interference of any mental process by irrelevant sensory stimuli pressing in upon its centers may be inhibited by a drainage of those sensory currents downward into some motor path which is kept open by muscular activity, such as twirling a pencil or fumbling with a watch chain during deep thought. Thus, inhibition is an essential element in cerebral life, which, on the one hand, makes possible the entrance upon the stage of consciousness of only one set of sensations at a time, and so preventing a hopeless jumble;

<sup>1</sup> L'inhibition dans les phénomènes de conscience, *Revue Phil.*, Aug., 1890.

<sup>2</sup> Handbook of Psy., Feelings and Will, pp. 36, 70.

and, on the other hand, provides for the arrest of any mental activity when it has served its purpose.<sup>1</sup>

Ladd: The field of consciousness at any one moment is limited. The selection of objects for this field implies the partial or total exclusion of other objects. The result of this limitation taken in connection with the unity of consciousness is a conflict and inhibition of ideas. But this conception should not be carried into the realm of the unconscious. To determine the inhibitive value and efficiency of ideas not in consciousness is impossible. Why some ideas triumph we do not know. In any complex mental state that is chiefly characterized by ideation the principles both of fusion and of inhibition combine to produce the result. Some tendencies take leading and predominating parts, others get relatively suppressed. The processes of fusion and inhibition go on simultaneously. What tendencies will prevail and what not, may be said to involve the entire past history of the psychical life.<sup>2</sup>

The different views of psychological inhibition expressed in the last few pages may in a general way be classified into five conceptions. The first four consider inhibition as a process entirely psychical.

1. Inhibition as an expression of the power which ideas, as such, exert upon each other: This view looks upon ideas as permanently existing entities persisting independently of their presentation to consciousness, and possessing the power of attracting and repelling each other directly. Inhibition is then the process by virtue of which some ideas are prevented from being presented to the soul because of the repellent force of other ideas either in or upon the threshold of consciousness.

2. Inhibition as obstructive association: Inhibition is the negative side of the associational process. The number of mental elements which can enter the field of consciousness at any one time is limited. What elements do enter is determined by the laws of association. The elements which do not enter are inhibited because the mind is already filled. Another way

<sup>1</sup> *Psy.*, Vol. II., pp. 581-585.

<sup>2</sup> *Psy.*, Descriptive and Explanatory, pp. 252-259.



of putting it is, that certain ideas are of such a nature that they do not fit into the chain of association as well as other ideas, and consequently are inhibited by their own nature.

It is practically the same conception to consider the attention process as the effective factor in determining what shall enter consciousness, and so negatively what shall remain out of consciousness. In either case the point to note is that inhibition is but a name. In fact, according to the above conception, there is no active process for which it stands. It is merely an attitude of the mind taken in considering the active processes of attention and association.

3. Inhibition as logical contradiction: A statement cannot be thought as being both false and true at the same time. This conception, of course, cannot be considered as adequate, nor, indeed, properly one of inhibition.

4. Inhibition as a mode of the will's activity: The will not only excites and directs mental activity, but it checks and controls it. This function is seen most frequently in checking and controlling the movements of the body.

5. Inhibition as a psychophysical phenomenon: This view considers the physiological side of consciousness—*i. e.*, cerebral activity. The activity of nervous energy in certain centers and paths inhibits its use in other centers and paths, and consequently the mental state appropriate to the activity of those centers. The drawing off of nerve currents from certain centers inhibits the continued activity of those centers. Of course, such a conception might be nothing more than the statement of any one of the first four views in coördinate physiological terms.

It is not intended that any psychologist should be set down as holding any one of these conceptions exclusively. But these are the limits within which the conception of inhibition has been applied by psychologists. This conception cannot be said to have any definite or well-defined meaning. Sometimes it is used to designate a purely hypothetical process conceived as taking place between the world of existence and the world of non-existence—*i. e.*, between conscious and so-called unconscious ideas. Sometimes it is a direct fiat of the will. Sometimes it is the mere condition of not being called into existence,

as is the case in association. Again, it may be the mere logical contradiction of two statements. Inhibition is a term which has been used to designate all kinds of mental conflict, hesitation and arrest.

So in answer to the question concerning the use of the term in psychology, it seems evident that it does not have the same definite and accepted meaning that is given it in physiology. In this latter science it is now recognized as a definite function of certain nerve fibers. In psychology, on the other hand, it is yet a very general and much used term, including in it the ideas of cessation, opposition and conflict. Logical, and even ontological, conceptions have been mixed up in the views of inhibition. From the nature of the objects of physiology and the objects of psychology we could not expect a paralleled application of the term in the latter science. Physiology deals with material objects wherein physical laws hold. Impenetrability, force and repulsion may very properly be regarded as characteristics of matter; but in what way can they be regarded as properties of ideas, or mental states? This question may be regarded as settled by the general discredit into which those "hideously fabulous performances of the Herbartian *Vorstellungen*" have fallen. Inhibition, as a process, implies impenetrability, force and consequent repulsion, and, therefore, has definite meaning in psychology only as we approach physiology—*i. e.*, enter the field of psychophysiology. This will be considered more fully later.

There is another consideration which makes against the conception of inhibition in purely spiritual terms. It comes out of the nature of the existence of ideas. The existence of an idea depends upon its presentation. There is no such thing as an unrepresented idea. Whatever else it may be before presentation, it is not an idea; nor can it be said to be an idea after it has passed from consciousness. Ideas are not entities which have existence outside of the conscious field; they do not persist like material bodies. The elements of mental states are constantly changing, so that we may say that an idea is different at every instant of its existence. In this sense we may hold that every idea perishes at its presentation. Now to say that an idea is



held out of consciousness by ideas in consciousness can have no meaning; for there *are no ideas out of consciousness*. That which is out of consciousness is not an idea, and as such is neither affected nor exerts any influence.<sup>1</sup> Inhibition of ideas by other ideas, then, is a pure abstraction. This consideration holds equally against the view of inhibition which makes it the negative side of association, in so far as this latter process rests upon the assumption that ideas as such affect ideas not yet presented.<sup>2</sup> Even granting the existence of unrepresented ideas, it would be a difficult task to explain how ideas *in* consciousness can exert a repellent force upon ideas *outside* of consciousness.

Because, obeying the laws of association, the train of conscious ideas takes one direction rather than another, this can hardly be considered sufficient ground to hold that other *possible* trains of ideas are inhibited. The world of existence cannot be supposed to exert an inhibiting influence upon a world of non-existence. If a child is born a girl, does that fact inhibit its being a boy? But just such conceptions of inhibition have been used by psychologists. James<sup>3</sup> speaks of the 'Law of the Inhibition of Instinct by Habit,' and gives as an illustration of this law the inhibiting influence which the selection and building of a nest have upon the bird's instinct to build nests. Stated more generally: After reacting upon one specimen of a class other specimens do not call forth the reaction. This is an unwarranted use of the term. It is equal to saying that the impulse to build nests is still active after the nest is built, but that it is held in check by a restraining influence. This, however, is not true. The impulse to build the nest is not inhibited; on the other hand, it runs its full course in an unrestrained manner, normally. There is no inhibition. The impulse ceases because *it is not inhibited*, because it has fulfilled its mission undisturbed. *It is an impulse to build one nest, not an impulse to build nests continually*. Animal instincts of this nature are impulses called forth under certain conditions; the conditions

<sup>1</sup> This, of course, does not deny the hypothesis of physical dispositions.

<sup>2</sup> To those who say that this is a mere matter of phraseology, I can suggest only that the use of the term *idea* in two senses leads to confusion. A more careful use of terms is advisable.

<sup>3</sup> Psy., Vol. II., p. 394.

being fulfilled by the realization of the impulses, then they no longer exist. If this were not so, what a smouldering mass of struggling impulses the ordinary animal would be, and what an amount of energy would be consumed in inhibition!

The conception of inhibition which considers certain ideas as opposed in content and, therefore, as excluding each other from the field of consciousness, may mean nothing more than logical opposition. Properly, there is no inhibition involved in this conception; on the other hand, the opposition depends upon the possibility of thinking ideas together, which brings out the relation of opposition. Only when ideas are presented at the same time can opposition exist. Psychologically, opposition in the content of states of consciousness does not bring about suppression; on the other hand, we regard it as one of the laws of association by virtue of which ideas are helped into existence. Opposition in the content of ideas is no more a principle of inhibition than likeness of content. The principle of contradiction is also a logical principle, and is not applicable here.

The inhibition which the will exercises seems to be most effective in the field of bodily movements, especially voluntary acts. Any voluntary movement may be arrested immediately by the will. But within the field of consciousness it does not have an immediate and direct effect. If one attempts to thrust out of consciousness an idea, or an emotion, the attempt serves only to intensify it. The more direct the effort the clearer will the idea become, and the more persistently does it remain. The will is successful in inhibiting mental states only when working through the motor adjustments of the body. If we wish to banish certain thoughts from our mind, we can do so only indirectly by inhibiting, the bodily adjustments which accompany such thoughts. A change of bodily activity tends to bring about a change of mental states.

After this hasty survey of the different conceptions of inhibition I am satisfied that its use in psychology should be confined to psychophysical phenomena. Its use in purely mental terms is either meaningless, or else involves the hypothetical creations of the Herbartian psychology. Almost universally the instances

of inhibition cited by the foregoing psychologists involve definite bodily activities, either within the field of sense perception or bodily movements. These instances fall under the following classes :

1. Inhibition of one sensation by another : A faint sound is inhibited by a loud sound ; a slight pain by a greater pain.

2. Inhibition of bodily movements by sensation : A sudden sight or sound may inhibit movements of walking, breathing or the action of the heart. Pain may inhibit the movements which cause it.

3. Motor activity may inhibit mental states: Activity in battle may inhibit fear. Motor activity inhibits the feelings of embarrassment. If, when trying to remember a name, some other name very similar is pronounced the first name is inhibited.

4. Emotions may inhibit bodily functions: Shame inhibits the action of the vasomotor muscles. Great dread inhibits the flow of saliva. Great grief inhibits the flow of the blood to the brain.

5. Will may inhibit the voluntary and half-voluntary movements of the body, and, to a certain extent, the involuntary muscles. Some people are able to decrease the activity of the heart at will.

It will be seen that in all the above cases of inhibition there is involved, as has already been stated, distinct and definite physiological activities which are closely connected with mental states. It is within this field of psychophysiology that the conception of inhibition has meaning from the psychological point of view, and inhibition in so far as it concerns psychology must be described and explained in terms of the psychophysical mechanism.

Experimentally I have investigated two phases of inhibition within this field :

(1) Inhibition of one sensation by another.

(2) Inhibition of mental states through suppression of their motor elements.

The next two sections are given to these questions.



## BINOCULAR RIVALRY.

If corresponding points of the two retinae are separately stimulated with two incongruous fields—*i. e.*, fields of sufficient difference to prevent their interpretation as a single field—the phenomenon of binocular rivalry appears.<sup>1</sup> For a time one field presses itself into consciousness, then the other takes its place. In this manner a continual shifting of the fields takes place. If a green glass is held before one eye and a red glass before the other, and the eyes turned toward the sky, the struggle of colors is very readily seen. A simple way of getting rivalry between the two eyes is to close both eyes, cover one of them with the hand and turn the face toward the bright sky. The dark and the light fields will be seen alternately.

The stereoscope affords a convenient means of separating the two fields. If a simple drawing of two perpendicular parallel lines is so placed in the stereoscope that it can be seen by the right eye only, and another drawing of two horizontal parallel lines is placed so that it can be seen by the left eye alone, the cross which results shows at the point of intersection a rivalry of the two sets of lines. For an instant they will appear as in *a*, Fig. 2. The next instant they will appear as in *b*, same figure. The fluctuations will continue indefinitely.

Several explanations of this phenomenon have been offered. Helmholtz was the first investigator who studied binocular rivalry carefully. He concluded that the change of fields is due to a change of attention—*i. e.*, the rivalry is psychical, and not physical. If the attention is held upon one of the fields, it will remain in consciousness to the exclusion of the other field.<sup>2</sup>

<sup>1</sup> Wundt, *Physiol. Psy.*, Vol. II., p. 211. Lectures, tr., pp. 195-210. Hermann, *Handbuch d. Physiol.*, p. 380. Helmholtz, *Optik*, p. 918. Hering, *Beiträge z. Physiol.*, p. 313. Exner, *Entwurf z. e. Physiol. Erk. Psy. Er.*, p. 72. Fechner, *Ueber einige Verhältnisse d. binocularen Sehens*, *Abhl. d. k. Sächs. Ges. d. Wiss.*, VII., p. 378. Panum, *Physiol. Untersuchungen über das Sehen mit zwei Augen*, Du Bois, Réymond's *Archiv*, 1867, p. 63. Woinow, *Ueber den Wettstreit d. Sehfelder*, *Arch. f. Ophthal.*, XVI., p. 194. Schön, *Zum Wettstreit d. Sehfelder*, *Klin. Monatsbl. f. Augenheilkdz.*, XIII., p. 356. Axenfeld, *Un phénomène de contraste binoculaire*, *Arch. Ital. de Biol.*, XII. Chauveau, *Sur la théorie de l'antagonisme des champs visuel*, *Compt. Rend.*, Vol. 13, p. 439.

<sup>2</sup> *Optik.*, p. 920.

He further states that the rivalry does not depend upon any organic structure or condition of the nervous system,<sup>1</sup> but upon mental conditions. The fact that rivalry does take place is explained by the fact that the attention is ever seeking something

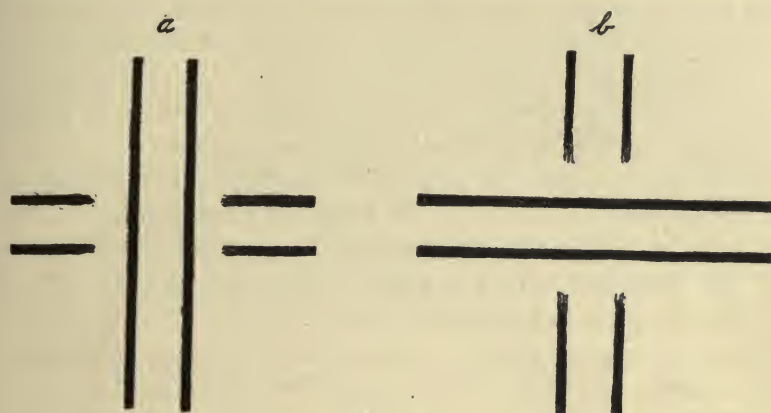


FIG. 2.

new. It does not ordinarily maintain itself in a state of rest, but is constantly changing. If we wish to hold any object in consciousness, we may do so by constantly finding new elements or aspects in it for consciousness. Accordingly, Helmholtz believes that the rivalry between the two retinæ can be controlled at will—*i. e.*, if the attention is held upon one of the two fields by force of will power, that field will remain in consciousness.

Hering<sup>2</sup> and Panum<sup>3</sup> consider that binocular rivalry is due *purely* to physiological conditions. Fechner<sup>4</sup> opposes the attention theory of Helmholtz, and Woinow<sup>5</sup> seems to agree completely with Helmholtz.

Chauveau<sup>6</sup> thinks that the phenomenon of binocular rivalry is central, and not peripheral, as Panum suggests. He proposes the hypothesis, as the physiological basis of the rivalry, that the central cells for corresponding points in the

<sup>1</sup> Ibid.

<sup>2</sup> Zum Lehre von Lichtsinne, pp. 380-385.

<sup>3</sup> *Op. cit.*

<sup>4</sup> *Op. cit.*

<sup>5</sup> *Archiv f. Ophthalm.*, Vol. XVI., pp. 194-199.

<sup>6</sup> *Op. cit.*

retinæ are connected with a single optical center, from which visual center the perception of sight arises. When corresponding points of the retinæ are stimulated by the same or like objects the corresponding central cells give the same report to the optical center, and hence there is no conflict; but when corresponding points are stimulated by different objects, then the reports which the single optical center receives conflict and perception is interfered with. In favor of this theory is the fact that parts of the field which fall upon adjacent points of the retinæ do not show rivalry, but are fixed in consciousness, showing that the retina itself does not possess any noticeable functional rhythm similar to the rhythm of binocular rivalry.

St. Witasek<sup>1</sup> reports that he was able, after practice with the stereoscope, to prevent binocular rivalry of different contours. He used the Zöllner figure, the parallel vertical lines for one eye and oblique lines for the other. He was attempting to get the illusion of the Zöllner figure when its parts were combined by means of the stereoscope. At first the illusion did not appear, because, owing to the rivalry, the lines did not appear simultaneously. After continued attempts he claims that he was able to prevent the rivalry, so that both sets of lines were present at the same time, making the completed figure. This prevention of the rivalry is *very remarkable*. No one has reported such a result before. Neither Helmholtz nor Woinow found that they were able to hold *both* fields in their experiments. They believed that by controlling the attention and fixing it upon one field they could exclude the other; but to hold two different fields in consciousness at the same time, under these conditions, is quite another thing. I have been working two years with the stereoscopic combination of different fields, and have been unable to discover any tendency of the kind mentioned. One of my problems during these two years has been just this attempt to control the rivalry, but I have gained absolutely nothing in my ability to do so; nor have any of my subjects been able to prevent the rivalry. The experiments bearing upon this point will be given later.

The object of my experiments was not so much to find an ex-

<sup>1</sup> *Zeitschrift f. Psy.*, Dec. 20, 1898.



planation for binocular rivalry, as to determine what conditions, both subjective and objective, affect it, and to what extent the phenomenon throws light upon the general problem of inhibition.

#### APPARATUS.

The stimuli for the retinae were red and green squares one centimeter in size. For the greater part of the experiments these two squares were crossed by five black diagonal lines. The lines upon the red field ran from the upper left-hand corner to the lower right-hand corner, while those upon the green field ran from the upper right-hand corner to the lower left-hand corner, so that when the fields were combined in the stereoscope the lines crossed each other at right angles. These fields were placed upon a black cardboard in proper positions for the stereoscope :

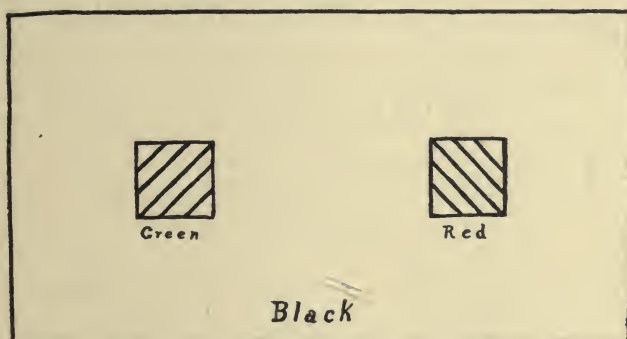


FIG. 3.

When this cardboard was placed in the stereoscope the squares fell upon corresponding points of the retinae. Red and green backgrounds for the lines were employed merely to aid the subject in determining when the changes in the fields came. If the lines were upon like backgrounds, perception of the exact time the changes occur was more difficult. The colored backgrounds changed in every case with the lines—*i. e.*, the right diagonal lines were always seen upon the red background and the left diagonal lines upon the green. The form of the lines was varied as the conditions of the experiments required. Red and green borders of different widths were pre-

pared, so that the colored backgrounds could be enlarged without extending the lines. These borders were placed around the fields. The light intensities were varied by means of shades and screens.

By means of a second's pendulum a moving field either for both or for one eye was arranged. This was done by fastening cardboard of the required color to the pendulum. This cardboard had upon it the black lines of the first fields, and was of such size that when the pendulum was set in motion a moving colored field with the lines upon it could be seen for all positions of the pendulum, through an opening in a screen. The card for the stereoscope had two square openings, one centimeter in size, cut out of it at the points where the red and green squares of the figure on page 21 are situated. Now if the stereoscope with this card in position were placed in front of the moving cardboard, each eye would be stimulated by a moving colored field, the movement, however, being restricted to the opening in the cardboard in the stereoscope. The rate of movement was regulated by the amplitude of the pendulum. It was an easy matter to arrange a moving field for one eye while the field for the other eye remained stationary.

These arrangements so far admitted only of the use of reflected light. It was found convenient to use transmitted light in some of the experiments. In order to do this the two red and green fields were made of gelatin films of those colors. Lines were made upon them by pasting strips of black paper across them. These lines were the same in number and direction as upon the fields already described. The films were placed in the openings, one centimeter square, of a black cardboard of proper dimensions for the stereoscope. Electric lights, arc and incandescent, placed behind these films gave the various stimuli.

In order to register the length of time each field remained in consciousness a kymograph carrying three writing pens was set up. Two of the pens were connected with two electric keys which were manipulated by the subject; the third pen was connected with a clock and wrote seconds. The kymograph was driven by a small electric motor at a uniform speed of one centimeter per second. The time line upon the records was a check



upon the rate of speed. The records were taken upon continuous roll paper. The subject did the recording by pressing the right key when he saw the right field and the left key when he saw the left field.

## EXPERIMENTS.<sup>1</sup>

### SEC. I. THE EFFECT OF WILL UPON RIVALRY.

The question of how far will power is able to control the rivalry of the two retinæ is the subject of this section. The stimuli were the right diagonal lines for the right eye and the left diagonal lines for the left eye. The right diagonal lines in all cases were upon the red background, and the left diagonal lines were upon the green.<sup>2</sup> At each sitting three records were taken: a normal record, a record showing the effect when the subject tried to hold the red field by will power and a record showing the effect when an attempt was made to hold the green field. During the normal record the subject allowed the change of fields to take place without attempting to influence it in any way. In the next two records an attempt was made to hold the fields by fixing the attention upon one of them and by dint of will power see it alone, to the exclusion of the other field. Each record was one hundred seconds long, or the length of time nearest one hundred seconds in which there were an equal number of changes for each of the two fields. The following table gives the results. The second and fourth columns give figures in percents of the whole time of each experiment; *e. g.*, subject 1 normally held the red field 51% of the time, but when he tried to hold it he could do so 70% of the time.

Every subject expressed the opinion at the beginning of the experiment that he could control the fluctuations of the fields. None of them had had any experience with the phenomenon of binocular rivalry.

<sup>1</sup> These experiments were begun in the Harvard laboratory during the year 1897-98, but the major part of the work here reported was done in the Columbia laboratory.

<sup>2</sup> Hereafter these fields will be designated by their colors, *viz.*, red field—green field. The rivalry, however, was between the two sets of lines.

TABLE I.

Subjects.	NORMAL.		WILL.	
	Per cent. of time seen.	No. of changes.	Per cent. of time seen.	No. of changes.
RED FIELD—RIGHT EYE.				
1	51	33	70	23
2	61	31	76	27
3	53	18	66	17
4	58	18	64	19
5	45	30	61	36
6	52	16	69	16
7	49	57	59	61
8	57	38	73	48
9	49	18	63	24
Av.	53	259	68	280
GREEN FIELD—LEFT EYE.				
1	49	33	68	40
2	39	31	74	34
3	47	19	50	20
4	42	18	46	17
5	55	30	70	36
6	48	16	56	19
7	51	58	64	58
8	43	39	77	53
9	51	19	58	25
Av.	47	263	63½	302

The averages for the red and green fields under normal conditions for six of the above subjects is given in Table II. These averages are from ten normal records taken at different times during the year.<sup>1</sup> All figures are expressed in per cents.

TABLE II.

Subjects.	RED.			GREEN.		
	Av. for 10 Normals.	Av. V.	Max. V.	Av. for 10 Normals.	Av. V.	Max. V.
1	52	2.	4.8	48	2.7	4.4
2	59	4.3	7.2	41	4.2	10.
3	53	1.8	3.1	47	2.1	3.7
4	59	3.	7.3	41	3.	7.4
7	50	1.5	3.5	50	1.	1.9
9	49	2.8	4.	51	1.8	2.5

<sup>1</sup> Each normal was one hundred seconds long.

From Table II. it will be seen that under normal conditions the length of time either field remains in consciousness is fairly constant for each subject.

From Table I. we get the following results :

(1) Each subject was able to increase the length of time a field was seen by fixing the attention upon it.

(2) But the number of fluctuations in the rivalry could not be controlled. The number of changes showed a tendency to increase under effort.

The question of eye movement demands consideration in this experiment. It was noted that during the normal records there was very little, if any, eye movement. At least we can say that it was at a minimum. But whenever any effort was made to hold one of the fields observation revealed the fact that the muscles of the eyes were very active. If it were the red field that the subject was attempting to hold, when it appeared the eyes were constantly moving from one point of the field to another; when it disappeared, and the green field took its place, the muscles of the eyes seemed to relax: for the activity which was so apparent when the red field was in consciousness disappeared with the field; when it appeared again the movement of the eyes was resumed. Each appearance of the red field was accompanied by activity of the eye muscles, while the appearance of the green field was accompanied by inactivity. When the subject attempted to hold the green field, then, the activity accompanied its appearance, and the inactivity the appearance of the other field. This movement of the eyes brought about a change of stimuli, owing to the nature of the fields, as well as an increase in the area of retinal stimulation.

Most of the subjects were not aware of these movements until their attention was called to them. If they were asked to eliminate them, the sense of will effort disappeared. The movements seemed to be a necessary element in the activity of the will power—*i. e.*, if the subject did not make these movements, it did not seem to him that he was trying to hold the field. With naïve subjects it was impossible to eliminate the movements. These movements would invariably appear whenever they were asked to hold one of the fields by force of will. In my own



case I was able, after some practice, to hold the attention upon one of the fields without making the eye movements.<sup>1</sup>

The preceding experiments were then made upon myself under the same conditions, except that so far as possible eye movements were eliminated. The eyes were fixed upon the center of the field and held there. Each experiment, as before, was one hundred seconds long.

TABLE III.

Experiments.	NORMAL.		WILL.	
	Per cent. of time seen.	No. of changes.	Per cent. of time seen.	No. of changes.
RED FIELD—RIGHT EYE.				
1	53	17	51	15
2	55	24	50	22
3	54	24	54	23
REVERSE—RED FIELD—LEFT EYE.				
4	47	19	48	16
5	44	17	53	17
GREEN FIELD—LEFT EYE.				
1	—	—	—	—
2	45	24	47	20
3	46	24	46	23
REVERSE—GREEN FIELD—RIGHT EYE.				
4	53	19	56	16
5	56	17	56	21

Table IV. gives the results of twenty normal experiments made upon myself.

TABLE IV.

	Per cent. of time seen.	Av. V.	Max. V.	Av. No. of changes for 100 sec.
Red.	49.7	2.6	8.	24.
Green.	50.3	2.6	8.	24.

The variations are expressed in per cents.

<sup>1</sup> Movements which are perceptible.

Table III. shows that elimination of the eye movements from the experiment takes away the power which the will seemed to exercise over the length of time either of the fields can be held in consciousness. It should be stated that I acted as subject myself in the first experiments, and obtained the same results as those of the other subjects.

It should be noted also that these results show no exception. All point uniformly in the same direction. The lengthened time, then, which seemed to be due to the will in the first experiments was due rather to eye movements. Now if this be true, re-introduction of the movements ought to give the same results as those in the first experiments. This I have attempted to do in the next experiment. In all the following experiments, except when otherwise stated, the results are taken from my own records.

They have been checked, when possible, by results from other subjects, and they have agreed with them in every case.

Sec. 2. *Eye Movements*.—The conditions of this experiment were the same as in the experiments of Section 1, with the exception that instead of attempting to hold the fields by will power, conscious eye movements were introduced. First, movements in the direction of the lines of the red field—*i. e.*, the middle line was fixated and the eyes moved back and forth along this line. Second, movements in the direction of the lines of the green field.

TABLE V.

Experiments.	Red, per cent. of time seen.	Green, per cent. of time seen.	No. of changes for each field.
MOVEMENT—RED FIELD.			
1	79	21	18
2	77	23	18
3	73	27	22
MOVEMENT—GREEN FIELD.			
4	40	60	22
5	40	60	19
6	43	57	20

For normals and variations, see Table IV., Sec. 1.



Another form of eye movement is introduced, if during the experiment the lines upon the fields are counted back and forth. This not only calls for movement of the eyeballs, but brings into activity the apparatus for accommodation as well, in that the counting of each line necessitates fixating it. The eye movement in this experiment is in the opposite direction to the corresponding movement in the preceding experiment. There it was along the length of the lines; here it is at right angles to the lines. In four cases the fields were reversed—*i. e.*, the red field was placed before the left eye, and the green before the right eye. These cases are numbered in the table 4, 5, 9 and 10.













TABLE VI.

Experiments.	Red, per cent. of time seen.	Green, per cent. of time seen.	No. of changes for each field.	
COUNTED LINES ON RED FIELD.				
1	68	32	21	} Reversed fields.
2	67	33	18	
3	69	31	16	
4	75	25	10	
5	73	27	16	
COUNTED LINES ON GREEN FIELD.				
6	35	65	17	} Reversed fields.
7	40	60	15	
8	45	55	17	
9	31	69	23	
10	37	63	20	

The number of changes in the normal records preceding each of the above was 22 per 100 seconds.

Sec. 3. *Figures.*—In Section 2 all the eye movements were made by conscious effort. In Section 3 no conscious eye movements were made, but such variations of the lines upon the colored squares were employed as would induce more eye movement in one case than in another. In some cases there were no lines at all upon one of the fields. In the first three series the red field was without lines, while the green field retained them. The result might be expected that the red field would induce very few movements, while the green field would have the opposite effect. In the next three series lines were

TABLE VII.

Experiments.	Red, per cent. of time seen.	Green, per cent. of time seen.	No. of changes for each field.
	 RED.	 GREEN.	
1	30	70	19
2	32	68	27
3	27	73	18
4	28	72	23
	 RED.	 GREEN.	
5	21	79	21
6	36	64	20
7	27	73	21
8	30	70	21
	 RED.	 GREEN.	
9	34	66	19
10	23	77	22
11	24	76	20
	 RED.	 GREEN.	
12	43	57	25
13	42	58	25
14	45	55	33
15	50	50	27
	 RED.	 GREEN.	
16	57	43	22
17	59	41	29
18	52	48	21
19	52	48	24
	 RED.	 GREEN.	
20	39	61	26
21	40	60	31
22	53	47	25
23	49	51	27

In experiments marked 3, 4, 7, 8, 10, 11, 14, 15, 18, 19, 22 and 23, the fields were reversed.

placed upon both fields. This might be expected to equalize the eye movements.

Inspection of Table VII. will show that those fields which would be expected to induce the greatest amount of eye movement remained longest in consciousness; but in cases where both fields contained lines the rivalry tended to approach a normal condition.

Sec. 4. *Moving Field*.—What would be the result if the movement were transferred to the external stimulus while the eyes were kept at rest as far as possible? Section 4 deals with the problem. The two fields were the same as those in Experiment 1—a red and a green field with diagonal lines; the red for the right, and the green for the left eye. The red field was kept in motion by means of the pendulum already described. The green field remained at rest. The areas of the two fields were restricted to one square centimeter each by a black cardboard in which had been cut square openings of that size. This card was placed in the stereoscope which stood before the swinging red field and the stationary green field in such a way as to give the required stimulus. Other than the moving field the conditions were the same as in the normal records.

TABLE VIII.

Experiments.	RED FIELD MOVING.		
	Red, per cent. of time seen without the green.	Green, per cent. of time seen.	No. of changes for each field.
1	59	41	25
2	53	47	24
3	60	40	21
4	57	43	17 <sup>1</sup>
5	53	47	15 <sup>1</sup>

<sup>1</sup>The number of changes in the normal records for this day was only 16 per 100 seconds. The day was cloudy and consequently quite dark. The influence which intensity of light has upon the changes will be discussed in a later section.

In all the preceding experiments the displacement of one field by the other had been complete, but in this experiment the results were exceptional. The moving lines of the red field were seen practically all the time. The stationary green field came and went with its usual regularity. The length of time it re-



mained, however, was somewhat shorter than under normal conditions. While the green field was present the lines of the red field were still seen moving back and forth, seemingly *through* the lines of the green field. This consciousness of the lines was not clear and distinct when the green field was present; at times it would reach the vanishing point. Then, when the green field disappeared, it would again return to its maximum degree of clearness. The figures in the second column express only the per cent. of time the red field was seen *alone*. The third column might be said to express the per cent. of time that the green field was seen *in spite* of the moving lines of the red field.

Sec. 5. *Muscle Contraction*.—The usual red and green fields with the black lines were used in this experiment. The new condition introduced was that of muscle contraction: First, contraction of the muscles of the arm and leg of the right side of the body. Second, contraction of the arm and leg of the left side of the body. This contraction was flexion of the arm and leg. The position was held rigidly during the experiment. As much strength as the subject could command was expended in the contraction. In the first series the muscles of the right side of the body were contracted, while the muscles of the other side were relaxed as far as possible. In the second series the opposite condition existed. As is the case in every experiment reported, a normal record was taken as a preliminary to every series. Table IX. contains the results from six subjects.

TABLE IX.

Subjects.	NORMAL.			MUSCULAR EFFORT, RIGHT SIDE.			MUSCULAR EFFORT, LEFT SIDE.		
	Per cent. of time seen.		No. of changes for each field.	Per cent. of time seen.		No. of changes for each field.	Per cent. of time seen.		No. of changes for each field.
	Red.	Green.		Red.	Green.		Red.	Green.	
1	50	50	41	66	34	35	50	50	40
2	51	49	67	46	54	55	48	52	53
3	52	48	14	46	54	18	49	51	19
4	55	45	25	60	40	22	52	48	23
5	51	49	15	47	53	20	49	51	27
6	57	43	25	59	41	21	57	43	24

Red field—right eye. Green field—left eye.



Table X. gives the averages of ten normal records and the variations of the six different subjects. All figures are in per cents.

TABLE X.

Subjects.	AVERAGE FOR TEN NORMALS.				
	Red.	Green.	Av. V. for red.	Av. V. for green.	Max. V.
1	59	41	4.3	4.2	10.
2	50	50	1.5	1.	4.
3	49	51	2.8	1.8	4.
4	58	42	5.	8.	10.
5	53	47	1.8	2.1	4.
6	59	41	3.	3.3	7.

A comparison of the figures in these tables gives no data for any definite conclusions. In some there is indication of disturbance of the normal rivalry, but there is no uniformity in its effect. In order to check these results the experiment was repeated upon myself with great care. The results are given in Table XI.

TABLE XI.

Experiments.	NORMAL.			MUSCULAR EFFORT, RIGHT SIDE.			MUSCULAR EFFORT, LEFT SIDE.		
	Per cent. of time seen.		No. of ch'ng's for each field.	Per cent. of time seen.		No. of changes for each field.	Per cent. of time seen.		No. of changes for each field.
	Red.	Green.		Red.	Green.		Red.	Green.	
1	48	52	24	52	48	21	59	41	20
2	52	48	26	57	43	17	59	41	19
3				56	44	15	62	38	19
4				59	41	15	58	42	17
5 <sup>1</sup>	55	45	27	55	45	23	53	47	17

Red field—right eye. Green field—left eye.

The effect seems to be the same whether the muscles of the right side or of the left side are contracted—*i. e.*, an increased length of time for the red field. The number of changes was decreased during muscular activity.

<sup>1</sup> In this case the fields were reversed.

<sup>2</sup> Averages of normals and their variations are to be found in Table IV., Sec. I.

If instead of the total time, we consider the length of each individual change or phase of the rivalry in the last experiment, we get the following averages :

Normal. <sup>1</sup>	Av. length of time of each change.
Red <sup>2</sup> field—50 changes,	2.0 seconds.
Green <sup>3</sup> field—50 changes,	2.0 seconds.
Right side contracted.	
Red field—68 changes,	3.1 seconds.
Green field—68 changes,	2.5 seconds.
Left side contracted.	
Red field—75 changes,	3.1 seconds.
Green field—75 changes,	2.1 seconds.
Average variation of normal records.	
Red field,	.46 second.
Green field,	.50 second.

Whatever may be the relation between the motor centers for the two halves of the body and the visual centers for the two retinae, there was no corresponding change in the functioning of the latter to agree with the change in the activity of the muscular apparatus. Under both conditions of activity the result was the same. The field seen by the right eye, the red field, remained longer in consciousness without a corresponding decrease in the length of time the other field was seen. The rhythm of the rivalry was somewhat slower under these conditions.

Sec. 6. *Colored Borders*.—Borders of the same color as the red and green squares were placed about them in order to determine what effect they would have upon the rivalry. Borders of different widths were employed, and four series of experiments made : 1st. A red border around the red field, and no border except the black background for the green. 2d. A

<sup>1</sup> The average of 1,200 changes and their variations will be found in Table XIII.

<sup>2</sup> Right eye.

<sup>3</sup> Left eye.

green border around the red field, and no border for the green field. 3d. A red border around the green field, and no border for the red field. 4th. A green border for the green field, and no border for the red field. In each case one of the eyes received only the impression from the usual stimulus, while the other eye received the added stimulus of the colored border, in the first case of the same color, in the next of the opposite color. Five subjects took part in the experiments, and altogether about two thousand changes were recorded. The results show that these borders had very little, if any, effect upon the rivalry of the original fields. The colored borders and the black background of the opposite field showed a rivalry independent of the rivalry of the squares, which fact suggests an interesting question for investigation: Under the same external conditions, in what respects does the rivalry of the peripheral areas of the retinae differ from that of the central areas?

Sec. 7. *Fields of Different Areas.*—The size of the green field in this experiment was reduced to one-fourth of the size of the red field, the latter being the same as in former experiments—one centimeter square.

These fields were so arranged upon a black card that when they were united in the stereoscope the green field fell upon the central portion of the red field. The following table gives the results:

TABLE XII.

Experiments.	Red, per cent. of time seen.	Green, per cent. of time seen.	No. of changes for each field.
1	36	64	19
2	34	66	25
3	31	69	26
4	32	68	19

Besides the rivalry recorded in Table XII. between the green field and the central part of the red field, there was also a rivalry between the outer part of the red field and the black surface of the card around the green field. Most of the time when the green appeared it displaced only its own area of the red field; so that it was seen surrounded by the outer portions of the red



field. Part of the time, however, the whole red field disappeared when the green appeared—*i. e.*, the green field was seen surrounded by its own background.

This background was the black cardboard upon which the fields were placed. So there was a rivalry between the retinal elements, stimulated with a plain black surface in one eye, and the corresponding elements, stimulated with the striped red surface in the other eye. The rhythm of this rivalry was much slower than that between the red and the green surfaces. The black surface was in consciousness less than one-tenth of the whole time.

When the green field was seen in the middle of the red field the adjacent areas of the retina of the right eye were in directly opposed relationship to the perceiving center or centers, for the stimulus coming from one part of the retina was effective, while the stimulus coming from its adjacent part was not. Part of the stimuli coming from the right retina were inhibited by the stimuli coming from the corresponding points of the other retina.

But the stimuli striking both the effective and non-effective portions of the retina were the same. So if the rivalry depends upon the physiological condition of the retina we should expect that those portions subjected to the same conditions would function in the same manner. The fact that they do not, together with the fact that the retinæ have no connecting fibers, seems to me to be strong evidence in favor of a central explanation of the phenomenon of binocular rivalry.

Sec. 8. *Rivalry Under Normal Conditions.*—Before considering the next experiments it will be necessary to look more closely to what takes place in binocular rivalry under normal conditions. Within what limits do the single phases of the rivalry vary? Does fatigue or practice affect the rivalry? The following figures will throw some light upon these and other questions. These figures are from twelve normal records taken at different times during the year, and are in each case records of one hundred consecutive changes. The red and green fields with black diagonal lines upon them were the stimuli used in all normal records.



TABLE XIII.

RED—RIGHT EYE.			GREEN—LEFT EYE.		
Changes.	Av. length of changes.	Av. V.	Changes.	Av. length of changes.	Av. V.
I	1.93	.45	2	2.24	.75
3	1.96	.55	4	1.82	.53
5	1.80	.40	6	1.96	.55
7	2.24	.90	8	1.80	.37
9	2.15	.76	10	1.93	.55
11	1.92	.60	12	2.10	.76
13	2.18	1.00	14	1.93	.63
15	1.65	.40	16	2.06	.35
17	2.16	.70	18	1.70	.38
19	1.71	.80	20	1.82	.49
21	1.94	.50	22	2.03	.49
23	2.23	1.00	24	2.28	1.00
25	1.76	.55	26	2.20	.63
27	1.55	.40	28	1.81	.50
29	1.80	.40	30	2.24	.63
31	1.66	.43	32	2.10	.64
33	1.90	.35	34	2.05	.55
35	1.60	.40	36	1.90	.56
37	2.00	.50	38	2.03	.48
39	1.66	.50	40	1.82	.39
41	1.54	.60	42	1.72	.35
43	1.35	.25	44	1.54	.35
45	1.61	.42	46	1.73	.44
47	1.74	.33	48	1.87	.62
49	1.77	.58	50	1.85	.51
51	1.78	.48	52	1.68	.34
53	1.73	.42	54	1.70	.38
55	1.95	.62	56	1.76	.30
57	1.94	.41	58	1.80	.61
59	1.90	.53	60	1.85	.51
61	1.84	.56	62	2.05	.74
63	2.00	.47	64	1.84	.60
65	1.81	.41	66	2.13	.90
67	1.72	.50	68	1.90	.47
69	1.75	.50	70	1.75	.57
71	1.33	.36	72	1.43	.65
73	1.85	.44	74	1.80	.36
75	1.63	.31	76	1.93	.54
77	1.65	.33	78	1.65	.42
79	1.95	.21	80	1.80	.43
81	1.73	.40	82	1.66	.36
83	2.10	.63	84	1.90	.23
85	2.40	.65	86	1.80	.48
87	2.45	.40	88	2.36	.60
89	2.23	.63	90	2.30	.76
91	2.20	.67	92	2.08	.41
93	1.46	.44	94	1.75	.48
95	2.63	.65	96	1.85	.37
97	1.61	.45	98	2.13	.80
99	1.80	.60	100	2.00	.78
Average.	1.88			1.90	

The average number of changes per one hundred seconds for each field was 26 +.

No fatigue nor practice effect is shown by the records.

Sec. 9. *Light Intensities*.—In all the previous experiments the visual stimuli were brought to the retinae by means of reflected light. In this experiment transmitted light was used. Square openings one centimeter in size were cut in the card for the stereoscope. These openings were covered with red and green gelatin films across which strips of black paper were pasted, making two fields like the ones of the former experiments. Instead of sunlight, electric lights were employed. The object of the experiment was to determine what effect changes of intensity of the light stimulus would have upon the rivalry. Five different intensities of light were tried. The experiment was performed in a darkened room. In order to diffuse the light properly one thickness of ground glass was placed between the films and the light.

Table XIV. gives the per cents. of time each field remained in consciousness for the different light intensities. Since the intensities of both fields were the same in all cases, we should not expect this to differ from the normal. Table XV. gives the average length of each change, or phase, under the different intensities.

TABLE XIV.

	Red, per cent. of time seen.	Green, per cent. of time seen.
Arc light, 1,000 c. p., at 50 cm. distance,	49	51
Incandescent, 80 c. p., at 50 cm., . .	46	54
Incandescent, 16 c. p., at 50 cm., . .	47	53
Incandescent, 16 c. p., at 400 cm., . .	48	52
16 c. p., hooded, very dim, . . . .	48	52

Table XIV. shows that as regards the ratio of the lengths of time which the red and green fields remained in consciousness the rivalry was normal.

TABLE XV.

	ARC.		INCANDESCENT.							
	1000 c. p. at 50 cm.		80 c. p. at 50 cm.		16 c. p. at 50 cm.		16 c. p. at 400 cm.		Very dim.	
	Length.	Av. V.	Length.	Av. V.	Length.	Av. V.	Length.	Av. V.	Length.	Av. V.
Red.	1.18	.23	1.37	.37	1.61	.49	2.08	.70	4.08	.73
Green.	1.21	.16	1.59	.21	1.78	.28	2.21	.50	4.39	.74
No. ch. 100 sec.	41		34		28.5		23.3		11.6	

From Table XV. we get the following points: The rate of fluctuation varies with the intensity of the light. (For the lowest intensity the light was not decreased beyond the point where the fields could be seen distinctly without straining the eyes, although it was intended to decrease it until this point was approached. For the highest intensity the light was as bright as possible without producing a dazzling effect.) With the lowest intensity the number of changes during one hundred seconds fill to about 12, while with the highest intensity it increased to 41. The length of the changes was correspondingly long for the lowest intensity, and short for the highest intensity, ranging from 4.39 for the lowest, down to 1.18 for the highest. From this we see that the length of the phases of the rivalry increased nearly four times, while the ratio of the red and green phases to each other remained normal.

If instead of lighting both fields equally, they are unequally lighted, then this ratio is changed. The following experiment shows this. The ordinary red and green squares illuminated by reflected light were employed. Two series of tests were made: First, the red field darkened. Second, the green field darkened. In both cases the darkened field received about one-fourth as much light as the brightened field. Table XVI. contains the results.

TABLE XVI.

	Red, per cent. of time seen.	Green, per cent. of time seen.	No. of changes for each field.
Red darkened, . .	39.	61.	22.
Green " . .	60.	40.	29.



The average length of each phase under these conditions is as follows :

TABLE XVII.

	RED.		GREEN.	
	Av. length.	Av. v.	Av. length.	Av. v.
Red darkened, . .	1.9	.45	3.00	.8
Green " . .	2.1	.80	1.36	.3

These figures show that when the two fields were unequally lighted the lighter field remained longer in consciousness. There is then an element to be considered here which did not show itself in the last experiment where the fields were equally lighted. There, a rise in the intensity of light caused an increase in the rate of the changes and a decrease in the time the field remained. Here, a higher intensity of light upon one of the fields caused an increase in the time it remained in consciousness, in spite of the fact that a high intensity of light might be expected to fatigue the activity of the retina more quickly. While both fields were equally lighted the shortened phases of the rivalry might find an explanation in the physiological fact that a strong stimulus causes the processes of assimilation and dissimilation of the nerve substance to go on more rapidly, and consequently the phases of the rivalry would take place more quickly. But such an explanation does not account for the fact that when the two fields were unequally lighted the brighter remained longer in consciousness. The psychical fact that a strong stimulus has a greater attraction for consciousness than a weak one enters into this phenomenon. In other words, the assimilative and dissimilative processes of the retina do not furnish an explanation for binocular rivalry.

Sec. 10. *Rivalry of After-images*.—Stimulation of the retinae with the transmitted light of the red and green gelatin films used in the first part of Experiment 8 gave very persistent and distinct after-images. Now, since the after-image in the one eye was sufficiently different from the after-image in the other eye not to fuse with it, and since they both had the same local sign, they were not seen at the same time. The re-



sult was that here, too, the phenomenon of binocular rivalry appeared. The changes of the images were complete and distinct. The records of this rivalry given below consider only the rivalry of the diagonal lines of the fields. The changes of the colors were not recorded. The length of direct stimulation in each case was one hundred seconds; the light was then turned off, the eyes lightly closed and the changes of the after-images recorded. Seven different tests were made. Table XVIII. gives the ratio of the total lengths of time the two after-images remained in consciousness for the seven tests. Table XIX. gives the average length of the individual phases.

TABLE XVIII.

Experiments.	Red, per cent. of time seen.	Green, per cent. of time seen.
1	50	50
2	49	51
3	51	49
4	56	44
5	52	48
6	50	50
7	49	50

TABLE XIX.

Experiments.	RED.		GREEN.	
	Average length of each change.	No. of changes.	Average length of each change.	No. of changes.
1	5.08	10	5.14	10
2	4.94	10	5.08	10
3	4.88	10	4.58	10
4	7.88	7	6.21	7
5	7.70	7	7.20	7
6	6.60	7	6.55	7
7	7.00	7	7.43	7
Ave.	6.30		6.02	

Each one of the seven tests was continued for one hundred seconds, or the time nearest one hundred seconds in which there was an equal number of changes for each image. The variation of the phases of the rivalry during a single sitting averages about 1.

There was a large difference in the lengths of the phases in

the different sittings taken on different days, which can be seen from the table.

The rate of fluctuation in the rivalry of after-images is much slower than in the case of direct stimulation. The stimulus from which the after-images resulted was the brightest intensity used in the experiment of Section 9, an arc light of one thousand candle power at a distance of fifty centimeters from the gelatin films. Under the direct stimulus the individual phases were only 1.1 and 1.2 seconds, while in the rivalry of the after-images they averaged 6.3 and 6.0. The number of changes per one hundred seconds for the stimulus was 41, while the after-images averaged but 8+.

The ratio of the time which the two images were seen is practically the same as in the normal records.

Sec. 11. *Fields of Same Color.*—In order to determine whether the possible different adaptation of the lens for red and green lights had any effect upon the rivalry of the red and green fields employed in the preceding experiments, two series of experiments were made in which the fields for both eyes were of the same color; first, red; second, green. Otherwise the character of the fields was the same as in the normal experiments with reflected light. The same number of black lines was employed and the same sized squares. The only difference between the two fields was that of the direction of the lines which made two sets of lines at right angles to each other in the combined field. The first Table (XX.) gives the ratio of the two fields, and the second (XXI.) the average lengths of the individual changes.

TABLE XX.

Experiments.	Right field, per cent. of time seen.	Left field, per cent. of time seen.	No. of changes for each field.
BOTH FIELDS RED.			
1	45	55	31
2	46	54	31
BOTH FIELDS GREEN.			
3	46	54	29
4	45	55	28

TABLE XXI.

Experiments.	Right field, av. length of each change.	Left field, av. length of each change.
BOTH FIELDS RED.		
1	1.40	1.77
2	1.44	1.70
BOTH FIELDS GREEN.		
3	1.56	1.83
4	1.60	1.92

These tables show that the rivalry under the above conditions was normal.

Sec. 12. *Monocular Rivalry*.—In all the preceding experiments we have been considering rivalry as manifested between corresponding areas of the two retinae. Is it possible to get a rivalry of diverse stimuli when they fall upon the same area of the retina of one eye? Will two different objects, when so arranged that their light rays fall upon the same point of a single retina, be seen as one or two? If, under these conditions, the objects have the same local sign, we should expect the stimuli to fuse. This is what takes place when red and green sectors are revolved upon a color wheel. The green and the red are brought upon the same point of the retina at practically the same time, and the different stimuli are fused and perceived as a gray. There is, in this case, no fluctuation of the red and green stimuli in consciousness—in fact they are not in consciousness at all. A constant and even gray presents itself. This fusion is what we should expect in every case where different stimuli are brought by any means upon the same point of the retina. By means of a prism I was able to superpose upon the same point of one retina the two red and green squares that have been used in the experiments in binocular rivalry. If the thin edge of a prism is brought as near as possible to the eye so that this edge is in line with the perpendicular plane which cuts the eye into right and left halves, then one-half of the field of vision is seen through the prism and the other half is seen directly; but the half seen through the prism is bent in the direction of the other half, so that a part of the one half is superposed upon the



other. Diverse stimuli are thus brought together upon the same point of the retina. Under these conditions the red and the green fields could very easily be placed so that they appeared to come from the same position in space. For convenience the prism<sup>1</sup> which I used was set in one side of a stereoscope frame, and the frame mounted upon a solid standard. This formed a rest, which is necessary in order to eliminate movements of the head.

The red and the green fields were placed in position on one side of the stereoscope so that one was seen directly, while the other was seen through the prism, and yet both seemed to come from the same place—*i. e.*, both fields fell upon the same retinal point.

Instead of complete fusion of the red and the green backgrounds, and a fixed appearance of the crossed lines, there was only a slight fusion of the colored backgrounds and no fixity of the lines. Both the red and the green backgrounds were seen at the same time—the red lying just a little behind the green, and appearing through it, as it were. The red field was most intense at the extreme left edge,<sup>2</sup> and gradually became less intense toward the right. The green field was most intense at the right edge, and decreased toward the left. At the center of the square, however, the two fields appeared with about the same intensities.

Now the interesting part of the experiment is that if the center of the field was fixated, a rivalry of the colors was perceptible. Neither disappeared entirely: but at times the red would appear very distinctly while the green would fade; then the red would fade and the green appear distinctly. The two sets of lines showed the same fluctuation, keeping pace with the changing of the intensities of the colors. Sometimes one of them would disappear altogether.

This rivalry of the colors and of the lines was much slower than the rivalry in binocular vision. A very slight movement of the eye to the right or to the left would cause changes in the intensities of the fields; so that extreme care was necessary in order to avoid the movements, or, at least, not to confuse

<sup>1</sup> 20° prism.

<sup>2</sup> The red field was placed on the left-hand side.



changes caused by them with the rivalry of the fields, which is independent of eye movements.

If one of the fields is so placed that its reflection appears on the surface of a plate of glass at the same point through which the other field is seen directly, then both fields are superposed for the two eyes at the same time. The apparatus for Ragona Seinà's experiment of mirror contrasts serves as a convenient means for superimposing the fields.

#### SUMMARY.

The length of time which the fields normally remain in consciousness was increased by direct will power.

Efforts to decrease the number of changes of the fields in a given time were unsuccessful.

With the so-called pure will efforts there were in every case accompanying eye movements.

Elimination of the eye movements decreased the ability to hold either of the fields.

The introduction of conscious eye movements was accompanied by a lengthening of the time of the field whose lines served as the guide for the movement.

Counting the lines upon either field increased the length of time that field remained in consciousness.

Figures which induced the greatest eye movement remained longest in consciousness.

The lines of a moving field remained in consciousness nearly all the time, but did not inhibit the normal rivalry of the two fields.

Contraction of the right side or of the left side of the body had the same effect upon the rivalry, viz., increased the time which the field before the right eye was seen.

Colored borders did not affect the rivalry.

Of two fields of different sizes, the smaller remained longer in consciousness.

Under different conditions adjacent parts of the retina showed different rates of rivalry at the same time.

Increase in the intensity of the light stimulus caused an in-

crease in the rate of the changes, while the ratio of the phases of the rivalry was normal and constant.

Of two unequally lighted fields, the lighter remained longer in consciousness.

After-images showed the same phenomenon of rivalry; but the changes occurred at a slower rate than in the case of direct stimulation.

When both fields were of the same color the rivalry of the two sets of lines was not affected.

Different stimuli falling upon the same area of the retina of one eye produced the phenomenon of rivalry.

There seem to be some facts in the foregoing experiments that point toward an explanation of binocular rivalry as a purely physiological process. There are others which point to its explanation as a psychical process. The effect which different intensities of light had upon the rate of fluctuation might be interpreted as a purely physiological effect depending upon the anabolic and catabolic processes in the tissue of the retina. On the other hand, the fact that, of two fields of different intensities, the brighter remained longer in consciousness, seems to point to the attention process as the effective factor. The effect which eye movements had upon the rivalry of the fields may also be looked upon as physiological. These movements brought into activity new parts of the retina, thereby relieving the parts which had been active in reporting the stimulus to the higher centers. This division of labor enabled the retina to maintain its activity for a longer length of time. Now supposing that these movements, as was the case, were made only when one of the fields was in consciousness, and that when the other field appeared the movements ceased: in the latter case there would be no change of retinal elements; therefore the activity of the retina reporting this field would run down much more quickly. Thus an explanation would be offered for the longer length of time which the other field remained in consciousness. But in all cases of eye movements the fact must be considered that *not one, but both* eyes moved at the same time. Take, for instance, the experiment where eye movements

were introduced when the red (right) field was seen. Now, movements along the lines of this field with the right eye were accompanied by movements across the lines of the green or un-presented field by the left eye. So there was a change of retinal elements in this eye as well. When, in the course of the rivalry, the green field appeared and eye movements ceased, then the fixity of the retinal elements was the same for the right eye as for the left. So far as a change of the retinal areas is concerned, the conditions were the same for both eyes. There was a point of difference, however, in regard to fixation. When the red field was in consciousness there was fixation of the lines of that field by the right eye, which was necessary in order to follow the lines. This element of fixation was not possible in the same way for the left eye—*i. e.*, there could have been no adjustment to a particular line as was the case in the right eye. There was, in the one case, a particular and definite motor innervation coming from the cortical centers, while in the other this was lacking. This element was present also to a more marked degree when the lines of one of the fields were counted. Our conclusion, therefore, is that an explanation is to be found, not in terms of the physiological functions of the periphery but in terms of the central processes. This does not mean that it will be in purely psychical terms, for it is evident that the physical movements of the eyes should be considered as an effective factor in the rivalry under the conditions of the experiments.

The introduction of simple figures which tend to induce eye movements strengthened the belief that bodily adjustments play an important part in the phenomenon. But since the effect of these movements is not accounted for in the sense organs of the periphery, it may lie in the relation of the movements to the cortical centers; in other words, in the *meaning value* of the movements for consciousness.

A *purely psychical* explanation cannot stand before the fact that purely mental states were not able to control or affect the rivalry. It continued in its usual rhythm even when the attention was fixed upon one of the fields. In the case of the moving field opposed by a stationary field, the strong attraction which a moving object has for the attention process did not



check the rivalry. The other field came and went with about its usual rate. When, however, the so-called pure act of attention was aided by some appropriate motor adaptation, then the effect upon the rivalry was most marked.

In the case of two unequally lighted fields, the brighter calls for a greater number of eye adaptations in the form of more definite and distinct accommodations of the eye to points upon its surface than is the case with the darker field. The darker the field the more even does its surface appear—*i. e.*, the less the degree and the fewer the differences in the surface; it approaches a perfectly plain surface in low intensities of light. Consequently fewer points upon its surface call for fixation. A plain surface is not explored by the eye as thoroughly as a diversified surface is—nothing new is to be gained by such exploration, and so the mind is accustomed to fill in the content at once without the trouble of further eye adaptation. On the other hand, the brighter the field the more points of difference on its surface appear, and consequently the greater the number and more definite the eye adaptations. This was most evident where the red square seen by one eye was opposed to the plain black surface seen by the other eye. Here the black surface was in consciousness but a very small part of the time, the brighter field crowding it out. The same effect appears in the experiments of Section 3, where a plain red square was opposed to the figures of the green square.

In view of these considerations it seems that either a purely physical or a purely psychical explanation of binocular rivalry fails. The true explanation must be looked for in the nature of the psycho-physical processes of the cortical centers, the activity of which depends, not only upon the incoming nerve stimuli, but as well upon the outgoing motor discharges. The character of the discharge which determines into what motor reaction it is to end, is what is meant by the 'meaning value' of bodily movements or adaptations.

Consciousness, from the above point of view, depends for its existence and character upon the transference of sense stimuli into motor paths.<sup>1</sup> This hypothesis considers the in-

<sup>1</sup> Münsterburg, *Physiological Basis of Mental Life*; paper read before the American Psy. Ass., N. Y., Dec., 1898. *Science*, March 24, 1899.



coming, or sense stimulation, and the outgoing, or motor innervation, as a single nerve process. There is no point of separation between them. The motor discharge is necessary in order that any central activity take place. This point will be discussed in its more definite aspect later.

Binocular rivalry, then, would be at once 'psychical' and 'physiological' in that it is dependent upon central processes, and is affected by the nature of the motor adaptations. Fundamentally it may be considered as a rivalry of discharging centers whose activity is inseparably connected with incoming sense stimuli.

The important part which the motor adaptations of the eyes played in these experiments in binocular rivalry forms a connecting link between the consideration of the effect which their inhibitions had here and the effect of inhibited motor responses in other parts of the body. I shall add, therefore, the report of some experiments on the inhibition of the motor elements of language expression and its effect upon conscious states.

### INHIBITION OF MOTOR REACTIONS.

As has been indicated, inhibition, in so far as it is a question for psychology, is most manifest in the psycho-physical processes. Ideas, feelings and emotions are intensified or inhibited according to the character and intensity of the bodily adjustments. On the other hand, mental states have an intensifying or inhibiting control over the bodily reactions and functions. Between the mental and the motor elements of the psycho-physical processes there are many cases of direct conflict and repression—*i. e.*, certain mental states have a direct inhibitory influence upon certain motor adjustments, and certain motor adjustments have a direct inhibitory influence upon certain mental states.

On the other hand, certain motor adjustments are very important for the mental states. Many conscious states show very plainly that they are accompanied at all times by particular motor adjustments. In many cases the major part of the content

of the mental state is that furnished by the motor adjustment. If they are in any way inhibited, the mental state is inhibited.

An instance of motor adjustment accompanying conscious states is the almost universal tendency to express ideas in words at the moment they are presented to consciousness—*i. e.*, to think in words. Some people become hoarse listening to a long lecture. Observation in such cases reveals the fact that the listener is repeating to himself the words of the lecturer. Usually this tendency stops with the rudimentary stages of enunciation. The muscles of the vocal cords, throat and respiratory organs are slightly innervated and adjusted, but the process goes no further. Sometimes, however, the enunciation is complete so far as the adjustment of the muscles of the vocal cords, throat and mouth cavities is concerned. There is a tendency to make these adjustments not only when we hear spoken words, but to make them in response to other stimuli. We are likely to utter the name of any object upon which the attention rests. If an object is taken in the hand without knowing beforehand what it is, the vocal organs are ready with its name at the instant of recognition. If, for any reason, the motor apparatus does not respond properly, there is an interruption in the conscious stream. In moments of surprise, anger, joy or grief the motor expression of mental states is unrestrained. In ordinary mental states these adjustments are so rudimentary and delicate that only the closest observation will reveal them. A slight alteration in the rate, direction or pressure of the breath may in many cases be the only clue to them. That they are present to some degree during the greater part of conscious processes I am convinced.

In the psychological tests made upon the Freshmen of Columbia University there was one memory test which called for the reproduction of a given series of numbers, under two conditions: first, the numbers were read aloud to the subject; second, they were shown to him. In the last case the numbers were placed upon cards and shown one at a time. In both cases it was noticed that nearly every one tested repeated to himself the names of the numbers as they were read or shown. The question suggested itself as to what would be the effect upon the

ability to remember the numbers if these motor responses were in any way inhibited. Accordingly a number of simple tests were made, the results of which follow.

### SECTION I.

The object of the following experiments was to test the memory under two sets of conditions. First, under conditions which in no way inhibited the tendency to pronounce the names of the memory series when given. Second, under conditions which inhibited this tendency.

In the experiments of Section I the series used was made up of combinations of the nine digits. The simpler arithmetical relations were avoided in the make-up of the series. The number of digits in the series depended upon the ability of the subjects. It varied from seven to eleven. After reading the series to the subject he was required to repeat it in its exact order if possible. Misplaced and inverted digits were counted as errors: the first as one, the second as two errors. When school children were subjects they were asked to reproduce the series upon a slip of paper. The numbers were read at a uniform rate of one per second. There were forty-two subjects. Three were graduates in psychology, one a graduate student in another department of the University, fifteen students in the Horace Mann High School, and twenty-three pupils in the fifth grade of the same school. The two conditions of this experiment were as follows: First, the subject was asked to remember the series in the easiest or most natural way. Second, the subject held his breath while the series were read to him. It was thought that the holding of the breath would inhibit the tendency to repeat the names of the digits. Subjects W, D, R and G were examined individually; the school children were examined in groups. Under the first condition of the experiment *every* subject repeated to himself the numbers of the series as they were given; under the second, holding the breath, this tendency was somewhat suppressed. The following table gives the results:



TABLE XXII.  
MEMORY SERIES (DIGITS) AUDITORY.

Subject.	REPEATED NAMES.		HELD BREATH.	
	No. <sup>1</sup> Given.	No. <sup>2</sup> Remembered.	No. <sup>3</sup> Given.	No. <sup>4</sup> Remembered.
W.	80	75	80	60
W.	100	78	100	56
W.	110	94	110	77
R.	80	59	80	49
D.	80	58	80	45
W.	90	90	90	44
G.	90	86	90	35
	630	540	630	366
5th Grade.	161	117	161	104
	161	117	161	107
	161	110	161	90
	161	113	161	90
	161	114	161	83
	120	98	120	77
High School	120	85	120	92
	120	80	120	72
	120	88	120	66
	1285	922	1285	781

The decrease in the number of digits remembered when the breath was held may be due not to the absence of the motor adjustments, but to a distraction of the attention caused by the attempt to hold the breath and recall a series, which was being read, at the same time. Even if we admit that the lessened ability to remember a particular series was due to distraction of the attention, there is this consideration: Many psychologists hold that the attention is largely a matter of motor adjustment anyway; so it is quite possible that the adjustments necessary for holding the breath, by inhibiting the motor adjustments for the memory series, made it impossible for the attention to remain undivided at its task. This, of course, still makes the inhibition of the motor elements the effective factor in the inhibition of the conscious states. I am inclined to think, however, that if the effort to hold the breath came from the higher centers, it certainly would act as a distracting element in the attention process. But holding the breath for the short period of seven to eleven seconds requires very little mental effort. We often



hold the breath for longer periods of time during mental activity without being aware of it.

In order to test the matter, another experiment was performed under the same conditions, with the exception that the enunciation of the names of the digits was required when the breath was held. It was found that it is possible to make the proper motor adjustments for enunciation when the breath is held, if one desires to do so. The element of adjustment which was not present in the last experiment during the series when the breath was held is voluntarily introduced by the subjects in this experiment.

This enunciation of the names required some effort, at least on the part of the subjects, so that there might be expected to be an added opportunity for distraction of the attention. Table XXIII. gives the results :

TABLE XXIII.  
MEMORY SERIES (DIGITS) AUDITORY.

Subjects.	REPEATED NAMES.		HELD BREATH BUT RE- PEATED NAMES.	
	No. given.	No. remembered.	No. given.	No. remembered.
G.	80	64	80	64
5th Grade.	168	140	168	119
	168	118	168	121
	168	119	168	113
	133	118	133	98
6th Grade.	133	95	133	85
	133	84	133	88
	133	92	133	97
	1116	830	1116	785
M.	70	62	70	64
G.	70	69	70	69
H.	70	56	70	56
	210	187	210	189

The difference in results under the two conditions was very slight. This seems to indicate that the distraction of the attention as such in the first experiment was not sufficient to account for the decrease in the number of digits remembered. Holding the breath often takes place during moments of mental effort ;

but in the case of the first experiment it took place under conditions which tended to inhibit the motor elements of the memory process, and to this fact I attribute the lessened ability to remember the series.

## SECTION 2.

Perhaps the most satisfactory way of inhibiting the adjustments for enunciation, on the part of the subjects is to call their attention to the tendency and ask them to refrain from making them. A little practice will enable most subjects to eliminate the greater part of such adjustments. This was the method employed in the following experiments. Another change was made in the character of the memory series. Instead of numbers, colors were used. Squares of colored paper ( $1\frac{1}{2}$  inches) were pasted upon white cards of a size convenient to

TABLE XXIV.  
MEMORY SERIES (COLORS) VISUAL.

Subjects.	REPEATED NAMES.		INHIBITED.	
	No. Given.	No. Remembered.	No. Given.	No. Remembered.
W.	70	61	70	21
W.	70	65	70	24
D.	70	63	70	37
D.	70	66	70	54
Dxt.	60	59	60	19
B.	70	67	70	24
R.	50	50	50	17
F.	60	57	60	24
	520	488	520	220
6th Grade	147	134	147	77
	147	109	147	91
	147	98	147	66
	147	128	147	97
	147	98	147	56
5th Grade	56	45	56	15
	21	10	21	9
	21	17	21	11
High School	21	17	21	9
	21	15	21	11
	21	18	21	10
(3 Subjects.)	21	11	21	4
	126	85	126	63
5th Grade	126	80	126	58
	1169	865	1169	566

handle. The colors were red, brown, blue, green, yellow, orange and white. The cards were shown to the subject one after the other at the rate of one per second. In most of the experiments seven colors were employed in each series. When, however, the subject was able to remember more a larger number was given. The ability of the subjects to remember a series of colors shown, under the two sets of conditions given at the beginning of Experiment 1, was the object of the following tests. Under the first condition the names of the colors in the series were formed in muscular terms as they were given. Under the second condition the subjects refrained from making the motor adjustments. They were asked to think the colors in purely visual terms, and to avoid the motor elements involved in enunciation.

This table shows a marked falling off in the ability to remember a series of colors when the names of the individual colors are not repeated. The errors were not bunched, but evenly distributed. The following tables give typical records taken directly from my note book. They represent a single sitting for subjects D, W and F. Seven colors were given in each series, and ten series given under each of the two conditions. The top row gives the number of colors in each series correctly reproduced under normal conditions. The lower row gives the number correctly reproduced when the motor elements were inhibited.

SUBJECT D.										
Normal, . . . . .	7	6	7	6	5	7	7	7	7	6
Inhibited, . . . . .	3	2	1	2	3	3	3	2	2	3
SUBJECT W.										
Normal, . . . . .	7	7	7	6	5	5	7	5	5	7
Inhibited, . . . . .	2	1	2	0	1	3	1	2	4	5
SUBJECT F										
Normal, . . . . .	5	5	6	6	6	6	5	6	6	6
Inhibited, . . . . .	3	0	2	1	2	3	1	6	4	2

Some of the subjects showed a marked tendency to substitute other motor responses to the stimuli when the vocal adaptations were inhibited. This tendency manifested itself in impulses to designate by finger or head movements spacial positions for the colors as fast as they were shown, which scheme was used as an aid to the memory. The last two records of subject W<sub>10</sub> were reported by him as cases where the following spacial scheme was used quite successfully as an aid in reproducing the colors: The colors were ideally arranged in the order of the spectrum, with the grays beneath them, thus:

R. O. Y. G. B.

White.

Brown.

Black.

Now when a color was shown, it was put into this ideal scheme and its position indicated by slight movements. This subject's space sense was almost entirely motor.

### SECTION 3.

This experiment is the same as in Section 2, except in the manner of presentation of the series. Instead of showing the colors, their names were read to the subject, so that he did not see them at all.

TABLE XXV.

MEMORY SERIES (NAMES OF COLORS) AUDITORY.

Subjects.	REPEATED NAMES.		INHIBITED.	
	No. Given.	No. remembered.	No. Given.	No. remembered.
6th Grade	112	64	112	61
	112	65	112	43
	224	129	224	104
S.	60	56	60	47
S.	60	58	60	37
S.	70	57	70	42
M.	70	66	70	33
H.	70	65	70	30
	330	302	330	189



The decreased ability to remember the series seemed to be due to the inability to get a full consciousness of the stimuli when the adjustments of the vocal apparatus were inhibited. One subject said that to ask him to remember a series of colors without allowing him to repeat the names to himself as they were presented, was to ask of him an impossibility, since if the motor elements were eliminated there would be no tangible content for the memory. Another subject was unable to imagine the colors in the reproduction until he had repeated the names.

The explanation of these results will be found, I believe in the impoverishment of the conscious content by the elimination of the motor elements. It is not to be denied that the effort necessary to inhibit the tendency to repeat the names might have been a distracting factor in the attention process. But to say that the attention was distracted in the above experiments may mean only that the attention process follows the motor adjustments of the body, and that the so-called inhibition of the ideas is due to conflicts of these bodily adjustments, or that distraction of attention is the antagonism of opposing motor discharges. So that the innervation of the vocal muscles which kept them from reacting in their usual way may have been the real cause of the inhibition of the memory images.<sup>1</sup>

Suppose other muscles of the body had been innervated to a like degree. This would have required as much attention, and would have been equally distracting. Is it reasonable to suppose that contracting the biceps of the arms rigidly while a memory series is presented would affect the ability to reproduce the series to the extent shown in the above tables? Certainly not; and the reason is just this: flexion of the muscles of the arms in no way interferes with the natural reaction of the muscles involved in vocalization. In Experiment I it was shown that when holding the breath did not interfere with the motor adjustments for naming the series presented there was no inhibiting effect in the memory process. I do not believe it reasonable to suppose that more effort was expended in these later experiments in refraining from making the motor responses

<sup>1</sup> I refer to the innervation of the vocal muscles which kept them from repeating the names.

to the presented stimuli. Therefore, our conclusion is that the inhibition of the motor responses in the above experiments had an inhibiting effect upon consciousness, and to this was due the failure to reproduce the series.

These results agree with those obtained by Theodate L. Smith, reported in the *American Journal of Psychology*:<sup>1</sup> Mr. Smith investigated the effect which the complex of throat, tongue and lip movement involved in articulation has upon the memory of nonsense syllables. His method of inhibiting the movements of the muscles of the throat and other vocal organs was to require the subject to count aloud while the series was shown to him. He found that from 13 to 18 per cent. more errors were made when the movements for articulation of the syllables were inhibited in this way. The following interesting points came out in his results: Syllables hard to pronounce were most frequently forgotten. There was a marked tendency to interchange the letters *b* and *p*, *t* and *d* when they appeared as the initial letter of a syllable.<sup>2</sup> Regarding the increase of errors during the series in which the enunciation of the syllables was suppressed by counting, the question arose as to whether this was not caused by the distraction of the attention due to counting, and not to the suppression of the motor elements. It was decided that the inability to remember was due to the absence of the motor elements, for it was found that counting did not affect the memory of series whose motor adjustments were made by other parts of the body than those employed in counting. This particular series used by Mr. Smith was made up of the characters of the manual alphabet. His subjects did not know the alphabet—*i. e.*, they could not associate the different positions of the hand with the letters of which they were

<sup>1</sup> July, 1896.

<sup>2</sup> These points are significant: When a syllable difficult to pronounce appears in a series it is just the concise and definite motor elements which are lacking in the presentation to consciousness—*i. e.*, the motor adjustments are not definitely formed. Hence the inhibition of the mental states involved. The interchange of *t* and *d*, *b* and *p*, is doubtless due to the great similarity of the muscular adjustments in their pronunciation. Abstracting from these, there is as great a difference between the contents of consciousness awakened by these pairs of letters as between other consonants, and there would be no reason for confusing them.

signs. Not knowing the names, there was no tendency to repeat them; but there was a tendency to form the different characters with the hand when their pictures were shown. His subjects were given a series of these pictures one after the other and required to reproduce it as well as they could. In one series they were allowed to make the characters with the hand as the pictures were shown. In the next series they were not allowed to do this. From 11 to 22 per cent. more errors were made in the reproduction of the second series.

After this experiment had been continued for some time, counting was introduced into the first series to see to what extent it would act as a distracting factor. It was found that the ability to reproduce the series was not decreased. It is reasonable to conclude that the increase of errors made in the nonsense syllable series was due to counting, not as a distraction of the attention, but rather to the fact that it suppressed the motor adjustment for the series.

All these results show the great importance which the motor elements of articulation have in the make-up of mental states. In many trains of thought the content is almost entirely composed of the muscular complexes involved in the language expression of the thoughts. The usual mental content accompanying such a word as 'bubble' is inhibited if one tries to think the word with the lips wide apart. On the other hand, vividness of a mental process is produced by an intensification of the motor elements accompanying the process. If, while engaged in some mental work, such as reading or adding a column of figures, one is distracted by stimuli foreign to the work, the attention may be held to the task by reading aloud—*i. e.*, an increase in the intensity of the appropriate motor adjustments increases the stability of mental processes. A general intensified motor activity accompanies any attempt to overcome mental distraction.

The inhibitory effect which the suppression of motor activity has upon consciousness is not limited to the vocal apparatus. It is general. The whole motor mechanism is involved in the psycho-physical processes. In general, *inhibition of the motor elements tends to inhibit consciousness.*



Emotional states manifest in a very marked degree accompanying bodily conditions which are necessary for their existence. If the muscles are relaxed, head bowed down and the shoulders stooped, one can readily imagine and really produce the feelings of sorrow. On the other hand, if the chin is elevated, the head raised, the shoulders thrown back, the lungs filled and the muscles contracted, feelings of sorrow are immediately inhibited. Each emotional state has its appropriate bodily adjustments. Suppression of these adjustments tends to inhibit the emotion. A change of the motor elements may change the nature of the emotion.<sup>1</sup> All primary emotions imply tendencies to movement. Why not extend this tendency to all mental states? The alert bodily adjustment, the quick flashing eye are everywhere signs of mental activity. The discerning teacher is familiar with this fact. She can quickly read the mental condition in the bodily attitudes of the children. Vigorous, active thought is not found in relaxed and inactive bodies. James says: "No impression, or idea of eye, ear or skin comes to us without occasioning a movement, even though the movement be no more than the accommodation of the sense organ; and all our trains of sensation and sensational imagery have their terms alternated and interpenetrated with motor processes."<sup>2</sup>

It is useless to multiply instances of bodily movements which accompany, excite or intensify mental activity. They are everywhere in evidence. It is doubtful if it is possible to think intently without figuring the ideas in terms of movement, or without the motor adaptations for the language expression.

In emphasizing the importance of the motor elements in emotional states the advocates of the kinæsthetic theory of the emotions consider an emotion as dependent for its character entirely upon the reverberation of the motor adjustments—*i. e.*, upon the complex of sensations coming from movements or adjustments of the body which are the direct reactions of stimuli. That these returning sensations have their particular values for consciousness there can be no doubt, but this is not the relation between consciousness and motor phenomena that I wish to

<sup>1</sup> Ribot, *Psy. of the Emotions*, tr., p. 265.

<sup>2</sup> *Psy.*, Vol. II., p. 481.



consider. These movements and adjustments from this point of view may be considered in the same way as other external stimuli. The question is, Why and under what conditions do stimuli reach consciousness at all? It is my belief that no stimulus, either external or internal, is presented to consciousness without a motor reaction as a basis of the presentation. In other words, the *condition of consciousness is the transference of the action of the stimulus into or toward motor activity.*

The value of the motor side of consciousness from this point of view is not the value of the sensations coming from acting muscles, but rather the more fundamental fact that incoming stimuli are transformed into outgoing channels. Necessary to this conception is the following hypothesis concerning the action of the brain centers in their relation to afferent and efferent nerve fibers. The condition for the presentation of any stimulus is the permeability of the motor paths from the cortical centers—*i. e.*, consciousness arises only when the cortical centers involved are ready to discharge toward the periphery. The character of the mental state is determined by the location of the discharging centers. Its existence depends upon the *condition* of the motor paths: if they are open, the physiological nervous process necessary for consciousness can take place; but if they are closed, it can not. This conception regards whatever activity or change takes place in the so-called sensory and motor nerves and their centers as a *single* and *unified* process. The analysis of this process into a sensory process and a motor process, and the implied supposition that consciousness may accompany one, or both, is opposed by the above point of view. The sensory and the motor processes are inseparable. Activity in one part of a nerve circuit means activity in every part of that circuit, or, perhaps a better way of expressing it, the cortical centers are active only in transferring stimuli, external or internal, into motor channels. If the stimuli find a blocked system of motor channels from any cortical center, there is no activity of that center, and consequently no consciousness corresponding to such action.

Such an hypothesis as the above will give a better working physiological basis for the explanation of the facts of conscious

life. The close connection which motor activity has to consciousness is better understood if we consider every sensation or idea as the psychical element in the transference of afferent nerve stimulation into efferent channels.

From the point of view of evolution of the higher from the lower forms of conscious life, should we not expect motor activity and consciousness fundamentally connected? In the simplest forms of life having consciousness, feelings and movements are, as far as we know, coexistent and undifferentiated. A certain feeling means a certain bodily movement. Since only a few simple bodily movements are possible the feelings are correspondingly few and simple. With the limitation of motivity there is a consequent limitation of consciousness. On the other hand, as the possibilities of movements increase through the higher forms, there is a corresponding increase in the complexity and variability of consciousness. In the lower animals every feeling or idea has an immediate motor expression which gets executed to a greater or less degree. In man many of these expressions fall short of complete execution. They are suppressed, and we are able to trace them, or find their existence only in rudimentary form—in mere tendencies to act. Civilization and culture tend to modify and refine the expression of the motor innervation accompanying thought. In the child the natural and direct expression of its thoughts are least repressed, spontaneity is greatest; gradually, however, as the simplicity of its mental life develops into more manifoldness and complexity there is a general leveling tendency manifested in the motor expression of ideas. At the same time there is an increase in the number, variability and accuracy of motor expressions. This increase is parallel to the development of consciousness. In terms of brain physiology this repression of outward and visible expressions may find its explanation in the increased number of associated centers, whose activity means the transference of stimuli into a larger number of motor channels, thus modifying or even suppressing each other's action. In the child, before associations are formed to any extent, a stimulus may affect a small cortical area or a single center only. Later, when the center has formed connections with a number

of associated centers, the same stimulus may call up through these centers various motor innervations which are antagonistic, and so neutralize or modify each other. The more complex the mental state—*i. e.*, the greater the number—and the more diversified the locality of simultaneously excited centers, the less are the chances for a direct and simple act.

The various motor discharges accompanying a complex mental state may expend themselves in impulses to act in opposite ways. Out of this possibility arises the phenomenon of inhibition. The limitations of the motor adjustments become the limitations of thought. If ideas were free from this physiological condition, there would be no reason why the soul could not attend to a multitude of them at the same time. The old theory that the body is the limit of the soul has a new meaning in this conception. In the simple cell form it is impossible to contract and expand at the same time, and the feelings appropriate to these activities are, therefore, exclusive. The same principle we wish to apply to the more complex forms of consciousness. States of consciousness depending upon mutually exclusive motor adjustments are not possible at the same time. Or, simultaneous stimuli calling for opposite motor reactions are not presented at the same time. Thus, from this point of view inhibition in so far as it pertains to consciousness is a phenomenon arising out of the limitation of the motor apparatus.

### APPLICATION TO EDUCATION.

Education, in so far as it pertains to school life, has proceeded almost entirely upon the principle of dualism of mind and body. It seems to have been the presupposition upon which our school methods are based that between the spiritual and the physical natures of the child there is such a difference of kind that their development must necessarily be independent of each other. Consequently it has happened that all, or nearly all, the energy of the school has been expended in instruction directed to the spiritual nature, upon the supposition that it can be reached directly and without regard to the physical nature which underlies it. At first the intellect alone was the object



of school instruction; gradually, however, since the abandonment of the old faulty psychology, we have come to recognize the fact that the soul does not develop in sections, but that the will and the feelings enter into every act of intellect. This marked a great advance in educational theories.

But as yet there is very little intelligent understanding of the importance and real nature of the motor elements in conscious life. It is true, Franke, Froebel, Pestellozzi and others have at various times emphasized the motor elements; but beyond the misconceived and badly applied kindergarten system and the manual training courses appearing in a few favored localities their efforts have not reached.

Our methods of school instruction are still largely based upon the dualism of mind and body. This conception has led to the neglect of what to my mind is a fundamental element in education—motor training. We look after what we call the intellectual nature of the child, but the bodily activities we leave to the play of circumstances. In fact, the first thing we do in school is to suppress as far as we are able the natural and spontaneous reactions of the child; we stifle the activity that is the foundation upon which conscious life is built. We imprison the child for hours each day in his seat; meanwhile we try to teach him *to think* without giving him a chance *to react*. The only reasons that our schools are not in reality institutions for the suppression of mentality is that, in spite of our attempts to suppress, the child does react. He reacts in some way to every thought that is presented. In the first years of life these reactions are very prominent.

For some time psychologists have been working upon the hypothesis that every mental state has a definite and particular expression in the body, but this law of dynamogenesis need not necessarily presuppose the inseparable connection of consciousness and the motor discharge. One might very well hold that every mental state tends to express itself in the motor mechanism, and yet conceive the motor expression to be merely a result following the mental state. The relation is more fundamental, as was seen in the pages of the preceding section; and this fundamental and necessary relation I wish to add to

the law of dynamogenesis, and to insist that this addition makes it of primary importance for education. It gives us a clearer conception of how to apply the principle of self-activity in educational procedure. It is the child's ability to react, to make bodily adjustments and purposive movements which gives him the sense of *self* as opposed to his environment. His ability *to act* upon things, *to do* something with them, discovers to him the power and dignity of selfhood. The sense of mastery over environment, together with its place in the formation of character and self-reliance, comes directly out of the ability to execute bodily movements. In short, it is at the point of intersection, where consciousness and motor activity cross each other, that we may look for *self-consciousness* and *self-activity*, for these two are *one*.

It is not my object to make a plea for the recognition of motor activity in education merely for the sake of physical training *qua* physical training, for this would be proceeding upon the principle of dualism just as much as the attempt to train the intellect alone. My point is that there is no such dualism. The child should not be regarded as having two natures. On the other hand, he should be regarded as a single being with but one nature. This nature is at once physical and spiritual. Its elements are conscious bodily acts, impulses, feelings, emotions, and thoughts, none of which are purely physical, and none of which are purely spiritual—*i. e.*, the simplest physical adjustment to central stimulation has in it psychical elements, and the most complex idea has in it motor elements. It is rather my object to call attention to the motor elements because of their fundamental importance in the development of a self-conscious and self-active being. Instead of allowing the bodily activities which express themselves in the motor apparatus to shift for themselves, I would have them as carefully trained, and in as systematic a manner as the so-called faculties of the mind. I would have as great efforts made to help the child systematize his acts as is now made to induce him to systematize his thoughts. I would hold it as important for the child's development that he *learn to do* as to *learn to think*. I would not have the school *inhibit*, but rather *direct* the motor activity of

children. All methods of instruction looking to these ends should, however, be tempered with the conception of the unity of mind and body. Instruction should be placed upon a monistic basis.

Some of our most advanced thinkers in education to-day are asking that the primacy of the will and the fundamental character of the feelings be recognized in the application of methods of instruction. They wisely assert that every mental state has its particular feeling value, and that no idea, however abstract, is without this element. Now since the feelings are primarily and inseparably connected with and dependent upon physiological adjustments we can readily see wherein the value of bodily adjustments and motor activity lies for instruction. Why should not the school recognize this principle, and, instead of disregarding the value of motor activity, seize upon it as a means to reinforce and intensify instructions? The close connection which the positions of the body have to the feelings and emotional attitudes should be recognized and studied by the teacher. A lesson in history calculated to inculcate manliness and courage, by its recital of deeds of great men, goes wide of its mark, if given to pupils who, either from long confinement or for any other reason, are in a generally relaxed muscular condition.

From the point of view to which this work leads, the value of manual training for the development of the mind—*i. e.*, as a culture study—finds its basis in the very nature of consciousness. Here we find an explanation of the fact that the boy who gains the ability to perform bodily adjustments in a decided, accurate and rapid manner is better able to think accurately and clearly, and why a hesitating and ineffective bodily reaction is the accompaniment of a weakened or confused state of mind.





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## After-Images

BY

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[Submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy in the Faculty of Philosophy, Columbia University, and being Vol. V., No. 3, of Columbia University Contributions to Philosophy, Psychology and Education.]

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## CONTENTS.

	Page.
INTRODUCTION.....	2
PART I. EXPERIMENTAL .....	3
Sec. 1. Apparatus and Methods .....	3
Sec. 2. Threshold .....	4
Sec. 3. Latent Period.....	11
Sec. 4. Duration.....	18
Sec. 5. Fluctuations .....	37
Sec. 6. Qualitative Changes.....	38
Sec. 7. Space-relations. ....	41
Sec. 8. Retinal Transfer.....	44
PART II. HISTORICAL AND DESCRIPTIVE.....	51
Sec. 9. Relation to Sensation, to Memory, and to Imagination	51
Sec. 10. History .....	54
BIBLIOGRAPHY.....	60



## ON AFTER-IMAGES.

### INTRODUCTION.

After-images were first described in the *De Somniis* by Aristotle, who regarded them as closely allied to the centrally excited images of the dream-state; they were rediscovered and described anew by St. Augustine, and again by the Arab Al-hazan (a student of Aristotle's works) in the eleventh century. Peiresc, in describing these appearances in the seventeenth century, thought that he had discovered a new phenomenon. Among other prominent investigators to note and experiment with the images were Boyle, Newton, Buffon, Goethe, the elder Darwin and Fechner.

These vestiges of sensation owe their present interest in large measure to their seemingly twofold character, being allied both to sensation and to memory- and imagination-images. To the earlier observers after-images were more nearly like the images of the imagination; later, they were considered almost a sensation; whilst most recently the original position of Aristotle is again prominent. In the history of after-images we seem to have an epitome of the interrelations of physics, physiology and psychology; and probably no other single phenomenon is so good an example of the growth of experiment and measurement in psychology.

The succeeding portions of the present monograph, apart from the bibliography, deal respectively with (1) an experimental analysis of the conditions affecting the production, the duration, the latent period, the space-relations, etc., of the after-image, and with (2) a history of the phenomena and their relation to sensation, to imagination and to memory.

In the bibliography I have given references to what seem the most important contributions on the general subject of after-images. In Professor Koenig's excellent bibliography<sup>1</sup> of

<sup>1</sup> Helmholtz, *Handbuch der Physiologischen Optik*, 2te Aufl.



vision will be found references to almost all the work on this subject done previous to 1894. In the numbers of the *Psychological Index*, and in the bibliographies in the *Zeitschrift für Psychologie* and in the *L'Année Psychologique* will be found references to current literature since 1893.

The experiments to be subsequently described were all made in the psychological laboratory of Columbia University; and as subjects eleven advanced students in psychology took part, viz., B, C, D, F, G, H, Ho, K, M, S and W. Other subjects, too numerous to mention individually, chosen for their *naïveté*, were used for check-experiments. To all I am greatly indebted for their help and suggestions throughout the progress of the research.

The terminology used in the succeeding portions is as follows: (1) Positive after-image is an after-image in which the image and its background bear the same intensity-relation as in the stimulus. (2) Negative image is one in which the relation of intensity is reversed. Thus, if the stimulus is a red cross on a black background, and the resultant after-image is projected on a white wall, the image will be darker than the background, and accordingly negative; if the image is projected on gray, it may be darker or lighter than the field and either negative or positive. In either case the image may be of the same color as the original or of a different color. Four different kinds of images must then be distinguished, viz.:

Positive	{ same-colored.	Negative	{ same-colored.
	{ other-colored.		{ other-colored.

Wundt's definitions, (1) that the image is positive when it is of the same or greater intensity than the stimulus, and negative when it is of lesser intensity;<sup>1</sup> and (2) that positive images are same-colored and negative, are other-colored,<sup>2</sup> have not been accepted by the most recent and best writers.

<sup>1</sup> Grundzüge der Physiologische Psychologie, 4 Ed., I., 513.

<sup>2</sup> Human and Animal Psychology, Eng. trans., p. 109.

## PART I. EXPERIMENTAL.

SECTION I. APPARATUS AND METHODS.—The apparatus used throughout the series on threshold, latent period and duration was that used by Fullerton and Cattell,<sup>1</sup> adapted by the writer for the present purposes. The accompanying illustration shows the instrument from the standpoint of the experimenter.

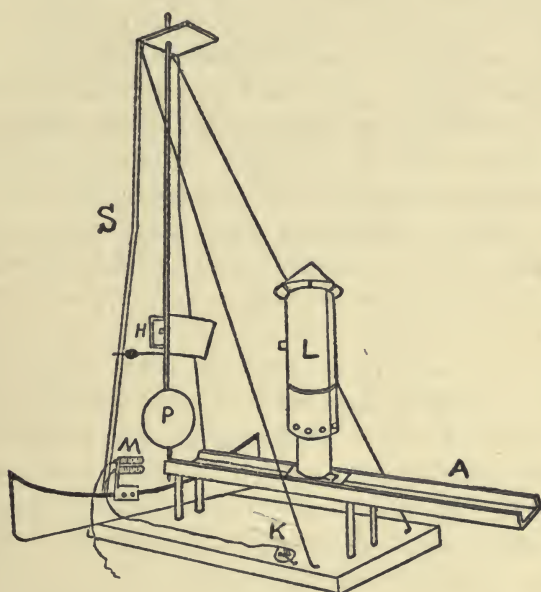


FIG. I.

The apparatus consists of a vertical screen (*S*) with an adjustable opening (*H*), of a long arm (*A*) and of a screen pendulum (*P*). At the opening (*H*) arrangements were made for inserting ground-glass plates for equalizing the illumination of the stimulus and for rapidly changing the areas. The long arm (*A*) carried a kerosene-lamp (*L*) (in the experiments upon duration and latent period an electric arc lamp), the changes in intensity being made by moving the source of light toward and from the observer. On the pendulum (*P*) was a screen, which

<sup>1</sup> Perception of Small Differences, p. 135; University of Pennsylvania Press, 1892.

in its usual position, as the pendulum is held up by the electro-magnet ( $M$ ), shuts off the light from the area to be illumined at ( $H$ ). When the pendulum is released, however, the light shines on the area for one second, and, returning, it is caught up by the electro-magnet. The key ( $K$ ) was used to make and break a circuit to the electro-magnet ( $M$ ) for releasing and holding the pendulum. Before the upright screen ( $S$ ) is a tachistoscope (not shown in the figure), with drop screens to give a light-stimulation of  $\frac{1}{1000}$ ,  $\frac{1}{100}$ , or  $\frac{1}{10}$  second. The whole instrument was placed in a dark-room before a cabinet with an opening opposite ( $H$ ). All experiments, unless otherwise noted hereafter, were made in a dark-room. The subject remained inside the dark-cabinet for from ten to fifteen minutes for adaptation before any experiments were made. Then, with the eyes on a level with the stimulating light, the area was uncovered, covered again, and the subject reacted in the manner appropriate to the series of experiments.

SECTION 2. THE THRESHOLD.<sup>1</sup>—Not every stimulus is followed by a sensation; a sound may not have the requisite intensity; the weight be not sufficiently heavy; the light not large enough or long enough continued. In like manner minimal amounts of energy, time and space stimulation will fail to produce an after-image. Some questions immediately suggested are: What intensity of light will produce the after-sensation? How large an area is necessary to get an after-image? How long must the stimulus last to leave its effect? Then, we may ask whether or not there is a relation between the varying changes in the light, whereby a small area may be counterbalanced by a longer stimulation or by a greater intensity, and *vice versa*.

*Time*.—As noted above, three variables were used, viz., time, area and intensity. With a fixed area and intensity, what time must the light stimulate the eye to produce an after-image? The lamp placed at 25 cm. from the screen  $S$ , giving an intensity of  $\frac{2}{25}$  c.p., and the area 64 sq. mm., were used

<sup>1</sup> Sec. 2 is largely reprinted from an article on The After-image Threshold; *Psychol. Rev.*, II., 130-136 (1896).



as constants throughout this series. The time was varied from  $\frac{1}{1000}$  to 1 second. Two intermediate steps were used,  $\frac{1}{100}$  and  $\frac{1}{10}$  second.

Adaptation completed, the subject sat in position, a stimulus was given, and the subject announced the appearance or non-appearance of an after-image. Usually thirty to forty minutes were taken for each day's experiments, the series being stopped before fatigue became apparent. For each subject the number of experiments upon each variation in time was one hundred. Where a greater or less number were made the small numbers in parentheses in the appropriate columns of the tables show how many tests of this kind were made. The experiments were all made with the eyes open, so as not to disturb the after-image. The subject's eyes were at the normal distance (about 30 cm.) from the stimulus; his head was steadied by a support. Rests were taken between the separate experiments to allow any trace of the previous image to disappear, a signal was given, five seconds were allowed for preparation and the light was shown.

The intensity of the light was very constant. The lamp was trimmed before the whole series of experiments; and the photometric determinations made before, during and after a sitting showed only the variations likely to occur in any series with the photometer used (Bunsen's). A fixation-point could not be secured throughout the experiments. At first this was a somewhat disturbing factor; but as the light used as a stimulus was so small and the after-image so indistinct, it was judged best not to have a fixation-point of light, owing to the confusion likely to result from mistaking it for an after-image. With practice, however, the observer learned to look for the stimulus in the proper direction; and in the case of the writer in not over 5 per cent. of the times was it necessary to focus the eyes consciously after any part of the light was seen.

The following table shows the results of the experiments for time obtained from two observers, C, an advanced student of psychology, and F, the writer, upon whom the experiments were made by another worker familiar with the apparatus. The table shows on the first lines the percentages of times that after-images appeared. The results were also grouped in



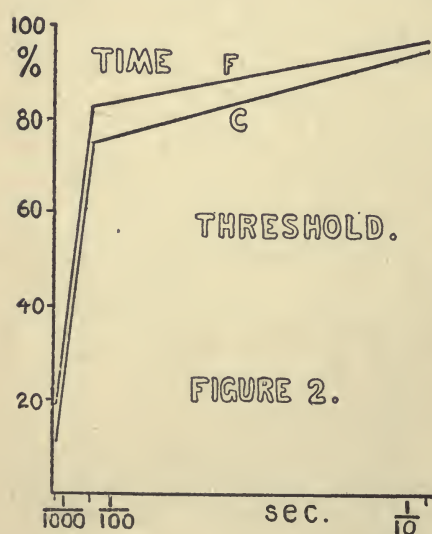
series of tens, and the variations of these groups were calculated. These variations are shown on the second lines, marked A. var.

TABLE I.—TIME.

Time in sec. Subjects.	1	$\frac{1}{10}$	$\frac{1}{100}$	$\frac{1}{1000}$
C. { Per cent. A. var.	97 3.5(70)	95 7.	75 17	12 10.8
F. { Per cent. A. var.	100 —	97 4.2	82.5 8.3(120)	19 7.4
Average.	99	96	79	15.5

As noted above, 100 experiments of each kind were made, except where another figure is shown in parentheses. Thus, for F, with the stimulus  $\frac{1}{100}$  second, 120 experiments were made; while for C, using one second stimulation, only 70 trials were made.

The accompanying curve shows the results graphically—the abscissa denoting divisions of time, the ordinate the percentage of times after-images appeared. Only the points for  $\frac{1}{1000}$ ,  $\frac{1}{100}$ , and  $\frac{1}{10}$  seconds are shown.



The shape of the curve indicates what might have been expected in accordance with the results of experiments upon other time-phenomena of vision. With the shortest time after-images seldom appeared; then with a slight increase in the duration of the stimulus there was a rapid rise in the number of appearances, followed by only a slight increase for longer stimulation.

The same character of curve is found in investigating the time it takes to see a light or a color.

When shown for a very short time the color can never be seen ; but as the time is slowly increased, it becomes very plain, but is seen no oftener and becomes no plainer if the time be further increased. In both cases (recognizing a color and seeing after-images) the stimulus at first is insufficient to make the sensation appear above the threshold of conscious experience. This threshold is then passed suddenly, and the stimulus soon reaches a point at which an after-image or the color (as the case may be) is always seen, unless numerous distractions of great intensity interfere.

✓ This effect of increase in the time of stimulation is quite in accord with the commonly accepted theory regarding the after-image: that the phenomenon, at least in its positive form, is due to inertia of the retina. From this point of view, the longer the light works upon the retina—the greater the amount of energy used—the greater will be the effect on consciousness.

✓ *Area.*—A corresponding, though not a proportionate, increase in the number of appearances was noted when the area was varied from  $\frac{1}{16}$  to 64 sq. mm. Six areas were used—squares of  $\frac{1}{4}$ ,  $\frac{1}{2}$ , 1, 2, 4 and 8 mm. on each side. These were made on ground glass, opaque paper being glued over all but the small area wanted. The error in making the larger sizes was less than  $\frac{1}{4}$  mm., which amount is inappreciable in comparison with the whole area. ✓ Difficulty was experienced in making the areas under 1 mm.; but from numerous ones made the best were chosen. In those chosen the error was not more than  $\frac{1}{10}$  mm. The smallest areas were measured with a magnifying-glass, and those were chosen in which the errors compensated each other.

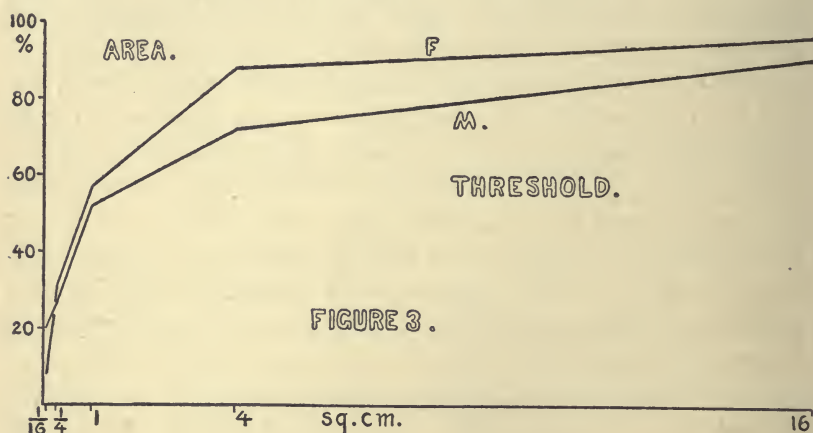
✓ The intensity of the light was kept at  $\frac{2}{25}$  c. p., and the time of stimulation was one second. With these constant, an after-image was very seldom seen when the smallest area was used, while the largest square produced an image at all times. The same precautions that were observed in the experiments upon the effect of time-changes were taken in this series and in all the experiments that are described later.

The following table and accompanying curve give the results obtained. The figures in the table are as in Table I.:

first are given the changes in stimulation, then the percentage of times the image was seen under these conditions and then the average variations of the experiments when grouped in sets of ten. The abscissa of the curve denotes the relative amount of the stimulus; the ordinate shows the percentage of times after-images were seen.

TABLE II.—AREA.

Area in sq. mm. Subjects.	64	16	4	1	$\frac{1}{4}$	$\frac{1}{16}$
M. { Per cent.	100	90	72	52	27	20
{ A. var.	—(50)	7.5(80)	14.8	12.4	9.2	6
F. { Per cent.	100	96	88	57	31	8
{ A. var.	—	4.8	8.8	13	15.4	8
Average.	100	93	80	54.5	29	14



Some preliminary work in which a different standard of intensity was used— $\frac{1}{20}$  candle power—shows the same effect of area-increase. These experiments were made before the general line of work was fully planned. Seventy experiments were made with each area, and the writer was the only subject. The results, because of their confirmatory character, are appended:

TABLE III.—AREA.

Area in sq. mm.	64	16	4	1	$\frac{1}{4}$
F. { Per cent.	96	89	67	41	19
{ A. var.	5	10	14.7	10	9.9



*A priori*, one might not expect such an increase in the number of appearances to follow an increase in the size of the stimulus. For, with intensity and a time of stimulation constant, one might well say that the structure of the eye is such that only the number of stimulated elements, not the stimulation of the individual elements, changes with any increase in size of the physical stimulus. This is undoubtedly true, and the result is paralleled by the results of experiments upon the extensive threshold of vision. A color or a light cannot be seen when only a small area is present; but on increasing the size it is readily perceived. This may be analogous to the so-called 'summation process,' or it may and probably can be resolved into a matter of the attention. As a matter of fact, the largest area may not produce any more after-images than the smallest. The apparent difference may be the result of inability of the smallest sensations to fix the attention.

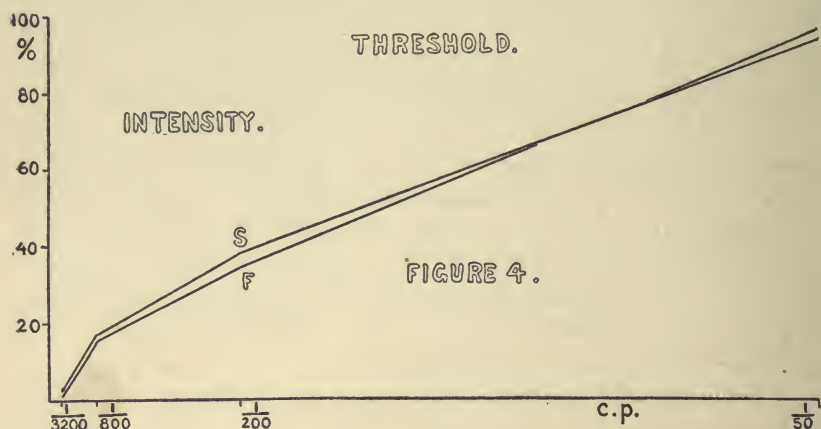
*Intensity.*—A similar series was made changing the intensity of illumination. The lamp was moved along the arm (A) of the apparatus (see Fig. 1) so as to give intensities varying from  $\frac{2}{25}$  to  $\frac{1}{3200}$  c.p. Three intermediate steps were chosen for convenience— $\frac{1}{50}$ ,  $\frac{1}{200}$  and  $\frac{1}{800}$  c.p. The amounts of energy in these experiments had an effect corresponding to what was found for the changes in the time and in the area of stimulation. The results obtained and a graphic representation follow:

TABLE IV.—INTENSITY.

Intens. in c.p. Subjects.		$\frac{2}{25}$	$\frac{1}{50}$	$\frac{1}{200}$	$\frac{1}{800}$	$\frac{1}{3200}$
S.	Per cent.	100	94	48	17	2
	A. var.	— (80)	7	25 (110)	20.8 (110)	3.6
F.	Per cent.	100	96	44	15.5	1
	A. var.	—	5.4	19.5 (130)	13.5 (130)	1.8
Average.		100	95	46	<del>46</del> 16.2	1.5

Two results in Tables I., II., III. and IV. should be noted: (1) the close correspondence of the observers, and (2) the comparatively large variation for the different individuals. The first of these observations is, I believe, of little significance.

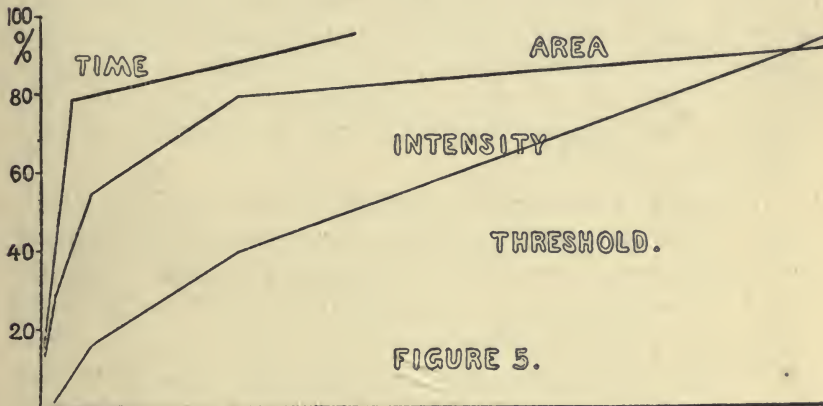
The great variation, however, seems to point to a considerable mental influence affecting the appearances. This will probably be more clearly brought out in the discussion of the experiments upon the influences determining the duration of the after-image.



*Summary.*—Experiments were made upon four trained subjects to find the amount of time-, area- and intensity-stimulation that would produce an after-image (conscious). It was found that to see an after-image 75 per cent. of the times the eye is stimulated, it is necessary to have as stimulation (1) for  $\frac{1}{100}$  second and 64 sq. mm. surface of light, an intensity of  $\frac{2}{25}$  c. p.; or (2) for one second exposure, an area of 4 sq. mm. and an intensity of  $\frac{2}{25}$  c.p.; or (3) for 1 second exposure, an area of 64 sq. mm. and a light of  $\frac{1}{100}$  c. p.

The average results for the three series are indicated in the following curve, which gives a general view of the relative effects of the physical units. It will be seen that the effects of time, intensity and extensity-changes are varied. This shows that for physiological or mental processes the units of physical energy do not counterbalance. The compensating relation seems to be a rather complex one. In its simplest form this relation may be stated as follows: squaring the time equals doubling the intensity or quadrupling the area; and *vice versa*, reducing the area to one quarter equals halving the intensity or taking the square root of the time.

Based upon the well-known phenomena of color-mixing by means of Maxwell's discs, the objection may be raised to these conclusions that lights of much less intensity produce after-images, inasmuch as colors fuse in any illumination. While this is true, it should not be forgotten, on the other hand, that the after-images producing the fusion are not consciously perceived as after-images. The usually low intensity of light in conjunction with a short time of stimulation does not produce an after-image that can be consciously noted. Moreover, the overlappings of images and succeeding stimuli produce a total effect which lacks many of the distinctive features of the simple after-image.



During the three series of over 3000 experiments only five times were negative after-images seen. These were noticed only with the largest area, the longest time and the greatest intensity, and always toward the close of an hour's session after the eyes had been stimulated forty or fifty times. This fact would seem to support the hypothesis that the negative after-image is due to eye-exhaustion.

SECTION 3. THE LATENT PERIOD.—That a light does not immediately produce an after-image is now well known. The observation seems to have been made first by Young,<sup>1</sup> in 1872,

<sup>1</sup>C. A. Young, Note on Recurrent Vision; *Taylor's Philos. Mag.*, 4 Ser. XLIII., 343-345, 1872.



who, in using a Holtz machine which gave a large spark, noted that if a single spark was made any conspicuous object in the darkened room was seen at least twice—with an interval of a trifle less than a quarter of a second—the first time vividly, the second time faintly; often the object was seen a third and sometimes (but only with great difficulty) even a fourth time. This interval between the sensation and the after-image has been called the latent period.

Young's observation seems to have been quite forgotten for several years till the appearance of Bidwell's note on the image following an illumined vacuum tube.<sup>1</sup> Bidwell found that, if a Geissler tube be revolved at the rate of once in three seconds, the tube is followed at a distance of about forty degrees by a ghost-like image of the original. If the rotation stopped, the ghost moved on, merging finally with the tube. The latent period in this case was about one-third of a second. The observation was repeated upon about ten people, with approximately the same results.

Independent observers<sup>2</sup> at the same time announced variations in the experiment, and the suggestion was made that the intermittent appearance of lightning might be due to a similar cause. The effect may be produced in a simple manner with a match or a piece of glowing coal revolved by hand. The ghost-image is seen to follow close after the stimulus. After some experience even a pencil or a bright-colored book when moved shows the phenomenon.

Later and more exact measurements of the appearances have been made by Bidwell,<sup>3</sup> Charpentier<sup>4</sup> and Hess.<sup>5</sup> Charpentier repeated Bidwell's observations and noted a second appearance after the sensation, as Young and Bidwell had. This, he adds, is easier to note in indirect than in direct vision. A similar os-

<sup>1</sup> S. Bidwell, On Certain Spectral Images Produced by a Rotating Vacuum Tube; *Nature*, XXXII., 30-31, 1885.

<sup>2</sup> Davis, Laurin, Newall and others.

<sup>3</sup> On the Recurrent Images following Visual Impressions; *Proc. Roy. Soc.*, LVI., 132-145, 1894.

<sup>4</sup> Réaction oscillatoire de la rétine sous l'influence des excitations lumineuses; *Arch. de Physiol.* (Ser. 5), IV., 541-553, 1892.

<sup>5</sup> Untersuchungen über die nach kurzdauernder Reizung des Sehorgans; *Archiv f. die gesammte Physiol.*, XLIX., 190-208, 1891.

cillatory effect was found by Charpentier in using a black disc with a comparatively small white sector. Revolving the disc once every second or every two seconds, and fixating the center, the white sector was seen to be crossed in places by dark bands. These seem closely related to the disappearances and the reappearances of the after-image, but need more careful study in the light of Exner's work upon the progress of a visual sensation.

Bidwell's later work is largely a repetition of his earlier observations, but using spectrum colors instead of the illuminated Geissler tube. A spectrum, revolving every  $1\frac{1}{2}$  seconds, was thrown on a screen and the ghost-image observed. The ghost followed the spectrum at a distance of about 50 degrees. The latent time was 0.2 second. It is interesting to note that Bidwell observed no image following red or violet light. The recurrent image of an illumined slit in an opaque disc was found to follow after 0.15 second. Another disc, 15 cm. diameter, having two opposite radial slits  $1\frac{1}{2}$  mm. wide, was then rotated one turn a second before illumined ground glass. As observed from a distance of  $1\frac{1}{2}$  meters, each slit seemed to give four and possibly five luminous images arranged like the ribs of a fan. The usual blue recurrent image could also be seen following the images of the radial slits at an angle of 80 degrees. Throughout, the eye was fixed upon the center of the disc. This observation is readily confirmed, but seems not to be understood. The fact that Bidwell speaks of the oscillations and the ghost separately seems to indicate that he did not believe they were similar in character. Even if we admit Charpentier's contention of an oscillatory process in the retina, which makes a white disc at any phase seem alternately dark and light, we could not admit it for the dark opaque disc of Bidwell nor for the after-image accompanying it.

One fact seems to have escaped notice—at least it has not been recorded. In the simple experiment with a burning coal or with a Geissler tube the ghost follows the stimulus; but, in addition, there is an after-image of the path taken by the light. In this image the oscillations described by Bidwell do not occur. None of the fluctuations usually apparent in the after-image are noted in this case, the image gradually fading away.

Experiments of a different character have been made by Hess. In his work moving objects were not used, momentary illumination of a stationary stimulus taking the place of the revolving discs. A photographic shutter gave exposures of  $\frac{1}{100}$  to  $\frac{1}{200}$  second. Hess discovered a negative after-image of very short duration following immediately upon the termination of the stimulus. This preceded what is commonly known as the positive after-image.

The nature of the experiments in the present work is more nearly related to the work of Hess than to that of Charpentier and Bidwell. A stationary light was used and the reaction-time taken. As in the work on threshold, I endeavored to find the relative effects of varying stimuli in the three respects of intensity, time and area. The apparatus used was essentially the same as that described in Section 1. The kerosene lamp, however, was replaced by an arc light of about 1000 c. p. The intensities, as given to the subjects, were 1,  $\frac{1}{10}$  and  $\frac{1}{100}$  c. p.; these were obtained as in the threshold-experiments.  $\frac{1}{4}$ , 1

and 4 sq. cm. in squares were used as areas, and the varying times were  $\frac{1}{100}$ ,  $\frac{1}{10}$  and 1 second. As constants were used  $\frac{1}{10}$  c. p., 1 sq. cm. and 1 sec. Only one of the factors was varied at any one time. A chronoscope was placed in series with a reaction key and the pendulum (*P*) and the drop tachistoscope. The observer was instructed to react the instant the after-image appeared, and the time between the end of the stimulus and the beginning of the after-image could thus be read direct from the chronoscope.

# sec. LATENT PERIOD

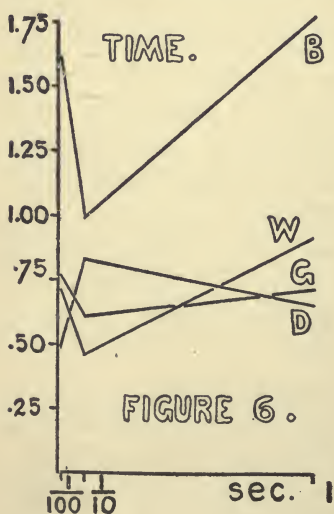


FIGURE 6.

The individual variation is quite large, owing to the mental attitude of the subject, one reacting to the first approximation



of an after-image, another waiting till he was certain that the appearance was what he sought. Although his attitude was constant with each individual, we shall not be able to compare the subjects, and to group them for an average representing a typical result. So also the individual attitude toward the stimuli made other variations in the results. To one the stimuli of greatest intensity were too bright; at times another could scarcely get an after-image with the smallest area.

Accompanying will be found the tables and curves for the average results for time-, area- and intensity-experiments upon four subjects. Twenty experiments were made upon each variable. In the tables the average time between the stimulus and the first appearance of the after-image is first given, and this is followed by the average variations of the twenty experiments.

TABLE V.

TIME : constants,  $\frac{1}{10}$  candle power, 1 sq. cm.

Stimulus in Seconds. Subjects.	.01		.1		1.0	
	Aver.	A. Var.	Aver.	A. Var.	Aver.	A. Var.
B.	1.62	.42	.99	.12	1.76	.74
D.	.49	.19	.83	.35	.65	.11
G.	.76	.22	.61	.23	.71	.18
W.	.71	.13	.46	.14	.92	.20
Aver.	90		72		1.01	

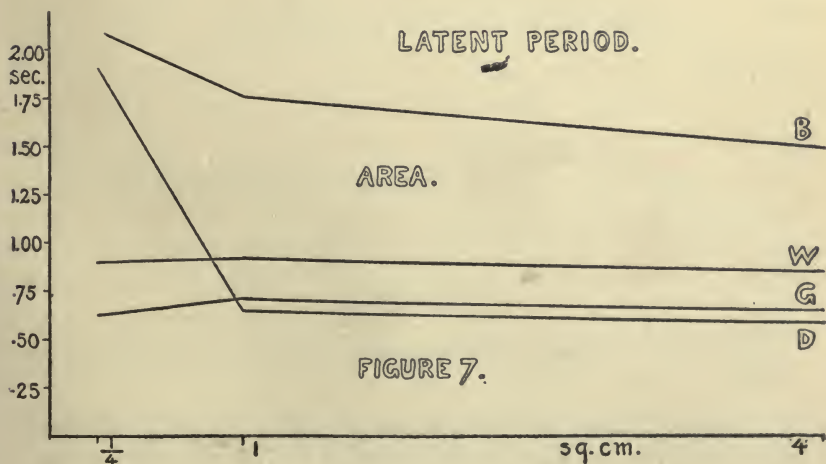


TABLE VI.

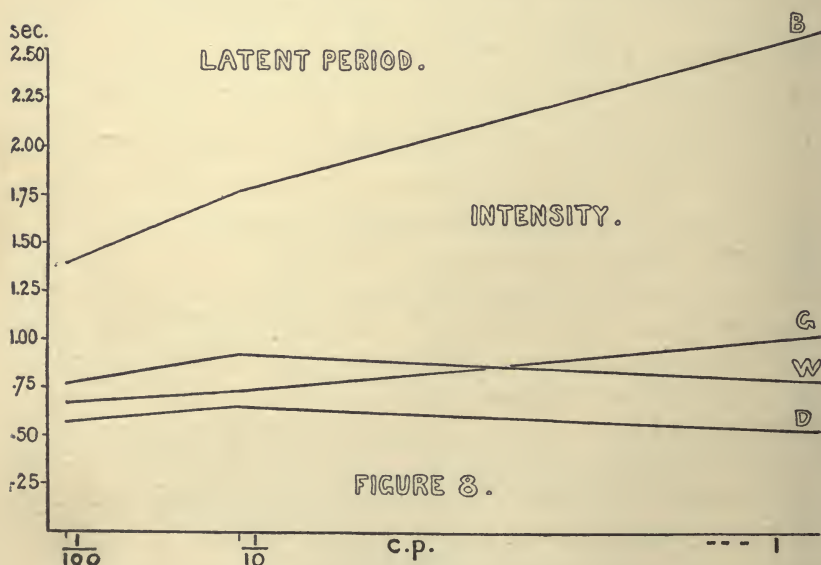
AREA: constants, 1 sec., and  $\frac{1}{10}$  c. p.

Area in sq. cm. Subjects.	$\frac{1}{4}$		I		4	
	Aver.	A. Var.	Aver.	A. Var.	Aver.	A. Var.
B.	2.11	.65	1.76	.74	1.50	.46
D.	1.91	1.25	.65	.11	.58	.07
G.	.63	.13	.71	.18	.65	.11
W.	.90	.15	.92	.20	.86	.13
Aver.	1.39		1.01		.90	

TABLE VII.

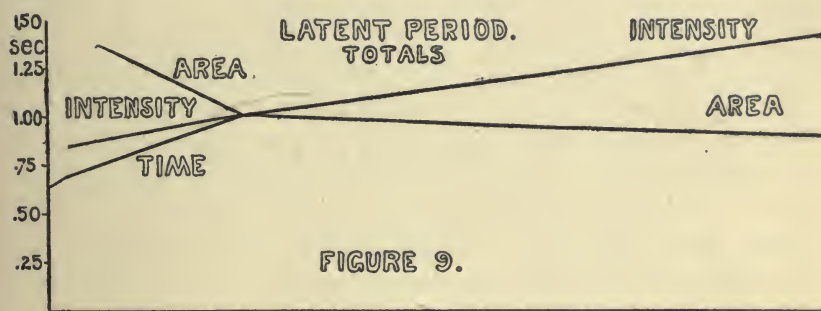
INTENSITY: constants, 1 sec., and 1 sq. cm.

Intensity in c. p. Subjects.	.01		.1		1.0	
	Aver.	A. Var.	Aver.	A. Var.	Aver.	A. Var.
B.	1.39	.34	1.76	.74	3.36	1.58
D.	.57	.08	.65	.11	.51	.14
G.	.67	.12	.71	.18	1.09	.33
W.	.77	.13	.92	.20	.72	.11
Aver.	.85		1.01		1.42	



*Summary.*—While the grouping and averaging of these diverse figures cannot be well justified, I have done so because,

in a general way, the figures of the averages seem to show what ordinarily would be expected. With the lesser times the eyes have not time to move and to get a sensation on many contiguous portions of the retina. Accordingly when the time of stimulation is short the after-images are clearer and more easily recognized. The larger areas have the advantage of giving after-images of considerable size, which attract the attention sooner than the smaller ones; while the great intensities seem to be so blinding in their effect that the image is very hard to recognize. I append a curve showing the comparative effects of the variables, but with full knowledge that its meaning should not be taken as absolute, but only as an indication of the results that a much larger number of individuals may approximate.



In the existing theories explaining the phenomena no reason is noted for any variation in this latent period. It seems probable that an explanation must be looked for in the mental attitude of the subject. Such an explanation would not only account for the great individual differences, but would also account for the great variation of the same individual under precisely the same physical conditions. The subject G, who is distinctly visual, accustomed to note variations in color, etc., apparently recognized the image sooner than any of the others who were not so trained. This mental attitude will also explain why the after-image of the largest areas was seen sooner than that of the smaller one. The attention in this case was probably attracted sooner to the large object. The conditions attending the changes in latent time for intensity- and time-ex-



periments are not so evident. In the intensity-experiments the subjects B and G, for whom the 1 c. p. stimulus was too strong, dominated thereby the totals. Considered in connection with the results of the experiments upon duration (Section 4), it seems likely that the attention is a predominant factor in all measurements of the phenomena.

This view is further strengthened if the average variations under the different conditions are considered. Usually those conditions which the subject considered favorable show the least variation. Thus, B felt that the  $\frac{1}{100}$ -second exposure was too short a time for him to see the after-image, and the great variation shows that there must have been some great disturbing factors. The same occurred with both G and B under stimulation of 1. c. p., which was considered by both as too strong.

— SECTION 4. DURATION.—The earliest accounts of the duration of after-images seem to be those given by Boyle and by Newton. In his work on color, Boyle relates that a man who had looked through a telescope at the sun without a protecting medium had his eyesight so injured that thereafter whenever he looked toward windows or other bright objects he fancied he saw a globe of light about the size of the sun. This appearance was a source of trouble to the man for nine or ten years.

To Locke, who had called his attention to this interesting observation, Newton wrote an account of a similar personal experience. The letter, which contains further observations of interest, is quoted fully:<sup>1</sup>

“The observations you mention in Mr. Boyle’s book of colours, I once made upon myself with the hazard of my eyes. The manner was this: I looked a very little while upon the sun in a looking-glass with my right eye, and then turned my eyes into a dark corner of my chamber to observe the impression made, and the circles of colours which encompassed it, and how they decayed by degrees, and at last vanished. This I repeated a second and a third time. At the third time, when the phantasm of light and colours about it were almost vanished, intending my fancy upon them to see their last appearances, I found, to my amazement, that they began to return, and by little and little to become as lively and vivid as when I had newly looked upon the sun. But when I

<sup>1</sup>D. Brewster, *Memoirs of the Life of Sir I. Newton*; Edinburgh, 1855, Vol. I., p. 236:

ceased to intent my fancy upon them they began to vanish again. After this I found that as often as I went into the dark and intended my fancy upon them, as when a man looks earnestly to see anything which is difficult to be seen, I could make the phantasm return without looking any more upon the sun; and the oftener I made it return the more easily I could make it return again. And at length, by repeating this without looking any more at the sun, I made such an impression upon my eyes that, if I looked upon the clouds, or a book, or any light object, I saw upon it a round bright spot of light like the sun, and, which is still stranger, though *I looked upon the sun with my right eye only, and not with my left, yet my fancy began to make an impression upon my left eye as well as upon my right.* For if I shut my right eye, or looked upon a book or the clouds *with my left eye, I could see the spectrum of the sun almost as plain as with my right eye,* if I did but intend my fancy a little while upon it; for at first if I shut my right eye and looked with my left, the spectrum of the sun did not appear till I intended my fancy upon it; but by repeating this appeared every time more easy. And now in a few hours' time I had brought my eyes to such a pass that I could look upon no bright object with either eye, but I saw the sun before me, so that I durst neither write nor read; but to recover the use of my eyes shut myself up in my chamber made dark for three days together, and used all means to divert my imagination from the sun. For if I thought upon him I presently saw his picture, though I was in the dark. But by keeping in the dark, and employing my mind about other things, I began in three or four days to have some use of my eyes again; and by forbearing to look upon bright objects recovered them pretty well, though not so well, but that, for some months after, the spectrum of the sun began to return as often as I began to meditate upon the phenomena, even though I lay in bed at midnight with my curtains drawn."

Several interesting observations are here noted. The one of most importance seems to be that regarding the effect of imagination and attention upon the after-image. This will be discussed later; as will also the one regarding the transfer of the image from the right to the left eye.

The philosopher Tetens, who seems in some way to have anticipated the modern idea of mental measurement, mentions that he had investigated the duration of the after-image. Only a few words of description are given, and details were left for a future paper, no record of which I have been able to find.

Some few records of the duration are to be found in Fechner's observation of the color-changes, but the material is so slight as to be of little value.

— Helmholtz notes that the duration and intensity of the stimulating light affect the duration and character of the resultant after-image: "The greater the intensity of the primary light the brighter is the positive after-image and the longer it con-

tinues."<sup>1</sup> "Greater intensity of the primary light gives the negative after-image greater clearness and duration."<sup>2</sup>

The most extended observation of the changes occurring in connection with the duration of the phenomena seem to be those by Titchener.<sup>3</sup> Used primarily for another purpose (see Section 8), the observations are of considerable value in this connection, because they were made upon subjects naïve as to the present problem. Dr. Titchener asked his subjects to record the duration of various after-images in a stimulated eye. These experiments, though few in number for the single observers, show in general the same effect as the more extended series by myself. The conditions were as follows: The subject was placed before a screen with a variable sized opening, which was to be illumined by a lamp. The photometric determination of the light is not given. The area- and time-stimulations were changed and the records taken. The light used was made vari-colored by the use of gelatin plates; white light was not used. The following table of results with red lights will illustrate the general tendency, and will act as a basis for comparison with the present work. In this table, which I have constructed from the individual records as given by Titchener, will be found the averages of at least three observers for each series. The maximum number of experiments made under any con-

TABLE VIII.

AREA OF STIMULATION IN MM.				
STIMULATION IN SECONDS.		30	40	50
	5		9.9 (14)	27.2 (9)
	10	12.5 (14)	17.4 (16)	26.7 (8)
	15	16.3 (15)	22. (14)	29.7 (7)
	20	19.1 (10)	23.9 (15)	39. (8)
	25	21.9 (11)	30.1 (10)	30.8 (7)

<sup>1</sup> Handbuch der Physiol. Optik, p. 503.

<sup>2</sup> Ibid., p. 505.

<sup>3</sup> E. B. Titchener, Ueber binoculare Wirkungen monocularer Reize; *Philos. Stud.*, VIII., 231-310 (1892).



dition by a subject was six, the minimum two. In parentheses will be found the number of experiments used in making the averages. The other figures give, in seconds, the average total duration of the after-image.

The individual variations from these averages are comparatively large, although each individual's results are quite constant. The variability is about the same as was found in my experiments upon the effect of time-, area- and intensity-changes.

The present series took its rise, however, largely from two incomplete minor studies made at the Columbia Psychological Laboratory, 1891-93. These investigations were begun by Mr. L. V. Southack and Miss E. G. Seebring, respectively; but the results and methods were unknown to the writer till after the present series was begun. The results, which have remained unpublished, I give in the appropriate place, with the corresponding present series. The method employed by both experimenters was to use an incandescent lamp, enclosed in a long box, to illumine a ground-glass Greek cross in the front of the box. Each arm of the cross was one square centimeter. Variations in intensity were obtained by changing the position of the lamp, and variations in time of stimulation were measured by means of a metronome. The results were written down by the observers after each experiment, the various fluctuations being noted as well as possible.

The apparatus for the present series was about the same as that used in the work on latent period; but for the chronoscope a kymograph was substituted. Upon this a continuous record of the after-image could be recorded. The time of stimulation was noted upon the drum, and the subject was instructed to close the key-circuit as soon as an after-image appeared, to keep it closed while the image lasted, and to break and make the circuit as quickly as possible when any changes in the character of the after-image took place. When any change was noted by the breaking or the closing of the circuit the subject told the experimenter its character, and this was indicated upon the kymograph-record. In this way not only the total, but the actual time of duration, the fluctuations and, to some extent, the latent

period could be determined. In this series the observations of the latent period were open to criticism owing to a slight degree of inaccuracy in recording the end of the stimulating light. Such an error was relatively large for the short times of the latent period, but was inappreciable for the comparatively long durations. As in the preceding work on threshold and latent period, the three physical units of energy were varied. A standard stimulus of 1 cm.,  $\frac{1}{10}$  c.p. and 5 seconds was chosen; and the variables were  $\frac{1}{4}$ , 1 and 4 sq. cm.,  $\frac{1}{100}$ ,  $\frac{1}{10}$  and 1 c.p., and 1, 5 and 10 seconds. It was found impossible to record the exact time of each fluctuation. The changes in the character of the image are not clear-cut, but there is a fading of one color into another. This could not be recorded as a distinctive color-change. Even the usually marked changes from the positive to the negative phases were not always clear-cut.

The following tables and curves give two results: the total time of the after-image—*i. e.*, the time from the end of the stimulus to the final disappearance of the after-image; and the actual time—*i. e.*, the total time minus the period when unseen. In the curves the total durations are denoted by the continuous lines, and the actual times by the broken lines.

*Time.*—The constants in these experiments were an intensity of  $\frac{1}{10}$  c.p. and an area of 1 sq. cm.

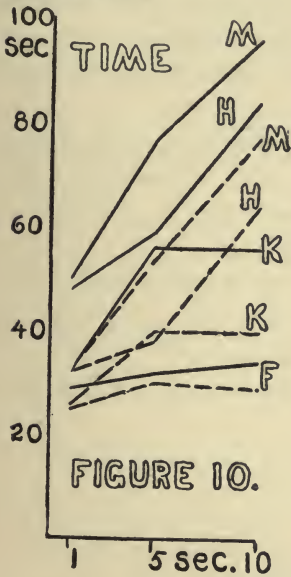
TABLE IX.  
TOTAL DURATION OF AFTER-IMAGE.

Time of Stimu- lation in seconds. Subjects.	1			5			10		
	Aver.	A.	Var. No. Exp.	Aver.	A.	Var. No. Exp.	Aver.	A.	Var. No. Exp.
K.	32		6	56		24	56		14
H.	48	10	10	59		15	84		27
F.	29		4	32		5	34		4
M.	50	13	10	77		29	96		30
Average.	39.8		50	56		50	67.5		50

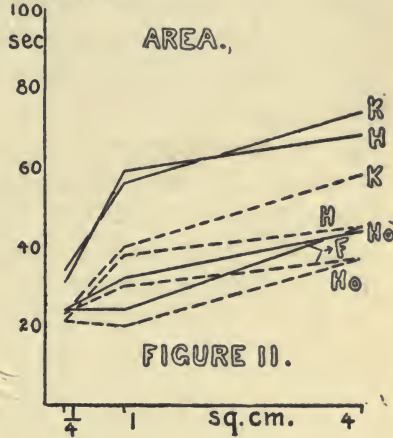
TABLE X.  
ACTUAL DURATION OF AFTER-IMAGE.

Time of Stimu- lation in seconds. Subjects.	I			5			10		
	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.
K.	26	4	20	40	12	20	40	9	20
H.	32	9	10	38	17	10	64	23	10
F.	25	2	10	30	5	10	29	3	10
M.	32	10	10	54	20	10	77	20	10
Average.	28.7		50	40.5		50	52.5		50

## DURATION



## DURATION



The corresponding series in Miss Seebring's experiments show the following results for the total time of duration:

TABLE XI.

Time of stimulation.	3			15			60		
	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.
First series.	21	9	5	47	6	5	107	5	5
Second series.	23	4	5	43	6	5	101	7	5
Averages.	22		10	45		10	104		10

Full records of the fluctuations were taken only for the second series, and the actual time that the image was seen can



be given only for these experiments. The following table shows the relation between the total and the actual duration :

TABLE XII.

Time of stimulation.	Total time.		Actual time.		No. Exper.
	Aver.	A. Var.	Aver.	A. Var.	
3 sec.	22.6	4.	10.9	3.9	5
15 sec.	42.6	6.	36.	4.4	5
60 sec.	101.2	7.	98.6	8.4	5

In Titchener's results the same effect is noted. The varied intensities and areas do not permit, however, of a direct comparison of the three series. But they have enough in common to show the rapid, but not proportionate, increase in duration due to the longer times of stimulation.

— *Intensity*.—With 1 sq. cm. and 5 seconds exposure as constants.

TABLE XIII.

TOTAL DURATION OF AFTER-IMAGE.

Intensity in c.p. Subjects.	.01			.1			1.0		
	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.
K.	40	12	20	56	24	20	70	19	20
H.	29	9	10	59	15	10	84	19	10
F.	26	6	10	32	5	10	50	3	10
Average.	31.8		40	49		40	68		40

TABLE XIV.

ACTUAL DURATION OF AFTER-IMAGE.

Intensity in c.p. Subjects.	.01			.1			1.0		
	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.	Aver.	A. Var.	No. Exp.
K.	28	6	20	40	12	20	52	9	20
H.	22	7	10	38	17	10	63	18	10
F.	25	3	10	30	5	10	50	3	10
Average.	25		40	36		40	55		40

Using a cross with 1 sq. cm. arms and 15 seconds exposure, Mr. Southack found for himself and a second observer the following results. The standard intensity, 1, was a 100 c. p.

incandescent lamp at a distance of 30 cm. from the ground-glass cross. The varying intensities were obtained in the usual manner.

TABLE XV.

TOTAL DURATION OF AFTER-IMAGE.

Relative Intensities. Subjects.	$\frac{1}{320}$			$\frac{1}{64}$			$\frac{1}{4}$			I		
	Av.	A.	No. Var. <sup>1</sup> Exp.	Av.	A.	No. Var. <sup>1</sup> Exp.	Av.	A.	No. Var. <sup>1</sup> Exp.	Av.	A.	No. Var. <sup>1</sup> Exp.
A.	8	5	20	10.6	1.5	20	11.2	1.7	20	60.7	10.7	20
B.	11.8	1.4	10	14.4	3.2	10	18.1	1.9	10	26.4	1	10

Area: constants, 5 sec. and  $\frac{1}{10}$  c. p.

TABLE XVI.

TOTAL DURATION OF AFTER-IMAGE.

Area of Stimulus in sq. cm. Subjects.	$\frac{1}{4}$			I			4		
	Aver.	A.	No. Var. Exp.	Aver.	A.	No. Var. Exp.	Aver.	A.	No. Var. Exp.
K.	34	15	20	56	24	20	74	24	20
H.	31	12	10	59	15	10	68	17	10
Ho.	24	3	5	24	4	5	45	15	5
F.	24	8	10	32	5	10	44	6	10
Average.	28		45	42.8		45	57.8		45

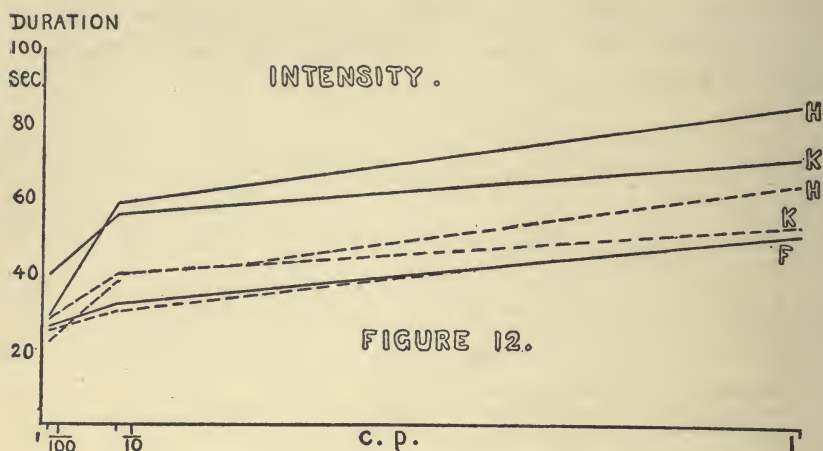
TABLE XVII.

ACTUAL DURATION OF AFTER-IMAGE.

Area of stimulus in sq. cm. Subjects.	$\frac{1}{4}$			I			4		
	Aver.	A.	No. Var. Exp.	Aver.	A.	No. Var. Exp.	Aver.	A.	No. Var. Exp.
K.	23	7	20	40	12	20	58	10	20
H.	21	10	10	38	17	10	45	9	10
Ho.	21	3	5	20	5	5	37	11	5
F.	23	5	10	30	5	10	37	11	10
Average.	22		45	32		45	45		45

The increase in duration for the greater intensities and the longer times of stimulation is easily explained on purely physiological grounds. In both cases more of the photo-chemical matter

<sup>1</sup> The average variation of the averages when the experiments were grouped in sets of five. This was the only calculation left by Mr. Southack. The records of the individual experiments could not be found.



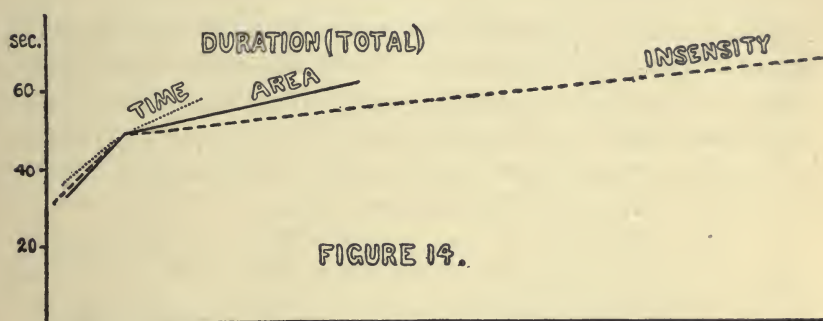
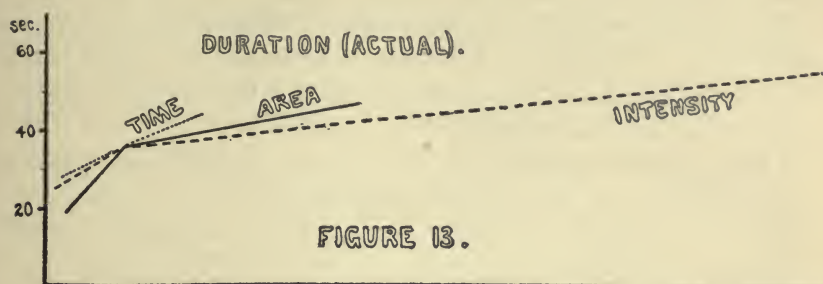
of the retina may be decomposed, and it takes the retina longer to come to a state of equilibrium. The effect of the larger areas, however, cannot be explained by such a simple process. In the latter case there is only an increase in the number of retinal elements stimulated, and not a greater stimulation of any one element. An explanation of the greater durations with the large areas must probably be looked for in another direction. In the threshold experiments the same effect was noticed, and it was concluded that the cause was mental. In this case, when the after-image is fading, a small light is hard to notice, and the large one, with its numerous points for the shifting of the attention, is easy to keep in view. The different durations for the large and the small areas is, accordingly, only an apparent one.

The relative effects of the physical units are shown in the accompanying curves, which are constructed from the averages of subjects H, K and F. The results from these three observers only were used because a slight variation due to the fourth subject would disturb the common point of coincidence in the three series. Fig. 13 represents the results of actual time of the after-image, while Fig. 14 shows the results for the total time.

Such a simple relation as was found in the threshold experiments between the units of physical energy seems not to



hold here. This is not surprising when it is considered that the longer time that the image is in view gives greater opportunity for outside influences to affect the process.



The constancy of the relation of the actual to the total duration should be noted. The average proportion is 76:100, while the variation for this is about 3.

*Quality of Light.*—If a spectrum projected on a wall is looked at for some time and an after-image obtained therefrom, it will be noticed, besides the qualitative differences, that the image of the central portion, the yellow and green parts, lasts relatively longer than that of the red or violet ends. This has sometimes been considered as evidence that the central portion of the spectrum is of greater intensity than the ends. This hypothesis seems at first sight to be the correct one; but at times it has not been sufficiently considered by investigators.

Bidwell<sup>1</sup> notes that while the after-image obtained from the

<sup>1</sup> Op. cit., p. 140.

whole spectrum was violet, he could get no after-image from the red and the violet of the spectrum. This fact he attempts to explain without considering the intensity differences, and he uses the observation to prove that the image is due to an excitation of the violet nerve-fibers only. However, the accuracy of Bidwell's observation has been questioned by Hess, who found that an image was produced by red lights whose wavelength was less than  $623 \mu$ .

Charpentier<sup>1</sup> found that the *persistance des impressions rétiniennes* was affected by the quality of the light; but though he believed the difference due to the differences in the intensity of the stimuli rather than to the quality of the light, his results were not reported in full.

In his work with transmitted light, Titchener<sup>2</sup> used various colored gelatins, which resulted generally in showing a longer duration for yellow than for red light. In these experiments, however, the relative intensities were not the same, and in this connection little value attaches to the results. The results obtained by Titchener for effects of other colors—violet, green, etc.—are incomparable for the same reason.

I have attempted to exclude the factor of illumination-differences by using gelatin films whose absorbent powers were known. The light was so arranged for each gelatin that the intensity was equal for all. The following table shows the results for stimulation of 5 seconds and an area of 1 sq. cm.:

TABLE XVIII.

	V.	B.	G.	Y.	O.	R.
Duration in seconds.	24	26	30	24	27	22
A. Var.	6	5	3	5	7	6

Ten determinations were made with each kind of light. On the first line will be found the average duration under the various conditions; and below, the average variations for each series of experiments. It will be noticed that practically no

<sup>1</sup> C. Rend. de la Soc. de Biol., Sér. 8, Vol. IV., p. 92, 1887.

<sup>2</sup> Op. cit.

difference is found to exist between the after-image-producing power of any colors. The variations make an overlapping of the times in such a way as to show that the slight differences noticed at first in the table are only apparent.

In this connection it would be interesting to test individuals who are partially or totally color-blind. Tests on such a class would probably be the only valid ones as indicating the part played by the quality and by the intensity of the light.

*Parts of the Retina.*—Purkinje, Aubert and Exner have noted independently that light-sensations continued longer in the fovea than on the periphery of the retina. These observations have been disputed by more recent investigators, however. The experiments, dealing only with the phenomenon of visual persistence, do not indicate what might be expected for other similar phenomena, particularly the longer continuations, such as after-images. In view of the fact that we can see less clearly with the peripheral portions of the retina,<sup>1</sup> and that these parts are more easily fatigued,<sup>2</sup> it seemed probable that the after-image would also be less distinct and would continue a shorter time. The supposition was proved correct by the results of the experiments.

Each of seven points along the inner side of the right eye were stimulated with a stationary light, and the total and the actual times of duration of the after-image were noted. As stimulus, I used a small lamp with an intensity of about  $\frac{1}{2}$  c. p.

TABLE XIX.

Angular distance from fovea.		0°	15°	30°	45°	60°	75°	90°
Total	Aver.	40	32	10	0?	0	0	0
Duration	A. Var.	8	9	4	—	—	—	—
	No. Exp.	10	10	10	5	5	2	2
Actual	Aver.	35	24	8	0?	0	0	0
Duration	A. Var.	7.5	9	5	—	—	—	—
	No. Exp.	10	10	10	5	5	2	2

<sup>1</sup> See Aubert, *Grundzüge d. Physiol. Optik*, 1876, p. 539 ff.

<sup>2</sup> *Ibid.*, p. 545.



This was looked at for about 5 seconds, and the angular distance subtended was about two degrees. The light was at all times at a constant distance from the eye (retina), viz., 30 cm. The table on page 29 shows the results of the experiment upon each of the points,  $0^\circ$  being the fovea.

— Not only were there differences in the duration of the after-image, but the fainter appearance of a sensation toward the periphery was duplicated in the after-images. No variations in color of the after-image corresponding to the different points tested was apparent.

This gradual, but finally absolute, lack of ability to distinguish an after-image is probably due to several factors. Aubert mentions that the periphery is more easily fatigued than the fovea; but he does not seem to consider that this may be partly mental and not entirely physiological. From observations made during the progress of the experiments it seems likely that the inability to attend to these things not in its immediate vicinity is the primary reason for the lack of images toward the periphery, and for the long durations at the fovea. At  $45^\circ$  it was felt at times that an image might be present, but so indistinct that it was impossible to give any definite answer. In the table these results are accordingly given as  $0$  (?).

A similar but more extended set of experiments upon the quality as well as the duration would be important for testing color theories, but this point of view was not considered till the present series had been finished.

*Individual Differences.*—Throughout the foregoing pages one or more individuals have been considered as giving typical results. That this is not strictly true, however, has been fully recognized; and from the material at hand I have endeavored to discover some of the individual conditions upon which the duration depends. The numerous difficulties in such a study are evident: each individual is not the same at any two moments, and various distracting thoughts cannot be excluded.

During the past few years there has been made at the Columbia laboratory a series of mental and physical tests. Among these was one on after-images, in which the duration, the color, etc., were noted. One great defect of these measurements is to

be found in the fact that there were many experimenters instead of one, making methods of experimenting and of recording to some extent dissimilar. In general, however, the results are fairly comparable, and are as accurate as the method of correlation used in this case. The individual differences of recording had little influence upon the measurement of the total time; but by some the actual time with the various fluctuations were not given at all. For this reason only the total time can be used as a basis for comparison.

The stimulus was a cross, with arms 1 sq. cm., illuminated by a 100 c. p. incandescent lamp in a box. The lamp was 30 cm. from a plate of ground glass, which was used to make the cross of uniform brightness. The subject was seated in a semi-dark room, and no adaptation time was needed. The cross was shown for ten seconds and then shut off by a screen. The subjects, having closed and covered the eyes without pressing upon them, informed the experimenter of the various phases as they occurred, and these were recorded as well as possible. Generally two trials were given.

The tests chosen for comparison with the results of the after-image test were sharpness of vision, color-vision and imagination-type. These were so chosen because they seemed closely allied to, or were thought to have an influence upon, the after-image. It would have been interesting to compare, in addition, visual and auditory memory, time of reaction to visual and to auditory stimuli, etc.; but at that time such tests were not included in the anthropometric series.

Sharpness of vision was tested with the Galton eyesight-tester. This is an instrument having at certain distances cards of numerals in diamond type which are to be read by the subject, who looks through a hole in a screen. The type was illuminated by a 100 c. p. incandescent lamp at a distance of one meter from the card, which was 37 cm. from the eyes. The normal distance for reading correctly eight or more out of ten numerals was for each eye 42-52 cm.

Color-vision was tested with spools of colored wools; but this, being only a rough-and-ready method of detecting color-blindness, did not show defects which did not amount to blind-

ness. Only six color-defectives were found, and their after-images may be paralleled by numerous other observers who were considered nearer normal. The results, accordingly, show no unusual variations.

The imagination-type was discovered by means of the following series of questions, part of which are the classic ones of Galton: Think of your breakfast-table as you sat down to it this morning; call up the appearance of the table, the dishes and food upon it, the persons present, etc. Then write answers to the following questions: Are the outlines of the objects distinct and sharp? Are the colors bright and natural? Where does the image seem to be situated? In the head? Before the eyes? At a distance? How does the size of the image compare with the actual size of the scene? Can you call to mind better the face or the voice of a friend? When violin is suggested, do you first think of the appearance of the instrument or the sounds made when it is played? Can you call to mind natural scenery so that it gives you pleasure? Music? The taste of fruit? Have you ever mistaken a hallucination for a perception; *e. g.*, apparently heard a voice or seen a figure when none was present?

Of the subjects who answered the questionnaire, 59% would be considered as of a distinct visual type, 4½% were distinctly auditory and 36½% would be considered of mixed type.

For comparing these results with the duration of the after-image, only the simple method of correlation was used. The results of the subjects that were considered as having hypernormal, normal or subnormal vision were grouped and the average duration of the after-image was calculated; a similar method was used for the imagination-type correlation. This method is not above criticism; but it is sufficiently accurate to show certain tendencies and influences. The results for the accuracy of vision in the right and left eyes, and for the type of imagination, are summarized in the following tables.

Objection may be made to this method of correlation, to the grouping the subjects and the use of one or, at most, two experiments, as giving a typical value for the individual. The objections would apply to all anthropometric series, and it has weight only when one attempts to apply results to individuals.



It is only in considering an individual as of a class that the figures have a value.

TABLE XX.

Acuteness of vision, right eye.	No. of cases.	No. of exper.	Av. durat. of after- image in secs.	Av. var.	Percentage of times no image was seen.
0-37 cm. Subnormal.	37	57	26		49
44-52 cm. normal.	63	92	37		27
61-85 cm. Hyper normal.	35	59	37		25

TABLE XXI.

Acuteness of vision, left eye.	No. of cases.	No. of exper.	Av. durat. of after- image in secs.	Av. var.	Percentage of times no image was seen.
0-37 subnormal.	37	57	32		46
44-52 normal.	76	106	34		26
61-72 hyper normal.	26	42	36		26

TABLE XXII.

Mental Type.	No. of cases.	No. of exper.	Av. durat. of after- image in secs.	Av. var.	Percentage of times no image was seen.
Visual.	77	121	36		26
Mixed.	47	65	37		32
Audile.	6	8	16		63

Probably the most interesting fact to be noted in the tables is not that the duration is longer for the visual type, or for the man with strong eyes, but that in these classes the total number of times when no image was noticed is small compared with the percentage for the audible type, or for those having weak eyes. — Granting that these variations are true criteria of the different classes, an explanation must be sought. It is easy to see why an individual of a visual type might have an after-image of longer duration than one of an audible type. Accustomed, as he is, to note principally those phenomena which appeal to his eyes, practice enables him to select the visual stimuli and to attend to these even if of little intensity. Accordingly, an after-image in its final stages, when very dim, would be noted by

him; whereas an auditory type of individual might neglect this minimal amount of sensation. A similar hypothesis would explain why audiles should see less images than visuals. Many of the images are undoubtedly of little intensity, and they would be unnoticed by those individuals unaccustomed to note small visual differences. This would reduce the differences to a question of the attention or of habit, rather than to a physiological process; and this explanation is in accord with what was noted above in the experiments upon the increase in the area of stimulation.

In a similar series made upon a class of seven boys by Dr. F. B. Brandt this explanation is further confirmed. Using a stimulation of 1 sq. cm. of the northern sky fixated for 15 seconds at a distance of 30 cm., Dr. Brandt found that those boys who held the highest rank in school-work, those who had learned to fix their attention, could see the after-image for a longer time than those who were dull and inattentive. The attention in this case may have been largely influenced by 'interest,' but the general result and conclusions are the same. The 'bright' boy is he who is interested in each novel experience; is he who attends with all his might to what is immediately present. During the course of the experiments this view was clearly indicated by a study of the actions of the subjects. A and B had been intellectual rivals throughout their school-work, and when B found that A was having after-images of longer duration than he, his interest flagged and it was difficult to persuade him to continue. While there are some variations in the relation between scholarship and the duration of the after-image, the results show that the three boys of excellent ability very easily outrank the other four. The following table gives the results of ten experiments upon each subject.

In conjunction with the attention, imagination undoubtedly plays an important rôle in these phenomena. Miss Washburn has noted that she was able to control voluntarily the colors of the after-image;<sup>1</sup> and in addition she mentions that when she visualized a color and tried to force the after-image to take this quality,

<sup>1</sup> M. F. Washburn, *Subjective Colors and the After-image*; *Mind*, N. S., VII., 25-34, 1899.

TABLE XXIII.

Subject.	Scholarship grade.	Duration of after-image in seconds.	Average variations.
A.	96%	320	36
B.	95	169	64
C.	92	267	65
D.	82	60	26
E.	81	69	51
F.	80	135	40
G.	79	42	31

‘the image of the visualized color was brought on sooner,’ and ‘held longer than usual.’ Miss Washburn concludes, as I do above, that the image is largely influenced by the attention, and that perhaps the imagination is to be explained as the calling the attention to any part of the image; the color is there, but it remains unnoticed till we are prepared for it. In this way the after-image would also appear to be in view a longer time.

*Miscellaneous.*—Besides these factors, other influences affect the duration of the phenomena. Helmholtz<sup>1</sup> noted that if an electric current be sent through the eye and the optic nerve, the image is changed in character and the time of duration is (probably) shortened. This change is analogous to that occurring when the eye is stimulated with light while an image is in view. Exner<sup>2</sup> found that the retinal circulation and pressure on the ball of the eye influenced the duration. Brewster<sup>3</sup> in 1834 noted that a smart blow on the head would stop an image in its course. Similar results were obtained from my subjects, but sometimes the image would also seem of less intensity. In the morning, when the eyes were fresh, the image lasted about 30% longer than in the evening or when fatigue was apparent.

— All these factors may be resolved into an influence of the attention on the one hand, or of the physiological process in the sense-organ on the other. Either explanation would suffice; but it seems that both should be considered. In conjunction

<sup>1</sup> *Physiol. Optik*, 509.

<sup>2</sup> Ueber die Funktionsweise d. Netzhautperipherie und den Sitz der Nachbilder; *Archiv f. Ophthal.*, XXXII. (2), 233-252, 1886.

<sup>3</sup> Accounts of Two Experiments on Accidental Colors; *Philos. Mag.*, 3 Ser., IV., May, 1834.



with the material of the foregoing paragraphs it seems likely that the mental element is very great—that is, the blow on the head or the general fatigue, the pressure on the eyeball, etc., are influences inhibiting the focussing of the attention upon the after-image. Particularly is this the case when these factors are brought in in an artificial manner that their effect may be observed. Voluntary winking, where the attention must be turned away from the sensation to the movement, shows the same effect.

— When the stimulus was not attended to, when the subject was distracted with a continued conversation, while all distracting influences ceased with the end of the stimulus, the duration and the intensity of the after-image seemed not affected. When however, distracting sensations, sounds and conversation were introduced during the progress of the after-image the attention was distracted sufficiently to cause an apparent shortening in the duration of the after-image by about 40%.

— *Summary.*—The after-image, particularly in its duration, is affected by many mental and physical conditions. The most influential of these seems to be the mental attitude of the subject. If he looks intently for the after-image, if he is accustomed to note visual changes, the image will apparently remain longer than if his attitude is more motor or auditory. When the attention is directed in its greatest intensity to the after-image, either from habit, because one is visual-minded, or voluntarily, the duration is about one-third longer than when the attention is not so directed. Various physical conditions—*e. g.*, having the image on the periphery of the retina, physical fatigue, etc., which give similar results—may also be considered in this class. Of the physical light-changes, the most influential seems to be the time of stimulation. This corresponds to what was found in the experiments on the threshold and latent period. The area- and intensity-changes also have effects similar to what was found in the experiments upon threshold and latent period; but no general relation of these effects seems to be discovered. Figures 13 and 14 show composites of the average results from Tables IX.–XVII., and from them the relative effects of the physical units may be noted. Contrary to

the generally accepted view, variations in quality (color) seem to have little effect unless there is, as usual, a change in intensity.

SECTION 5. FLUCTUATIONS.—In all discussions of the after-image one of the questions that has been largely neglected is that regarding the fluctuations from positive to negative, its disappearances and reappearances. Immediately upon the close of the stimulus an after-image due to the continuation of the light is seen. A vacant period (the latent time, see Sec. 3) follows, and then an image which in turn fades away into another color or disappears and reappears several times. This fluctuation was noted by R. W. Darwin,<sup>1</sup> and has been variously ascribed to many physiological causes connected with the sense-organ—to eye-movements, to winking, to pressure on retina, etc. During the progress of the present work I endeavored to note the various effects of these conditions. Most of the experiments were made in a dark-room, and the image was looked for with eyes open. Pressure on the eyeball was thus prevented, but the fluctuations continued. Winking, if unconsciously done, disturbed the image momentarily, but had no lasting effect. However, if consciously done (*i. e.*, when the attention was directed away from the after-image to the movements of the eyelids), the image seemed lost for an appreciable time. The numerous involuntary eye-movements could not be controlled. ✓ The eyes tended to *follow* the image, which would remain in view while these movements were not inhibited. If consciously prevented, if the eyes were brought back to what appeared to be their normal position, if the eyes were consciously rotated while the phenomena lasted, the image would disappear, only to reappear if consciously brought back—*i. e.*, if the attention was again directed to the organ of sensation rather than to the movements of this organ. ✓ From these observations of my subjects and myself I am inclined to believe many of the fluctuations are mental in character, just as we have seen the duration to be largely a mental matter. In other words, the various

<sup>1</sup> R. W. Darwin, *New Experiments upon the Ocular Spectra of Light and Colours*; *Phil. Trans.*, LXXVI., 313-348, 1786.

phases are influenced more by the mental attitude of the subject than by the physiological condition of the retina.

Helmholtz noted that violent movements of the body changed the character of the after-image from positive to negative,<sup>1</sup> and that an electric current sent through the optic nerve changed the intensity of the after-image present.<sup>2</sup> This may also be seen to agree with the attention hypothesis.

In some respects the fluctuations of the image arising from stationary stimuli are analogous to those noted by Bidwell<sup>3</sup> and by Charpentier.<sup>4</sup> However, the images noted by these investigators were so complex that it seems uncertain whether we are dealing with the same phenomena. The Bidwell recurrent images are explained satisfactorily upon physiological grounds; but in repeating the experiments I was led to believe that at least the brightness of the various bands was due in large measure to the fixation of the attention.

The changes in the physical stimuli seemed to have no effect in the present case. The after-images when they lasted longer did not have more fluctuations, but the various phases remained for longer times. Such a great variation in the number and in the character of the fluctuations was noted that it seemed impossible that any general relation held true. Opposed to these results, however, are observations by Helmholtz<sup>5</sup> and by Miss Washburn.<sup>6</sup> Helmholtz noted that the duration of the stimulus had an influence upon the negative as well as upon the positive phases; but he only makes this general statement without further detail. Miss Washburn also observed variations in the fluctuations due to intensity-changes of the stimulus.

SECTION 6. QUALITATIVE CHANGES.—As one of the most difficult and most striking of the phenomena to be explained, the color-changes of the after-image have attracted a great amount of attention. But throughout the various accounts of the color-changes it seems to have been assumed that the experimenter was normal, and what occurred in his eyes would or

<sup>1</sup> *Physiol. Optik*, p. 504.

<sup>2</sup> *Ibid.*

<sup>3</sup> *Op. cit.*

<sup>4</sup> *Op. cit.*

<sup>5</sup> *Op. cit.*, p. 505.

<sup>6</sup> *Op. cit.*, p. 27.



should occur to another, if he saw the same light. This belief has led in some measure, I fancy, to the disagreements regarding the phenomena. The individual element seems not to have been considered by any one. In the experiments noted in Section 4 it was found that the various observers differed greatly in their account of the color-changes; and, moreover, an individual did not always see the same colored after-image when stimulated with the same light. At one time, all the images would be seen as light alone, at another, they appeared gray or reddish. This change appeared with all my subjects. The difference was also noted by R. W. Darwin,<sup>1</sup> who considered the changes as due to his making too many experiments at one time, his eye being 'not quite free from the spectra of the colours previously attended to.' The difference of individuals is naïvely noted by Miss Washburn in her recent paper. In this paper she says her subjects had to be *drilled to see a normal image* (*i. e.*, like her own).<sup>2</sup>

From the after-image experiments in the series of anthropometric tests (see Sec. 4), it was found that "the after-image, when first seen, was sometimes positive and sometimes negative: and the colors varied greatly, being distributed in the first place as follows: negative or dark, 33%; light or white, 29.4; blue, 13.7; purple, 9.8; green, 5.9; yellow, 3.9; red, 2.0; miscellaneous, 2.0."<sup>3</sup>

Various trials were made of physical conditions to discover whether or not the change was due to these; but for the conditions tested (position of eyes in relation to light, accommodation, adaptation, changes in intensity, duration and area of stimulus), nothing definite was discovered.

Fechner found that various durations of the stimulus gave different colored after-images, but I found no such definite relation with the comparatively long times used by me. The change in quality of the after-image under any one condition—*e. g.*, 5 seconds—seemed as great as that observed between 5 seconds and 1 second stimulation. The only uniform change

<sup>1</sup>Op. cit.

<sup>2</sup>Op. cit., p. 27.

<sup>3</sup>Cattell and Farrand, Physical and Mental Measurements of the Students of Columbia University; *Psycho. Rev.*, III., 618-648, 1896. See especially p. 645.

noted was that with the greater intensities and the longer stimulations the after-image appeared oftener as negative. With medium intensities such as I used this result is almost never noted; but the change is clearly brought out in the results of the experiments upon threshold (where only  $\frac{1}{8}$  of 1% were negative) compared with the results upon duration (where fully 80% of the images had negative phases).

The quality (or intensity) of the image was changed by Helmholtz by sending an electric current through the eye. "If one has developed in the eye a negative after-image and then sends an electric current through the eye and the optic nerve, \* \* \* the negative after-image becomes darker, and if an image is just on the borderland between positive and negative, it can be made negative by sending a current through the eye."<sup>1</sup> This change is somewhat analogous to what occurs when an after-image is produced and external light is then admitted to the eye.

I can attempt no explanation for these individual differences. The suggestion has been made that the differences may be due to a varied sensitiveness of the retina to light-waves of different lengths. Such an explanation seems, however, too hypothetical when one considers the great differences. One subject saw the after-image always as green; to another it was always red; to a third it varied in the white-black series. The explanation could not be stretched in any case to cover the change in an individual under practically the same conditions. The importance of these differences for theories of color-vision will readily be recognized. If later experiments are made, a most careful study of each subject's color-vision should also be made.

The control of the color of the after-image should again be noted. Miss Washburn attempted to control the color-changes, but concludes that the control is not real, but apparent. In her case the attention intensified certain colors that were present, but which would be unnoticed under ordinary circumstances. Such an explanation of the effects of attention probably could not be made to include the variation of one individual from another. The problem needs more detailed and careful study.

<sup>1</sup> Op. cit., p. 509.

An attempt to change voluntarily the character of the after-image from positive to negative has been uniformly unsuccessful in my own case; Miss Washburn seems to indicate that this was done by all her subjects, and it may be that being a poor visualizer accounts for my inability.

SECTION 7. SPACE-RELATIONS.—In his characterization of the difference between after-images and the images of the imagination, Fechner considers as one of the greatest differentia the spatial character of the phenomena. The after-image is, according to him, of two dimensions only, while the images of the imagination are tridimensional. This statement has not often been disputed, but there seems little truth in it as a statement of universal fact. Professor Hyslop found that if a “picture hanging obliquely on the wall, say thirty degrees, more or less, and I look at it while lying on a bed or lounge and then look at the wall vertically near me,\*\*\* I notice that the after-image *does not lie in the plane of the wall, but in the same position relatively to the plane of vision as in its real position.*”<sup>1</sup> In other words, we have here a case where the after-image was distinctly opposed in character to what Fechner said we have it.

In view of these contradictory statements, it seemed advisable to test the matter more thoroughly, using several subjects. A few simple experiments were devised for the purpose. The subjects, naïve as to the purpose of the experiment, were asked to gaze fixedly for fifteen seconds at one corner of a highly colored rectangular block, and to project the resultant after-image upon a white wall about four meters distant. Note was to be made of size, color, etc., but no suggestion was given regarding the tridimensional character. Throughout the experiments the eyes were open. A brightly lacquered round resonator, two brightly colored books placed like a V with the apex turned away from the subject, and two incandescent lamps placed at different distances from the eyes, were also used as stimuli. Some of the observations with the various stimuli are as follows:

<sup>1</sup> J. H. Hyslop, Experiments in Space Perception; *Psychol. Rev.*, I., 588, 1894.



“Projected from the wall, but not distinctly.”

“Seemed to stand back. Solid.”

“Some suggestion of solidity, but no definite outline.”

“Perhaps (?) solid.”

“Vague feeling of projection from wall.”

Most of the results, however, seem negative. With closed eyes, on the other hand, the image became more nearly like the imagination-images; and while there was a feeling of insecurity, the images acquired the tridimensional character of the mental products. This may be due (in some cases it was due) to the superposition or to the strengthening of the after-image by the imagination-image. Here we must also consider the effects of the attention. We ordinarily see what we wish to see, and the numerous questions regarding the space-relations may have tended to suggest to the subjects the answer sought. With some subjects, however, no amount of experiments or suggestions could get them to say anything beyond that the image seemed ‘almost solid.’

If my own attitude is to be considered typical, I should say that the image appears at first distinct only in outline. The differences of light and shade which we note in the original solid object do not appear in the after-image till we look for them, and then it is difficult to say whether the differences are really due to the imagination or whether they are in the after-image. The time it took many of my subjects to note the third dimensions would indicate that this space-character was ‘read into’ the sensation rather than given.<sup>1</sup> This much, however, seems clear, that if there is any appearance of solidity connected with the after-image, it is not nearly so evident as it is in sensation or in imagination. The fact that the image is projected by us into space, and is there localized, leads one to believe that the idea of depth is not wholly wanting in the ordinary after-image. This would seem to class the phenomena rather as perceptions than as sensations.

<sup>1</sup> Professor Hyslop advises me that almost seventy-five per cent. of the students in his introductory classes fail to perceive at once the third dimension in stereoscopic pictures. Many succeed after the suggestion has been given, but some fail to obtain the usual result even after many experiments.

Stereoscopic experiments, in which the sensation was one of solidity, gave no indication of apparent solidity in the resultant after-image.

The size of the image, as is well known, apparently increases or decreases if we look respectively at a far or at a near object. This apparent change in size is due to the fact that a certain portion of the retina is concerned with the seeing of any after-image, and the amount of space subtended by this portion of the eye increases as the square of the distance of the wall or screen. When projected on the same screen the size of the after-image from any light is approximately constant for all observers; the few variations noted are probably due to hasty judgments or poor observations. It may be that the moving of the eyes during the stimulus produced in these cases a larger image, more of the retinal elements having been stimulated. A curious fact in connection with the apparent change in size is that if a long image is projected upon a receding wall one end is seen wider than the other. A more wonderful and more puzzling phenomenon is mentioned by James. If an after-image is projected on a flat surface 'resembling a receding screen,' the image takes on the form it would have if seen under similar actual conditions just noted. Such a result points strongly to mental influences, which we have seen are important factors in all conditions of the after-image, viz., attention and imagination. The experiment makes it evident that the phenomena are closely related to the imagination and to perceptual processes. The experiment was repeated by several of the subjects, but at first all united in affirming no such change. The suggestion made, however, to one individual gave the effect. It is impossible to tell from James' remark whether the observation is original with him or whether it is quoted.

To discuss the apparent size of the after-image seen when the eyes are closed leads one into all the difficulties that have followed the discussion of the moon's size. It is interesting to note, however, that when asked the size of the image resulting from a cross with arms one decimeter square as viewed at a distance of three meters the average of the sizes noted by

observers was 7.5 cm. and the image was said to be about 1.87 m. from the eyes. The variation is large and the individual answers show characteristics of the several observers. The subject might repay fuller and more extended observation.

SECTION 8. THE RETINAL TRANSFER OF THE AFTER-IMAGE.—One of the questions of considerable interest raised by Newton in his letter to Locke was that regarding the transfer of an image from the stimulated to the unstimulated eye. "Though I looked with my right eye only" (at the sun), he says, "and not with my left, yet my fancy began to make an impression upon my left eye as well as upon my right. \* \* \* With my left eye I could see the spectrum or the sun almost as plain as with my right."

Brewster<sup>1</sup> independently noticed the phenomenon, and it has been discussed fully by many observers, notably by Helmholtz, Fechner, Charpentier and Titchener. A complete discussion of previous views will be found in Titchener's<sup>2</sup> article, and only a brief résumé of the general views need here be given.

It will readily be seen that the transfer may be explained in any of the following ways: (1) The appearance is an extension of the well-known phenomena of binocular contrast. (2) There is a functional connection between the retinae, whereby one is affected by what affects the other. (3) The after-image has its seat in the brain, and not in the retina. (4) The transfer is only apparent.

Fechner<sup>3</sup> and Helmholtz<sup>4</sup> are the sponsors of the first theory. The former stimulated one eye with a bright-colored light, and the other was darkened or stimulated with a very weak gray light. According to him, the unstimulated eye saw the contrasted color during the continuation of the stimulus, and this left its effect (a true after-image, apparently) in this eye. In other words, the after-image was not transferred, but the op-

<sup>1</sup> Article on Accidental Colours, in Edinburgh Encyclopedia.

<sup>2</sup> E. B. Titchener, Ueber binoculare Wirkungen monocular Reize; *Philos. Stud.*, VIII., 231-310, 1893.

<sup>3</sup> Fechner, G. Th., Ueber einige Verhältnisse des binocularen Sehens; *Abh. d. k. Sach. Ges. d. Wiss.*, VII., 481, 1860.

<sup>4</sup> Op. cit.



posite nerve-fibers of the unstimulated eye were excited during the progress of the sensation, whence an after-image was produced. This explanation, accordingly, is only a different form of the next theory.

In support of the co-excitation hypothesis many other physiological phenomena may be cited. The most evident of these are the facts that both eyes are used as a single instrument; that they are moved together; and that if one eye is in the dark while the other is in light, the iris of the former will expand as the iris of the latter does. Similar other points indicate that the two eyes are controlled as one. Charpentier<sup>1</sup> is the main exponent of this theory; but his experiments upon the varied sensitiveness of the two eyes do not seem to have been well chosen or conclusive.

Parinaud,<sup>2</sup> Ebbinghaus<sup>3</sup> and Binet<sup>4</sup> are supporters of the third explanation. Almost the same experiments that convinced Fechner and Charpentier were used by them in making the new hypothesis. It is taken for granted that the image is seen by the eye that is open, and to them the only satisfactory explanation is that the image lies in the cerebral center of vision, and not in the retina.

While accepting the same general point of view, Titchener introduces a new series of experiments to prove that the transfer is real, and is not an apparent one. The experiments included observations upon (1) the relative duration in the stimulated and in the non-stimulated eye; (2) the variations in the fluctuations; (3) the color-changes, and (4) the differences in brightness. The results of these experiments by Titchener are summed up as follows: (1) The image in the non-stimulated eye lasts a shorter time than in the stimulated eye. (2) The relative time which the two phases of the secondary (*i. e.*, in the non-stimulated eye) image take is in no way similar in the primary eye. (3) The phenomenon occurs also under conditions when a mix-

<sup>1</sup> Charpentier, A., *La lumière et les couleurs*, Paris, 1888. *Sur les connexions fonctionnelles des deux rétines.* *C. Rend. de la Soc. de Biol.*, 8 Ser., II., 364, 1885.

<sup>2</sup> *C. Rend. de la Soc. de Biol.*, 13 May, 1882.

<sup>3</sup> *Archiv f. die gesammte Physiol.*, XLVI., 498, 1891.

<sup>4</sup> *La Psychologie du raisonnement.* Paris, 1886, p. 45 ff.

ture of both fields of vision is made impossible. (4) There is a constant difference in brightness between the negative complementary images in the two eyes. (5) Experiments upon a monocularly color-blind person gave evidence of the second image.

On the other side of the question we find Delabarre. To him the secondary after-image is an illusion; the transfer is only apparent. Considering the experiments made by Parinaud and Binet, he says: "A serious difficulty in settling the question lies in the well-known impossibility of separating the visual fields of the two eyes. Whether one eye or both are open; whether they are focussed on the same point or are held parallel, or squinted, or even jammed into all sorts of relative positions by fingers inserted into their sockets, the field of each will appear to coincide with the field of the corresponding portion of the retina of the other. If an after-image be found in both together, one image only will be seen whatever their relative positions; and if the image be found on one alone, it will yet be seen in the corresponding portion of the field of the other, provided that the brilliancy of the second field be not so great as to obscure the much weaker sensation of the image. In reality, in this experiment the after-image never does appear on the left field until the last eye has so greatly darkened as to allow it to be seen; and in the periodical increases in brilliancy of the left field the image disappears. It will thus be seen that a retinal seat of the after-image explains all the facts as easily as does a central seat. Hence the assumption of the latter by M. Binet and others is entirely superfluous \* \* \* not merely superfluous, but impossible."<sup>1</sup>

The results of five experiments tend to strengthen this position: (1) There is a difference in brightness of the image when the stimulated eye is open or shut. (2) With an image in the right eye, open it and the image is clearly seen; open the left eye with the right closed, and the image is blotted out. Such an effect is probably due to binocular mixture. (3) With both eyes open, the image being in the right, place a pencil or

<sup>1</sup> Delabarre, E. B., On the Seat of Optical After-Images; *Amer. Jour. Psych.*, II., 326-328. 1889.

similar object before the left; no change in the image occurs. Place an obstruction before the right, and the image disappears temporarily or permanently. (4) Obtain a strong image with the right eye; if projected on a white surface, it appears *light green*. If both eyes are covered tightly, so that no light gets in, the image is *dark green*. If the eyes are closed, but not covered, and light gets in, the image is *rose-colored*. Now "obtain an image with the right eye, then close and cover it and open the left eye. Now the left field darkens and the *dark-green* after-image appears; this gives place to the *rose-colored* image if the covering is removed from the right eye, and the eye kept closed; and this in turn to the *light green* if the right eye is opened. These three colors can be made to succeed one another indefinitely without in any way interfering with the open left eye, which, alone, according to M. Binet, is the source of all the visual impressions present." (5) In retinal rivalry, on obtaining an image with the right eye, look with both eyes through colored glasses at a background. When the color seen with the right eye is predominant the image is seen; when the color seen with the left eye is predominant no image is apparent.

Professor Titchener's experiments, like Binet's, may also be explained by considering the image a peripheral as well as a central event. The variation in brightness is explained from the peripheral point of view, to my satisfaction, by Delabarre's considerations. The apparent difference in the duration of the image is readily understood when one learns that pressure of any sort on the eye will disturb the image. The mere closing of the eyelid would suffice for this. When Titchener speaks of the occurrence of the phenomena even under 'conditions when a mixture of both fields is impossible,' he seems to forget that the two eyes always act as one, and that it is impossible to tell (by ordinary inspection) in which eye a sensation is. One of the subjects used in the present research, a man who had worked during two years upon binocular rivalry, advises me that he is now unable, even after such great practice, to tell in which eye the sensation is. It seems probable, in view of other experiments noted above, that Dr. Titchener's subjects were in-



fluenced to a great extent by *suggestion*. It has been seen what a great part this plays in the duration and in the spatial character of the after-image; and it is not unlikely that a similar influence is felt when looking for an image with an unstimulated eye.

To my mind, the only experimental results that might indicate the central seat of the after-image are the well-known experiments with suggested after-images. Binet gives the following account of the experiments:<sup>1</sup> "Wundt has shown that the simple image of a color, imagined for a long time, gives rise to an after-sensation of the complementary color. If one mentally looks fixedly for some time at a red image, he perceives a green tint upon opening the eyes and looking toward a white surface. This experiment is difficult to repeat, for it is necessary to have a power of visualization that not every one has. To take me as an example, I am not even able to imagine a color clearly. I am a very mediocre visualizer, so it is not surprising that I cannot obtain the colored after-image. But my friend Dr. Féré does this readily. He can represent to himself a red cross sufficiently intense to see on a piece of paper a green cross following it; moreover, he sees not only the color but the shape." A similar experiment has been made with hypnotized subjects. A red cross is suggested and then suggested away, and the green after-image noted. In the case mentioned by Binet, however, it seems not to have occurred to him that possibly the resultant after-image was suggested just as was the original red cross. The experiment has not been made in a rigidly scientific manner, and is inadequate. I have been unable to get a sufficiently good visualizer who was at the same time naïve regarding after-images, and I have not been able to repeat the experiment with due precautions.

The case with a full hypnotized subject seems not well authenticated. However if the question of its authenticity be waived for a moment, the question immediately arises, "Is the after-image the result of seeing the imaginary red cross, or is it not in this case also a new suggestion?" The precaution not to give such a suggestion seems not to have been taken, and this

<sup>1</sup> La Psychologie du Raisonnement, Paris, 1886, p. 41.

would vitiate the experiment. Even if this precaution were taken and the same result found, the central hypothesis would not be proven. Other experiments with hypnotized subjects would seem to indicate that from suggestion certain tissues may be modified so as to leave thereafter a noticeable effect. This is true of the experiments in which a blister is produced by suggesting the application of some known medium which ordinarily will produce such an effect. With such a case explained upon purely peripheral grounds, would it not seem probable that the eye was also so stimulated when the red cross was imagined?

Delabarre suggests the following experiment, a trial of which might aid in giving some negative testimony. Hypnotize a subject and suggest a sensation that he may get a good after-image. Then by suggestion paralyze the sight of this eye. If no after-image is seen in the other eye, it will indicate that there is no transfer; and if an image is presumably seen, "it will merely indicate that the paralysis of the right optic nerve has not been complete."<sup>1</sup> Delabarre's radical statement should have its *will* toned to *may*. The result in either case would not decide whether the image was central or peripherally transferred.

An experiment which may indicate a line for investigation has been tried by me. I attempted to stimulate only the optic nerve and the cerebral visual centers to discover what effect, if any, was left by such stimulation. Electrodes were placed upon the head, and when the circuit was closed and broken a vivid flash of light was produced. All the experiments gave negative results. No after-image was produced. It would be supposed that, if the image was of central origin, the stimulation of the central centers would give such an after-image. In this experiment it is not certain that the retina was not stimulated, but that the brain center was (either directly by the electricity or by the retina, which was in turn stimulated by the electric current). It would seem as if an after-image should have been produced in either case if the seat was cerebral.

Another experiment, which gives only a partial and negative answer to the question of transfer, has been made by me.

<sup>1</sup> Op. cit.

That portion of the right eye which corresponds to the blind spot of the left eye was stimulated to see whether there would be an apparent transfer. If a transfer (apparent) occurred, it could not be accepted as real, since with the corresponding portion of the left eye we can see nothing. If no image is apparent on opening the left eye, we learn nothing new, and neither theory is benefited. The fact is, an image appeared upon opening the left eye, and we are left to conclude that the transfer in this case *is* apparent and in other cases it is *likely to be* apparent.

Such an experiment is of value in that it is a link in the evidence tending to exclude one hypothesis. The image seems not to be transferred, but is either central or peripheral. The central situation seems to me improbable in view of the results of Delabarre's experiments and of the experiment on brain stimulation. The changes in the image under objective changes in the organ originally stimulated indicate a peripheral seat only in the stimulated eye; and the interference with the stimulated eye interfering with the progress of the image indicates that the after-image is not transferred.



## PART II. HISTORICAL AND DESCRIPTIVE.

SECTION 9. RELATION OF AFTER-IMAGES TO SENSATION, TO IMAGINATION AND TO MEMORY.—The intimate relation of after-images to sensation, to memory and to imagination makes the phenomena of great psychological interest. Seeming to be the connecting link between sensation and the idea, the study of this relation is of considerable epistemological importance.

The differences between visual after-images and the images of the imagination have been thoroughly discussed by Fechner.<sup>1</sup> His conclusions are summarized by James as follows:<sup>2</sup>

"*After-images* first coercive; seem unsubstantial, vaporous; are sharp in outline; are bright; are almost colorless; are continuously enduring; cannot be voluntarily changed; are exact copies of the originals; are more easily got with shut than with open eyes; seem to move when the head or eyes move; the field within which they appear (with eyes covered) is dark, contracted, flat, close to the eyes and the images have no perspective; the attention seems directed forward toward the sense organ in observing after-images." On the other hand, "*imagination-images* feel subject to our spontaneity; have, as it were, more body; are blurred; are darker than the darkest black of the after-images; have lively coloration; incessantly disappear and have to be renewed by an effort of the will (at last even this fails to revive them); can be exchanged at will for others; cannot violate the necessary laws of appearance of their originals; *e. g.*, a man cannot be imagined from in front and behind at once; the imagination must walk around him, so to speak; are more easily had with open than with shut eyes; need not follow the movements of head or eyes; the field is extensive in three dimensions, and objects can be imagined in it above or behind almost as easily as in front; in imagining, the attention feels as if drawn backwards towards the brain. Finally Fechner speaks of the impossibility of attending to both after-images and imagination-images at once, even when they are of the same object and might be expected to combine."

The above account is true only of Fechner himself; he remarks that results from other individuals show certain characteristic differences. Dr. Lay finds that he agrees with Fechner only in the following particulars: the after-images are coercive, cannot be voluntarily changed, seem to move with the eyes, and the attention is directed forward toward the sense organ, while the images of the imagination are directly opposite

<sup>1</sup> *Elemente der Psychophysik*, II., 468 ff.

<sup>2</sup> *Principles of Psychology*, Vol. II., 50.

in these particulars.<sup>1</sup> Regarding the other differences, it is not clear whether Dr. Lay disagrees with Fechner regarding the characterization of the after-image or of the images of imagination.

From the numerous observations of my subjects and myself any one of these qualifications of the after-image may be contradicted. The fact that we do not see an after-image after every visual sensation, the fact that it requires an amount of attention to perceive it, would indicate that the phenomenon is not so coercive as Fechner believed. The after-images are not colorless; sometimes they are colored more lively than ordinary sensations, and often their color is more intense than that of the imagination-images. The images are not continuously enduring, but have many fluctuations. We have already seen (Part I, Sec. 7) that the after-image often has the appearance of solidity. The probable reason for the usual two-dimensional character lies in the fact that nearly all objects bright enough to produce after-images are of two dimensions only. The colored paper, the window panes, the gas flame, etc., are plane surfaces and in the sensation the effect of solidity is not gotten directly from these, but rather from their surroundings, which usually have not the requisite intensity for the production of after-images. To Fechner's differences it may be added that the after-image is sharp and clear only if very near the point of fixation, while the imagination-image may be a clear representation of a scene all of which could not be noted by the eye at one time. James adds as a universal proposition that the after-images seem larger if we project them on a distant screen, and smaller if we project them on a near one, while no such change takes place in mental pictures.<sup>2</sup>

The name of memory-after-image is given by Fechner to the instantaneous positive effects of sensation.<sup>3</sup> These images are distinguished from ordinary after-images by the following characteristics: (1) Their originals must have been attended to

<sup>1</sup> W. Lay, *Mental Imagery*, p. 2. Monograph Supplement No. 7 to *The Psychological Review*, 1898.

<sup>2</sup> *Principles of Psychology*, p. 51, note.

<sup>3</sup> *Elemente der Psychophysik*, II, p. 491 ff.

only such parts of the compared originals as have been attended to appearing: this is not the case in common visual after-images. (2) The strain of attention toward them is inward, as in ordinary remembering; not outward, as in observing an ordinary after-image. (3) A short fixation of the original is better for the memory-after-image; a long one for the ordinary after-image. (4) The colors of the memory-after-image are never complementary of those of the original. It is difficult to state the relation between the memory-after-image and the memory-image, but the two seem almost equally different from the true after-image. Many memory-after-images have an overpoweringly coercive quality. Examples of this are numerous. A revolting scene will often leave a lasting memory-image. The sight of some one drowning or a railway accident will remain in the mind for weeks; the details will appear in consciousness unexpectedly, and will overpower us as did the originals. But the effect is not produced so often with the visual as with the auditory memory-after-images. The snatches of melody that one hears continually in the mind's ear after a concert, or the popular tunes that often rise in consciousness and compel us to hum them over and over again, have a coercive quality unparalleled in the phenomena of after-images.

In many ways the after-images can be considered a true sensation. One of the chief sensational conditions is present, viz., a change in the sense organ; and many of the more mental conditions of the sensation are equalled. At times it is impossible to say whether it is an after-image or a sensation that is in consciousness. A friend relates that his child, who had looked at the sun and then turned around, tried to point out to a sister the second sun (*i. e.*, the after-image, which was *real* to him). Older people sometimes make the same mistake, and correct the error only from later observations. Such an uncertainty is due primarily to a wonderful clearness of outline, brightness of color and great intensity of the after-image. To these *differences* Sully<sup>1</sup> adds "definiteness of localization (either in the field of objects if the eyes are open, or in the dark field if they are shut)." Such conditions are not often produced in one's daily

<sup>1</sup> The Human Mind, I., p. 178.



life, but may be obtained by suitable experiment. On the other hand, the moving of the after-image with the eye, its fluctuations, its negative quality, its (usually) plane character, and the fact that it is not doubled by lateral pressure upon the eyeball, differentiate the after-image from a sensation.

SECTION IO. HISTORY.<sup>1</sup>—The phenomena variously known as after-images, recurrent-images and ocular spectra (Germ. *nachbilder*; Fr. *couleurs accidentelles*, *persistence des impressions*) seem to have been noted first by Aristotle. He compared the images of the dream state to them, and speaks of them as if they were familiar to his audience. The account is as follows :

“It is evident that when we look at anything for a long time, and then turn away our eyes, the sensation continues; just, for example, as when we look at something dark after having looked at the sun, it happens that on account of the force from the light still remaining in our eyes we see nothing (*i. e.*, of the shaded object). And if, after having looked at a color for a long time we turn away our eyes, this same thing happens; and if we should turn away our eyes after having looked at the sun or some other bright object, it happens that the eye sees first the same color, this color then changes to red, then to purple, and after becoming black disappears.”<sup>2</sup>

Later the after-image is mentioned by St. Augustine, and by the Arab Alhazin, who was probably attracted to their study from reading Aristotle's works.

In the seventeenth century (according to Helmholtz, in 1634) Peiresc “observed 1000 times that when he had looked upon the window distinguished with wooden bars and squares of paper, he carried the form thereof some time after in his eyes; but with this difference, that if he kept his eyes shut, he seemed to behold the bars dark, and the paper squares white, as he had at first seen them; but if he looked with his eyes upon a dark wall, then the paper squares seemed dark, and the bars of the same whiteness with the wall.”<sup>3</sup>

Kircher, Mariotte, Boyle, Fabri, in the succeeding century

<sup>1</sup> A short historical résumé will be found in Helmholtz, *Physiol. Optik*, 2 Aufl., pp. 836-837.

<sup>2</sup> *De Somniis*.

<sup>3</sup> *Life of Peiresc*, London, 1657, Book IV., p. 101.

made slight additions to our knowledge regarding the appearances. Newton's account of his memorable after-image (see Part I., Sec. 4, p. 37) was sent to Locke about this time. In it he seems to be the first after Aristotle to call attention to their intimate relation to the more (so-called) mental images.

Buffon<sup>1</sup> in 1743 related his experience with after-images projected on differently colored backgrounds. He found that the image fused with the background, and formed a color which was a combination of the true color of the after-image and that of the background. On account of the variety of the phenomena and because he was unable to account for all the appearances, Buffon called the after-images '*couleurs accidentelles*.' These experiences of Buffon were later confirmed by Gergonne, who made a number of new experiments similar in character.

The first theory after Aristotle's (=continuation of the stimulus) was that of Jurin (1758?). Apparently considering only the negative phases, he regarded the after-image as due to a process in the retina the reverse of what went on in normal sensation. In expression this view is strikingly like some most recent ones, although Jurin had not the same ideas as the more modern writers.

Scherffer<sup>3</sup> (1761), noting the negative images on a light background, supposed the phenomena to be caused by a temporary loss of sensibility of the retina for one color. He thought that the eye, having undergone a prolonged action from rays of a certain color, lost momentarily its sensibility for a weaker stimulus of rays of that color. Thus, on looking at white the eye is stimulated by rays of light of different color—red, green, blue, etc., and the retina recombines these into white. Now, after having looked for a time at red, the retina becomes fatigued for rays of that color, and when a white object is then fixated the various rays composing white are seen with the exception of the red, thus producing a bluish-green image.

<sup>1</sup> Mémoires de l'Acad. des Sciences de Paris, 1743, p. 213.

<sup>2</sup> Essay on Distinct and Indistinct Vision, p. 170 ff, of Smith's Optics, Cambridge, 1738.

<sup>3</sup> Dissertation sur les couleurs accidentelles; *Jour. de Physique*, XXVI., 1785.

Plateau<sup>1</sup> conclusively shows that this theory is inadequate in that it does not account for the negative after-images which are seen perfectly in most complete darkness. A later theory proposed by Scherffer was that the after-image is due to a prolongation of a feebler stimulation produced by rays different from the dominant color. For example, in looking at a red square we see not only red light, but also some blue and green light, and when the red is taken away the blue and green, which have not been too intense to overpower the eye, continue to be seen, thus producing the after-image. Were this true, it would be an example of a lesser light having a greater effect than one of great intensity.

De Godart<sup>2</sup> is responsible for two (so-called) theories. The first of these is a very fanciful one. Arranging the colors like the tones in a musical scale (black, blue, green, red, yellow, white), he believed that the direct continuation of a sensation was as much lower than white as the sensation was higher than black. A sympathetic action was set up in the retina just as sympathetic tones are noted on a musical instrument. "Such a theory," Plateau remarks, "scarcely needs refutation." The other theory of De Godart is as follows:

Voici une autre théorie de ces phénomènes: c'est de dire tout uniment qu'une fibre ébranlée par un objet reste incapable de donner la sensation d'un autre, aussi longtemps qu'elle conserve l'impression du premier, et que les différentes couleurs étant expérimentées par des portions d'une même fibre d'autant plus, courtes que le ton de la couleur est plus vif, c'est la partie qui n'a pas joué qui excitée par le blanc la faire, donne la couleur accidentelle.<sup>3</sup>

This seems to be only a badly conceived, a poorly expressed theory of insensibility like that proposed by Schaeffer; and coming, as it does, without elaboration toward the close of his paper, it seems to indicate that the hypothesis is not well considered by the author.

Darwin<sup>4</sup> (R. W.), having considered all the known facts,

<sup>1</sup> Essai d'une Théorie générale comprenant l'ensemble des apparences visuelles qui succèdent à la contemplation des objets colorés. \* \* \* la persistance des impressions de la Rétine, les couleurs accidentelles, etc.; *Ann. de Chimie et de Physique*, LVIII., 337-406, 1835.

<sup>2</sup> *Jour. de Physique*, VIII., 1776.

<sup>3</sup> *Ibid.*

<sup>4</sup> R. W. Darwin, *New Experiments on Ocular Spectra of Light and Colours*; *Philos. Trans.*, LXXVI., 313-348, 1786. Also found in E. Darwin's *Zoonomia*, 4 Am. Edit., 1818, Vol. I., p. 443-466. See also Vol. I., p. 10 ff.



attempted to group them into four classes: (1) Images owing to a less sensibility of a defined part of the retina. (2) Images owing to a greater sensibility of a defined part of the retina. (3) Images that resemble their object in color as well as form. (4) Images that are of a color contrary to that of their object. From the consideration of these different facts, he was led to believe that a part of the retina became fatigued by a color and became insensible to rays of that color, and that this part of the retina then took up a mode of action opposite to that which produced the sensation. The details of this theory are interesting in view of what is at present known of the retina's action. He says "the effect of the activity of the retina may be to alter its thickness or thinness, so as better to reflect or transmit the colours which stimulate it into action."<sup>1</sup> Possibly "the muscular actions of the retina constitute the sensation of lights and colours; and the voluntary repetitions of them, when the object is withdrawn, constitute our memory of them."<sup>2</sup>

Contrast was used as an explanation by C. A. Prieur (or Prieur de la Côte d'Or. [Fechner]). Numerous observations were made in the succeeding years, but they were only slight variations from the previous work. They are unimportant.

In 1835, Brewster<sup>3</sup> discussed the various color-changes, and concluded that the primary color and the color of the after-image existed in the retina simultaneously, in the same manner as a fundamental tone and its harmonic. After the primary light has ceased the color of the after-image continues. This is almost identical with the first theory of de Godard. In constructing his theory Brewster seems to rely greatly upon the various phenomena of simultaneous contrasts, considering these as representatives of the after-image process.

Having considered the inadequacy of each of the foregoing theories from Jurin to Brewster, Plateau<sup>4</sup> made a careful ex-

<sup>1</sup> *Philos. Trans.*, LXXVI., p. 348.

<sup>2</sup> *Ibid.*

<sup>3</sup> In *Edinburgh Encyclopedia*, Vol. I., article *Accidental Colours*. Also *Philos. Mag.*, IV., 354, 1834.

<sup>4</sup> *Ann. de Chimie et de Physique*, LVIII., 337-406.

amination of all the known facts preparatory to constructing a new theory. His theory, which follows, is partly a combination of the ideas of his predecessors, particularly those of Jurin and Darwin. He says, "we must conclude that the accidental image results from a particular modification of the organ, which spontaneously gives us a new sensation." "When the retina has undergone the action of rays of a certain color, it resists the action of that color and tends to regain its normal state with a force more and more intense. Then if the excitation is suddenly removed, it returns to its normal state by an oscillatory movement as much more intense as the action has been prolonged." The first primary image, which he considered a prolongation of the stimulus, was noted by him, and he concludes "that when the retina, after having been excited for some time by the presence of a colored object, is suddenly removed from this excitation, the sensation produced by the object continues to exist for a very short time, after which the retina spontaneously takes on a state opposite to the first, whence there results the sensation of the accidental color.

Dove, Scoresby, Grove, Seguin, Brücke and Aubert noted the after-images of moving objects, and the appearance and reappearance of the images under increased and decreased eye illumination. Brücke and Aubert noticed the after-images resultant from instantaneous illumination by the electric spark.

Fechner, who lost his eyesight mainly because of his long-continued study of after-images from very bright lights, proposed the theory which has usually been associated with the name of Helmholtz, viz., that the positive phase of the image is a continuation of the stimulus, and the negative and complementary phases are due to retinal exhaustion. This hypothesis is the one accepted by Wundt. The theory is inadequate because it does not account for an after-image whose course (fluctuation) is as follows: Pos., neg., neg., pos., pos., neg., neg., neg., etc.

Hering regards the positive after-image as a continuation of the stimulus, and the negative phases as reactions of the visual elements to a state of equilibrium, the 'assimilation or dissimilation in some of the visual substances.' The theory of light.

sensation proposed by Mrs. Franklin would account for the phenomena in approximately the same manner. It will be noticed that both these theories are in terms, though not wholly in sense, the same as Darwin's.

Accepting the general theoretical position of the Young-Helmholtz hypothesis of color vision, Bidwell would account for the phenomena as due to a 'reaction of the violet nerve fibers only.'<sup>1</sup> Four reasons are given for this view: "(1) With white light the recurrent colour is violet. (2) In the recurrent spectrum of the complete spectrum no colour but violet can be detected. (3) A pure red light, however intense, gives no recurrent image. \* \* \* (4) The apparently blue colour of the ghost of simple spectrum yellow is just as well produced by a compound yellow consisting of green and red, the latter of which is inert when tested separately." It should be remarked, however, that some of these observations have been disputed, and there is always danger of accepting a theory which is proved by experiments devised after the acceptance of the theory.

It is to be regretted that all these theories have made little addition to our knowledge regarding the phenomena. Except the work mentioned in Part I., not very much has been learned regarding the conscious after-images since Fechner's time. The mixing of colors by means of discs and the summation effects of intermittent retinal excitation, have largely been considered during this time, but in a historical account of the appearances, these investigations have little place.

<sup>1</sup>On the Recurrent Image following Visual Impressions; *Proc. Roy. Soc.*, LVI., 140, 1894.



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## The Accuracy of Voluntary Movement

BY

R. S. WOODWORTH.

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## CONTENTS.

	PAGE
INTRODUCTION.....	I
Psychological importance of the study of voluntary movements.....	2
Method of estimating the accuracy of ordinary movements...	4
PART I. LITERATURE .....	7
PART II. METHODS .....	16
Graphic registration of hand movements.....	16
Advantages of the scheme of making each movement in turn the 'normal' for the following movement.....	19
Methods of average error and of right and wrong cases.....	20
Statistical treatment of results .....	25
PART III. RELATION BETWEEN THE ACCURACY OF A MOVEMENT AND ITS SPEED.....	27
When the eyes are used, the accuracy diminishes as the speed increases.....	28
When the eyes are not used, the accuracy does not vary much with the speed.....	30
Differences in accuracy between the two hands: 3 points of superiority of the right hand.....	32
Individual differences. ....	36
Results obtained on keeping constant the interval between successive movements, and varying the speed of the motion alone: the accuracy diminishes rapidly as the speed increases .....	37
Result obtained on keeping the speed constant, and varying the interval alone: the accuracy diminishes as the interval is prolonged.....	39
Initial adjustment and current or contemporary control of a movement: accuracy of initial adjustment is favored by a short interval; accuracy of current control by a low speed	41
The optimal or most favorable interval: indications of its length.....	43
Speed probably does not interfere with the accuracy of the initial adjustment.....	52
PART IV. ACCURACY AS DIVIDED BETWEEN THE INITIAL ADJUSTMENT AND THE CURRENT CONTROL.....	54



Demonstrations of the existence of a current control, <i>i. e.</i> , of re-adjustments following the initial adjustment.....	54
Relative amount of re-adjustment in different sorts of movement.....	55
Attempt to estimate the accuracy contributed by the initial adjustment as compared with that contributed by later adjustment.....	59
PART V. TEST OF WEBER'S LAW.....	62
Difficulties: necessity of controlling the speed.....	62
Results: the increase in error is too slow to agree with Weber's law, and too rapid to agree with the law proposed by Fullerton and Cattell.....	64
PART VI. ERROR OF PERCEPTION AND ERROR OF MOVEMENT.....	67
Method of isolating the two in case of movements governed by sight.....	67
The sum of the two is less than the observed total error; the residuum is regarded as an error in the central adjustment.....	68
PART VII. SENSORY BASIS FOR CONTROL OF MOVEMENT....	71
Any sense may be used as the sensory basis for controlling the extent of a movement.....	72
Advantages of the visual and auditory senses.....	72
When the eyes are used, the muscle and joint sensations are disregarded.....	73
When the eyes are closed, the extent of movement is judged not by sensations of resistance, nor by sensations of position, nor by the duration of the movement, nor purely by association with visual space, but by sensations which are primarily sensations of the extent of movement.....	76
Accuracy of reproduction under various conditions.....	77
PART VIII. EFFECTS OF FATIGUE AND OF PRACTICE .....	88
Fatigue curves for accuracy of movement.....	88
The function of accurate movement, though demonstrably subject to fatigue, is only slightly so.....	88
Fatigue increases the variability of a performance.....	94
Results of other authors tend to the conclusion that <i>all</i> simple central adjustments are only slightly subject to fatigue.....	96
A single discrepant result of one author criticised .....	96
Practice decreases the variability of a performance; variability means improbability .....	99
Practice affects no improvement at very high speeds; this result generalized.....	102

*CONTENTS.*

v

The time of day does not perceptibly affect the accuracy of movement. ....	105
PART IX. THE BEST MOVEMENT FOR WRITING.....	106
A mode of writing, suggested by some of the above tests, is experimentally compared with the usual modes, and found to possess advantages in point of ease and of speed .....	106





## THE ACCURACY OF VOLUNTARY MOVEMENT.

### INTRODUCTION.

In all sorts of psychology, save one, there is of late an increasing interest in the motor side of consciousness. Physiological and abnormal psychology are busy studying the involuntary and automatic movements that are connected with conscious or subconscious processes. Explanatory psychology is making use more and more of factors connected with muscular tension or with reactions to stimuli. Even abstract concepts are now interpreted, by some of the best students of logic, as types of reaction to classes of stimuli. In short, the evident fact that man is not merely perceptive and intellectual, but distinctly active or reactive, is being pushed to a position in our study more worthy of its fundamental importance. Those also who are trying to apply the results of psychology to educational problems are—the best of them—emphasizing this same fact, and insisting in accordance therewith that the child shall be educated not in learning or thinking alone, but in well-directed and vigorous action. The same impetus is seen in the introduction of manual training and other practical activities into the schools.

In view of all this interest, it is somewhat surprising that the subject of movement has received so little attention from one of the great departments of psychological research. We have as yet no psychophysics of the voluntary movements. By this I mean that we have no large mass of detailed study into the normal relations of voluntary movement to consciousness.

We have nothing in this line that can compare with the immense amount of work done on the relation of perception to the stimulus perceived; or, to widen our view, we have nothing in the general subject of voluntary movement that can compare in completeness with the work done and still doing in all departments of sensation.

It is further noticeable that when the topic of voluntary movement is treated, it is nearly always from the point of view of the *perception* of movement. It is not the movement as produced, but as perceived, that has been the object of study. Much has been written on the sensations or perception or memory of movement, but scarcely anything on the production of movement.

The observation may here suggest itself that this neglect of the mere production of movement is quite fitting in psychology, inasmuch as movement enters into consciousness only as perceived. The objection is however not well taken. Movement enters consciousness not only as perceived, but as intended. And the relation of the movement executed to the movement intended is just as important in the study of conscious life as is the relation of the perception to the stimulus perceived. It is quite true that the anatomical and physiological details of the process by which a muscular movement is carried out have no direct bearing on the consciousness that is concerned with the movement. But it is no less true, on the side of sensation, that the anatomy and physiology of the sense organs have nothing directly to do with the consciousness of sensation. Consciousness knows no more of the physiological process by which its sensations reach it than of that by which its intentions are executed. Particular interest attaches to the details of the incoming process, because they condition the details of consciousness. Yet, on the other side, the details of the outgoing process are conditioned by consciousness; and consciousness, like other things, should be studied by its effects as well as by its causes. In short, there is no theoretical reason why the outgoing process should not be studied equally with the incoming, by those who are trying to come to an adequate conception of the conscious life of the human organism. We regard this conscious life as built up on the basis of the reflex arc. We have, so far, studied the afferent portion of this arc. We must advance to the study of the efferent portion. And especially we must endeavor to study the arc as a unit, to trace cause and effect from sense stimulus to muscular response. And we should do this not simply in cases of involuntary response, but in cases of

voluntary action. We should, namely, trace the effect upon voluntary actions of varied conditions of sensation. A striking instance of what I mean is afforded by the old observation of Duchenne and of Sir Charles Bell, that a patient whose muscular sense was abolished could execute voluntary movements like extending the hand, when his eyes were open, but either very imperfectly or not at all when his eyes were closed. The outgoing current was apparently impossible when both incoming currents were checked.

The present study is concerned partly with the relations of incoming and outgoing currents in normal individuals. The question raised is not as to the possibility of any movement at all, but as to the relative accuracy of the movement under the control of different senses. A thorough and detailed study of this whole field would be a useful addition to psychology. At present the study of psychological elements is concerned almost wholly with sensation. We see programs of courses written in these terms—"Beginning with a study of sensation, the course proceeds to the higher mental processes—memory, reasoning, emotions, and finally will." This assumes that the elementary study of consciousness must start exclusively from the side of sensation, as if the will were exclusively a 'higher' process. It ignores the fact that movements begin as early in life and as far down in life as any sensation. It fails to see that there is a development of voluntary acts from lower to higher just as truly as there is a development of intellectual life from sensation to abstract thought. And it further overlooks the close dependence of intellectual development on the elementary reactive and voluntary processes. We need, alongside of our elementary study of sensation, a study of the elements of the active side. And since the primary action, the primary volition, consists in bodily movements, that elementary study will devote itself to an analysis of voluntary movements as related to the consciousness that intended them and to the perceptions by which they are governed. Just as we base our conception of sensation on a study of sensations, and our theory of association on a study of associations, so we should base our conception of the will on a study of volitions, and primarily of voluntary movements.



This has, indeed, been the basis on which the best modern discussions of the will have been founded. They have used the material at hand, largely of a clinical character. And this is, of course, a valuable kind of material. But as in the study of the functions of the brain clinical and physiological material supplement and correct each other, so we may reasonably expect that a detailed study of the elements of voluntary action would coöperate to a fuller understanding of the will and of the relation of consciousness to its acts.

In an extended study such as is here conceived, the present paper aims, of course, to fill but a very modest place. Just as the study of sensation has consisted largely in an examination of the accuracy of sensations, so a study of movement will be largely an examination into the accuracy of movement. This side of the subject has hitherto received less attention than has the maximum of movement (dynamogenesis, fatigue experiments, etc.). Practically, however, the accuracy of movement is the more important of the two. It is the accuracy of a movement that makes it useful and purposive. While some few movements require only brute force of a comparatively ungoverned sort, in most cases there must be a considerable degree of control and adaptation to a particular end. The movement must have a particular direction, a definite extent or goal, a definite force, a definite duration, a definite relation to other movements, contemporaneous, preceding and following. Even in comparatively unskilled movements it is remarkable how many groups of muscles must coöperate, and with what accuracy each must do just so much and no more.

An Italian day-laborer breaking stones in the street or hammering on a hand drill, is not classed as a skilled laborer, and yet the movement of swinging the large hammer—requiring as it does the concerted action of muscles of all the limbs and of the trunk, each of which must contract in proper time and force—is executed with such precision that the hammer hits the drill every time.

It is not difficult, by use of the theory of the probable distribution of cases about their average, a theory which undoubtedly holds with approximate correctness for all ways of hitting at a

target, and hence for all movements directed to a definite end, to calculate the degree of accuracy of any movement. We have only to determine within what limits the movement must remain in order to escape a 'miss,' and then to count the number of misses in a large number of trials. It is clear that a movement so accurate that though often repeated it seldom misses its mark must, in the great majority of trials, fall far within the limits of a miss. If a marksman can put ninety-nine shots out of a hundred within a certain ring of a target, he can put nine out of ten within a ring only a half or third as large. Knowing the percentage of cases in which the error exceeds certain limits, we can determine by means of the probability curve, or by the corresponding table, the value of the average error, the error of mean square, or any of the standards. The reliability of our determination would increase as the square root of the number of cases observed. The only difficulty with this method is that the number of cases counted would generally have to be large. In any movement which was at all well-practised and efficient the proportion of misses would be very small, and hence the number of cases counted must be large in order to get the true proportion of misses. But if this proportion is too small to be accurately determined within a reasonable time of observation, we can still determine an upper limit beyond which the average error does not lie.

The following is an example of the use of this method of computing the accuracy of every-day movements. I stood for an hour watching four Italians pound on two hand drills. As the two couples kept time with each other, it was easy to count the blows and the misses. I counted 4000 blows in all, and in that number there was but a single miss. Moreover, that one was the first blow after a rest, and was made by a man who seemed less skilled and confident than his companions. So that the average of 1 in 4000 is too high rather than too low. The radii of the drill and of the hammerhead were each about two centimeters. The deviation from the center sufficient to constitute a miss measured therefore 4 centimeters. If then an error of 4 centimeters occurs in this movement once in 4000 trials, the *average* error would be, according to the probability curve,

about 0.9 cm. This is small in comparison with the whole movement, since the distance traveled by the hammerhead is at least a meter and a half, or over 160 times the average error. In other words, the error in direction—that of extent is not here involved—averages about one-third of a degree. Yet these laborers swing the hammer almost carelessly, using the eyes to guide them only for the last half of the blow.

In smaller movements, such as those of writing, the absolute though probably not the proportionate value of the average error is still smaller. In letters a centimeter in height I have found the error to average about half a millimeter, or one twentieth. Much more accurate and undoubtedly most accurate of all our ordinary movements, are those of the vocal organs. The adjustments of the vocal cords for pitch, of the breath for loudness, and of the cavity of the mouth for the quality of a tone, must be very fine indeed. The accuracy of pitch may be roughly determined by a method similar to that just used in the case of hammer blows. A large share of us are able to strike a given tone time after time without appreciable discord. The error necessary to constitute a miss will be measured by the smallest difference in pitch that can be detected. This is given as 0.2 of a vibration per second; but that is undoubtedly too small for ordinary quick observation. If we multiply it by 25, and take 5 vibrations per second as the measure of a perceptible miss, we shall be sufficiently conservative. Any one who can sing well enough to make a perceptible discord only once in 20 notes would then, according to the probability curve, have an average error of not over 2 vibrations per second. In order to compare this with the errors in other movements, we need to express the 2 vibrations as a fraction of the extent of the whole movement. The denominator of this fraction would, of course, vary with the size of the interval jumped, and also with the absolute pitch. If the pitch be middle C, or let us say a pitch of 250 vibrations per second, and if the jump be an octave, then the interval, measured in vibrations, is 125 when the jump is from the C below, and 250 when from the C above. And the average error of the singer, under the conditions assumed, would be  $\frac{2}{125}$  for the jump from the C below,



and  $\frac{1}{125}$  for the jump from the C above. In striking the next C higher, since the vibration rate is doubled, the same percentage of misses would mean an average error of half the size, in proportion to the whole movement or jump. When we consider that along with this precision in pitch there must go a precise adjustment of the current of air and of the parts of the mouth, and that the whole combination of movements must be executed with great promptness, we can hardly doubt that the movements employed in singing a high note are the most accurate under our control. This accuracy is largely the result of the extremely delicate sense—that of pitch—that furnishes its guide. And, indeed, when this sense is applied to the movements of the arm and fingers, the latter can by long practice attain an accuracy perhaps equal to that of the vocal cords. Evidently the same line of reasoning that has been applied to the movements of the cords can be applied to the movements of the violinist's hand, and if the violinist is able to strike a given pitch, and jump a given interval, with the same accuracy and speed as the singer, the same fractions as were obtained above would represent his average error. Probably the movements of the violinist's left hand are, in point of extent, the most delicately graded of any rapid manual movements. It is known that the good violinist must begin very early and grow up in art. The movements of the piano player need not be so accurate in extent, as a much greater leeway is allowed by the breadth of the keys. The remarkable feature of the movements of a skilled pianist is rather their combination of speed and accuracy with great variety and extreme complexity.

Enough has been said, by way of introduction, to show that very great accuracy is attained in many of our voluntary movements, and to suggest also that the sources of this accuracy are by no means well understood.

## PART I. LITERATURE.

On the particular topic I have investigated, that of the accuracy of voluntary movements under various conditions, there have been but few papers published. And most of these have

been concerned primarily with the accuracy with which a movement was perceived, rather than the accuracy with which it could be made. I shall, however, summarize briefly the results of a few investigators who have dealt with closely allied problems. Some are concerned with the least perceptible movements of different joints, some with the constant errors that occur when one movement is imitated in extent by another, some with the accuracy of the muscular sense of position, and some, finally, with the degree of accuracy which the muscle sense enables us to attain in repeating a given movement. In all of this work visual control of the movement was excluded.

Goldscheider's fundamental studies in the least perceptible movements of different members led to the following results:

(1) The least perceptible movement, measured in angular terms, varied with the different joints, being smaller where the bones are longer, as in the shoulder and hip, than in the joints of the hand and fingers. Of the long joints, the shoulder gives the smallest threshold, and after it the elbow, knee, hip and foot, in this order. The least perceptible movements of the different joints are apparently proportional to the relative size of their habitual angular movements.<sup>1</sup>

(2) For any one joint, the least perceptible movement was the same in any direction.<sup>2</sup>

(3) The threshold for the perception of passive movement is about as small as for active. Hence no innervation feeling can be presumed to aid in the judgment of active movements.<sup>3</sup>

(4) The perception of the movements is not based, then, on innervation feelings. Nor is it based on sensations of pressure from the surface, for these sensations can be shown not to aid but rather to disturb and blunt the sensibility to small movements.<sup>4</sup> Nor can the muscle sense, in the strict meaning of that term, be supposed to give us information regarding these minute *passive* movements.<sup>5</sup> The sensation which arises in the muscle itself is, except in case of fatigue and pain, a dull, dif-

<sup>1</sup> *Archiv f. Anat. u. Physiol., physiol. Abth.*, 1889, 486, 487.

<sup>2</sup> *Ibid.*, 480.

<sup>3</sup> *Ibid.*, 1889, Suppl. Band, 207.

<sup>4</sup> *Ibid.*, 1889, 491.

<sup>5</sup> *Ibid.*, 495.

fuse sensation, not like that of movement. Moreover, if the finger (say), that the muscle should move, be held fast, and the muscle be made to contract by means of electricity, we get no sensation of movement. As soon as the finger is permitted to move, the sensation of movement is felt. The real origin of the sensations which inform us regarding small movements of our members is to be found in the surfaces of the joints.<sup>1</sup>

(5) Is there a special sensation of movement, or is there simply a perception of the initial and final positions of the limb and a (*quasi*) inference of the movement? There must be a special sensation of movement, for the following reasons: (a) by means of superficial anæsthesia the sense of the position of a member may be destroyed without appreciable blunting of the sense of movement; (b) the delicacy of the sense of movement is closely dependent on the speed of the movement, whereas the sense of initial and final positions would not be concerned with the speed; (c) introspectively we have a sense of movement which is not the same as the sense of position.<sup>2</sup>

Hall and Hartwell,<sup>3</sup> in studying 'bilateral asymmetry of function,' compared movements of the two arms intended to be equal. When the arms were moved in opposite directions from a position in the medial plane, the right hand (of right-handed persons) made the longer movement, provided the arms were moved simultaneously. If the movements were successive, this tendency partly disappeared.

Loeb,<sup>4</sup> in a series of experiments similar to these last, obtained somewhat different results. Whether the movement of the right hand was exaggerated or not depended not simply on whether the person was right-handed or not, but in addition on the extent to which he was accustomed to use his hands.<sup>5</sup> If a manual worker, he exaggerated the left hand; if not, the right.

<sup>1</sup> *Zeitschr. f. klin. Med.*, 1889, XV., 108-111.

<sup>2</sup> *Arch. f. Anat. u. Physiol., physiol. Abth.*, 1889, 493 ff.

<sup>3</sup> *Mind*, 1884, IX., 93-109.

<sup>4</sup> *Pflüger's Archiv für die ges. Physiologie*, XLI., 107-127; XLIV., 101-114; XLVI., 1-46.

<sup>5</sup> The results of the various authors on this question are summarized by C. S. Parrish (*Am. Jour. Psych.*, VIII., 265-267), who concludes that the question cannot yet be called settled.



As this constant difference remained, though in less degree, when one of the movements was passive, it could not be due, wholly at least, to the difference in the amount of effort required to move the two arms. Moreover, this difference disappears when the movements of the two arms, instead of being simultaneous, are successive. There is still a difference, uniform in direction for any subject; but it is not in favor of the right or the left hand, but of the first or the second movement, with whichever hand made. From this fact Loeb concludes that the constant difference between simultaneous movements of the right and the left hand is not the result of different sensation-masses from the two arms. The judgment of the extent of a voluntary movement is based largely on the will impulse. Two simultaneous movements, being produced by the same will impulse, are judged to be the same. But the prime basis for judgments of the extent of movement is the duration of the movement.

This last supposition does not accord with the fact established by Fullerton and Cattell, that the duration of a movement is much less accurately judged than the extent.

When equal movements are attempted from different positions of the arms, that movement always turns out to be the shorter, the muscles concerned in making which are, at the beginning of the movement, in the more contracted state. This signifies that the more a muscle is contracted the less effect a given nerve current will have on it. We always feel as equal any movements that we make for equal. The judgment of the extent of voluntary movement depends on the impulse (intention or innervation), and not on sensations from the moving parts. Since weighting one of the moving arms does not introduce a constant error, it follows that not the tension of a muscle but only its length, *i. e.*, its degree of contraction, determines what effect a given voluntary impulse shall have on it.

In attempting to make two equal movements at different speeds we fall into a constant error, the faster movement being regularly too long. Close attention to a movement diminishes its speed and so its length.

Delabarre<sup>1</sup> comes to the opposite conclusion from Loeb as

<sup>1</sup> Delabarre, Ueber Bewegungsempfindungen, Freiburg i. B., 1891.

to the basis for judgment of the extent of movement. He says: "Movements are judged equal when their sensory elements are judged equal. These sensory elements need not all have their source in the moving parts. All sensations which are added from other parts of the body, and which are not recognized as coming from these distinct sources, are mingled with the elements from the moving member and influence the judgment." Anything that tends to increase the sensory elements of a movement, without attracting the notice of consciousness to the fact of this increase, produces an over-estimation of that movement. Attention to a movement has this effect. Resistance to the movement, when unperceived, has the same effect. So also has a contracted position of the muscles at the beginning of the movement. The result is here, as in the other case, that the movement is shorter than was intended. This fact (which Delabarre is able to confirm, however, only in rather extreme cases) is interpreted by him and by Loeb in opposite ways. For Delabarre, the increased sensation per unit of movement is the cause for the shortness of the movement. The extent of the movement is controlled by a simultaneous judgment of the sensations arising from the movement. For Loeb, on the contrary, the extent of the movement is determined entirely by the original impulse and by the irritability of the muscle, and the judgment is based wholly on the impulse. A fact discovered by Fullerton and Cattell helps us toward a decision. They found that even when movements were intended to be equal the subject could still tell, in considerably more than the probable one-half of the cases, whether his error was positive or negative. These authors state the result definitely for the force and the time of movement. But it appears also in extent of movement. In fact, we all know that there are occasional instances in which a movement gets away from us, and we realize by its feeling that it is not what we intended. In such cases the judgment of the movement is evidently based not on the intention, but on sensory elements.

When Delabarre applies his main principle to the case of successive movements it takes this form: "A movement seems greater in memory than in execution." That is, in attempting to reproduce a previous movement the constant error is positive.

Long distances were found to be reproduced with greater accuracy than shorter. Movements in some directions were less accurate than in some others and harder to judge.

Fullerton and Cattell<sup>1</sup> have made the first and only thorough test of Weber's Law as applied to the perception and reproduction of movement. Their general result is that Weber's Law does not hold, even approximately. Everywhere (save in the *time* of movement) the error of observation increased more slowly than the stimulus. The authors propound a substitute for Weber's Law which has so much to commend it both in its closeness of agreement with the facts and in its reasonable theoretical implications that it should always be considered in tests of Weber's Law. Their proposed law is that the error of observation increases as the square root of the stimulus. The interpretation they give to this law is none of those that are ordinarily given to Weber's Law. They regard it as having neither physiological nor psychological nor psychophysical significance. Its significance is physical—one might almost say, mathematical or statistical. This interpretation is based on the fact that what is directly measured by the ordinary psychological methods is not a quantity of sensation, but an error of observation. Hence the mathematical rule for the combination of errors would apply here. If the act of perceiving a given magnitude is in any sense a combination of the acts of perceiving its parts, then the average errors in the perception of the parts would be combined into a total average error; which would, however, be less than the sum of the component average errors, because the component errors would be as likely to be in opposite directions as in the same. In such a situation the theory of probability finds that the combined error should be equal to the square root of the sums of the squares of the component errors. Hence the error in the perception of a magnitude containing a certain number of units would be proportional to the square root of the number of units. All this applies to the variable error, not to the constant error.

This conception of the error of perception is easy to grasp

<sup>1</sup>On the Perception of Small Differences, with Special Reference to the Extent, Force and Time of Movement, 1892.



in the cases of space, time and force magnitudes ; more difficult perhaps in case of intensity of sound and light. It is especially easy in the case of movement. Suppose in attempting to draw a line a decimeter long I am subject to an average error of 5 millimeters. If now I aim to draw a line 2 decimeters long, by drawing first one decimeter and then adding another, the average error of my single decimeter would evidently come in twice. But inasmuch as the errors may be either positive or negative, and if combined by mere chance may act either with or counter to each other, the average error of the double line would not be as great as 10 millimeters. Theory and experiment (see the authors' paper, p. 97) agree that it would be equal to the error of a single decimeter multiplied by the square root of two. If a line three decimeters long be drawn in the same way, the average error of the whole would be that of a single decimeter multiplied by the square root of three. And so on. Now the hypothesis of the authors is that even when the two-decimeter line is drawn continuously, without sharply marking off the separate units, the errors of the single units are combined in the same way, subject, however, to a possible variation from the strict rule, as the result of the continuity. This same conception can be applied with equal readiness to any experiment in which a given stimulus is reproduced. And it is about as easy to apply to the visual perception of length. In the perception of intensities it seems less easy, since the stimulus is not here perceived as a sum of units, or as a compound at all. If, however, it shall turn out that the judgment of intensity is largely a matter of muscular adjustment, then the conception of the authors will be there too of easy application.

So much for the theory of the proposed law. As a matter of observation, it accords fairly closely, and much more closely than Weber's law, with the facts obtained by the authors in the judgment of extent, force and time of movement. The authors do not find their law followed with entire strictness, and argue that entire strictness should not be expected. The physical or statistical law of the combination of errors would be modified in application to any special sense by peculiarities arising from the physiology and psychology of that sense.

In regard to the constant error in the reproduction of a given length of movement, the authors find, as a result of a far larger mass of material than either Loeb or Delabarre apparently had available when they formulated different rules, that while small distances are regularly exaggerated, large distances (700 mm.) are regularly made too small. The perception of the extent of a movement was found to be more accurate than that of its force, and this in turn than that of its duration.

One more result obtained by Fullerton and Cattell is of importance in the present study. They were able to analyze the variable error in reproducing a movement into an error of perception and an error of mere movement. Part of the total error is attributed to inaccuracy of perception, and the remainder to the failure of the movement to obey our intention. In case this failure is at all marked we are able to detect it. The subjects were therefore required after making each movement to judge, or at least hazard a guess, whether it was too long or too short. The results given for force and for duration of movements show that the percentage of right guesses is too large to be attributed to mere chance, but that the error of perception is considerably larger than the error of movement. As was remarked above, the possibility of detecting differences between the movement intended and the movement actually executed shows that the judgment of the movement is not based, as Loeb maintained, wholly on the intention.

Bowditch and Southard<sup>1</sup> determined the accuracy with which a point could be hit, with eyes shut, when it had first been located by the eye or by touch. The result was that the average error when the point was originally located by the eye was 11.4 mm., while when located by the touch and muscle sense it was 19.2 mm. "It would thus seem (p. 235) that the knowledge of position in space obtained through the sense of sight is nearly twice as accurate as that obtained through the sense of touch." The authors also studied the effect of varying the interval elapsing between the location and the re-finding of the point. Up to two seconds the accuracy increases; above that, decreases. The curve so obtained they call the 'curve of forgetfulness.'

<sup>1</sup>A Comparison of Sight and Touch; *Journal of Physiology*, III., 232-245.

Münsterberg, among his very suggestive studies of movement, has given us some which touch on the general topic of the present paper. In the *Gedächtnisstudien*<sup>1</sup> he takes up a problem similar to that of the paper just quoted, viz., the effect of the lapse of time on the memory for extent of arm movements. His result is that the accuracy of memory increased from an interval of 2 seconds up to an interval of 10 seconds, and beyond that decreased. In his preliminary report on *Lust und Unlust*<sup>2</sup> he studies the accuracy of reproduction of an arm movement, as affected by different emotional conditions. The normal extents of movement were learned by heart, and imitated during these conditions. The results were that languor produced a negative constant error, while bodily vivacity produced a positive constant error. Seriousness gave a negative constant error; gaiety a positive, probably because of unusually strong or weak innervation of the antagonistic muscles. Pleasure gave a positive constant error for extension of the arm, and a negative constant error for flexion; whereas unpleasant emotions gave exaggerated flexions and too small extensions. The bearing of these observations on the theory of the emotions does not concern us here. But it is important to note that the emotions can be sources of inaccuracy in our movement. In view of the intimate relation between emotion and bodily movements, and specially in view of the correlations suggested by Münsterberg, it is not improbable that many of the unaccountable variations in accuracy which often appear are the result of fleeting emotions. Nothing probably will introduce so much inaccuracy into a series of movements (and I speak from my own observations as well) as a sudden strong emotion.

W. L. Bryan<sup>3</sup> studied the growth of children (6-16 years) in the power of rapid and of accurate movement. The growth was found to be much more pronounced in rapidity than in accuracy. Improvement in the latter was visible principally from six to eight years of age. The maximum rapidity, more-

<sup>1</sup> Münsterberg, *Beiträge zur experimentellen Psychologie*, 1892, IV., 81-88.

<sup>2</sup> *Ibid.*, pp. 218 ff.

<sup>3</sup> On the Development of Voluntary Motor Ability; *Amer. Jour. of Psych.*, 1892, V., 123-204.



over, though differing much for different joints, and in different individuals, was for the same joint of the same individual remarkably constant. The accuracy of movement was subject to much wider variations. The right (preferred) hand was generally, but not always, superior to the left in both speed and accuracy.

The movements used by Bryan in the study of accuracy were of two sorts. The first consisted in drawing a straight line from one point to another 30 mm. distant; the second in the insertion of a stylus into a small hole, starting from a point 6-10 mm. from the hole. In each experiment the apparatus was so arranged that any error exceeding a certain small value would betray itself by making an electric contact and producing a click of a telegraphic sounder. In criticism of these as methods for studying the accuracy of movement, it may be observed (1) that the movements studied were exclusively very small, and (2) that the *speed* of the movement was not controlled nor recorded. As we shall soon see, the accuracy of a movement varies with its speed, so that the mere statement that such a movement or such a person is so accurate has no definite meaning unless the speed is specified.

## PART II. METHODS.

The experiments which form the basis of the present paper were carried on during the year 1898-99 in the Psychological Laboratory of Columbia University. The subject of accuracy of movement was first brought to my attention as a fruitful field for study by Professor Cattell, to whom I am indebted also for many helpful suggestions during the progress of the work.

My experiments have been carried on almost exclusively by the use of various forms of the graphic method. The particular advantage of this method was that it allowed the movements to be made at any desired speed and interval. It also allowed the performance of a large number of tests in a short time, leaving the measurements till afterward. In this way I have been able to accumulate a number of tests (over 125,000 movements have been used in preparing this paper) that would have been quite

impossible in any other way. The subsequent measurements took, indeed, a large amount of time; but as they were very simple and mechanical in character, they could be made by an assistant.

In studying the accuracy of the *extent* of a movement, I used a kymograph rotating on a horizontal axis, and carrying a continuous roll of paper 24 centimeters wide, at a rate of 1-5 mm. per second. Over the drum was fitted a desk-like cover. This concealed the paper, except a narrow area, which showed

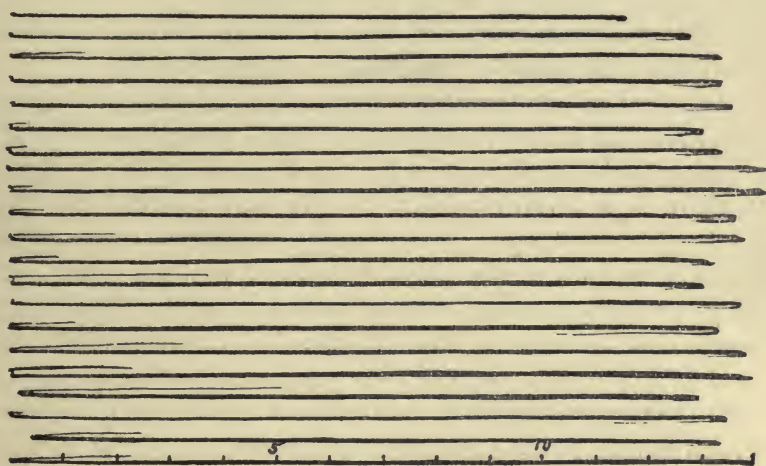


FIG. 1. Sample tracing of lines ruled on the kymograph, each required to be equal to the preceding. Reduced to  $\frac{2}{3}$  original size. A centimeter scale is drawn in on the last line.

through a slot parallel to the axis of the kymograph. Along the nearer edge of this slot was a brass straight-edge, which could be moved to and fro, and so vary the width of the slot. At one end was fixed another piece of brass to serve as a 'stop,' or, better, as a starting-point for the movements. When the desk-top was in position the brass straight-edge lay 2-3 mm. from the surface of the paper beneath. A hard drawing pencil, beveled to a very tapering point, and held by the fingers in the ordinary position, was now inserted through the slot so that its point rested on the paper. The movement consisted in ruling a line along the straight edge in either direction. Then, as the

paper moved slowly by, the movement was repeated at regular intervals indicated by a metronome, and the record came out with the appearance represented in Fig. 1.<sup>1</sup>

The principal advantage of this method lies in the quickness with which a large number of movements can be made and recorded. If it is desired to study the relation of the accuracy of movements to the interval between them, or if for any reason it is necessary to have the movements follow each other at a small interval, some automatic registering apparatus is indispensable. Especially is this true in the case of fatigue. In order to induce fatigue the movements must of course follow so quickly as to allow little time for recuperation between them.

An apparent disadvantage of the particular arrangement here adopted is that the movement of the pencil point does not represent exactly the movement of the hand which holds the pencil. As there is a certain amount of free play between the hand and the point of the pencil, and as the resistance to the motion is applied mainly at the point, it is clear that the hand must always move somewhat further than the pencil. This can be observed to be the case, and the more so in proportion to the speed of the movement and to the pressure upon the pencil.

But though this would be a slight source of error if the study were primarily of the *perception* of movement, it is not so when the emphasis is laid on the *production* of movement. In the latter connection the movement must always be studied with respect to some result aimed at. In the present method the result attempted is the drawing of a line of a certain length. This attempt lies nearer common experience, and is more definite and tangible, and so a fairer test of accuracy, than the attempt to make a mere movement of the hand which shall have a certain extent.

The experiment was varied in several ways. Sometimes the normal was a seen line, which the subject was to copy time after time. Sometimes the line was reproduced from memory. Sometimes a line was previously drawn on the moving paper, at right angles to the direction of the movements, and the move-

<sup>1</sup>The cost of the apparatus was in part defrayed by a grant to Professor Cattell from the Elizabeth Thompson Science Fund.



ments were required to terminate just on that line. But the most useful method was to require each line to be equal to the one made just before. However much error had been committed in that preceding line, no correction was to be attempted in the new line. The subject was impressed with the idea that his sole duty was, without reference to the errors he had committed in the past, to make the present line equal to that immediately preceding. The width of the slit was so adjusted that the subject saw the line he had just made. The advantage of this method lies, first of all, in the stimulus it gives to the attention. When a large number of repetitions of the same movement are attempted the attention flags and the movement becomes more or less automatic. But by this method the normal is constantly changed, though but slightly. Automatism is excluded. There must be each time a new adjustment to fresh conditions. In the second place, this method is almost necessary if a series of movements is to be made with the eyes closed. If the eyes are closed, and a single normal is required to be repeated time after time, the test is one of memory rather than of accuracy of movement; but when the normal is in each case the line just made, the error of memory is mostly excluded. Another very marked advantage of this method is that each movement is in every way comparable to that which it imitates. In the usual method of psychological experiments one stimulus is presented as a standard and the other as a quantity to be compared with the standard. The attitude of the observer toward the two is different. The one he stores in memory; the other he compares with a memory image. From this difference in attitude arises a constant time-error. In the reproduction of movements this error is specially in evidence, as has been remarked by Delabarre. The normal movement and the reproduction are made at different speeds, and with different innervations of the muscles antagonistic to the movement. As contrasted with this source of error, the method which makes each movement first a reproduction of the preceding, and then a normal to the following, insures that each movement shall receive the same quality of attention and be carried out in the same way.

Another advantage of this device of making each movement the normal for the following is that the calculation of results, the most tedious part of the use of the graphic method, is much simplified. Instead of measuring the whole lines and computing their average length and average error, we need measure only the difference between the end of each line and the end of the next. These differences give the errors directly. Moreover, if, as often happens, two or more successive errors lie in the same direction, it is not necessary to measure them separately, but only their sum. And still further, if we get the sum of all the errors in one direction, and also the difference between the first and last lines of the series, we can easily compute the sum of the errors that lie in the other direction, and so the total and the average error.

The 'constant error' is also readily obtained by this method, being simply the difference between the first and the last lines divided by the number of lines less one. The curve itself gives a direct demonstration of the direction and amount of the constant error. This may be seen by reference to Fig. 1. Here, as in most cases, except where the speed is so great as to make it difficult to complete the movement, the constant error is positive (often more strikingly so than here). This figure also illustrates the method indicated above of calculating the average error. The sum of all the positive errors is 62 mm. Now if the sum of the negative errors were equal to that of the positive, the constant error would be 0, and the last line would be of the same length as the first. Since the last line is 17 mm. longer than the first, the sum of the negative errors must be 17 mm. less than that of the positive errors. We may express the calculation in a general formula. If  $p$  be the sum of the positive errors and  $c$  the last line minus the first, then  $p-c$  represents the sum of the negative errors, and  $2p-c$  represents the total error. On dividing by the number of trials we get the average error. If it is desired to get the pure variable error, the correction can easily be made, first finding the constant error. As, however, the constant error is usually very small, the correction is not worth making.

The average error seems at any rate to be the best general measure of the accuracy of movement, when the normal is *perceived* without much constant error. Evidently the accuracy of a movement cannot be stated wholly in terms of the variable error. Its accuracy must be tested by the closeness with which it attains its goal. The constant error and the variable error may well be isolated and studied separately. But for a complete measure of accuracy the two must somehow be taken in connection with each other, and there seems no better way of connecting them than to leave them combined as nature made them.

There is one source of error in the method of making each line equal to the preceding: since the normal varies in length, the different movements in the same series are not strictly comparable. As the positive constant error is cumulative in its effect, the normal tends to become longer and longer. But this can be practically avoided by making the series short, or by dividing it according to length.

Of the four 'psychophysical measurement methods,' I have used principally that of 'average error.' Whatever may be the difficulties in the application of this method to other branches of psychological research, it is here the easiest in practice and the



most defensible in theory. In measuring the accuracy of marksmanship the most direct and complete method would evidently be to measure the distance of each shot from the bull's-eye, and obtain the average of these distances, or, better, to construct the distribution curve of the errors. As target practice is a type of all experiments in accuracy of movement, the same method can be used in them all with the same justification.

I have made use also of a sort of method of right and wrong cases. It resembles the counting of misses in a target practice, in place of measuring all the errors. Since the hits at a target are distributed around a center in accordance with the law of probability, it follows that if we count the number of hits that lie beyond certain limits, we can compute by means of the probability curve the number that lie within any given limits, or determine the limits within which any given proportion of the hits must lie. More generally, when a series of measurements is distributed around a known center in accordance with the probability curve, we can determine the whole curve by determining any one point. This is the principle of the method of right and wrong cases. We take a given difference and determine in what proportion of the cases it is perceived aright. And then, we argue, we know the whole distribution of the perceptions around the normal in question. The method of right and wrong cases is thus seen to be a selection from the method of average error. The latter determines every part of the distribution curve; the former, only one or two points of the curve directly, and the rest by theory. If, however, the distribution in any sort of experiment is not in accordance with the probability curve—as would be the case if the action were performed sometimes in one way, sometimes in another—then the method of right and wrong cases does not enable us to determine the true form of the curve, as the method of average error would.


Yet the method of right and wrong cases, or, at least, the method of counting misses rather than measuring all errors, has the advantage of saving time, and also enables us to make many hits at the same target. The close accumulation of hits around the center does not disturb our measurement, if we need only count the hits that lie outside of certain limits. Knowing this



number, and knowing beforehand the whole number of hits, we are able to compute the average error, as has already been done for the case of swinging the hammer. Or, we may fix on some proportion to be left outside—a proportion, for instance, that will give the average error or the error of mean square—and find by trial the limits to correspond. I will now describe the experiments in which I have used these devices.

In the first, which may be called the ‘coördinate paper experiment,’ the movement consisted in hitting with a pencil point at the center of each in turn of the small squares in a sheet of coördinate paper. The coördinate paper used was ruled in squares of either one-quarter or one-fifth inch sides (6.4 or 5.1 mm.). The squares within a block of one square inch were aimed at in the order of reading. After a large number of hits had been made the misses were counted. A hit that fell outside of the square or on its sides was counted a miss. A miss would thus measure longer in the corners than in the middle of the sides. But this introduced no error into the calculation, since the accuracy would in any case be proportional to the per cent. of misses. The disadvantage of this method is that a large number of hits must be made in order to get a reliable determination of the per cent. of errors. Its advantage is that the subsequent calculation of results requires only counting instead of measuring. This is a point of considerable importance in studying fatigue, since, in order to get fatigue, an immense number of hits is necessary, and some device for shortening the calculation is therefore almost indispensable. Besides that, it was desired not to confine the study exclusively to a single sort of movement, but to extend the observations to a reasonable variety of movements. The act of hitting rapidly at a series of targets is quite different from that of ruling a line equal in extent to another. It requires accuracy of direction as well as of extent. And it turned out also to be a more complicated act than that of hitting repeatedly at the same target. The motion is double; besides the vertical striking movements, there is a horizontal motion of the hand along the row of squares. The control of each is to some extent independent of the other. At the faster rates, however, it was not possible to aim each hit separately;

the horizontal movement of the hand along a row, and the four or five striking movements for that row, had all to be performed as one act. It is perhaps on account of the complexity of this movement that its accuracy was subject to great variation at different times. For this reason it does not commend itself as a test in individual psychology, or for possible clinical purposes.

Better adapted, because giving much more uniform results, is a device which may be named the 'three target experiment': 

Three dots about a millimeter in diameter were made on a sheet of paper at mutual distances of 15 cm., forming thus an equilateral triangle. The sheet was fastened to a table, at a definite position with respect to the subject of the experiment, so, namely, that two dots were about 5 cm. from the nearer edge of the table, and the other dot beyond. One of the two nearer dots—the one on the left when the right hand was to be used, and the one on the right when the left hand was to be used—was brought into the subject's medial plane. A metronome prescribed the speed.

Beginning at the further dot, and going round and round the triangle in a direction opposite to the hands of a watch, the subject aimed in succession for each dot. Exactly 50 hits were made at each dot. Then, to measure the accuracy of the result, the method was simply to find, by trial with compasses, the radius of the circle which would just enclose 34 of the 50 hits, and leave 16 outside. Those counted were, of course, the 16 lying outside. The center of the circle was so chosen that the hits were distributed around it about equally on each side. It represented the center of distribution of the hits. The distance of this center from the dot aimed at gave thus the constant error, which could be determined in direction as well as in extent. The radius of the circle gave the variable error of mean square, which is such that 68% of the cases lie within that distance from the average.

Doubt may naturally be raised as to the accuracy of this graphic method of measuring the results. I have tested it in two ways: (1) by making duplicate copies of a few sheets by the use of carbon paper, and measuring the same records twice independently, with an interval of over a month. The two sets of measurements differed on an average by 5.5%—a difference due largely to one or two palpable mistakes, but after all by no means excessive when we remem-

ber that the probable error of the measurements in question for a series of 50 hits is 6.7%. On compounding the two errors we find that the error of measurement increases the error due to chance from 6.7% to only 8.2%. The other way of checking the accuracy of the measurement was to compare its results with those of a more painstaking method. The latter consisted in having the targets made on millimeter coördinate paper, getting the distribution of the hits along each axis, and combining the results so obtained. The differences between these two methods of measuring the records were too slight to be considered, as far as concerns the variable error. As concerns the constant error, they differed considerably. I should not depend therefore on the graphic computation for an accurate knowledge of small constant errors. For the variable error, this method gives all the accuracy we need, with the expenditure of but a very small fraction of the time required in the other method. Of course, the method of simply measuring the distance of each hit from the target would be of no service when it is desired to separate the constant and the variable errors.

One distinct advantage, in the interest of accuracy, of the mode of computation adopted lies in the fact that it need attend only to the outside third of the hits. Any method which requires separate attention to the hits lying close to the target fails when the hits are closely bunched, since it is then impossible to decipher the record. On the whole therefore this graphic device probably gives as accurate results as can be attained by any method of measuring many hits at the same target—and for my purpose some repetition was necessary, in order that the speed might be controlled.

The object of using three targets instead of one was twofold: to increase the difficulty of the process and thus prevent to some extent automaticity of movement, and further to give a means for controlling the extent of the movements made. In work with a single target, it is difficult, and almost impossible if the speed is greatly varied, to insure that the hand be raised to the same height every time. The consequence is that the results at different speeds are not comparable. But here the horizontal distance to be covered between each two hits is so large that the hand is never raised high, and such differences as there may be in the heights are slight in comparison with the whole distance moved. To the device of putting a stop vertically over the hand I object, because it introduces a disturbing element. The rule should be so to arrange the experiment that the subject can devote his whole attention to the execution of his task without the interposition of vexatious "thou shalt" or "thou shalt not."

Details of procedure, as adopted in the several experiments, will be given in the form of notes to the tables which record their results.

The subject of accuracy of movement lends itself so readily to measurement and a quantitative treatment that my results have largely taken a numerical form. I have attempted to reach exact determinations of the accuracy under different conditions of speed, practice, fatigue, sensory control, etc. The numbers obtained are, perhaps, not very instructive in them-



selves; but a comparison of them leads to the detection of some of the influences which affect the accuracy of movement.

If the error is decidedly less under certain conditions than under certain others, it is clear that the former conditions contain some influence beneficial to accuracy. The amount of this influence is not the most important thing, though I have tried to determine it with some degree of certainty. The principal thing is the discovery of the influence in question. In such a search psychology cannot rely on introspection. We cannot tell from introspection what guides our movements. Nor can we rely on common observation, for in such matters it is inaccurate and far from concordant. We have to rely on a quantitative determination of the degree of accuracy obtained under different conditions. Here we have a method of psychology which does not depend upon introspection. And it seems undeniable that this method ought to be applied in as many fields as possible. It has been already applied in memory and in some problems of perception. Give the 'subject' some difficult task to perform under certain conditions from which he cannot escape (much as in a game); then vary the conditions, and measure and compare the success of his efforts, and you have a method which permits in the subject a direct and naïve attitude toward the problem that is set him, and which enables the experimenter to analyze at his leisure the factors which contributed to the given end. Undoubtedly, this method might be skilfully adapted to various departments of psychological research. It is readily, almost inevitably, applied to the problem of the accuracy of movement. The task set the subject is as direct as that of hitting a target. Only, the conditions are varied and the effects on his accuracy measured.

The italicized figures in the tables are to be disregarded except by those who wish critically to estimate the reliability of the numbers given and the significance of the differences between them. The figure in italics gives *the error of the average*—not indeed the 'probable error,' but the 'error of mean square,' which is proportional to it, being 48% greater. My reasons for choosing this measure of the reliability instead of the probable error are that the error of mean square is always

computed first and the probable error from it; and that, to my mind, the larger error gives a more desirable measure. The chances are nearly *even* that the true value of the average differs from the value obtained by not more than the probable error; while they are about 2 to 1, more exactly 683 to 317, that the true value differs from the value obtained by not more than the error of mean square. Now what we want is a determination that has a balance of probability in its favor. Odds of 2 to 1 are really too small, but they are better than even chances. In order to get limits within which the determinations are really reliable, we must multiply the error given in the tables by 2, 3 or 4. The chances that the true value of the average does not differ from the value obtained by more than twice the error in the table are 20 to 1; that it does not differ by more than three times that error, 360 to 1; that it does not differ by more than four times that error, over 16,000 to 1. When therefore an inference from a determination would no longer hold if the value were changed by not over once or twice the error given, the inference can be said to have at most a degree of probability; but when the inference would remain unaltered, though the value were changed by 4 times the given error, then the probability becomes a practical certainty.

Since most of the inferences to be drawn from the tables are drawn from the *differences* which appear between the several determinations, it may be well to remind the reader of the rule for obtaining the probable difference between two averages. If the error of the first is  $a$ , and that of the second is  $b$ , then the chances are 2 to 1 that the true values of the two averages do not differ from each other by more than  $\sqrt{a^2 + b^2}$ —that is, if the two groups from which the averages are made have really no essential difference from each other. If then the actual difference between the averages of two groups exceeds this value, the chances are 2 to 1 that the difference is not the mere effect of chance, but significant of some difference between the phenomena measured. If the actual difference exceeds 3 or 4 times this value, it is practically certain that we have a genuine difference between the two groups. If for instance we wish to see how much *a priori* probability there is that the difference between the average errors 8.6 and 11.8 in the first line of Table I, is significant, we obtain the square root of the sums of the squares of the individual errors, which gives us  $\sqrt{1.17}$ , or about 1.1. The odds are 2 to 1 that two averages having these individual errors will not differ from each other, as the effect of pure chance, by more than 1.1. As the actual averages differ by 3.2, or three times 1.1, the odds are about 360 to 1 that the two averages represent different phenomena, or that their difference is sig-



nificant. In other words, the studied movements with eyes shut are, in all probability, really as well as apparently more accurate than the automatic movements. Of course, this purely mathematical treatment leaves out of consideration the extreme variations which often appear in human performances, and which, while not due to the numerous small causes, the combined action of which we call chance, are still unaccountable and irrelevant to the function that is being studied. For this reason, a cautious reader will be on his guard, and allow for each observed average or difference a somewhat wider margin of possible error than the purely mathematical treatment would show. But, on the other hand, lists of figures without some such appended measure of their reliability are a snare. One cannot tell how much they may be worth. To give the number of observations from which the average is made is not sufficient, since the reliability depends also on the *variability* of the series. To give the mean variation alone is insufficient, since the reliability depends also on the number of observations. To give both the number of observations and their mean variation supplies all the necessary data, but leaves the computation to the reader. The error of the average, as given in these tables, affords the reader a direct measure of how much reliance can be placed on the average, from a purely theoretical point of view. This error of the average is obtained from the mean variation and the number of cases, by dividing the mean square deviation (1.253 times the mean variation) by the square root of the number of observations. Or, in most of our tables, since the average is itself an average error (practically identical with the variable error), the error of that average is obtained by dividing it by the square root of *twice* the number of observations. Conversely, the number of observations can be approximately computed from the tables, by dividing the square of the average by twice the square of its error. These numbers of observations are quite unequal in different cases, ranging from 50 to 300.

Some doubt may arise whether my work is really psychological or physiological. The field of voluntary movement undoubtedly lies, like the field of sensation, in the borderland between the two sciences. On which side of the border the present studies lie, I have not thought it worth while to attempt to decide.

### PART III. RELATIONS BETWEEN THE ACCURACY OF A MOVEMENT AND ITS SPEED.

It is clear that the study of accurate movement must consider at every step the speed of the movement. Two movements are not necessarily the same because they have the same length. If one is more rapid than another, a factor is thus introduced which will very conceivably affect the accuracy. We must, somehow, be able in our experiments to know the speed of the movements. The method here adopted for this purpose



is based on the ease with which we can 'keep time.' The *metronome* has been my constant companion during all this work. The subject has been required to make one movement at each stroke of the metronome, which might be set anywhere from 40 to 200 strokes per minute. In case still greater speed was desired the subject was required to make two or three movements to each stroke of the metronome. After a little practice almost everyone can meet these requirements without devoting any attention to keeping time.

Beside the methodological necessity of taking account of the speed of a movement, the relation of accuracy to speed is in itself worthy of study. Common observation teaches us that the accuracy diminishes when the speed becomes excessive, but further than that we shall have to rely on experiment. Will the accuracy diminish regularly, as the speed is increased, or will there be an optimal rate, at which the accuracy surpasses that at either faster or slower rates? How much more accuracy is attainable at low speeds than at high? Is the curve the same for the two hands, and the same with eyes shut as with eyes open? For answers to all such questions we must turn to special experiments.

I have tested the relation of accuracy to speed in several sorts of movements, which may however all be grouped into two classes: the ruling of lines on the drum, and the hitting at targets in different ways. I shall base my discussion of the matter on the results gained in ruled lines, and bring in the other experiments to confirm or to illustrate particular points. The movements of the experiments recorded in Tables I.-IV. were made by the method, defended above, of requiring each line to be equal to that which immediately preceded. Besides the tables, which give separately the results obtained from the individual subjects, I introduce composite diagrams representing the average results for the four subjects. These diagrams will form the basis of the exposition, and the tables will be directly referred to only in noting individual differences.

The relation of the accuracy of a movement to its speed is presented in Fig. 2. Since the ordinate is proportional to the average error of the movement, a rise in the curve denotes an

increase in the error, and consequently a decrease in accuracy. We see therefore that in a general way the movement loses accuracy as its speed is increased. As the number of movements per minute is increased from 20 to 200—as, therefore, the speed is increased tenfold—the error increases sixfold. We cannot

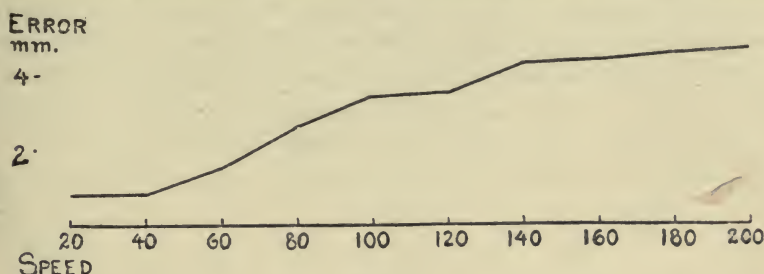


FIG. 2.

reduce the result to so simple a formula as that the error is proportional to the speed. Here the error increases too slowly to be proportional. In the left hand, as we shall see, it increases too rapidly to be proportional.

On looking more closely, we see that it is not even true that equal increments of speed produce equal increments of error. The line of ascent is not straight, but steeper in the middle portion than at either end. In fact, at each end there is a portion in which no perceptible increase in error attends the increase in speed. Movements at 40 per minute—that is, at intervals of 1.5 seconds—are on the whole quite as accurate as movements at 20, with intervals of 3 seconds. And movements at 140, 160, 180 and 200 are all about equally accurate. These facts will be studied more in detail in later connections. Here, by way of general explanation, it may simply be suggested that an interval of 1.5 seconds allows time for all the fine adjustments at the end of a movement—all the *groping about*, or adding on of slight additions—that can be done in an interval of 3 seconds. There is, therefore, a lower limit beyond which decrease in speed does not conduce to greater accuracy. And at the upper end there is a limit beyond which increase in speed does not produce much further inaccuracy. The reason is that beyond a speed of 140 to 160 movements per minute it is no longer

possible to control the movements separately. Much has to be left to the automatic uniformity of the hand's movements, and this, as we shall see, does not diminish as the speed increases.

In formulating this inverse relation between speed and accuracy we have been limiting our view to movements that were regulated by sight. If we now take into account movements designed to be equal but made without the help of sight, we shall find this inverse relation to hold no longer, at least as a general rule. Fig. 3 shows the relation that obtains when the

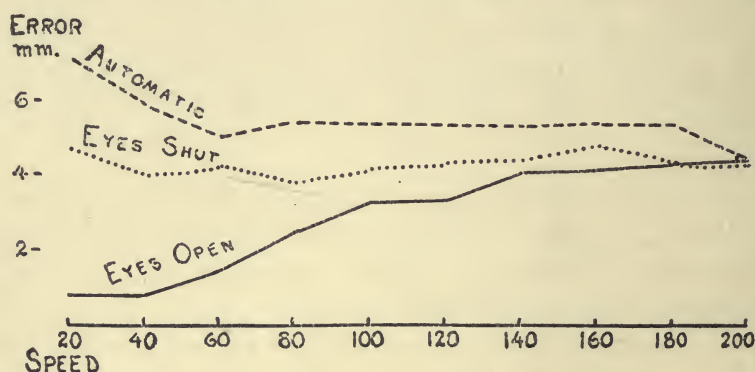


FIG. 3. Relation of accuracy to speed. Right hand.

eyes are closed, and also when the movement is careless or 'automatic.' (For explanation of the 'automatic' movements consult the text to Tables I.-IV.) We see that the automatic movements gain slightly in uniformity as the speed is increased, while the studied movements made with closed eyes are almost equally accurate (or inaccurate) throughout. The correlation between accuracy and speed is much slighter than when the eyes are used.

As between the three sorts of movement, we notice that that which is governed by the eye is much the most accurate at low speeds, and that the movement with eyes shut, though less accurate than this, is still decidedly better than the careless movement. But though this is true at low speeds, it is less and less true as the speed is increased. The gradual decrease in accuracy when the eyes are used, and the gradual increase in the uniformity of the automatic movements, finally bring all the



curves to about the same level. From 140 on, the accuracy is about as great blind as seeing and at 200 no effort avails to improve on the automatic uniformity of the movement. We may put these results in another form as follows: at high speeds the accuracy contributed by voluntary attention, using either the muscle (joint) sense or the eyes, amounts to zero. By decreasing the speed we greatly increase the accuracy due to visual control, but do not increase that due to the muscle sense.

All these inferences have been drawn from movements of the right hand. Fig. 4 shows that the left hand confirms them,

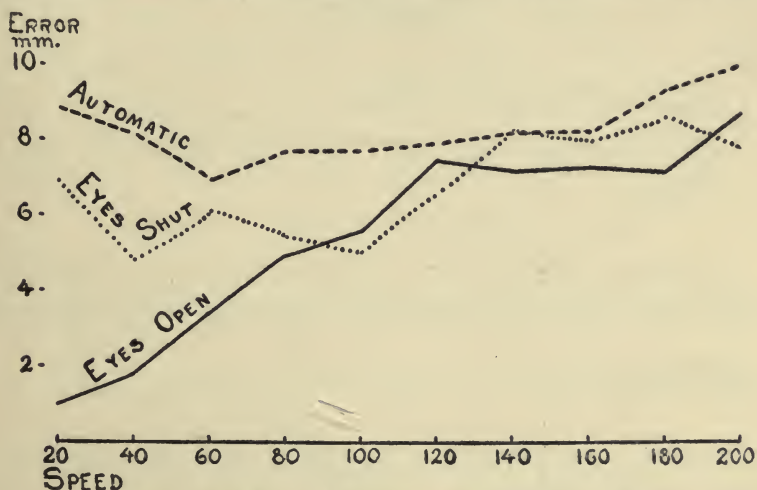


FIG. 4. Relation of accuracy to speed. Left hand.

with one exception. We do not find at the beginning of the curve for eyes open a flat portion, as in the right hand. An interval of 1.5 seconds is not here as good as one of 3 seconds. The accuracy diminishes rapidly up to the rate of 120 movements per minute, and from there up remains practically constant. The curves for automatic movements and for movements with eyes shut betray no decided tendency, no closer correlation between accuracy and speed than obtains under similar conditions in the right hand. The only general tendency is a sagging of both curves in the middle. The minimum of error—that is, the maximum of accuracy—occurs at intermediate speeds.

The point at which attention ceases to add any accuracy to automatic movement comes here at a lower speed than with the right hand. The interweaving of the curves begins as low as 100.

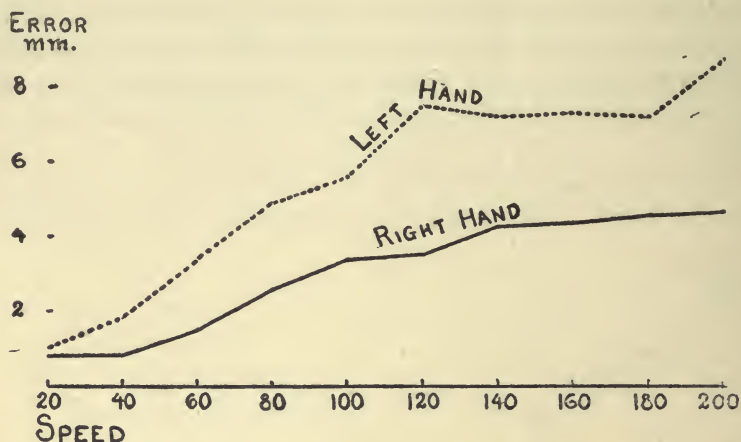


FIG. 5. Relation of accuracy to speed. Eyes open.

In order to compare more easily the accuracy of right and left hands, and thus to contribute a little to the study of bilateral

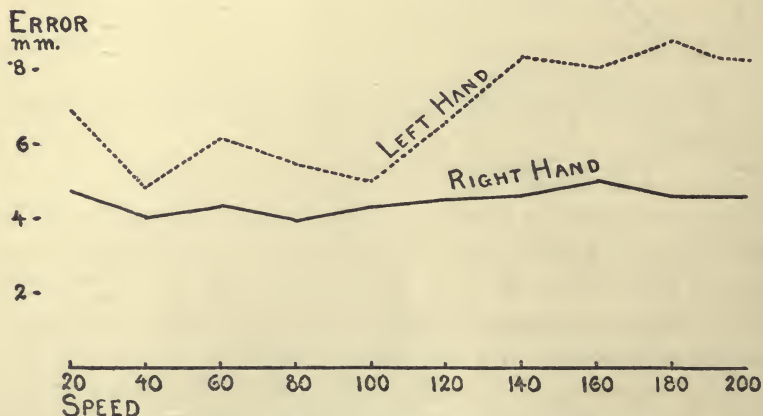


FIG. 6. Relation of accuracy to speed. Eyes closed.

asymmetry of function, the corresponding curves of Figs. 3 and 4 are paired off in Figs. 5, 6 and 7. Fig. 5 enables us to compare the accuracy of movements controlled by aid of the eye.

At the slowest rate employed, the accuracy attained by the left hand is practically the same as that attained by the right. But as the speed is increased the two curves diverge more and more, showing that the left hand is much more quickly and extremely affected by speed than the right. We find in Fig. 6 that the superiority of the right hand over the left is marked when the eyes are not used, and in Fig. 7 that the same is true



FIG. 7. Relation of accuracy to speed. Automatic movements.

even of the uniformity of automatic movements. In both cases the superiority is clearly visible at the lowest rates, but still more so at the high speeds.

These differences between the hands are not peculiar to this one sort of movement, but appear also in target experiments of different kinds, as is shown in Tables XII. and XIV.

The advantages of the right hand over the left, in point of accuracy, would accordingly seem to be three :

(1) While not capable of greater precision than the left when plenty of time is allowed, it can be controlled much more rapidly. Slow movements can be made as accurately with the left hand, provided the eyes are used—which means, provided a delicate sensory control is used. If plenty of time is allowed, either hand can probably be adjusted as fine as the naked eye



can see. But if the speed is increased beyond a certain point—a point which will have different positions for different kinds of movements—the left hand can no longer be controlled as closely as the right. The seat of this superiority of the right hand is probably in the motor centers. The same may be said of the next point of superiority.

(2) Even in automatic movements, the right hand shows a greater uniformity than the left. This is presumably an effect of the greater practice that the right hand has had in making uniform movements. It furnishes a basis of regularity on which the voluntary accuracy of the right hand can build.

(3) Inasmuch as the right hand gives better results also when the eyes are closed, it would seem that the muscle, joint and skin sensations from the right arm are probably more delicate than those from the left. Yet it is possible that (3) is simply an effect of (2).

TABLE I.

MM.	RIGHT HAND.						LEFT HAND.					
	EYES OPEN.		EYES SHUT.		AUTOMATIC.		EYES OPEN.		EYES SHUT.		AUTOMATIC.	
20	1.1	.1	6.7	.5	6.5	.5	2.3	.2	8.6	.6	11.8	.9
40	1.3	.1	6.0	.4	5.2	.4	2.5	.2	5.3	.4	10.3	.7
60	2.2	.2	6.4	.4	5.8	.4	4.4	.3	6.8	.5	9.3	.6
80	3.8	.2	6.2	.5	7.4	.5	6.7	.5	7.0	.5	9.5	.6
100	4.0	.3	6.1	.4	6.2	.4	8.0	.6	6.7	.5	9.7	.7
120	3.3	.2	7.2	.5	7.9	.5	9.0	.6	7.4	.6	8.5	.6
140	5.4	.4	6.3	.5	6.0	.4	7.5	.6	10.4	.8	9.3	.6
160	6.1	.3	6.3	.3	6.9	.4	7.6	.4	9.8	.7	9.3	.6
180	5.8	.4	5.8	.4	7.9	.5	7.5	.5	8.8	.5	11.3	.7
200	6.0	.3	5.6	.3	5.0	.3	9.6	.5	8.4	.5	10.4	.7

TABLE II.

MM.	RIGHT HAND.						LEFT HAND.					
	EYES OPEN.		EYES SHUT.		AUTOMATIC.		EYES OPEN.		EYES SHUT.		AUTOMATIC.	
20	0.9	.2	4.1	.3	4.7	.3	0.9	.1	6.0	.5	8.7	.6
40	1.0	.1	3.6	.2	4.6	.3	3.1	.2	4.7	.3	6.7	.4
60	1.6	.1	4.1	.2	4.0	.2	4.4	.3	5.6	.4	6.0	.4
80	1.6	.1	2.4	.1	4.1	.2	6.2	.6	4.7	.3	10.5	.8
100	2.9	.2	4.4	.2	4.7	.2	5.3	.4	5.2	.4	11.0	.8
120	2.8	.2	3.9	.2	3.8	.2	7.2	.6	5.7	.3	8.9	.6
140	3.2	.2	4.3	.1	4.6	.2	8.3	.5	7.7	.5	8.5	.6
160	4.2	.2	4.3	.1	4.8	.2	6.3	.4	8.0	.6	7.4	.4
180	4.2	.2	4.6	.1	5.1	.3	8.2	.4	9.3	.6	8.8	.5
200	4.4	.1	4.8	.2	4.8	.2	9.8	.5	8.5	.4	9.8	.5

TABLE III.

MM.	RIGHT HAND.						LEFT HAND.					
	EYES OPEN.		EYES SHUT.		AUTOMATIC.		EYES OPEN.		EYES SHUT.		AUTOMATIC.	
20	0.4	.1	2.1	.2	10.0	.6	0.5	.1	6.2	.7	6.5	.7
40	0.6	.1	2.7	.3	8.0	.4	0.8	.1	4.4	.4	7.1	.7
60	1.3	.1	2.9	.3	5.6	.3	1.9	.2	5.9	.7	5.4	.6
80	3.2	.2	3.1	.3	4.9	.3	4.2	.4	4.4	.4	4.8	.5
100	3.3	.2	3.3	.3	5.7	.3	3.7	.4	3.1	.3	3.9	.4
120	3.5	.2	3.6	.3	4.8	.3	5.6	.6	6.8	.5	6.4	.6
140	3.8	.3			5.9	.3						
160	4.0	.3	4.9	.4	5.2	.3	10.4	1.0	7.0	.6	7.5	.6
180	4.4	.3			4.7	.3						
200	4.7	.5	4.6	.4	4.4	.2	9.3	.8	7.2	.5	6.0	.5

TABLE IV.

MM.	RIGHT HAND.						LEFT HAND.					
	EYES OPEN.		EYES SHUT.		AUTOMATIC.		EYES OPEN.		EYES SHUT.		AUTOMATIC.	
20	0.6	.1	5.7	.3			0.4	.1			8.3	.6
40	0.4	.1	3.8	.3			0.9	.1			8.4	.5
60	0.8	.1	3.8	.2			3.0	.2			6.8	.3
80	1.9	.1	3.8	.3			2.6	.2			5.9	.3
100	3.4	.1	3.4	.2	3.2	.2	5.5	.4			6.0	.2
120	4.2	.3	3.3	.2	3.9	.2	7.1	.5				
140	4.7	.3	3.2	.2	4.2	.2	5.8	.4				
160	3.4	.2	4.3	.3	4.0	.3	5.0	.3	7.3	.5	8.7	.7
180	4.0	.2	3.5	.2	4.9	.3	5.8	.3	8.1	.6	9.0	.6
200	3.8	.2	3.3	.2	3.4	.2	6.6	.4	7.5	.5	10.2	.7

Relation of accuracy to speed; lines ruled on kymograph. Each line required to equal the preceding. In the column headed 'MM' are given the number of movements per minute, as prescribed by a *metronome*. The other columns give *average errors in millimeters*. In the columns headed 'open' the eyes were used in guiding and checking the movement. In the columns headed 'shut' the eyes were shut and the guidance depended on the tactile senses. In the column headed 'automatic,' though the eyes were open, they were not directed to the work, but wandered around the room or out of the window. These movements were entirely careless—'automatic' I have called them. They were not automatic in the sense used in speaking of automatic writing, but more in the sense in which walking is automatic. Most of my subjects soon learned to attend to these movements just enough to keep them going. No attention was devoted to making them equal.

My object in introducing such automatic movements was to obtain a sort of *zero mark* for the accuracy produced by voluntary effort. A certain amount of uniformity may be expected in a repeated movement, even if it be left to itself. The additional accuracy produced by voluntary control starts with this automatic uniformity as its basis, or as its zero mark.

Of the four persons who served as subjects in these experiments, D, F and W were men of 25-30 years, students of psychology, and P was a young lady, a clerk in the laboratory. Others who served in occasional experiments, G, H, Sp, Dx, were also students of psychology, and Sn a law student.

Having now drawn the general conclusions that proceed from the composite curves, we next turn to the tables and look for individual differences.

We notice that the individuals differ markedly in accuracy; but that it is not always the same individual that is most accurate at all speeds or in all the varieties of the experiment. Decidedly the least accurate of the four is P, then comes W, while D and F are close contestants for first place. The same general standing was maintained by these four subjects in another form of test.

The individual curves, if drawn, would have about the same shape as the composite curve, and the conclusions reached above would be substantiated in the individual cases (with one exception).

Thus, when the eyes are used the accuracy of the right hand always shows the same slow change at low and at high speeds, with abrupt changes at certain intermediate points, as at 80 or 100.

The left hand is at 20 always about as good as the right (eyes open), but rapidly becomes less accurate as the speed is increased. The right hand is in all other cases more accurate than the left.

In the movements made with shut eyes no distinct correlation appears between speed and accuracy, *except* in two series. In D's right hand the accuracy *increases* quite perceptibly with the speed up as far as 140. And in F's right hand the accuracy *decreases* as the speed is increased, in exactly the same way as when the eyes are used. This is a distinct exception. This subject visualized very strongly, but I fail to see why this should help his right hand and not his left. I can only infer that this subject has a much finer muscular and joint sensibility in his right arm than the rest of the persons tested.

The individual curves of automatic movements show no strong tendencies, yet they present some fairly-marked differences. In right-handed automatic movements no definite tendency in any direction is shown except by F, who gives an increase of uniformity with speed. In the left hand, only W shows no definite tendency; each of the others has a minimum



of error—*i. e.*, a maximum of accuracy—at some intermediate speed: D at 80, F at 100, P at 120.

We must now examine the results obtained, with a view to analyzing them and detecting the factors which make for accuracy and for inaccuracy.

Our first question is as to whether the decrease in accuracy at high speeds is due to the increase in speed *per se*, or to the diminution of the interval between successive movements. All along, the increase of speed has been brought about wholly by making the metronome beat faster, thus compelling a more rapid movement, but at the same time diminishing the interval between successive movements. Now inasmuch as some time is undoubtedly needed, at each trial, for the perception of the new normal, and for adjusting the movement to this perception, it is quite conceivable that the decrease in accuracy at high rates may be due to insufficiency of time for perception and adjustment. The adjustment here referred to is of course the initial adjustment. The later and finer adjustments would be interfered with by the speed rather than by the interval.

In order to answer this question it is only necessary to vary the speed and the interval independently. This is accomplished most simply by setting the metronome at a slow rate, and requiring the movement to be now slow, now rapid, but always at the same long interval. The results appear in Table V.

TABLE V.

	EACH — PRECEDING				RULING TO A LINE.			
	AV. E.		C.E.		AV. E.		C.E.	
All slow.	0.9	.1	+0.3	.1	0.6	.1	+0.3	.1
All fast.	4.5	.4	+0.1	.6	4.9	.4	+0.2	.4
Alternately { Slow.	0.8	.1	+0.4	.1	0.5	.1	+0.3	.1
{ Fast.	4.6	.4	+1.1	.5	4.3	.3	+1.7	.4

Interval constant (one second), but speed varied. Errors in mm. Subject W. The experiment consisted in ruling lines on the kymograph. The speed was not exactly regulated, the slow movements being simply as slow as was possible at the prescribed interval, and the fast movements much more rapid.

The table gives the results obtained when the eyes are used. When the eyes were closed, and the movement was alternately slow and fast, the results were quite different, as follows:

With the metronome set at 40,

	Average Error.	Constant Error.
Slow movements gave . . . . .	13.9	-12.7
Fast " " . . . . .	13.0	+10.6
With the metronome set at 60,		
Slow movements gave . . . . .	12.5	-11.6
Fast " " . . . . .	13.7	+12.9

The results may, therefore, be summarized as follows:

1. When the movement is controlled by the eyes it is much less accurate at a high speed than at a low—and that although the interval remains the same.

2. But when the eyes are shut changes of speed have no perceptible influence on the *average* error.

3. When the eyes are shut differences in speed do, however, introduce different *constant* errors. The faster movements are, almost without exception, too long, the slower too short. The subjective measure for the length is different at the two speeds. This result agrees with one obtained long ago by Vierordt and Camerer,<sup>1</sup> and confirmed by Loeb.

TABLE VI.

INT. IN MIN.	SUBJ. D.	SUBJECT F.				SUBJECT P.				SUBJECT W.						
		SER. 1.		SER. 2.		SER. 1.		SER. 2.		SER. 1.		SER. 2.		SER. 3.		
$\frac{1}{200}$	3.0	.3	4.5	.5	2.8	.2	4.9	.6			4.8	.5	4.0	.4	4.4	.1
$\frac{2}{200}$	6.7	.4	5.1	.5	5.0	.5	4.4	.5			3.8	.4	2.1	.2		
$\frac{3}{200}$			5.2	.4	2.0	.2									3.7	.2
$\frac{4}{200}$	4.0	.3	6.6	.6	6.9	.6	5.9	.8	3.9	.5	4.9	.6	3.9	.3	4.5	.2
$\frac{5}{200}$	6.9	.7	6.6	.6	6.6	.7									4.2	.2
$\frac{6}{200}$	4.7	.4	9.1	.9	5.9	.6	4.9	.5	5.6	.6	5.4	.7	4.9	.5	5.0	.2
$\frac{7}{200}$	5.0	.4	6.0	.6	5.3	.5	4.8	.5	5.4	.6						
$\frac{8}{200}$	4.7	.4	6.5	.7	10.4	1.0	5.2	.6	6.1	.6	6.5	.5				

Speed constant, but interval gradually increased. The metronome was set at 200. Where the interval is recorded as  $\frac{4}{200}$  sec., for instance, the meaning is that one movement was made at each fourth beat of the metronome. The interval was counted off by an assistant or by a bell on the metronome. In some of the series, small movements, in time with the single beats of the metronome, but uncontrolled as to length, were kept up during the intervals. In other series the hand simply rested during the intervals. Again, in some of the series the normal was in each case the preceding line, in other series the movement was required to terminate at a seen line. As follows:

Subject D, intervals vacant, each = preceding.

Subject F, ser. 1, intervals vacant, ruling to line.

ser. 2, " " each = preceding.

Subject P, ser. 1,

ser. 2, movements during interval, each = preceding.

Subject W, ser. 1, intervals vacant, ruling to line.

ser. 2, movements during interval, ruling to line.

ser. 3, " " each = preceding.

<sup>1</sup> See Vierordt. Der Zeitsinn, 1868, p. 148.

In the preceding experiment the interval was constant and the speed varied. In Table VI. the opposite is the case. This was accomplished by setting the metronome at a *high* rate, and requiring each movement, while consuming the time of a single beat, to be executed now at every beat, now at every second, now at every third, etc. In this way the speed was kept (approximately) constant, while the interval was increased from  $\frac{1}{200}$  to  $\frac{10}{200}$  of a minute. The results show that the correlation between interval and accuracy is by no means so close as that between speed and accuracy, but that, on the whole, the *accuracy diminishes as the interval is prolonged*. This unexpected result will be discussed after we have presented more complete evidence that it is a fact. This is found in Tables VII.-XI., in which both quantities are varied, but independently.

TABLE VII.

[illegible]

TABLE VIII.

[illegible]



TABLE IX.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	140	160	180	200
$\frac{1}{20}$	0.9 .1	1.8 .2	5.0 .5	3.2 .3	6.4 .7	6.0 .6	9.5 .9	6.8 .7	10.3 1.0	9.8 .9
$\frac{1}{40}$		1.3 .1								
$\frac{1}{60}$			2.2 .2							
$\frac{1}{80}$				3.8 .2						
$\frac{1}{100}$					4.0 .3					4.4 .5
$\frac{1}{120}$						3.3 .2				
$\frac{1}{140}$							5.4 .4			
$\frac{1}{160}$								6.1 .3		
$\frac{1}{180}$									5.8 .4	
$\frac{1}{200}$										5.5 .6

TABLE X.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	140	160	180	200
$\frac{1}{20}$	0.6 .1	5.1 .5	2.2 .2	5.2 .6	4.3 .4	4.3 .5	5.2 .6	4.9 .5	5.0 .5	10.4 1.0
$\frac{1}{40}$		0.5 .1		3.9 .4		4.4 .4		6.3 .6		6.6 .7
$\frac{1}{60}$			1.5 .2			3.9 .4			4.8 .4	
$\frac{1}{80}$				2.3 .2				3.7 .3		
$\frac{1}{100}$					3.8 .3					5.0 .5
$\frac{1}{120}$						3.3 .3				
$\frac{1}{140}$							2.9 .2			
$\frac{1}{160}$								3.1 .2		
$\frac{1}{180}$									4.5 .3	
$\frac{1}{200}$										2.8 .2

TABLE XI.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	160	200		
$\frac{1}{20}$	0.1	0.2	3.2 .3	3.2 .3	3.3 .3	3.7 .4	3.0 .3	6.5 .7		
$\frac{1}{40}$		0.1						6.6 .6		
$\frac{1}{60}$			0.6 .1							
$\frac{1}{80}$				2.8 .3						
$\frac{1}{100}$					4.4 .4				5.1 .5	
$\frac{1}{120}$						3.5 .4				
$\frac{1}{140}$										
$\frac{1}{160}$							3.2 .3			
$\frac{1}{180}$										
$\frac{1}{200}$								4.5 .5		

Interval and speed varied independently. The interval was varied as described in connection with Table VI. The speed was varied by requiring a movement to occupy the time of a single beat of the metronome, and setting the metronome at different rates; keeping the interval constant meanwhile by requiring a movement to be made only with every second, third or fourth beat of

the metronome. Thus in the uppermost line the interval was kept constant and the speed varied by making a movement at *each* beat when the metronome was at 20, at each *second* beat at 40, each *third* at 60, each *fourth* at 80, and so on. The interval was thus kept constant at three seconds, and the speed was successively proportional to 20, 40, 60, 80, etc.

VII.	is from subject D, each = preceding
VIII.	" " " W, " "
IX.	" " " P, " "
X.	" " " F, " "
XI.	" " " F, ruling to seen line.

Along the horizontal lines in the tables, the interval is constant and the speed varied; and we see that the error everywhere increases with the isolated speed. Along the vertical lines, the speed is constant, but the interval is varied; and we see that the error almost always diminishes as the interval is shortened. Finally, along the main diagonal, and along other oblique lines which can be traced, the speed and interval are varied simultaneously as in the ordinary experiments; and the result is the same as was obtained above in those experiments. We notice a more rapid increase in error along the horizontal lines than along the oblique lines—a consequence of the inverse relation of accuracy to the interval.

The conclusions to be drawn from these experiments are evidently these:

(1) The great factor operating against accuracy is the mere speed of the movement.

(2) The short intervals allow plenty of time for perception and initial adjustment—that is to say, for so much perception and adjustment as the rapidity of the movement allows to be put to use.

(3) If we distinguish between the *initial adjustment* of a movement to the desired length, and the *current or contemporary control* exerted over it during its progress, we see that the conditions of current control are not changed by altering the interval preceding the movement, in independence of speed. The current control would be influenced by the time occupied by the movement itself, but not by the interval preceding. The unfavorable influence of a long interval must therefore be exerted solely on the initial adjustment. There is something about a

short interval that is more conducive to an accurate adjustment of the initial impulse. What that something may be is suggested by the fact, already established, that a fair degree of uniformity appears even in automatic movements, and that this uniformity is rather increased than decreased by high speed. There is a certain momentum of uniformity, which would, of course, be favored by ease of rhythm, and apparently also by a short interval between movements. Whatever additional uniformity is contributed by attention to the initial adjustment is probably favored by the same conditions. The nerve centers will more readily discharge in a given way, if they have but very recently discharged in the same way.

It has thus developed that our original method of increasing the speed of movements, by decreasing the interval allowed between them, involved the contrary action of two influences: speed proper, tending to inaccuracy; and shortness of interval, tending to uniformity. The former was the stronger influence, so that the resultant effect was to produce a decrease in accuracy. But beyond a certain point this decrease scarcely continued; shortness of interval balanced speed.

The bad effect of speed consists in rendering impossible a delicate *current control*, in preventing those later and finer adjustments by means of which a movement is enabled to approximate more and more closely to its goal. On the other hand, the bad effect of a long interval is exerted on the *initial* impulse. The initial impulse becomes less certain as the interval that has elapsed since the last similar impulse increases.

If this interpretation of the results be correct, two inferences follow and may be tested. First, any sort of movement in which the current control is so imperfect that the accuracy depends mostly on the initial adjustment will be expected rather to gain than to lose from hastening the beat of the metronome. The resulting increase in speed would then do no harm, while the decrease in interval would do good. Second, any movement of which the initial adjustment has to act in opposition to the momentum of uniformity will lose accuracy *very rapidly* as the beat is hastened. The speed will do harm, and the shortened interval no good. Each of these inferences is substantiated by facts.



An example of imperfection in current control is afforded by almost any movement with the eyes shut. If the reader will consult again Figs. 3 and 4, and Tables I.-IV., he will notice that the general tendency is for the accuracy to increase with the speed, at least up to a certain point. This tendency would be more marked in the composite curves if it were not for the unusual accuracy shown by subject F at the low speeds, pointing apparently to an ability on his part to perceive the extent with a fineness sufficient to permit of good current control.

Further examples are afforded by those columns of Table XII. which record the results obtained with eyes shut. In this 'three-target experiment' the current control of a movement was very slight indeed when the eyes were not used. We see in the results a tendency to a minimum of error at middle rates. The tendency is unmistakable in the right hand. Here again we see, therefore, that increasing the speed increases the accuracy up to a certain limit. And since the accuracy of the movement is here determined almost entirely by the initial adjustment, we conclude, as before, that diminishing the interval improves the initial adjustment.

Evidence along this same general line is afforded by the 'curve of forgetfulness' established by Bowditch and Southard in the case of locating a point with the hand, and by the similar curve established by Münsterberg for the memory of the extent of arm movements. Both of these results have been summarized in the section devoted to the literature. In both cases it was found that, up to a certain limit, increasing the interval decreased the accuracy. That the limits differed widely in the two cases is perhaps because of the difference in the function studied, and perhaps because Münsterberg did not consider intervals smaller than 2 seconds. It is possible that there may be another maximum of accuracy below that limit. That this supposition is probably correct my results lead me to believe. There seems to be a distinct tendency for the accuracy of the reproduction to increase, when the eyes are shut, from 20 to 80 or 100—that is, from an interval of 3 seconds down to one of 6 or 7 tenths of a second.

The question of an *optimal interval*, which has been close at

hand for several pages, is thus now squarely before us. We have found that decreasing the interval acts beneficially to the accuracy of the initial adjustment, but always up to some limit. Is there then some particular interval which is best adapted to secure accuracy in the initial impulse, which is just long enough to secure the maximum accuracy which that impulse can attain? Can we point out and determine such an interval? The proper way to settle the question would be to turn to our table of results when the interval was varied independently of the speed, and look for minimum errors. On consulting Table VI. and running up the vertical columns in Tables VII.-XI., we get rather discrepant indications. We find no indication of an optimal interval greater than one second. So much at least is certain. Decreasing the interval down to one second is pretty sure to increase the accuracy. And the appearances are that it may be profitably decreased still further. Subjects D and F seem to find the shortest interval allowed better than anything longer. Subject W, on the contrary, seems to find 0.6 sec. ( $\frac{2}{3}$  min.) better than 0.3 sec. It is to be regretted that the conditions of the experiment did not permit the use of intervals between those values, while keeping the speed constant. It is entirely possible that the optimal interval lies within this gap. It is also likely that it is longer for the left hand than for the right. As far as we can judge from the curves of automatic movements and of movements with eyes shut (Figs. 3 and 4), we should judge that for the right hand all intervals from '80' on (*i. e.*, from three-fourths of a second down to three-tenths) were about equally good, the very smallest possibly a little better than the rest; and that for the left hand the optimum would lie almost anywhere between a second and a half second.

These results do not enable us therefore to locate an optimal interval with any high degree of probability, nor even to establish the existence of such an interval with certainty. It is however practically certain that such an interval exists. We have clearly seen that decreasing the interval does at first increase the accuracy. But this increase can hardly continue clear down to an interval zero. There must be a turning-point somewhere. These results suffice to show that the turning-point lies below

one second, and that for the left hand it probably lies above 0.3 sec.

If we now look back over our evidence for the 'first inference' made above, we shall find it satisfactory. We inferred that *if* the speed of a movement interfered with its accuracy only by interfering with the current control, and not by interfering with the initial adjustment, *and if* shortening the interval between movements favored accuracy only by helping the initial adjustment, and not by helping the current control, *then* a movement which had to depend for its accuracy on its initial adjustment should become more accurate as the speed increased, provided the interval decreased at the same time. The initial impulse would be helped, and there is no other source of accuracy to be injured. We found that this was true within certain limits. To explain the limitation, we have the very probable assumption of an optimal speed. We have qualified our first rule, that 'the accuracy increases as the interval is shortened,' by the addition of 'within certain limits.' And within these limits, as far as examined, we have found our inference to hold. Everything looks as if we were right in supposing the beneficial influence of shortening the interval to be exerted primarily on the initial adjustment.

Our 'second inference' above will be more easily established. We inferred that any sort of movement of which the initial adjustment had to act in opposition to the momentum of uniformity would lose accuracy very rapidly as the speed is increased. In such a case the decrease in interval would not begin to compensate for the increase in speed. We should have quite a different curve from that of Fig. 5, in which the highest speeds are scarcely less accurate than the intermediate. In the movements on which Fig. 5 is based the automatic uniformity came in and helped at high speeds. But suppose the movements were such that automatic uniformity would hinder rather than help the initial adjustment—then the power of current control would be lost without anything to take its place.



TABLE XII.

MM.	SUBJ. D.	SUBJ. F.	SUBJ. W., RIGHT H.		SUBJ. W., LEFT H.	
	R. H. OPEN.	R. H. OPEN.	OPEN.	SHUT.	OPEN.	SHUT.
40	0.1	0.3	2.0	21.3	2.8	15.6
80	0.2	0.6	2.6	14.1	4.3	14.0
120	2.0	3.1	3.8	13.6	5.6	20.2
160	4.1	5.6	5.2	9.3	8.6	12.1
200	6.5	7.5	7.3	11.6	10.5	19.1
240	6.8	9.0	8.2	10.4	10.3	15.2
280	13.3	10.6	9.6	11.5	14.5	22.1
320		15.5	12.1	19.5	20.1	21.3
360			13.4	20.3	26.2	26.8
400			28.3	24.7		

'Three-target experiment.' Subjects D and F with right hand, and open eyes. Error (of mean square) of each average = 6% thereof, except in column 3, where it = 3%.

Such a movement is that of the 'three-target experiment,' Table XII. Here each initial adjustment is a repetition not of the last, but of the third before. Between, two movements of quite different direction have been made. And anything like automaticity is excluded by the angular character of the whole movement. The natural tendency to cut the corners is not a help, but a hindrance to accuracy. Thus the 'momentum of uniformity,' if operative at all, is so only to a slight extent. The result is that, in proportion as speed interferes with the current control, the accuracy is lost. There is no flattening out of the curve at the higher rates, but the error increases finally by great jumps, making the last part of the curves the steepest.

This is the result with eyes open. That with eyes closed, giving a minimum of error at intermediate rates, has already been discussed. If our assumption of an optimum interval is correct, and if that interval be located for the present movement at about 160—as seems likely from the columns for eyes closed—then not all the increase in error at high speeds would be due to the failure of contemporary control. The latter supposition would in any case be hardly justified; for very little readjustment of a movement is possible above about 200. But if the optimal interval has been passed, then the initial adjustments also will be becoming inaccurate, and the combined loss in accuracy will be very great.

TABLE XIII.

	60	80	100	120	140	160	180	200	240
Aimed at each.	2.1 .1	2.6 .1	3.2 .1	3.8 .2	4.4 .2	5.2 .2	6.1 .2	7.3 .3	8.2 .3
Aimed at only one.	1.9 .2	1.4 .1	2.1 .2	2.0 .2	2.0 .2	3.2 .3	3.1 .3	3.8 .4	3.7 .4

'Three-target experiment.' Subject W. In the first row the subject aimed at each target in succession, as in the experiments of Table XII. In the second row target No. 1 only was aimed at, the others being hit at carelessly; the eyes remained fixed on No. 1. Only the hits at target No. 1 were counted.

That there is an optimum interval below 200, and that not all the increase in error is due to contemporary control, results from a slight variation of the experiment. Since it was found difficult at high speeds to move the eyes around the triangle of targets fast enough to get a good aim at each one, I tried the effect of aiming at only one of them, but still carrying the hand around to the triangle and hitting more or less at random near the other targets. The result was that the task became, introspectively, much easier, and that the error was much reduced. This result can be seen in Table XIII. It means that a large part of the error at high speeds was due, not to failure of the current control of each movement—for the speed of the movement was the same as when every target was tried for—but to the failure to get a good aim at the start; in other words, to the lack of a good initial adjustment. Inasmuch as the error remains almost constant from 60 to 140 when only one target was aimed at, although the increase of speed made the current control of each movement less accurate, we may infer that the initial adjustment was all along becoming more accurate, and, therefore, that the interval was becoming more favorable. But at about 160 the rapid loss in accuracy points to a deterioration of the initial adjustment. At about that point the optimal interval must have been left behind. This agrees fairly well with the location of the optimal interval at 160, from the columns for eyes shut in Table XII. It is quite probable that the best interval would be longer in the more difficult movement of the 'three targets' than in the more automatic movement of ruling equal lines.

TABLE XIV.

MIN.	RIGHT HAND.			LEFT HAND.		
	1ST SER.	2D SER.	AV.	1ST SER.	2D SER.	AV.
20	7	0	5.4 1.0	2.8	1	2.4 .7
40	6	3	6.0 1.1	2.8	0	2.2 .7
60		6		2.3	2	2.2 .7
80	7	0	5.4 1.0	3.3	2	3.0 .8
100		6		6.0	8	6.4 1.1
120	7.5	9	7.4 1.2	8.5	9	8.6 1.3
140		8		14.5	13	14.2 1.6
160	9.5	7	8.8 1.3	14.8	19	15.6 1.6
180		16		17.3	18	17.4 1.7
200	12.5	10	11.4 1.4	22.3	18	21.4 1.8
240	9	15	10.2 1.3	28.8	30	29.0 2.0
280	19	23	20.2 1.8	40.0	43	40.6 2.2
320	18	27	19.8 1.8	43.0	48	44.0 2.2
360	22	26	22.3 1.9	52.3	57	53.2 2.2
400	28	38	30.2 2.0	61.5	61	61.4 2.2
480	33	52	36.9 2.2			

'Coördinate paper experiment.' The targets were quarter-inch squares. A block of sixteen was done at one rate and then the rate was changed. Subject W. Each first series includes 400 movements at each speed. Each of these series includes experiments of several different days. The second series for each hand includes 100 movements at each speed—all of them done the same day, several weeks after the close of the first series. *The entries in the table give per cents. of misses.*

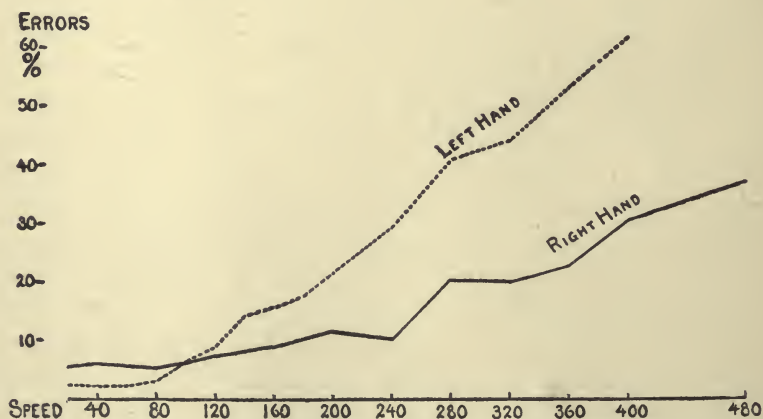


FIG. 8.—Relation of accuracy to speed, in the 'coördinate paper experiment.'

A movement somewhat similar to the last, one in which the new adjustments are not repetitions of the old, and in which, accordingly, increase in speed brings decrease in accuracy, is



the movement of the 'coördinate paper experiment.' The results are recorded in Table XIV., and presented to the eye in Fig. 8. The increase in error is fairly uniform with the left hand, except that it does not begin till 80. The right hand, however, shows some irregularities, which may be significant. There are some speeds which give greater accuracy than those on either side. This would appear more strongly in the records for separate days. Much may be attributed to mere chance; but probably not all is to be so explained. There was generally a feeling on the part of the subjects—and several subjects expressed the same feeling—that this or that speed was preferable to those just faster and just slower. The preferred speed was not the same for different subjects, nor always for the same subject at different times. No importance would attach, therefore, to determining its exact value in a given experiment. But the reason for the optimal speed is worth seeking. Subjectively, it was that this or that was a more convenient rhythm for the movements. Above about 200 the movements were too near together to receive separate attention. It was necessary to adjust for 2 or 3 or for a row of 4 or 5 all at once. In other words, a sort of automatism entered. It was not exactly automatism, but rather compoundness of voluntary impulse. One such impulse produced several hits. This affords an opportunity once more for an optimal interval between the hits, and such an interval makes its appearance, and that consciously, though not always at the same point. It is quite possible, therefore, that the slower ascent of the right-hand curve up to 240 is an expression of some advantage found in the shortened interval, an advantage which partially compensated for the increase in speed. But above 240 this compensation was lost, both initial adjustment and current control suffered, and the decrease in error was rapid.

Another fact which hints at some peculiarity of the initial adjustment came to light in this coördinate paper experiment. Before relating the fact one or two details of the procedure must be described. After a block of 16 squares had been hit at, the metronome was shifted, and the next block done at a higher or a lower rate. Usually the experiment started at either the slowest or the fastest rate, and proceeded gradually to the other extreme, and then back again (*e. g.*, in the order 20, 40, 60 \* \* \* 480, or else 480, 400, 360 \* \* \* 20). It

seemed to the person making the movements that he could do better when the speed was being increased than when it was being decreased. On examining the records, this was found to be the fact, especially as regards the middle rates, from 120 to 320 hits per minute, inclusive. If we lump together everything between these limits, we get the average per cent. of errors in each series of Table XIII., as follows:

Right hand, 1. series, speed increasing, 12%; decreasing, 13%,					
"	"	2.	"	"	10%; " 19%.
Left " 1. " " " 22%; " 25%.					
"	"	2.	"	"	23%; " 26%.

The difference is not always striking in amount, but is fairly regular in occurrence—23 cases out of the 30 from which these gross averages are formed. This result is, perhaps, what would be expected. Any difficult movement, we should guess, would be best done by beginning deliberately and gradually working up to a passable speed. This is not, however, always true. On testing it in lines ruled in the drum, I found the following average errors for the middle rates:

Eyes open, subj. D, speed increasing, 2.9 mm.; decreasing, 2.6 mm.			
	F,	3.6	2.6
	P,	4.0	4.3
	P <sup>1</sup> ,	6.2	6.6
Eyes shut,	D,	3.3	3.1
	P,	6.2	9.2
	P <sup>1</sup> ,	7.5	6.8
Automatic,	P,	5.9	6.4

Sometimes increasing speed gives better results; sometimes decreasing. We must conclude that the advantage of the increasing speed is no general law for all sorts of movements.

A third instance of a movement where the initial adjustment is not a repetition of that which precedes is afforded by the experiment of making each line ruled on the kymograph the least possible amount longer or shorter than the line before. When that is the requirement each movement is adjusted to be different from the one preceding, not the same. This turns out to be at high speeds a much more difficult task than that of making equal movements. The 'momentum of uniformity' enters here, not as an aid, but as a hindrance. When the speed becomes too great to allow of much current control (secondary adjustment) we cannot fall back on automatic uniformity, but must make the utmost efforts to vary the initial adjustment by as little as possible—which turns out to be a good deal. Consequently the error does not stop or retard its increase at an intermediate rate. See the Table.

<sup>1</sup> Left hand.

TABLE XV.

MM.	FIRST SERIES.			SECOND SERIES.		
	+	-	AV.	+	-	AV.
40	2.4	2.5	2.5	3.2	3.7	3.5
60	3.7	3.8	3.8	5.3	5.7	5.5
80	5.3	6.6	6.0	7.7	7.5	7.6
100	6.8	7.1	7.0	9.1	9.7	9.4
120	10.7	10.5	10.6	11.1	10.9	11.0
140	9.3	13.3	11.3	13.7	13.3	13.5
160	17.8	19.1	18.5	16.2	17.7	17.0
180	13.1	19.5	16.6	16.8	16.3	16.6
200	23.4	29.5	26.5			

Method of least possible increments. Lines ruled on the drum. The 'first series' started with a very short line, and increased each time by as small an amount as possible until the limits of the paper were reached (150-200 mm.); then the reverse process followed. In the 'second series' 10 positive increments were followed by 10 negative increments, and so on back and forth.

On comparing the least possible *positive* increments with the least possible *negative* we find the negative generally the larger. This reminds us of Weber's law. But the cause is probably to be sought, not in perception, but in adjustment. It is harder to adjust a movement to be just shorter than the preceding one than to be just larger, even as it is easier to sing or play a *crescendo* than a *decrescendo*, easier to sing up the scale than down.

The method of least possible increments of movement was employed by Fullerton and Cattell, who subjected it, and with it the method of least noticeable difference in general, to searching criticism, showing that there was no standard least noticeable difference for all persons nor for the same person at all times, and that in order to be capable of any sort of precise application the method must be combined with that of average error or with that of right and wrong cases. These difficulties with the method appear very clearly in the original tracings of the experiments here discussed. Some subjects were satisfied with a standard difference which, by the inevitable variation of their attempts at it, gave quite a large percentage of errors. Other subjects insisted on a standard difference so large that practically no errors occurred. Evidently the just noticeable difference—or smallest possible increment—meant different things to different persons. Their results should not be compared without attention to the percentage of errors—without, in short, determining the distribution curve of the differences made.

In the experiments recorded in Table XV., however, the standard difference was sufficiently large to reduce the percentage of errors very low, and the results are fairly comparable. The purpose of introducing this other method was simply to get another variety of the demand for accuracy, in order to see whether the relation between accuracy and speed would still follow the same general law.



By all these instances of movements under conditions which prevent automatic uniformity, our 'second inference' is pretty well established. In all such cases we see that the error, instead of remaining almost constant throughout the higher speeds, becomes rapidly greater. And the reason for this seems to be that the initial adjustment cannot benefit by the shortness of the interval, since the new adjustment is not a repetition of the old.

All this discussion serves, therefore, to make more probable the conception with which we started. It seemed reasonable to suppose that the bad effect of speed consisted in reducing the completeness of the current control, and that the good effect of a short interval was felt primarily by the initial impulse. Facts, as far as we have found them, support this conception. Only, we have been induced to modify our rule that shortness of interval favors accuracy by recognizing the probable existence of an optimal interval, below which the accuracy of the initial adjustment is rapidly lost.

We have all along been overlooking one possible bad effect of speed. We have urged that speed would interfere with the current control of a movement, but have quietly assumed that it would not interfere with the initial adjustment. Yet it is not hard to conceive that the initial adjustment might become more difficult as the speed increased. A slight error in the duration of a movement would mean a greater error in extent, when the velocity is greater.

To offset this argument, we have both general reasoning and observation of our own results. The general reasoning is founded on the fact established by Fullerton and Cattell, in their oft-cited monograph, that judgment of the time of a movement follows very closely Weber's law.<sup>1</sup> If one movement takes twice as much time as another, it will be subject to twice the variable error in point of time. But as its velocity will be only half as great (the extent being assumed to be practically the same), its error in extent will not be affected by the difference in its duration, or, what amounts to the same thing, by the difference in its velocity. If, therefore, Weber's law holds for

<sup>1</sup> Fullerton and Cattell, p. 108

the duration of a movement, it follows that the speed of a movement does not interfere with the accuracy of reproducing a movement—when, that is to say, the movements are made with eyes closed, and are judged as wholes, or depend for their accuracy on their initial adjustments. Doubling the speed does, indeed, double the error in extent that would be produced by a small error in the time of the movement; but as doubling the speed halves the time, it also halves the error of the time. And thus the two balance each other.

Distance is equal to the product of the velocity and the time:  $s = vt$ . When, therefore,  $s$  is (practically) constant, as here,  $v$  and  $t$  are inversely proportional to each other.

If we look at the question in the light of our experiments, we have two observations to make:

(1) Introspectively, it is easier to adjust a fairly rapid movement than a very slow one. The fairly rapid movement is more of a unit, and can be adjusted all at once to a greater degree than the slow movement.

(2) The tables give no sign of a bad influence of speed where the accuracy depends on the initial impulse—*i. e.*, when the eyes are closed. Nor do the automatic curves show increasing irregularity at high speeds, as they ought if high speed conduced to variability in extent. These statements must not, indeed, be left too absolute. There are some signs that, at the highest speeds considered, these movements become less accurate or less uniform. These signs were interpreted to mean that the *interval* had passed its optimum. It is possible that the speed, too, was becoming so great as to be in itself a cause of inaccuracy in the initial adjustment. It is quite probable that when the speed passes the bounds of common use the initial adjustment becomes difficult. It is possible that there is an optimum speed from the standpoint of the initial adjustment. All we can claim with certainty from our own results is that the mere increase in speed, within the ordinary range, does not interfere within the initial adjustment; and that its great disadvantage lies in its prevention of the later and finer adjustments.

#### PART IV. ACCURACY AS DIVIDED BETWEEN THE INITIAL ADJUSTMENT AND THE CURRENT CONTROL.

The phrases 'initial adjustment,' 'finer adjustments' and 'current control' have been bandied about so freely in the last few pages that the patient reader doubtless hopes they will soon be either given a rest or else made to give an account of themselves. We shall do the latter first.

If the reader desires a demonstration of the existence of the 'later adjustments' which constitute the most evident part of the 'current control,' let him watch the movements made in bringing the point of his pencil to rest on a certain dot. He will notice that after the bulk of the movement has brought the pencil point near its goal, little extra movements are added, serving to bring the point to its mark with any required degree of accuracy. Probably the bulk of the movement is made by the arm as a whole, and the little additions by the fingers. If now the reader will decrease the time allowed for the whole movement, he will find it more difficult, and finally impossible, to make the little additions. Rapid movements have to be made as wholes. If similar movements are made with eyes closed, it is soon found that the little additions are of no value. They may bring us further from the goal as likely as nearer. We have no exact knowledge of where the goal is, and so cannot use our finer adjustments.

Another demonstration can be had by drawing a free hand line joining two points. The line will record the changes in direction, and so give us an insight into the later adjustments as far as they are applied to the direction of the movement. Increasing the speed or shutting the eyes produces the same effects as before. There is, however, one new fact that appears and gives us an insight into the character of the first adjustment. If lines of considerable length, say a foot or two, are made at a rapid rate, the changes in direction will probably be found to be about the same in them all. They all start out at nearly the same angle from the true direction and make about the same sweeping curve around to the goal. This curve is not a simple arc, but bends back on itself. As this curve ap-



pears, moreover, when the eyes are closed, the changes in its direction cannot be due to later adjustments. The initial impulse takes the hand along a curve. We aim around a corner; not according to geometrical straight lines, but according to the make-up of our arm. From the geometrical point of view, the simplest movement that we can make—the movement as determined solely by its first impulse, and not complicated with later adjustments—is still a complex affair. The initial adjustment is itself complex. It includes the innervation of different muscles one after another. The coördination adapted to produce a straight line is probably more complex than that to produce certain curves. The first impulse includes also a command to stop after a certain distance. These later effects of the first impulse are probably in some degree reflex. The proper continuation of a movement which has been started seems, from pathological cases, to be dependent on the preservation of the arm's sensibility. Yet the first impulse of a movement contains, in some way, the entire movement. The *intention* certainly applies to the movement as a whole. And the reflex mechanism acts differently according to the difference in intention. We must suppose that the initial adjustment is an adjustment of the movement as a whole.

A graphic demonstration of the later adjustments in the matter of extent of movement is not so easy as in the matter of direction. But by means of a rapidly rotating kymograph it can be accomplished. By this means a curve of the speed of the movement—similar to the curve of muscular contraction—is obtained, and any little additions to the movement can be detected.

As representing the standard curve of a movement governed entirely by its initial adjustment, we take the curve of 'automatic' movements. Since no attention was paid to the extent of these movements, there is no call for later adjustments. With this standard we may compare the curves obtained when each movement was required to imitate the preceding, or to terminate at a given line. We may compare movements also at different speeds, and of the right and left hands. The comparison will reveal the causes of the differences in accuracy between these several movements. See Figs. 9 and 10 :

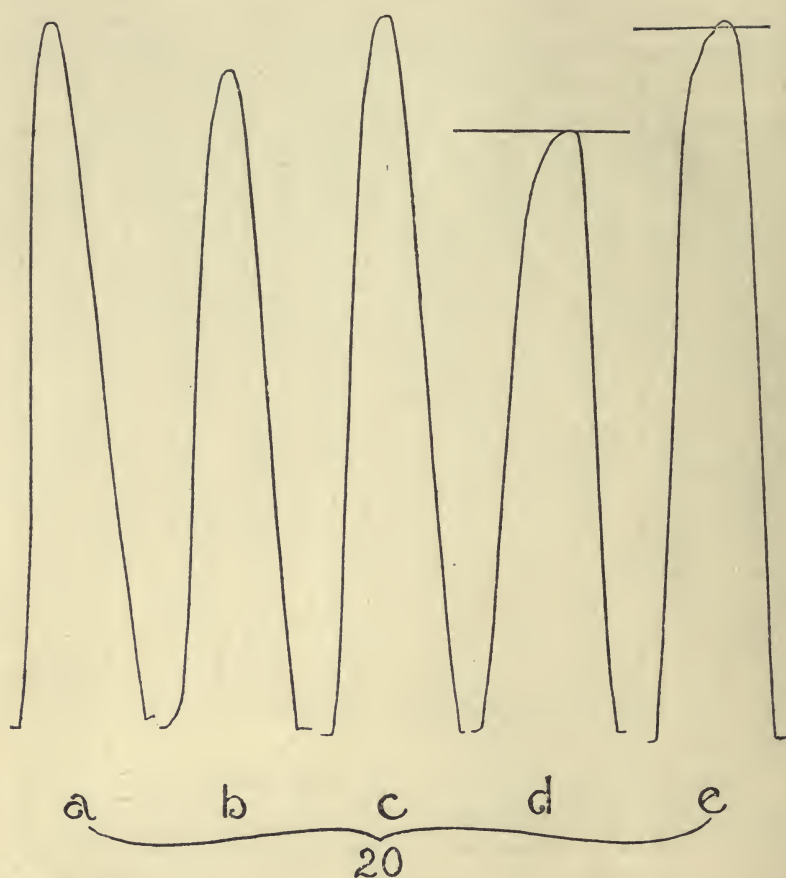


FIG. 9.

Tracings on a rapid kymograph of different sorts of movements: *a*, automatic; *b*, eyes shut, left hand; *c*, eyes shut, right hand; *d*, eyes open, left hand; *e*, eyes open, right hand. Rate, 20 movements per minute. The movements with eyes shut were required to be equal, each to the preceding. Those with eyes open were required to terminate on a line previously ruled on the paper. Selected records (not continuous). Reduced to  $\frac{2}{3}$  original size.

The most striking difference between these tracings is that between the sharpness of their tops. At the slowest rate the automatic movement gives a sharper top than any other, and the studied movements with eyes open the bluntest. The order of sharpness is: automatic, left hand with eyes shut, right hand with eyes shut, left hand with eyes open, right hand with eyes open.

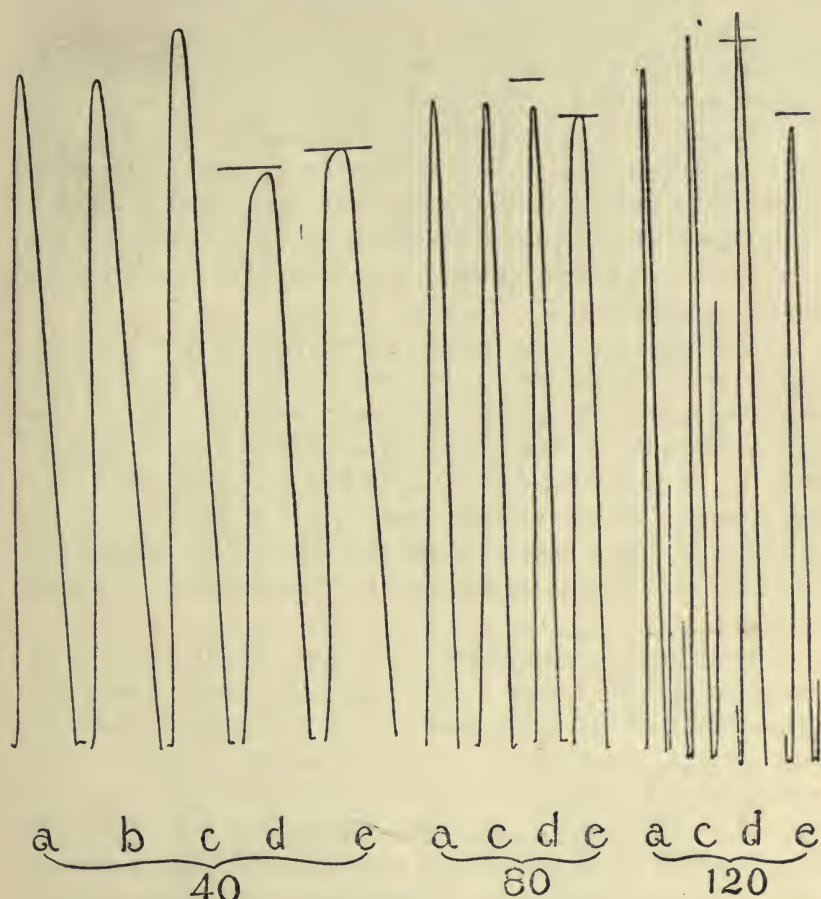


FIG. 10.

Same as Fig. 9, but at rates of 40, 80, and 120. Letters have the same meanings as in Fig. 9.

The blunt top is an expression of extreme slowness of movement at the close. To make up for this, the beginning and middle of the movement, and also the return to the starting-point, are considerably hastened. This slowness at the end is useful because it allows for the fine adjustments. Evidences of these can be seen in the curves. They are visible as irregularities in direction or as marked differences from the run of the automatic movement. Some of the fine adjustments consist in little *additions* to the movement, carrying it beyond where it



would otherwise have gone; and others in a *subtraction* or inhibition of the movement, making it shorter than it would otherwise have been. The latter seem to give the best results; the former seem to be corrections of mistakes. The best type of later adjustment is that which brings the movement so smoothly to a stop that no sharp change in direction is possible. This type is the best, since it is clearly the least awkward. The whole movement runs smoothly to its desired end, without any break or correction.

Turning now to the records of movements at more rapid rates, we see the differences between the different sorts gradually disappear. At 40 the differences are still present; the time allowed is still sufficient for nearly all the later adjustment that can be profitably used. At 80 only the right hand, eyes open, shows any perceptible broadening of the top; at 120 even this is almost gone. Above this the later adjustments are about *nil*, and all movements have to depend on their initial adjustment.

These tracings demonstrate the truth of our previous assumption that the loss of accuracy at high speeds was due to impossibility of later adjustments. Or, as it was expressed, the *bad effect of speed is exerted on the current control of the movement*. A rapid movement does not allow time enough for the later adjustments. The later adjustments are reactions to stimuli set up by the movement, and a rapid movement does not allow for the reaction-time. That this is a sufficient explanation of the bad effect of speed, at least up to 120, is seen on comparing the degree of loss of accuracy with the degree of failure of the later adjustments.

The tracings serve also to bring out clearly the differences between the hands. At the slowest rate, the left hand, as well as the right, has plenty of time for its later adjustments, and, therefore, it attains about equal accuracy with the right. The adjustments of the left hand are, however, more awkward. As the speed is increased the left hand loses its fine adjustments much more quickly than the right. We found before that one point of inferiority of the left hand was that, though it could be accurate slowly, it could not combine accuracy with speed.

We may now be more explicit, and say that the left hand can make the fine later adjustments of a movement slowly, but not rapidly. Another point of inferiority that was noted was a less delicate tactile sense. This also appears in the tracings. When the eyes are shut and the speed is low the right hand gives a less pointed top than the left. This means that the right hand did more in the way of later adjustment, undoubtedly because it had a keener sense of the normal length.

The presence of later adjustments can be detected in many common movements, as, for instance, in singing. Ordinary singers do not always strike the note accurately at one jump, but must feel around a little after reaching the neighborhood, guiding themselves by the sense of pitch. Probably the ordinary run of violinists, and certainly the beginner, find their notes in this groping way. The path to skill lies in increasing the accuracy of the initial adjustment, so that the later groping need be only within narrow limits; and through increasing the speed of the groping process, so that finally there seems to be no groping at all. The later adjustments are combined with the bulk of the movement in that smooth and graceful way which we picked out from our tracings as the most perfect type. Whether the great virtuosos do away entirely with the later adjustments and achieve their wonderful accuracy by means of the first impulse, would be an interesting thing to find out. The speed and *verve* of their performances make it difficult to suppose there is anything there of the nature of groping. Yet these artists have had to work up through the groping stage, and it is likely that some traces of the process by which they reached perfection should remain in the perfected result. The later adjustment is probably there, but it is made with perfect smoothness, and has by long and efficient practice attained the sureness and the speed of a reflex.

The question, how much of the accuracy of a movement is attained by the initial adjustment, and how much is left to the later adjustments, is difficult of accurate answer. We cannot take the accuracy attained with closed eyes as a measure, for there is some attempt even with eyes closed at later adjustments. And, besides, the accuracy of the initial impulse may increase

with the clearness of the normal. Our *aim* is better with eyes open than with eyes closed. A better measure would be the accuracy attained by use of the eyes at high speeds. Since the power of later adjustments is mostly lost at high speeds, the degree of accuracy remaining must be attributed to the initial impulse. But we have found the accuracy of the impulse to be greater at short intervals. So that if we were to take the total accuracy attained at high speeds (and short intervals) as equal to that portion of the accuracy at low speeds which is due to initial adjustment, we should be making the adjustment more accurate than it really is. It would seem proper, however, to take *the accuracy attained at high speeds and long intervals* as approximating to the desired measure. The mere speed, it has been argued, does not interfere with the accuracy of the initial adjustment. What affects it is the interval. If then the interval remains the same, and only the speed is varied, the accuracy of the initial adjustment will remain constant. And if at any speed we can assume the later adjustment to be *nil*, then the accuracy remaining will be that of the initial adjustment at that interval. If then we turn to Tables VII.-XI., and examine the horizontal lines, we shall probably find approximately the measure we are seeking. We have to allow for some chance irregularities, and perhaps can do no better than to take the error at 200 as representing that of initial adjustment when uncorrected by later control. According to this measure we find the final error to be at low speeds very much less than the error of initial adjustment. The final error expressed as a per cent. of the error of initial adjustment is as follows :

	Speed 20.	Speed 40.	Speed 100.
Subject D	9	3	37
" W	16	11	74
" P	9	—	91
" F	6	8	76
" F	2	2	86

From these numbers we infer that the initial impulse contributes but a small part of the accuracy of slow movements, but a good share of all the accuracy that remains at 100.

It is quite possible that the numbers in the first two columns are too small. My reason for supposing so is that the initial adjustment, as measured by this



method, turns out to be sometimes less accurate than the automatic movements at the same rate. It seems proper to regard the latter as furnishing a zero of accuracy, and anything below this must be due to some disturbance. Perhaps the disturbance consists in the unusual combination of high speed and long interval. At the same time it is quite possible that the initial adjustment of careless movements is fully as accurate as that of careful, when each movement is a repetition of the one before.

The error of initial adjustment can be more readily calculated for the *direction* of movement. In the experiment, described above, of drawing a free-hand line to connect two points, we have a record of the direction in which it started as well as of the point to which it finally came. The direction in which it started is an indication how accurate the first aim was; it represents the initial adjustment. If we produce the line in the direction in which it started, we shall see where the first adjustment alone would have sent it. We can see how far it would have come from the goal, and compare that error of the original adjustment with the error finally made. We can also detect the successive corrections that were made, and see how much accuracy each added. The only difficulty with the method is that not all the changes in direction are real attempts at correction. As stated above, the movement, at least when rapid, proceeds naturally by a curved path. Some of the changes in direction are, therefore, provided for in the initial adjustment. By practice we can distinguish pretty well between such changes and those which record a correction. And this sort of confusion is comparatively absent in slow movements.

I will give the results of one such test. The lines were about 50 cm. in length. They all started at a common center and radiated toward dots which served as goals. They were required to keep time with a metronome beating at a slow rate. When the metronome beat 20 times a minute the average error of the initial adjustment was 23.0 mm. and the final error 2.1 mm. When the beat was 40 a minute the average error of initial adjustment was 38.8 mm., the final error 4.8. The final error was, at the slower rate, 9% of the initial, and at the faster rate 12%. We see here the error in initial impulse increasing as the interval is shortened, contrary to what we observed

above. But this need not disconcert us. The movements are here much longer, and not mere repetitions of each other; and the perception of the new target was difficult at short intervals. Hence, the optimal interval is longer than in the simpler movements.

The general conclusion is then fairly clear. The accuracy of the original impulse is slight compared with that added by the later adjustments, when the speed is low and the eyes are used; otherwise, almost as great. In other words, in the situations which permit great accuracy that accuracy is due mostly not to the initial adjustment of the movement as a whole, but to the current control, consisting of finer adjustments.

#### PART V. TEST OF WEBER'S LAW.

It would be impossible to go through a study of this kind without taking one's turn, sooner or later, at Weber's law. It is quite true that Weber's law applies to sensation and not directly to movement. Yet the same formula might be used in speaking of movement. It might be conjectured that the error in making a movement was proportional to its length. We wish to test this conjecture.

In order to test it, we must take account at every stage of the speed. This is not so essential when the eyes are closed, since then the accuracy is not much influenced by the speed; but it is absolutely necessary when the eyes are used. If time enough is allowed and the target is plainly seen, one movement can be made just as accurately as another. A movement of an inch cannot be terminated any more exactly than a movement of a foot or of a mile, provided we have all the time we need. In other words, later adjustments of equal minuteness and accuracy can be appended to the long movement. The movement is not adjusted as a whole, or not simply as a whole. The final adjustment is of very minute portions, whether the whole movement be long or short. The process is similar to the comparison of lengths by the eye. If we see the lengths separately, we are subject to a considerable error, which increases with the length; but if we are allowed to place them

to suit ourselves, we put them side by side, flush at one end, and then can detect any difference at the other end that is large enough to see at all with the optical apparatus at our command. By this method a small difference is detected with equal certainty whatever the length of the lines compared. What we finally perceive is not the whole lines, but small parts of them. So it is in making movements, provided we are under no limitations as to speed.

In order then to give Weber's law a fair test in movements governed by the eye, it is at least necessary to limit the time of the movement. Even this, however, does not fully answer the purpose, since when equal times are allowed the bulk of the longer movement is made so much faster than the slower as to allow almost as much time as its close for fine adjustments. This is specially true when the time allowed is considerable. More rapid movements, not being able to make use of later adjustments, are made as wholes, and afford a fair chance to test Weber's law. Yet even here a difficulty arises. Should the movements of different length be allowed equal times, or should they be compelled to be of equal speed? The latter is impracticable in any strict sense, since the speed of a movement is by no means uniform. And if the longer of two movements were given a proportionately long time, so as to insure equal *mean* velocities in the two, the result would inevitably be that the bulk of the long movement would be made so fast as to allow time for fine adjustments at its close, and thus the long movement would prove the more accurate. Our tables will show such cases.

Nothing better seems practicable, therefore, than to give the long and the short movements equal times, and see what they will do. The results appear in Tables XVI., XVII. and XVIII. In order to insure constancy of the normal, and also to avoid all uncertainty of perception and confine the errors to those of movement proper, the experiments were performed by the method of ruling up to lines previously drawn on the kymograph paper at right angles to the direction of the movements. These lines were at distances from the fixed starting-point of 5, 10, 15 and 20 cm. respectively.



TABLE XVI.

MM.	N = 50 mm.			N = 100 mm.			N = 150 mm.			N = 200 mm.		
	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.
40	0.1	+0.1	0.1	0.3	+0.3	0.3	0.4	+0.3	0.4	0.8	0.	0.8
80	0.5	+0.4	0.5	1.0	+0.3	1.0	1.5	+0.5	1.5	4.4	0.	4.4
120	1.3	+0.4	1.3	2.9	+1.5	2.8	3.4	+1.1	3.4	4.1	-0.1	4.1
160	1.8	+0.7	1.7	3.9	+2.6	3.2	5.5	+2.1	4.9	3.6	-0.4	3.6
200	2.4	+1.2	1.9	3.4	+1.8	3.3	4.6	-1.8	4.4	5.6	-1.7	5.5

TABLE XVII.

MM.	N = 50 mm.			N = 100 mm.			N = 150 mm.			N = 200 mm.		
	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.
20	0.2	0.	0.2	0.1	+0.1	0.1	0.1	0	0.1	0.1	0	0.1
40	0.6	+0.5	0.6	0.3	+0.2	0.3	0.3	+0.1	0.3	0.1	0	0.1
60	0.4	+0.4	0.4	0.7	+0.5	0.7	0.6	+0.4	0.6	0.7	+0.1	0.7
80	0.6	+0.5	0.6	1.4	+0.1	1.4	1.7	-1.0	1.6	1.4	-0.2	1.4
120	1.9	-0.3	1.9	2.9	-1.9	2.4	3.1	-2.0	2.9	5.3	-3.7	4.5
160	1.7	-0.9	1.5	3.7	-2.3	3.1	5.9	-5.3	3.9	4.4	-1.4	4.5
200	2.8	-2.1	1.9	3.5	-1.2	3.2	7.3	-5.5	5.1	4.1	-1.7	4.5

TABLE XVIII.

MM.	N = 50 mm.			N = 100 mm.			N = 150 mm.			N = 200 mm.		
	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.	AV. E.	C. E.	V. E.
20	0.1	+0.1	0.1	0.6	+0.3	0.6	0.6	+0.4	0.6	0.6	+0.3	0.6
40	0.2	+0.2	0.2	0.5	+0.2	0.5	0.6	0	0.6	0.6	+0.5	0.6
60	0.8	+0.5	0.8	1.2	+0.6	1.2	1.6	+0.6	1.5	1.7	+0.2	1.7
80	1.1	+0.2	1.1	1.7	+0.1	1.6	2.2	+0.1	2.1	2.6	+0.1	2.6
100	1.2	+0.3	1.2	1.7	0	1.6	2.3	+0.3	2.4	3.0	+0.1	3.0
120	1.6	+0.9	1.5	1.6	-0.5	1.6	2.4	+0.3	2.5	2.8	-0.1	2.8
140	1.4	+0.6	1.5	2.3	-0.7	2.3	3.3	+0.5	3.3	3.4	-0.5	3.2
160	1.6	+0.8	1.2	2.7	+0.8	2.6	3.0	-0.4	2.7	4.2	-0.4	4.3
180	1.5	-0.3	1.5	3.1	-0.6	3.0	3.9	-0.7	3.7	4.0	-2.6	3.4
200	2.0	-1.3	1.8	2.5	-0.2	2.5	5.2	-2.1	4.6	6.4	-1.1	6.2

Test of Weber's Law, ruling to seen lines. Right hand:

XVI., subject D.

XVII., " P.

XVIII., " W.

Where no error of an average is given, the error is less than 0.05.

Taken in the gross, these results show, indeed, an increase in error as the extent of movement increases, but by no means a proportional increase. The deviation from Weber's law is almost always in one direction. Out of 66 chances to test the law (each normal being compared with the normal next larger, at every speed), 7 show a fairly exact agreement with the law,

14 show an increase in error too large for the law and 45 an increase too small for the law. Now we should, of course, expect, in these comparatively short series (about 100 cases for each average), that there would be considerable deviations from the typical curve. But the deviations should be distributed around the type as a center, lying about equally on each side. Here they preponderate so strongly in one direction as to afford evidence against the proposed law. The error increases with the extent, but not so rapidly as the extent.

We had expected, however, that the error would increase too slowly at low speeds, and had agreed to use only the high speeds for a serious test. But when we confine our attention to the speeds above 120 we find the same showing. Out of 33 tests 6 agree closely with Weber's law, 6 show an increase too large for the law and 21 an increase too small. There is nothing, therefore, to indicate that the exact relation of Weber's law holds, and much to show that it does not, but that the error of a movement increases more slowly than the extent.

On testing the other formula which is in the field, that of Fullerton and Cattell, according to which the error should increase in proportion to the square root of the magnitude, we find that the observed increase is too great. Out of the 66 tests 11 show a close agreement with the law, 10 show too slow an increase in error and 36 too rapid an increase. Out of the 33 better tests 6 show a close agreement, 7 too slow an increase, and 20 too rapid an increase. The odds against this law are not quite so heavy as against Weber's, but the probability is that the typical rate of increase lies between these two formulations. This applies, let it be remembered, to comparatively short movements controlled by sight, with no chance for error in the perception of the normal, and with equal times allowed for movements of different lengths. In movements with closed eyes, again, the actual increase of error has been found to be less rapid than Weber's law requires. Fullerton and Cattell found their law to hold very well in that case. Most of the error was then error of perception, whereas that found in the present experiments is the error of movement. More precisely, it is the error of the initial adjustment so far as this is not corrected by later adjustments.

Tables XVI.-XVIII. show differences in the constant error at different lengths and speeds. The constant errors are for the most part small and uncertain, yet the general run of them is fairly uniform with all the subjects. The C. E. is positive at low speeds, but becomes negative at high speeds; and it tends to become negative sooner for a long movement than for a short. The cause of the negative C.E. is simply the difficulty of getting through with the whole movement when the interval is short.

If the first of two movements is twice as long as the second, but the second is made twice while the first is made once, the total distance moved will be the same in both cases. And the average speed will be the same. Will the error be the same? On consulting the tables we find that the error is very far from the same. There are many such pairs of movements in the tables—*e.g.*, 50 mm. at the rate of 80 per minute, with 100 mm. at the rate of 40 per minute; or 100 mm. at 160 per minute, with 200 mm. at 80 per minute; also 100 mm. at 120 per minute, with 150 at 80 per minute. On picking out such a pair we usually find that the longer movement shows the smaller average error. This is so in 37 out of the 45 such pairs in the tables. If two short movements are combined into a long one, which is allowed the time of these two, it will have less error than the two, and not only so, but less than either one. The cause of this can hardly be that the initial adjustment of the longer movement is better, for we have found that the initial adjustment is less accurate for long movements than for short, and less accurate at long intervals than at short. The real cause is doubtless that the longer movement, by reaching a much higher speed in the middle of its course, saves time for finer adjustments at the end. The shorter movement has twice as many fine adjustments to make, and a smaller total time in which to make them. Hence its greater inaccuracy.

When this experiment is repeated with eyes closed the opposite result is obtained. The more extensive movement at the longer interval shows the greater error. This is brought out by Table XIX., though in a different sort of movement. The cause of the greater error is probably twofold. The perception of the longer normal is less accurate, and the adjustment is less accurate at the longer interval.



TABLE XIX.

	10 cm., MM. 200.	20 cm., MM. 100.	30 cm., MM. 66 $\frac{2}{3}$ .	40 cm., MM. 50.	50 cm., MM. 40.
L. H. Eyes open.	3.0	3.5	3.0	2.7	2.5
R. H. Eyes shut.	11.2	9.3	16.5	16.4	20.6

An experiment much like the 'three target experiment,' but only two targets were used, and of these only one was carefully aimed at. The pencil point was carried between hits to near the other target, so as to give a movement of the required extent. As the interval is lengthened the extent of the movement is lengthened in equal ratio. The measurement of results proceeded as in the 'three target experiment.' The errors given are variable errors of mean square. Error of each determination, 1-10 thereof.

#### PART VI. ERROR OF PERCEPTION AND ERROR OF MOVEMENT.

Fullerton and Cattell have already been quoted as analyzing the gross error in the reproduction of a normal length into two factors, by the device of requiring the subject to judge the movement as well as make it. As it was found that he could to some extent detect errors in his own movements, the inference was that part of the gross error was due to uncertainty of the hand and arm; but mathematical analysis led to the conclusion that the error of pure movement was only half as large as the error of pure perception. These results were gained in the case of movements made with closed eyes.

A similar problem presents itself in the case of movements controlled by the eye; but the solution proceeds naturally by a different method, as follows: Three sets of experiments were made in the reproduction of a given length. In the first set the normal was presented by a straight line placed on the desk covering the drum, but several inches from the slot where the reproductions were made. Here are present both an error in the perception of the normal and an error in the movement. In a second set the error of movement was excluded by having the subject lay off the distance *deliberately* on lines which had previously been ruled. In the third set the error in perception of the normal was excluded, or at least reduced nearly to zero, by placing the normal so close to the reproduc-

tion that no difficulty remained in seeing when the reproduction was long enough. This was accomplished by laying off the distance beforehand on the kymograph paper and denoting its end by a distinct line perpendicular to the direction of the movements. The movements were required to terminate at this line. Now the interesting question was whether these isolated errors of perception and of movement would add up properly and produce the combined error as found in the first set. In order to tally, the constant error in the compound process should be equal to the algebraic sum of the constant errors of the component processes, while the variable error of the compound process should equal the square root of the sum of the squares of the variable errors of the component processes. The results for the variable error, as being the more reliable test, are given in Tables XX. and XXI.

TABLE XX.

NORMAL.	16 mm.		33 mm.		50 mm.		72 mm.		96 mm.		128 mm.	
MM.....	25	50	25	50	25	50	25	50	25	50	25	50
Error of Perception .....	.5	.5	1.2	1.2	1.1	1.1	2.1	2.1	1.7	1.7	1.3	1.3
Error of Movement.....	.1	.3	.5	0.5	1.5	1.2	1.3	0.9	0.8	1.1	1.1	1.0
Theoretical comb'd. Err.	.5	.5	1.3	1.3	1.8	1.6	2.4	2.3	1.9	2.0	1.7	1.6
Observed comb'd. Err...	2.2	1.4	1.7	1.6	2.9	1.6	2.7	2.1	3.7	5.1	3.6	2.5

TABLE XXI.

NORMAL.	50 mm.			72 mm.			128 mm.		
MM.....	40	80	120	40	80	120	40	80	120
Error of Perception.....	2.3	2.3	2.3	3.7	3.7	3.7	6.7	6.7	6.7
Error of Movement.....	.4	.6	1.9	.5	1.0	2.2	.3	1.5	2.7
Combined, theoretical.....	2.4	2.4	3.0	3.7	3.8	4.3	6.7	6.8	7.2
Combined, observed .....	2.9	3.2	3.6	5.1	5.0	2.6	4.6	6.9	5.5

Error of perception and error of movement. XX., subject W; XXI., subject P. The experiment is described in the body of the paper.

It will be noticed that the combined errors, as observed, are generally greater than as calculated. (A table of constant errors would show the same.) This is very markedly and surely the fact with Subject *W*, less surely with Subject *P*, where there are, indeed, three striking exceptions.<sup>1</sup> These ex-

<sup>1</sup>The experiments were, of course, made without any inkling as to what the result would be.

ceptions are coincident with decidedly small observed errors in the compound process, and are probably to be explained by the fact that it was much easier, after making a few imitations of the seen normal, to cease to gauge the movement each time by the normal, and instead to repeat the last imitation. If the subject should yield to this tendency—and the original tracings look as if this was here the case—the effect would be to increase the uniformity of the reproductions and diminish the variable error.

On the whole, therefore, an *unexplained residuum* of error, or, we may say, of variability, appears in the combined process. Besides the error due to imperfect perception of the normal and the error due to the disobedience of the hand and arm, there must be some third source of error. What this source can be is at first thought mysterious; but we must remember that the total error is not, in complete strictness, *composed* of an error of perception and an error of movement. The total error inheres in the reflex arc as a whole, beginning with the sensation and reaching clear around to the movement. Inaccuracy doubtless creeps in at various points along this arc. In particular, there may be a lack of precision in connecting the perception with the motor mechanism. Such a source of inaccuracy might be introduced by lack of sufficient time to adjust the motor discharge to the perceived normal. There is undoubtedly an adjustment time—a time required by the current to pass from the visual to the motor center and set the latter into proper activity. And it is quite probable that when the time allowed for this ('initial') adjustment is barely sufficient, the adjustment may be only partial. Such a lack of sufficient adjustment time was observed in certain experiments described above, as in the 'three-target experiments' at high rates. (See pages 47 and 61.) In that experiment, however, the new adjustment was in each case quite different from the last. The more alike an adjustment is to that which immediately precedes it, the easier and surer it is, and probably the shorter is the adjustment time. We have found that in ruling a series of lines the time necessary for the best adjustment attainable (the 'optimal interval') is quite short, shorter perhaps than one-third of a second.



This would make us doubt that the third source of error which we are now seeking can be a lack of sufficient time for adjusting the movement to the seen stimulus. Consequently, although this is the first source that naturally occurred to us, we shall have to abandon it and seek further.

A better suggestion is that the adjustment is disturbed by the inaccuracy of the perception. When the perception is easy and precise the adjustment will be precise, and introduce but little error beyond that of perception; but when the perception is inaccurate and indefinite the adjustment will itself introduce much variability. Now when we see a line in one place and reproduce it in another we have, to guide our movement, only a rather vague and indefinite sense of the normal length. We not only are subject to a considerable error in perceiving it, but feel little confidence in our estimate. It looks *about* so long. Within wide limits we should be very sure we were right, within narrow limits not at all sure. Hence, perception has no exact instructions for adjustment, and adjustment must act largely at random. We are somewhat at sea in the initial adjustment. If we simply erred in our perception, but were sure we were right, we should adjust wrongly indeed, but still precisely; and little additional variability would creep in at the adjustment. But since we have no exact idea of the size of the normal, since we do not know exactly what movement we want to make, our adjustment is indefinite, and introduces additional variability.

And, besides this indefiniteness of the initial adjustment, there is a complete absence of the later and finer adjustments, which we have called, collectively, the current control of the movement. In order to make use of later adjustments we need a keen perception of the length of the normal. If we feel certain of our perception to within a centimeter only, we naturally shall not make fine adjustments a millimeter in length. Thus indefiniteness of perception interferes with both the initial adjustment and the current control.

Another instance of indefinite perception, leading to inaccuracy of adjustment, is found in the movements made with the eyes shut. On comparing the accuracy attained by the tactile sense

with that attained by sight when the normal is a few inches away from its imitation, we find the visual perception no longer so superior to the tactile. Counting out constant errors, and using only the variable errors, we have the following comparisons :

Subject	P, at rate	40, eyes open, 4.6; shut, 6.0.
"	" " "	80, " " 6.9; " 6.2.
"	" " "	120, " " 5.5; " 7.2.
"	W, at rates 20-25,	" " 3.6; " 4.1.
"	" " "	50, " " 2.5; " 3.8.

The advantage of sight is largely lost when the imitation is no longer placed close alongside of the normal.

This discussion may be summarized as follows: besides the inaccuracy of perception and the inaccuracy due to failure of the movement to obey our intention, there is an inaccuracy in the intention itself, or in the process of adjusting the movement to the perception. The error due to this source is slight when the perception is accurate, but considerable when the perception is vague and uncertain, since then the force of the motor discharge is selected partly at random.

## PART VII. SENSORY BASIS FOR CONTROL OF MOVEMENT.

Having considered the more strictly motor elements (speed, extent) in the process of making an accurate movement, and to some degree also the central elements (adjustment), we may next devote some attention to the sensory part of the process. Our question will be as to what sensations are relied on for governing the extent of a movement. We shall have two cases to consider: that in which the eyes are used, and that in which the eyes are not used. In the first case our problem is whether the eyes alone are relied upon for information of the normal length, or whether the muscle and joint sensations are also relied on to a certain extent. In the second case our problem will be to discover whether the extent of a movement made without the aid of the eyes is judged in terms of its force, or its time, or its terminal position, or its association with visual space, or finally in terms of sensations of motion proper.

As a preliminary to the first problem, we may note that any sense whatever may conceivably serve as the sensory basis for controlling the extent of a movement. Those which actually do so serve are the 'muscular,' tactile, visual and auditory sensations, and probably also in some cases the sense of smell. Of these the visual and auditory give the most accurate information, largely because the breadth of their sensational fields makes possible a *superposition* of two sensations (that of the normal and that of the imitation), and so a more exact comparison than can be made where memory has to be relied on for one sensation. We can see the target *while* we hit at it, we can hear the pitch *while* we attempt to strike it. But we cannot *feel* a certain length or direction of movement while we attempt to reproduce it. Hence, as shown above, we do not refine upon our first adjustment by secondary adjustments. But, as we have found, these secondary adjustments are hardly possible anyway save at a low speed, so that at high speeds the muscle sense gives as good results as the eye. The reliance of the muscle sense on memory brings about a second source of inaccuracy when we attempt a long series of reproductions of the same normal. The constant errors accumulate, and chance errors are uncorrected, so that the later attempts may be very far wrong.

These peculiarities of the muscle sense control may be readily tested by *writing with the eyes closed*. It will be found (1) that, at the ordinary speed of writing, a single letter or a short word can be formed as well with the eyes closed as with them open; (2) that, however, when the speed is low enough to permit of fine secondary adjustments, the eyes assist greatly in forming the letters just right; (3) that extreme slowness is a disadvantage rather than an advantage when the eyes are not used; and (4) that if several words are written with the eyes closed, the alignment is lost or some other constant error makes itself evident.

In speaking of the visual or auditory sensations as controlling a movement, we seem to be getting into a mystical difficulty. How can any sensations govern a movement, save the sensations that come from the moving member itself? My answer to this objection is that the visual or auditory sensations which



we get from the movement of a member are as really entitled to be called sensations of the moving member as are the tactile and joint sensations. The mere fact that the organ of sense is not located in the moving member makes no real difference, except, probably, in the universality and firmness of the association between the movement and the sensation. The connection of joint sensations with the movements which produce them is, for consciousness, or for the central regulatory apparatus, no absolutely *a priori* connection, but a matter of association. When we make a certain movement we experience certain muscle and joint sensations, and these become firmly associated with the movement. But we may also experience certain visual or auditory sensations, and these, if as regularly present, may become as firmly associated as are the others. Both are entitled to be called sensations of the movement. The most universal and effective association of this kind is without doubt that between the movements of the vocal organs and the resulting sounds.

In this way, by association, the control of a movement may come to depend on sensations of any sort. The accuracy of the movement is limited only by (1) the degree of fineness of the sensations used; (2) the degree of firmness of the association between the sensation and the movement; (3) the degree of steadiness of the member used and, as always, (4) by the degree of deliberateness which is permitted. If we increase the fineness of the visual perception by means of a magnifying glass, we shall be able to adjust our movements more minutely than with the naked eye. But we shall need practice, in order to overcome the old association between a certain visual space and a certain extent of hand movement. We shall need, up to a certain limit, more time for each movement. And the fineness of our adjustments will be finally limited by an unavoidable trembling of the hand.

If, therefore, any sense may control a movement, we have to ask what the result will be when two senses are each in a position to do the work, one, however, being finer than the other. Will the movement obey two masters, or will it cleave to one and despise the other? The answer to which my experiments

point is that in obeying the more accurate sense it entirely disregards the other.

The question was first suggested to me in the following connection. When a series of lines is ruled on the kymograph, each required to start at a fixed block and to end flush with the preceding line, it sometimes happens that a line does not begin far enough back. Accordingly if it ends where it should, it will be shorter than the preceding line. A friend called my attention to this as a possible source of error, arguing that when the eye said to the movement, 'Far enough!' the muscle sense would say, 'Not long enough yet!' and the movement made, being a resultant of these two influences, would be longer than it should be. On examining a large number of such instances, however, it was found that there was no tendency for a line which began short to be prolonged at the end. Two such instances appear in Fig. 1. The inference seemed to be that the movement entirely disregarded the muscle sense and relied solely on the eye.

This inference is confirmed by a study of the constant errors made when the eyes are used as compared with those made when the dependence is on the muscle sense. If a large constant error appears in the use of the muscle sense, some traces of this would be expected to remain when the eyes were used—that is, if the muscle sense still had anything to say about the extent of the movement. But in various experiments which serve to test this point the constant error is found to be quite regularly in the opposite direction. If we suppose then that the muscle sense is still attended to when the eyes are used, we have to assume the presence of a large constant error due to the use of the eye, a constant error of opposite sign to that of the muscle sense. But no such constant error appears when the distances are laid off deliberately, in what is evidently entire dependence on the eyes. We must conclude, therefore, that the muscle sense is not attended to at all in these cases.

For instance, in ruling on the drum lines which were required to begin at the same point as the last (not determined by a block) and to have the same length as the last, the constant errors of both beginning and length were found to lie in opposite directions according as the eyes were used or not. Again, in ruling lines to imitate a constant seen normal, the constant error was found to be contrary in the two cases.

A negative result in such tests often appears, but does not disprove our view, for it may happen that sight does give a small constant error, and this may be in the same direction as that of the muscle sense. But the large number of positive instances are just so many proofs of situations in which the muscular sense is not relied on when sight is used. Introspectively, we feel after making a series of movements with eyes closed that opening them frees us, at one stroke, from all the uncertainty and error of the muscle sense. The basis of control seems entirely different.

There are, indeed, certain constant errors due not to the muscular perception of a movement, but to peculiarities of the movement itself, which, though reduced, are not entirely removed by the use of the eyes. Such errors may be the result of the speed of the movement, or—as in the ‘three target experiment’—of the anatomical peculiarities of the moving member. But these errors are distinctly not errors of perception.

When therefore two senses are available for the control of a movement, the more accurate will be obeyed and the other simply ignored—provided accuracy of the movement is desired. If, however, the muscle sense gives as much accuracy as is desired for the purpose in hand, then it is the more accurate sense that is disregarded, or used only for avoiding the accumulation of constant errors. This is true in writing, and often in playing the piano. In writing we never look at the movements of our fingers, but only at those of the pen-point. In fact, we do not really look at the pen-point, but rather at the letters that have just been written. As already stated, a single letter can be rapidly written as well without as with the eyes. As, therefore, a careful following of the pen-point with the eye would serve no purpose, that painstaking operation is eliminated. If the reader will attempt to look closely at the point of his pen as he writes, he will be immediately aware that this is not what he ordinarily does, but a much more difficult and fatiguing process. We by no means follow the *l* or the *y* up or down to its end. Watch the eyes of a person who is writing, and you will see that they move very little. What we use the eyes for is to keep track in general of where we are, so as to preserve the alignment and spacing, so as to keep an equality in the letters, and so as to



avoid losing our way when in the midst of a word and so misspelling it. In forming the letters, therefore, we come to depend mostly on the muscular and tactile sensibility.

In urging that the muscle (tactile, etc.) sense is disregarded when the eyes are depended on, I do not mean to say that it is disregarded except as concerns the extent of the movement. The coördination of the muscles is something about which the eye tells us nothing, and for the control of the movement in that respect we are undoubtedly dependent on the sensations from the moving member. When these sensations, or at least those from the skin, are removed by numbness, we are unable to write or sew, or button and unbutton, or play the piano. We cannot supply the lack of skin sensations by the eye, simply because we never have looked at the movements of our fingers, etc., but always at the result we were accomplishing. Consequently, we have no association between the visual sensation of the moving fingers and the proper impulse to set the muscles into coördinated action.

The truth of the whole matter seems, therefore, to be that the general and coördinating control is vested in what James calls the resident sensations, but that the closer control which is needed in hitting a target or making one line equal to another is left entirely to the eye when that is used.

But suppose the eye is not used, so that the judgment of the extent of a movement must be based entirely on the resident sensations. The question arises, What, exactly, are the resident sensations used? Do we sense the amount of motion directly, or do we infer it from some other sensation? Do we judge the movement in itself, or by the time or the force exerted, or by the position from which it starts and that at which it ends? Do we, when our eyes are shut, make one movement equal to another by making it equal in time or in force, or by making it coincident in position, or after all by simply making it equal in extent? Before examining each of these possibilities in turn, I shall illustrate by experimental results the following general statement:

The extent of movement is not judged exclusively in terms

It has been said that the judgment of position is influenced by the extent of the movement leading to that position, and that in one situation at least reproduction of a length is more accurate than that of position. Other sorts of experiment have shown the same thing.

With eyes shut, draw a triangle, and without lifting the pencil or opening the eyes continue around the same triangle a second time, aiming to hit the same corners. The result will probably be that the corners will be missed, but that the lengths of the sides will not be much disturbed thereby. Measurements of several scores of such triangles have shown that on the whole the errors in the positions of the corners are greater than those in the lengths of the sides. The judgment of the length of a movement is thus shown to be independent of the judgment of position, and to some extent better than it.

Once more, let it be required to make a movement of a constant length in a constant position. Repeat this for twenty times, with the eyes closed. It will very likely be found that the line has strayed from its position, but has retained very nearly its original length. If this change in position were conscious, we might suppose that allowance were somehow made for it. But, as it is unconscious, we must conclude that the length of the line is gauged, to some extent at least, independently of the sensations of position. If the judgment were wholly in terms of position, then there ought to be no tendency, when the beginning of a line deviated from the normal in a certain direction, for the end to deviate in the same direction. There should be no feeling of sufficient or insufficient length, but only of a right or wrong point of ending.

If the perception of the extent of a movement meant simply the perception of the limiting positions, it is hard to see what basis there would be for making one line half or double as long as another. There would certainly need to be a complete system of positions, so interrelated in our consciousness (or subconsciousness) that between any two there was a certain third, which by long association, was known to be half way between. There are two objections to such a supposition. First, in view of the infrequency of the act of drawing one line half as great as another, such a mass of association is more than improbable. Second, there is no *one* intermediate position which is recognized as the true ending for the half line. We do not grope around to find a position which feels just right. We do not feel specially confident when we finish our attempt. The feeling is not at all that of finding a familiar or a suitable *spot*, but like that of judging a *magnitude*. We try, not to hit a certain terminus, but to make a certain (approximate) quantity of motion.

A sensation of position is indeed, when well considered, a more complex affair than a sensation of extent of movement. In order to perceive a position exactly, we need to attend to a large number of sensations from various parts of the member in question. The sensations of a movement may come principally from a single joint. However this may be, it is certain that the sense of position is very inaccurate unless fortified by the sense of movement. Unless, that is to say, the position is arrived at in the different attempts *by the same route*, it will be very imperfectly found. To test this statement, choose a mark on the table, place the forefinger at some other point, and using the eyes bring

it to the mark. Close the eyes, and repeat the movement. It will be found that the mark can be hit with a fair degree of accuracy, provided the start is made from the same point. But take the finger from the mark, and, carrying it around in a circle, attempt to hit the mark again. The error will probably be considerably greater, at least till the hand has learned to *approach* the mark in the right way.

One further experiment, perhaps more conclusive than the others, may serve to close this discussion. It shows that the sense of the length of a small movement is much more accurate than the sense of position. It starts with the query: If the extent of a line is gauged wholly by its terminal position, why should not a long line be reproduced with as much accuracy as a short line? There seems no reason why the sensation of the terminal position should be less acute. The only doubt that arises is whether the length of the line may not be a cause of forgetfulness of the terminal sensation. Now if that were the true explanation, an alternation of long and short lines, the long ones designed to be equal among themselves and the short ones equal among themselves, should affect both sorts with an equal degree of forgetfulness of their terminal positions, and so make the absolute error the same in both long and short lines. But the result of the experiment is not so. In one such series, in which the longer lines were three times the length of the shorter, the average error of the longer lines was about twice that of the shorter lines. Another form of the same test is to make a series of equal short lines, swinging the arm to the side between each two. On thus swinging the arm the average error in position amounted to 18.5 mm., that of length (in lines 45-65 mm. long) to only 5.4 mm. But if the length of these lines was determined by the sense of position, the disturbance of the latter should have caused an equal disturbance of the former.

There is indeed one further set of evidence against the view that judgment of extent is only judgment of position, and that is the fact that movements of pretty fair approximation to equality can be made in different positions. This immediately suggests, however, another possibility that has not as yet been sufficiently considered. May not sensations of position be so closely associated with points in visual space that certain pairs of terminal positions shall be recognized as marking the ends of equal spaces?



At first I seemed to find pretty good evidence in favor of this hypothesis. If it were true, then the visual distortion of space ought to reappear in these movements. For instance, vertical lines ought to be over-estimated in comparison with horizontal. In trying to make a pair of equal lines, one vertical and the other horizontal (as the eye would see them), the horizontal should be the longer. This I found to be the case in myself and in most other persons on whom I tried the experiment. But later I found that in some persons the illusion was reversed. One person who tried 373 times made the horizontal line too short in 64% of trials, equal in 9%, too long in 27%; the horizontal line averaged about 8% shorter than the vertical. Another person made the horizontal line too long if it were from left to right and if the vertical line were drawn toward the body, but made the vertical line the longer if the directions were reversed. (Out of 226 cases of the first sort, 62% gave the horizontals too long, 8% equal, 30% too short. But out of 99 cases of the second sort, 66% were too short, 14% equal, 20% too long.) In still another subject the illusion could be removed by seeing to it that both movements were made with an equally free, gliding motion. And finally, in my own case, I found that the illusion vanished and was reversed at a certain length of line. In drawing such pairs of lines on the blackboard I found that the horizontal line was decidedly longer, averaging in fact 42% longer than the vertical, provided the vertical line was less than 20 cm. in length; but that it was perceptibly (15%) shorter when the vertical was over 80 cm. in length; and that there was a gradual shading off of the positive error into the negative, the transition point being at 30-40 cm. when the horizontal movement was below the shoulder, and at 40-60 cm. when above. All these facts went to show that the illusion was an affair of movement solely, and not dependent on association with visual space. The reason for the frequent exaggeration of the horizontal line seemed to be the usual reason for such exaggeration, as suggested by Delabarre, and already exemplified in this paper, namely that it is a freer motion than the vertical. The reason for the reversing of the illusion in long lines seemed, introspectively, to be that a short line is more easily made on the blackboard in a horizontal than in a vertical direction, but that a long line is more easily made in a vertical (descending) direction.

If then this 'illusion' turns out to furnish no evidence in favor of the visual position hypothesis, there are other facts which are evidence against that hypothesis. The extent of movement can be judged with some approach to accuracy, even in case of awkward and unusual movements which can hardly have any association with the visual space. Such are movements of the feet, of the hand behind the back, and of the body in swaying or in bending the knees (see Nos. 11, 18, 19 in the list above). Another argument is of the same nature as that offered above in the case of halving a movement. Introspectively, we do not feel as if we were searching for a point, but as if we were estimating a magnitude. Those who visualized

strongly in my experiments visualized not the terminus of the line, but the whole process of drawing the line. It is undoubtedly true that when the eyes are shut visualization enters very largely into the judgment of the length of a line. But the visualization is rather of the extent directly than of the positions.

It would be useless to pretend that these elements which I have denied to be the basis of the judgment of extent are never used as helps. Undoubtedly we may often judge partly by the time or force or position. But it appears clear that none of these are the fundamental thing, and that there is a judgment of the extent of movement directly. Another limitation: this does not mean that there are any sensations which by a necessary or a *priori* connection tell us of the extent of a movement. The sensations which we receive from our joints and skin and muscles probably tell us nothing definite or accurate until they have been formed into a system—drilled into a well-disciplined signal corps. But, when thus drilled, they inform us regarding the extent directly, and not around through some other element. And they are, in all strictness, sensations of motion.

This last statement, that there are sensations of motion as such, may arouse dispute in the minds of some. Some will be disposed, on philosophical grounds, to assert that motion is never directly sensed, but always inferred. In a recent influential book, which bears the name of psychology, and which is written by an author who, though not claiming to be a psychologist in the modern sense, is an eminent scholar in allied branches, we find the following made as a statement of fact:<sup>1</sup>

"We cannot form a mental picture of any activity of any kind whatever. We cannot picture even a movement in space, although we may picture the two places between which the motion occurs. So, too, becoming and change cannot be pictured in the mind, although we may picture the states of being before and after the transition. We may picture an object as here or there, but not as moving."

This statement does not of course deny directly that there are sensations of motion, but only that motion is an element of mental imagery. But it seems probable that any element that

<sup>1</sup> W. T. Harris, *Psychologic Foundations of Education*, 1898, p. 24.



appears in sensation may also appear in mental imagery. To deny absolutely, except on the basis of exhaustive investigation, that motion exists in mental imagery, would be to deny that there are sensations of movement. Now the existence of sensations of motion may be regarded as well established by the researches of Exner for the eye, and of Goldscheider for the joint sensibility. The present paper simply adds the fact that sensations of motion are used in controlling the extent of a movement. In regard to mental imagery of motion, it unquestionably exists. As I am myself a poor visualizer, my testimony as to the visual imagery of motion may not be worth much. Such visual images as I can call up are always *fleeting*. I cannot hold an image fixed before the mind's eye. It constantly changes, goes and comes, and wavers here and there. The flitting past of scenes from a car window is easier to call up than the outlines of a building. On inquiring among the visualizers, I find that some find it perfectly easy to picture an object as moving—just as easy as to picture it at rest—and that others find it difficult, largely for the reason that they try to follow the moving image with the eye. But even suppose that no picture of the motion of an object were ever, by any one, experienced in visual terms—that would not be a sufficient demonstration that no picture of motion could be attained. It is a rather late date to identify all mental imagery with the merely visual. Auditory imagery—such as hearing music—is distinctly a picturing of change. And motor imagery is a picturing of motion. No one, perhaps, will have difficulty in imagining himself as walking or as waving his hand in a circle or figure 8. That slight actual movements almost inevitably occur in connection with such imaginings does not eliminate the fact that the most of the picture is imagined.

The attempt to ascribe our ideas of motion to a purely 'intellectual' origin falls, not only before these empirical facts, but also before the logical difficulty advanced by James<sup>1</sup> that "we can only infer that which we already generically know in some more direct fashion." Unless we had some direct means of perceiving movement no congeries of sensations of position would ever suggest the idea of movement to us.

<sup>1</sup> Elements, II., 171.



After the masterly way in which James has thrown into relief the *streaming* character of our thought, and in particular the feelings of motion, transition and relation, such a discussion as the present is perhaps unnecessary. It serves, however, to set aside a theoretical objection to our conclusion that the extent of movements is gauged primarily in terms of sensations of extent of movement.

#### PART VIII. EFFECTS OF FATIGUE AND OF PRACTICE.

The graphic method, by permitting a repetition of the same act time after time at very short intervals, is well adapted for the study of the fatigue of any function. I have therefore sought to determine to what extent the power of accurate movement was impaired by a long series of accurate movements. Two sorts of movements were tested, both of which have been already described. One consisted in ruling on a kymograph a series of lines, each of which was intended to be equal to its predecessor. The other consisted in attempting to hit with a pencil inside each in turn of a series of squares of one-quarter or of one-fifth inch sides, provided by coördinate paper. The experiments were tried at various speeds of movement, with each hand, and with eyes closed as well as open. I have tried only a few fatigue experiments on other persons than myself, but such as I have tried have given results so fully comparable with those obtained on myself that the general conclusions are well established.

Typical fatigue curves are given in Figs. 11, 12, 13, taken respectively from Tables XXII., XXIII., XXIV. They have in common the following general features:

(1) Although an enormous number of repetitions of the movement was made, yet the loss in accuracy was comparatively slight. Five thousand, or even twelve thousand repetitions leave the average error quite near the original amount. Were the accuracy of voluntary control entirely used up, as the power of voluntary contraction soon becomes in the ergograph experiment, we should find the movements no more uniform than the 'automatic' movements studied above. As was there

suggested, the degree of uniformity of these automatic movements furnishes a sort of zero mark for the accuracy due to voluntary effort. This zero mark would stand at about 4 in Figs. 11 and 12. The zero mark in the target experiments, Fig. 13, would, according to the theory of chance hits, and also according to an experiment in *carelessly* hitting at the squares,

TABLE XXII.

NO. OF 500	1ST 100.		2D 100.		3D 100.		4TH 100.		5TH 100.		AV.	V.	
1	2.1	.2	1.1	.1	0.5	.1	0.8	.1	0.8	.1	1.06	.03	.43
2	1.4	.1	1.3	.1	1.3	.1	2.3	.2	1.9	.1	1.62	.05	.36
3	1.1	.1	1.7	.1	1.6	.1	1.6	.1	1.7	.1	1.51	.05	.18
4	1.3	.1	1.4	.1	1.9	.1	1.4	.1	1.6	.1	1.52	.05	.18
5	1.1	.1	1.4	.1	1.2	.1	2.0	.1	1.6	.1	1.53	.05	.29
6	1.5	.1	1.4	.1	1.7	.1	1.6	.1	1.9	.1	1.61	.05	.16
7	1.7	.1	1.7	.1	1.8	.1	1.1	.1	1.5	.1	1.56	.05	.21
8	2.1	.1	1.3	.1	1.6	.1	1.9	.1	1.6	.1	1.60	.05	.24
9	1.2	.1	1.6	.1	1.7	.1	2.7	.2	2.2	.2	1.88	.06	.46
10	2.7	.2	1.9	.1	1.5	.1	1.5	.1	1.4	.1	1.71	.05	.38
Average for whole series,											1.56	.02	.29

Fatigue. Lines ruled on drum, each equal to the preceding, 60 per minute. Subject D.

The 5000 lines ruled are divided into hundreds, and the average error for each hundred is given in the table. Five consecutive hundreds are combined into a larger group, the average error of which is given in heavy faced type. In the column headed 'V.' is found the mean variation of the errors for hundreds from the errors for their 500; and at the bottom of this column is found the average of these mean variations. The mean variation is introduced in order to furnish a measure of the variability or the uniformity of the performance at different stages. The italicized figures give, as before, the 'errors of mean square' of the adjacent averages. Some suspicion may arise that these errors of the averages are smaller than they should be, since if obtained from the mean variation of the 500, they would often be much larger. The reason for the difference is that the errors of the averages are computed on the assumption that the 500 form a homogeneous series, that no decided effect of practice or fatigue enters during the group. This assumption is sometimes not justified, as for example in the first 500 of the present table. But the assumption is necessary if the comparison is to be made between arbitrary groups of 500. On account of the arbitrariness of the grouping, it is well to consult the average errors not only for groups of 500, but also for the single hundreds. If for instance we examine in detail the first 500 of this table, we see a very rapid improvement (practice effect) from the first to the second and third hundreds. If we drop the first hundred, we have left a series of 400 movements in which the average error was but .8 mm. This is much lower than that of any later group of 400. Thus the full effect of fatigue is better seen than if only the groups of 500 are considered.

TABLE XXIII.

No. OF 500.	1ST 100.		2D 100.		3D 100.		4TH 100.		5TH 100.		AV.	V.	
1	2.6	.2	2.4	.2	2.3	.2	2.1	.2	2.1	.2	2.29	.07	.16
2	1.9	.1	1.8	.1	1.9	.1	1.6	.1	2.1	.2	1.84	.06	.13
3	1.4	.1	1.7	.1	2.1	.3	1.8	.1	1.7	.1	1.75	.05	.17
4	1.8	.1	2.4	.2	1.7	.1	1.3	.1	1.8	.1	1.78	.06	.24
5	2.3	.2	1.9	.1	2.0	.1	1.7	.1	2.1	.2	2.00	.06	.16
6	1.8	.2	2.0	.2	2.1	.2	2.9	.2	2.8	.2	2.28	.07	.41
7	2.5	.2	2.8	.2	1.7	.1	1.9	.1	2.0	.1	2.18	.07	.38
8	2.1	.2	2.5	.2	2.1	.2	2.2	.2	2.1	.2	2.19	.07	.12
9	2.4	.2	2.2	.2	2.4	.2	1.8	.1	1.7	.1	2.10	.07	.28
10	2.6	.2	1.8	.1	2.1	.2	2.4	.2			2.21	.08	.28
Average for whole series,											2.06	.07	.23

Fatigue. Lines ruled on drum, each equal to the preceding, 60 per minute. Subject W. *Eyes Closed.*

TABLE XXIV.

No. of 1000.	1ST 200.		2D 200.		3D 200.		4TH 200.		5TH 200.		AV.		V.
1	16	3	21	3	14	2	18	3	29	3	19.1	1.2	5.3
2	22	3	20	3	22	3	22	3	19	3	20.8	1.3	3.0
3	19	3	25	3	19	3	20	3	27	3	21.8	1.3	3.6
4	25	3	22	3	25	3	18	3	18	3	21.4	1.3	3.5
5	23	3	24	3	18	3	20	3	18	3	20.2	1.3	3.4
6	27	3	14	3	22	3	18	3	23	3	20.4	1.3	3.7
7	18	3	25	3	19	3	20	3	21	3	20.5	1.3	4.0
8	26	3	19	3	27	3	31	3	27	3	25.7	1.4	5.3
9	27	3	17	3	19	3	20	3	23	3	21.1	1.3	3.5
10	20	3	26	3	40	4	29	3	30	3	28.9	1.4	6.5
11	26	3	21	3	24	3	29	3	19	3	23.3	1.3	3.4
12	25	3	29	3	18	3	30	3	26	3	25.4	1.4	5.7
Average for whole series,											22.4	.5	4.2

Fatigue. 'Coördinate paper experiment.' Targets,  $\frac{1}{2}$ -inch square. 200 hits per minute. *The numbers give per cents. of misses.* Subject W. 'V.' gives the mean variation not of the groups of 200, but of groups of 100.

At the end of each hundred hits there was a pause of  $1\frac{1}{2}$  secs. At the end of each 2,000, there was a pause of 15-20 secs., while the paper was changed and the metronome wound.

*Subjective observations:* Fresh and buoyant at the start. Not conscious of much fatigue during the experiment, except in the elbow, the neck, the eye and the ear (from the continual din of the metronome). Toward the close, felt hot and uncomfortable. On stopping, the face was found much flushed, the arm was lame for a few minutes, and the feeling of general fatigue was very strong.

be at about 95% of errors. Having these zero marks we are enabled to compute what may be called the *Coefficient of Fatigue*. This will be a fraction indicating what part of the maximum



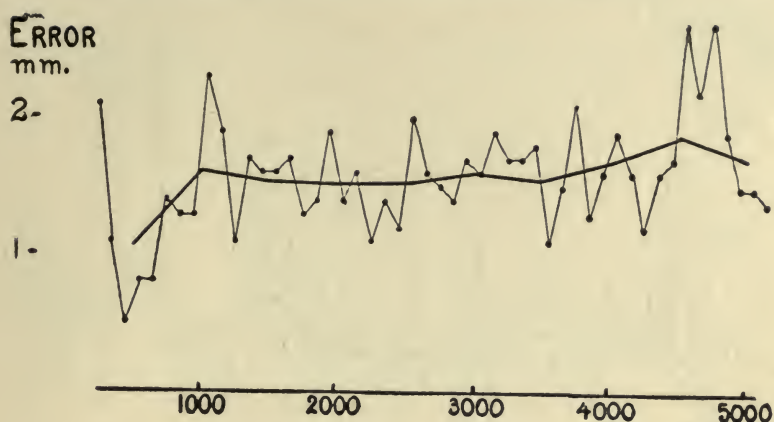


FIG. 11.—Fatigue. Lines ruled on drum, each equal to the preceding. Subject D. See Table XXII. The heavier line connects the points whose ordinates represent the average error of successive groups of 500 (next to last column in Table XXII.); the light line is similarly the curve by hundreds. The dots are for the purpose of bringing out more clearly the location of the average errors for the separate hundreds, and so indicating the variability of the performance at different stages.

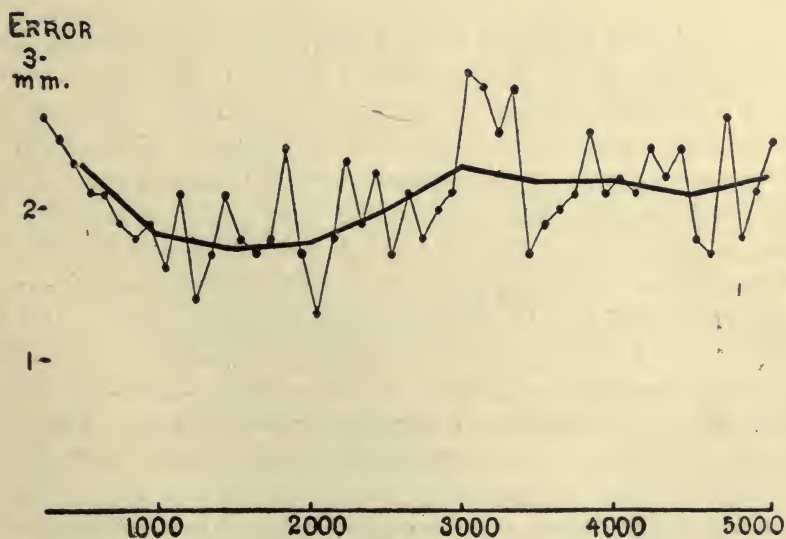


FIG. 12.—Fatigue. Lines ruled on drum, each equal to the preceding, 60 per minute. Eyes shut. Taken from Table XXIII.

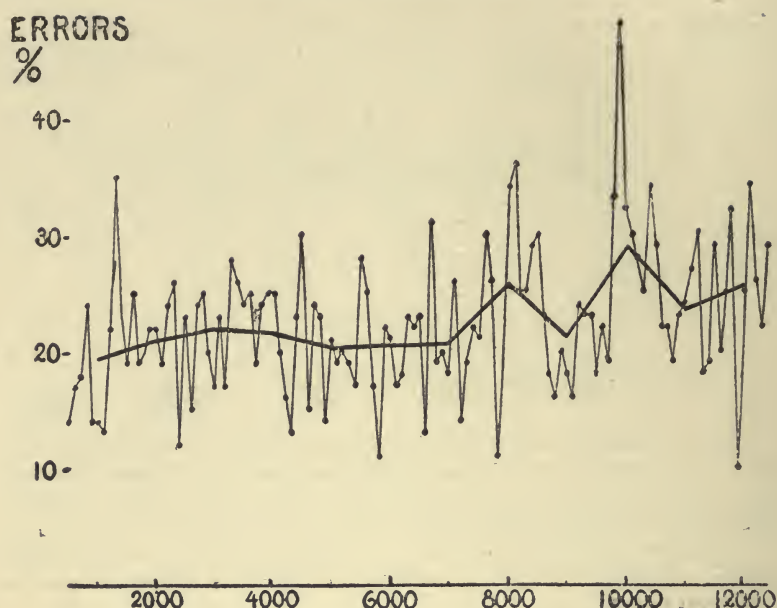


FIG. 13.—Fatigue. 'Coördinate paper experiment.' 200 hits per minute. The heavy line is plotted by thousands, the light line by hundreds. From Table XXIV.

ability is lost in fatigue. From Table XXII., for instance, the coefficient of fatigue is computed as follows: The best thousand is the first, with an average error of 1.06 mm. The worst is the ninth, where the average error is 1.88 mm. The increase from the minimum to the maximum error is thus 0.82 mm. But the whole *possible* increase would be from 1.06 mm. to about 4 mm., or 2.94 mm. Thus the actual increase in error is 28% of the possible. In other words, 28% of the accuracy due to voluntary effort is lost. Or, the coefficient of fatigue is 28%. If we take best and worst hundreds, instead of thousands, we shall of course get a larger coefficient, in this case 63%, the highest I have found, except in two other peculiar experiments. But the averages of the separate hundreds are too much subject to chance, and also to wanderings of the attention, to give a trustworthy determination. By the same method the coefficients of fatigue for the other experiments have been determined as follows.

Table XXIII.....	24%
Table XXIV.....	13%
Table XXV.....	6%
Table XXVI.....	1%
(By hundreds, however, 48%.)	
Table XXVII, MM. 30.....	8%
“ MM. 40.....	17%
“ MM. 60.....	2%
“ MM. 100.....	>100%
“ MM. 200.....	>100%
“ Left Hand.....	11%
Table XXVIII, MM. 120.....	1%
“ MM. 200.....	8%
(By hundreds, 16%.)	
“ Subject S.....	4%

In the cases where the coefficients are given in terms of hundreds also, there seemed to be good reason for it in that fatigue made its appearance within the first 500 or 1000. In the two cases which give a coefficient of more than 100% the movement was, from the start, very near the zero mark, and overreached that mark perhaps simply by chance. The computation of the coefficient of fatigue in such cases is really impossible. The full records and the original tracings show that fatigue was, indeed, very marked in the series with MM. 100, but almost absent in that with MM. 200. Since, in fact, the accuracy at MM. 200 is not appreciably better when care is taken than when the movements are automatic, fatigue would not be expected to appear.

The coefficients of fatigue show, therefore, that the fatigue effect was slight. In the ergograph experiment the coefficient would amount to nearly 100%, within a few score of movements. Here it does not exceed 30%, even in very long series. It is smaller in the target movements than in the ruling movements, because in the latter the zero mark lies so much nearer to the maximum of accuracy—because automatism does so much more and leaves so narrow a margin for the effects of voluntary effort.

(2) Though slight, however, the loss in accuracy is unmistakable. And this is not simply a failure of attention, for toward the close of every series especial effort was always made to do as well as at the first. The experience of subject D, who executed the series of movements recorded in Fig. 11, is interesting in this connection. He tried especially hard during the last 500, and on being asked if he could do as well as at the



start, replied that he was sure he could. Yet the record shows that, though he recovered to a considerable extent from the inaccuracy of the preceding 500, yet he was by no means able to come up to the average of the first 500, nor to equal in any single hundred the accuracy of the best hundreds at the start.

(3) At the beginning of the series there is usually a gradual improvement, continued for several hundred movements. This effect of 'warming up' or practice is not always present, but is quite generally the fact.

(4) There is a large amount of oscillation in the curve for separate hundreds. This is more marked at certain parts of the curve than at others. In general, we can say that it is greatest at the start and at the periods of fatigue. This fact is brought out better by examination of the column headed 'V' in the tables, which gives the mean variation of the averages for the separate hundreds composing a group of 500 or 1000 from the average of that 500 or 1000. If chance alone entered, the 'V' should be proportional to the 'Av.' But it will be seen that this is by no means the case, and that on the whole there is a disproportionate decrease of the variability toward the middle of the series, and a disproportionate increase wherever the influence of fatigue is visible in the size of the average error. These relations do not always hold good, but they are sufficiently in evidence to warrant a statement that *practice decreases the variability, fatigue increases it.*

To these typical fatigue records are added Table XXV., which gives a case of fatigue coming on very early—within the first thousand—recovered from somewhat in the second thousand, but not entirely recovered from even by the unusual attention and effort of the last thousand.

Table XXVI. also, added because it comes from another subject, shows a degree of fatigue quite early in the series.

Tables XXVII. and XXVIII., finally, give in condensed form the records of the other experiments on fatigue. They show the same general relations, especially the slightness of the effect of fatigue, at various different speeds. Observe that the 'automatic' movements become neither more uniform nor perceptibly less uniform as the series is prolonged up to 1000. Observe also that the left hand shows, within a series of 2500, more effects of practice than of fatigue. The adjustment of the left hand seems to be no more easily fatigued than that of the right. The great decrease in average error from the first to the second entries in the last column of Table XXVIII. is due to the fact that the subject was urged just at this point to do his best, and responded by quite a new way of making the movement. The fatigue curve begins properly with the second entry.

TABLE XXV.

No. OF 1000	1ST 200.		2D 200.		3D 200.		4TH 200.		5TH 200.		AV.		V.
1	6	2	7	2	13	2	19	3	17	3	12.6	1.1	4.7
2	10	2	13	2	9	2	10	2	13	2	10.8	1.0	1.6
3	18	3	13	2	15	3	16	3	17	3	15.7	1.2	1.5
4	11	2	15	3	13	2	12	2	9	2	11.8	1.1	1.6
Average for whole series,											1.27	.5	2.4

Fatigue. 'Coördinate paper experiment.' Targets  $\frac{1}{4}$  inch square. 160 hits per minute. *The numbers give per cent. of misses.* Subject W. Shows speedy fatigue and partial recovery.

TABLE XXVI.

No. OF 500.	1ST 100.		2D 100.		3D 100.		4TH 100.		5TH 100.		AV.		V.
1	1.9	.1	2.6	.2	3.2	.2	2.7	.2	3.8	.3	2.82	.09	.52
2	2.8	.2	3.2	.2	3.1	.2	2.7	.2	2.5	.2	2.86	.09	.23
Average for whole series,											2.84	.06	.38

Fatigue. Lines ruled on drum, each one equal to the preceding, 60 per minute. Subject P.

TABLE XXVII.

NO. OF 500.	MM. 30		MM. 40		MM. 60		MM. 100		MM. 200		AUTOM. MM. 60		LEFT HAND. MM. 60	
	AV.	V.	AV.	V.	AV.	V.	AV.	V.	AV.	V.	AV.	V.	AV.	V.
1	1.04	.13	.76	.05	1.12	.10	3.35	.23	4.32	.46	4.36	.30	2.39	.24
2	1.26	.21	.78	.10	1.18	.14	4.23	.61	3.80	.44	4.60	.36	2.53	.39
3	1.08	.10	1.12	.10					4.32	.50			2.11	.32
4	1.28	.23	1.26	.37					4.20	.56			2.54	.23
5			1.32	.22					5.30	.56			1.90	.12
6			1.20	.08					4.54	.73				
Av.	1.17	.17	1.07	.15	1.15	.12	3.79	.42	4.42	.54	4.48	.33	2.30	.26

Several fatigue experiments recorded by five-hundreds only. Lines ruled on drum, each equal to the preceding. Subject W. 'V' gives the mean variation of the averages of separate hundreds from the average of their 500.

TABLE XXVIII.

NO OF 1000.	SUBJECT S, MM. 120.		SUBJECT W, MM. 120.		SUBJECT W, MM. 200.	
	AV.	V.	AV.	V.	AV.	V.
1	(11.3)	(3.4)	30.1	2.0	24.7	4.1
2	1.5	1.0	30.5	1.9	30.3	1.5
3	1.6	1.2	30.6	4.7	24.5	1.9
4	2.1	0.9			25.4	1.7
5	4.7	1.7				
6	5.6	2.4				
Av.	3.1	1.4	30.4	2.9	26.7	2.4

Several fatigue experiments, recorded by thousands only. 'Coördinate paper experiment.' 'V' gives the mean variation of the averages for separate hundreds from the average of their thousand.

The results seem sufficient to warrant the following positive statements :

*The continued repetition of an accurate movement produces but a slow and slight decrease in its accuracy.*

*This decrease, though slight, is after a sufficiently long series unavoidable, and is not the result of mere failure of attention.*

In other words, the central apparatus for the precise adjustment of a movement is susceptible of fatigue, but only slightly susceptible.

From what is so far known regarding the special fatigue of similar functions, it is safe to assume that these statements hold good for all simple central adjustments. Thus Cattell found<sup>1</sup> that during 15-20 hours of almost continuous reacting, the simple reaction time, the perception time and the association time were each lengthened, though but slightly. Bettman<sup>2</sup> records a series of 1000 'reactions with choice,' performed in a little over two hours. The averages of the successive groups of 200 reaction times were 323, 328, 336, 355, 349σ. The series of reaction times obtained by Patrizi<sup>3</sup> were hardly long enough to show the effect of fatigue. They do serve to confirm the statement that the effect of fatigue in these simple central adjustments is slow and small.

The only investigations known to me which contradict this statement are several carried on in Scripture's laboratory. And the contradiction is here more apparent than real. For the functions studied (reaction time to flashes of light in a dark room,<sup>4</sup> accommodation time,<sup>5</sup> monocular and binocular estimation of depth<sup>6</sup>) all required hard use of the muscles of the eyes, and led in all cases to great discomfort in the eyes themselves. In other words, there was great *peripheral* fatigue, and to that, undoubtedly, rather than to fatigue of the central mechanism, is to be attributed the impairment of function.

One however of these experiments in Scripture's laboratory,

<sup>1</sup> Wundt's *Phil. Stud.*, 1886, III., 489-492.

<sup>2</sup> Kraepelin's *Arbeiten*, I., 156.

<sup>3</sup> *Arch. Ital. de Biol.*, 1894, XXII., 189-196.

<sup>4</sup> *Studies from the Yale Psych. Lab.*, IV., 15-19.

<sup>5</sup> *Ibid.*, III., 87-90.

<sup>6</sup> *Ibid.*, III., 68-87.



bearing the name of Henry E. McDermott,<sup>1</sup> is more closely in line with the present paper, and leads to the conclusion that the power of perceiving and controlling the *force* of a movement is subject to very rapid and extreme fatigue. It is, therefore, of some importance in the present connection to see whether this conclusion is correct. It will be worth our while to examine the paper in some detail, and it will easily be seen that the conclusion is founded on a faulty handling of the results.

The experiment consisted in a series of attempted repetitions of the same pressure on the dynamometer. The force required was not the maximum, but moderate. Having made any pressure as a normal, the subject was required to close his eyes and reproduce the same pressure time after time, up to 30-60 times. This, it will be seen, was very similar to my experiments, except that this is on the force of a movement, and mine on the extent. The result was that the force of the contraction, though varying considerably, on the whole increased little by little, so that the last contractions of the series were much stronger than the first. Hence, of course, the error of the later contractions, measured from the first contraction as their normal, was much greater than the error of the earlier contractions. This is interpreted to mean that the accuracy of the judgment of force became less and less accurate, and this is regarded as a fatigue phenomenon. But nothing can be clearer than that the greater error in the later trials is due to a forgetting of the normal. The positive errors of the successive experiments constantly accumulated. Except when such an error was detected by the subject, no attempt would be made to correct it, but the *last* preceding contraction would be taken as the normal. This the author himself seems to recognize as at least a possibility, when he says that the contractions 'were intended to be equal to the first one (or to the preceding ones).' (The difficulty of deciding what the intention actually was in these experiments shows the value of the method used in the present investigation, of making each equal to the preceding, and so eliminating the uncertainty of protracted memory.) Inasmuch, then, as the normal was in each case at least probably the preceding contraction, the fairest test of the relative accuracy in different parts of the series would be to find the difference between each contraction and the preceding, and treat this as the error. On doing this it is found that the error *decreases* rather than increases as the experiment progresses. We have, within the limits of the short experiment, not a fatigue but a practice effect.

The other sign of fatigue—variability—is also detected in the results, and used to fortify the conclusion. But instead of measuring the variability of successive parts of the same series, the method was to take 6 series of 60 each from the same person, done on successive days, and find the mean variation of the first trials from their average, the mean variation of the second trials from their average, and so on up to the mean variation of the sixtieth trials from their average. Naturally, the mean variation increased from the first trials to the sixtieth. But this is purely a statistical or chance result. Blindfold six men, and, starting them from the same point and in the same direction, tell them to

<sup>1</sup> *Ibid.*, II., 72-75.

go straight ahead. They will, of course, diverge more and more as they progress. Their mean variation about their average position will continually increase. But this does not mean that they cannot walk as straight after 100 steps as after 10. Even if their ability to walk straight continually improved, they would still continue to diverge. The same is true in the experiments in question. Theoretically, the mean variation should increase as the square root of the number of trials—that is, if the whole errors were *variable*. The presence of a constant error would diminish this ratio of increase somewhat, but by no means reduce it to zero.

Evidently, if the progressive variability is to be measured, it must be measured in each series separately. When the mean variation of successive groups of ten is thus found, the result is that the variability *decreases* during most of the series.

To prove these statements, I will add the average error and the mean variation for successive groups of 10, in all the separate experiments that are recorded in full.

The average error in dekagrams, if the normal is supposed to have been in each case the preceding contraction, is, for successive groups of 10, and for the different subjects of the experiment:

	1	2	3	4	5	6	7	8
A. G.,	3.0	2.7						
F. C.,	3.7	2.5	2.3	1.6	2.1			
N. B.,	2.0	1.3	3.9	2.5	2.4	2.3	2.8	1.7
J. R. N.,	4.1	3.4						
D. J. R.,	3.8	2.0	2.6					
Miss F.,	4.6	4.5	6.6	3.0	6.8			

The mean variation of the single trials in each group of 10 about their average is as follows:

	1	2	3	4	5	6	7	8
A. G.,	3.2	1.8	2.5					
F. C.,	4.2	2.6	1.8	1.6	2.3			
N. B.,	1.4	1.4	3.4	1.9	1.2	2.2	2.8	1.9
J. R. N.,	4.5	2.0						
D. J. R.,	6.0	3.5	2.8					
Miss F.,	4.3	4.6	4.3	6.5	6.6			

These figures reveal much more of a practice effect than of fatigue. The average error and the variability tend to decrease, and that though the absolute values are on the increase. In only two subjects, N. B. and Miss F., is there anything that looks like fatigue, and even here it is recovered from so fully that it is probably either accidental or due to wandering of the attention. I am far from asserting that it is perfectly correct to regard each trial as an imitation of the preceding. I have no doubt that errors were sometimes observed and allowed for. On plotting the curves for N. B. and Miss F., I find places where the subjects apparently became conscious that they were using too much force, and accordingly moderated it. This cannot very well be allowed for in a mathematical treatment, and probably no treatment would be really correct. But there can be no doubt the treatment here given is superior to that which disregarded the constant error and identified the forgetting of the normal with a fatigue of the power of perception and adjustment.

With this apparent exception removed we have a consensus of results up to date, all leading to the formulation of a general law, namely, that, except where attention wanders, or where muscular fatigue interferes, *the continued exercise of a motor mechanism interferes slightly but only slightly, slowly but only slowly, with either the speed or the accuracy of that mechanism.*

This conclusion is by no means contrary to common experience. We are familiar with facts that go to prove it. Any simple action, provided it does not fatigue the muscles, can be repeated time after time with no appreciable loss of power. A blacksmith or carpenter can hit nails on the head all day. A piano or violin player can keep up his remarkably swift and precise movements for an hour or two. After writing a long time we are unable, indeed, to write either as rapidly or as exactly as at the start; but this is largely a muscular fatigue, if feelings can be trusted. The day laborers, mentioned early in the paper as making 4000 hits with but a single miss, were, when I made my observation, in the last hour of the day's work. At the close of a six-day bicycle race I watched the movements of the riders, and tried to detect some sign of fatigue of motor control. But, as far as I could see, they rode as smoothly and steered as straight as a perfectly fresh rider.

*Practice.* The beneficial effects of 'warming up' have already been exemplified in the fatigue curves. It has also been noted that practice decreases the variability of a performance. I add here another bit of evidence, taken from a somewhat different sort of movement. See Table XXIX.

If decrease in variability is a mark of practice, it follows that a low variability is a sign that the effect of practice has already been accomplished, and consequently that not much further improvement is to be expected. Of two persons, for instance, who can do a given act—such as a compound reaction—in the same average time, that one has the better chance to improve whose separate trials differ more among themselves. It is not certain that he will improve the more, because he may not try so hard. But he has the better chance. I do not know that this fact is established in just this form, but it is a fact that



as a person approaches his maximum speed or accuracy, his individual trials agree more and more closely with each other. All practice curves show this. If a person should reach a point at which his trials were quite unvarying, the meaning would be that he had reached an absolute limit to improvement in that line. The only possibility of further improvement would be to hit upon some *new way* of doing the act in question.

TABLE XXIX.

	1ST 50.		2D 50.		3D 50.		4TH 50.		5TH 50.		6TH 50.		AV. FOR 300.	
Av. Err.	19.5	2.0	18.0	1.8	16.5	1.7	17.8	1.8	16.5	1.7	15.5	1.6	17.5	.7
Var. Err.	8.7	.9	6.5	.7	7.0	.7	7.9	.8	6.8	.7	4.8	.5	6.7	.3

Effects of practice on the average error and on the variability. The experiment consisted in closing the eyes, making with a pencil point a dot on a sheet of paper on the table before you, carrying the hand to the side, and immediately bringing it back and making another dot as near as possible to the first. The eyes were then opened, and the two dots connected with a line to show that they belonged together; eyes closed again, and experiment repeated. Thus the errors were known, and there was chance for correction. Three hundred such movements were made at one sitting, and the average and variable errors computed for each successive group of fifty. The result is that while the average error decreases, the variable error decreases proportionately much more.

There is a theoretical basis for this assertion in the conception of variability. The variability of a species or of a performance is conceived as the result of the varying combination of a lot of small causes, some of which act in one direction and some in the other—some acting, let us say, to increase, and others to decrease, the accuracy of a movement. Improvement consists in a selection of those causes which make for accuracy, and an exclusion, as far as may be, of those which make for inaccuracy. The more one of these sets of causes is excluded, the more uniform will be the result. And, conversely, the more uniform the result, the less sign of the presence of causes making for further accuracy, but not yet grasped and controlled; and the less sign that there are causes making for inaccuracy which are only partially repressed, and may be still further prevented from interfering. In more ordinary language, we may say that variability means that the act is done in different ways. Improvement consists in the selection of the better

ways and the exclusion of the worse. A high variability means that there are widely different ways in which the act is done, so that the field for selection is wide. A low variability means that the act is always done in nearly the same way, so that improvement will be only slight, unless we get out of the ruts, and not merely select, but invent some new way.

As we have seen, a high variability is a sign not only of lack of practice, but also of fatigue. By weakening the selective influence, fatigue brings again into play the causes that make for inaccuracy, and so restores the performance to its original variability. It may even increase the variability beyond its original degree, since fatigue itself seems to be more or less in-

TABLE XXX.

MM.	20	50	80	120	160	200	240	280	320	360	400	480
1st 50.	2	14	14	24	16	22	22	32	30	38	52	30
2d 50.	4	8	6	6	8	14	2	24	16	22	26	40
3d 50.	8	4	8	10	6	4	6	14	14	12	24	32
4th 50.	10	0	6	4	10	8	12	18	6	32	10	32
5th 50.	6	2	8	4	20	10	4	20	24	16	32	32
6th 50.	6	10	8	4	10	12	16	22	22	16	18	34
7th 50.	16	4	2	0	4	12	0	8	16	12	28	34
8th 50.	2	6	2	4	0	12	10	18	16	24	36	32
9th 50.	0	12	0	14	12	14	16	25	33	37	43	60
10th 50.	0	0	0	4	2	6	14	21	21	14	33	43
Av.	5	6	5	7	9	11	10	20	20	22	30	37

TABLE XXXI.

MM.	20	40	60	80	100	120	140	160	180	200	240	280	320	360	400
1st 50.	20	10	12	10	24	8	16	18	20	16	24	48	42	36	54
2d "	6	4	0	0	4	8	12	6	12	16	22	40	20	40	48
3d "	0	6	8	8	16	14	18	8	26	34	30	32	34	44	60
4th "	4	2	0	4	2	6	20	20	14	20	18	42	56	64	68
5th "	6	2	2	2	10	0	6	22	26	22	16	44	32	50	60
6th "	10	6	2	0	14	6	6	18	10	18	38	34	50	56	64
7th "	2	0	2	6	6	14	22	14	20	22	36	38	54	60	54
8th "	0	0	2	2	2	4	6	10	14	20	46	32	46	58	58
9th "	0	0	0	0	4	12	14	4	12	22	36	46	50	46	58
10th "	2	0	4	4	8	14	14	23	25	29	40	44	56	60	67
11th "	0	0	0	0	8	4	12	14	10	6	21	42	38	54	56
Av.	2	2	2	3	6	9	14	16	17	21	29	41	44	53	61

Practice. 'Coördinate paper experiments.' Targets,  $\frac{1}{4}$  inch square. Table XXX., right hand; Table XXXI., left hand. Nos. give per cent. of misses.

termittent, and the selective influence would thus itself be subject to variation, and introduce variability on its own account. A high variability may, therefore, be a symptom either of possible improvement, or of approaching dissolution. Between the two, only the 'history of the case' can decide.

Besides the evidences of practice in the fatigue curves there are given in Tables XXX., XXXI. and XXXII. the results of practice continued for several days or weeks. The first two of these give in temporal order the results already summarized in Table XIV. The movement studied was that of aiming in succession at each of the squares in coördinate paper. The squares here were of quarter-inch sides. As the experiment was not designed to test practice, but the relation of accuracy to speed, no care was taken to do the work in equal portions or at equal intervals. The first eight fifties of the right hand were done within a week; the first nine of the left hand within three weeks, beginning about a month after the series of the right hand. The last two fifties of both hands were done on a single day, two weeks after the close of the first left-hand series and ten weeks after the close of the first right-hand series.

The results of the Tables may be summarized as follows:

- (1) The improvement is not steady, but irregular.
- (2) Where improvement appears, it generally appears very early and is rapid.
- (3) After the interval, the accuracy is at first low, sometimes even lower than at the beginning of the first series. But here improvement is very rapid.
- (4) Improvement does not appear equally at all rates of speed. The lack of improvement at the slowest rate of the right hand is a matter of no significance, being due simply to an uncertainty as to how the movement should be made when there was a superfluity of time. More significant is the difference that appears in either hand between the moderate and the extreme rates. The moderate rates show a sure and permanent improvement; the very rapid rates show practically no improvement, but on the whole a deterioration. This is seen most quickly by comparing the records for the first fifties with the general averages at the bottom of the table. Which rates shall be called moderate



and which extreme, is a question to be answered separately for each hand. The right hand has a fair degree of control up to 400 movements per minute, and improvement is in evidence up so far. But at 480, which is about the limit for mere rapidity of movement, to say nothing of precise control, no improvement whatever appears as the result of practice. The left hand shows very marked improvement at the slowest rates, but none that is permanent or sure above 100. Here we have another fact in bilateral asymmetry of function. We have also a result which enables us to make somewhat more precise the common knowledge that practice does much more good in some sorts of work than in others.

We might expect, *a priori*, that equal amounts of practice would produce proportional amounts of improvement in acts similar in general, but differing in some one element, as here in speed. If one act were twice as accurate as another before practice, we might expect it to be about twice as accurate after practice also. But we find that, in our table for the left hand, the error at 20, at first more than  $\frac{1}{3}$  of that at 400, decreases to  $\frac{1}{8}$ ,  $\frac{1}{27}$ , and finally to less than  $\frac{1}{80}$ . The slow act improves quickly; the similar rapid act improves not at all.

Since the difficulty introduced by high speed is a difficulty in control, the suggestion readily occurs to generalize the statement just made as follows: *When an act lies within easy range of control, it improves rapidly with practice.* The awkwardness of unfamiliarity readily disappears. *But when, for any reason, such as excessive speed, the act lies near the limits of control, it can be done as well at first as after a moderate degree of practice.* Undoubtedly it is susceptible of improvement, but the practice necessary is much more extensive.

An example of this distinction, where speed is not the differentia, is found in the relative effects of practice in singing different notes. A note that lies within easy range improves with comparatively short practice; but a very high note, or a note at the transition between different 'registers,' makes no corresponding improvement. The high notes can be sung almost as well at first as after a moderate amount of practice; the notes that lie within easy range may improve very quickly.

This difference between the effects of practice at high and at low rates is confirmed by Table XXXII. which records a series of experiments by a different method and on a different person. The movement tested was the 'three-target' movement already described, and was executed with the left hand. As this experiment was designed specifically to test the effects of practice, it was arranged as methodically as was feasible. Three experiments were tried each week day, and at each experiment three sets of movements were made, one at a slow, one at an intermediate and one at a rapid rate. The results as related to speed are that practice effects a rapid and permanent improvement at the slow rate, but very little, if any, at the higher rates.

TABLE XXXII.

DATE.	MM. 40.		MM. 120.		MM. 200.	
Mar. 11.	3.5	Av. for 5 days.	4.2	Av. for 5 days.	8.3	Av. for 5 days.
13.	3.3		3.5		7.5	
14.	2.5		3.5		6.8	
15.	2.4		3.3		6.6	
16.	1.0		3.1			
17.	0.7	2.54	3.0	3.52	7.0	7.30
18.	0.5		3.5		7.0	
20.	0.4		3.1		7.5	
21.	0.3		3.7		7.2	
22.	0.5	0.48	3.5	3.36	7.2	7.18
23.	0.6		4.0		7.9	
24.	0.3		3.7		7.2	
25.	0.3		3.3		7.0	
27.	0.6		3.6		7.0	
28.	0.5	0.46	3.5	3.62	6.5	7.12
29.	0.3		3.5		6.9	
30.	0.3		3.5		6.9	
31.	0.6		3.7		6.8	
Apr. 4.	0.4		4.2		7.7	
5.	0.5	0.42	3.5	3.68	7.0	7.06
6.	0.3		3.9		7.1	
7.	0.3		4.0		8.1	
8.	0.3		4.0		8.1	
10.	0.3		3.9		7.2	
11.	0.3	0.30	4.1	3.98	8.7	7.84
12.	0.4		3.5		7.1	
13.	0.3		3.8		6.9	
14.	0.5		3.8		6.9	
15.	0.5		4.0		8.1	
17.	0.5	0.42	4.1	3.84	6.7	7.16
26.	0.4		3.6		6.3	
27.	0.8		4.3		7.7	
Av.		0.60		3.67		7.28

Effects of practice. Subject P. 'Three-target experiment,' with left hand. The errors recorded are errors of mean square, determined as for Table XII.

Two other facts appear in this experiment, though not recorded in the table. In order to find out whether the accuracy of movement was affected by the time of day, three experiments were tried each day, one at 9 A. M., the beginning of the day's work, the second at 2 P. M., immediately after lunch, and the third at 5.30 P. M., at the close of a day's clerical work. The result was that the accuracy was not perceptibly affected by the time of day. The errors averaged as follows:

	40	120	200
Morning.	0.5	3.7	7.3
Noon.	0.4	3.7	7.1
Night.	0.4	3.7	7.3

This result harmonizes well with those of a few other experiments which are not yet, however, extensive enough to warrant positive statements. As far as they go, they indicate that neither slight doses of alcohol nor great general fatigue produce any decided effect on the accuracy. The control of these simple movements is apparently so well ingrained in the nervous system that moderate changes in the general condition of the system do not affect the power of control. On the other hand, there do appear considerable variations in the accuracy at different times, variations which are not accounted for in terms of practice, or fatigue, or time of day, or mere variations in the effort of attention. Some of these unaccountable variations appear in Table XXXII. Many others might be added. The causes of these variations, at present unknown, are evidently more important than the familiar causes, fatigue, practice, time of day and slight doses of drugs.

One more fact came out of the practice experiment. Practice of the left hand helped the right also. Before the series with the left hand began, and again after it was completed, a single experiment was made with the right hand. The errors were as follows:

	40	120	200
Before.	3.1	3.9	7.1
After.	0.7	3.8	6.6

The right hand did somewhat better than the left before and about the same afterward. The right hand shows a de-



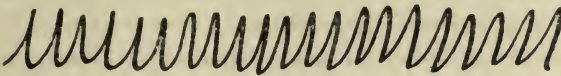
cided improvement at the rate for which the left had improved, but no appreciable improvement at the rates for which the left had not improved. These results show (1) that the transference of the efforts of practice from one side of the body to the other—a transference which has been established in other investigations as taking place from the right side to the left—also takes place from the left side to the right; and (2) that it is not the mere practice that has this transferred effect, but only *successful* practice. Undoubtedly the right hand, if itself practised at the rate of 120, would make a decided gain. It does not gain from the practice of the left hand, because the left hand itself does not gain. Where the left hand does gain the right shares the benefits in almost equal measure.

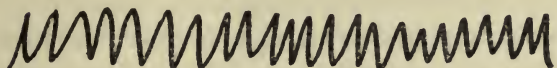
#### PART IX. THE BEST MOVEMENT FOR WRITING.

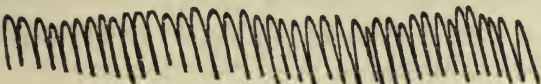
In the experiments recorded on page 85 it was found that a side-to-side swing of the wrist and forearm was likely to be made longer than it should be in comparison with a movement of the fingers or of the full arm perpendicular to that. The reason seemed to be that the side-to-side movement was freer and easier. In following up this suggestion, it was found that that movement was also more rapid, more steady and accurate in direction, but somewhat less accurate in extent. Since these facts led to the query whether this movement could not be profitably used in writing, a more complete study was made of the ease, speed and accuracy in extent and direction of this movement, and of two that are commonly used in writing. One of these is the finger-and-thumb movement, as usually taught to children; and the other a movement of the full arm from the shoulder, which is also sometimes taught under the name of 'forearm motion.'

The experiment consisted in making series of movements, back and forth like a string of small *u's* or *m's*, such as may be seen in Fig. 14. For the finger and full arm movements the paper is held as in ordinary writing. For the wrist movements it is best to let the top of the paper slant over to the right (in case of right-handed persons), so that the direction of the series

as a whole shall be nearly toward the body, or, more exactly in line with the forearm. In this last movement a backward motion of the whole arm carries the hand along the line, while

 fingers.

 full arm.

 forearm

Columbia University } fing.  
Columbia University } RH.  
forearm

Columbia University } fing.  
Columbia University } LH.  
forearm

FIG. 14. Different movements in writing. 'R.H.' = right hand, 'L.H.' = left hand. Reduced to  $\frac{7}{10}$  original size.

the side-to-side motion of the wrist and forearm makes the separate strokes. The results obtained are as follows:

1. As regards ease, the full arm movement, if hastened, is by all means the hardest. It requires the expenditure of the most energy and shakes the whole body. As between the other two, different persons give different judgments. Some prefer the

side-to-side movement, others the more practised finger movement. There is little doubt that, aside from practice, the side-to-side movement is easier. It is instinctively chosen for such movements as erasing. It is made with a much simpler coördination than the finger-thumb movement. The latter, as has been shown by the researches of Duchenne<sup>1</sup> and of Obici,<sup>2</sup> is a complicated affair. It requires, for instance, the simultaneous extension of the first joint of the forefinger and flexion of the second and third joints, and *vice versa*. The full-arm movement has no firm fulcrum, and so shakes the trunk. The forearm movement is the simplest, and resting the elbow provides a firm fulcrum. One can see approximately how the three will appeal to an unpractised hand, by trying them with the left hand. Besides being the simplest in coördination, the forearm movement has over the finger movement the advantage of being made with comparatively large muscles. The ordinary writing movement is made largely with the little muscles in the hand itself<sup>3</sup> (interosseal and lumbrical). The continued use of the small muscles is more liable to lead to cramp than the continued use of the large muscles. It is found that writers who use the full arm motion are much less subject to writer's cramp than those who use the ordinary motion.<sup>4</sup> The muscles concerned in the 'side-to-side' motion, though not so large as those that make the full arm motion, are large enough. On the whole, therefore, the forearm motion would doubtless be, after practice, the easiest of the three.

2. As regards speed, the forearm is demonstrably the best. Make three series of movements like those in Fig. 14, at the fastest possible rate, and time the series. It will probably be found that more back-and-forth movements can be made in a given time by a forearm motion than by either of the others, and that the separate movements of the forearm are also more extensive. Such, at least, has been my observation. Out of 21 persons whom I have tested in this way, there were but four

<sup>1</sup> G. B. Duchenne, *Physiologie des mouvements*, 1869, pp. 173-175.

<sup>2</sup> *Recerche sulla Fisiologia della Scrittura*; *Rivista di Freniatria*, XXXII., pp. 625-643, 870-893.

<sup>3</sup> See Duchenne, loc. cit.

<sup>4</sup> See Dana, *Text-book of Nervous Diseases*, 4th ed., 539-548.



exceptions, none of which was at all marked. The average number of movements per second (double movements, including both back and forth) was :

Finger movement,	5.3,	with a mean variation of	0.8.
Full arm	"	5.4,	" " " 0.7.
Forearm	"	6.5,	" " " 1.2.

As between the finger movement and the full arm movement there is no advantage in point of speed. But the forearm movement averages 23% faster than the finger movement.<sup>1</sup>

The left hand gives the same general result. I have tested only four persons, three of whom showed the greatest speed in the forearm movement. The averages were: fingers, 4.0 movements per second; full arm, 4.5; forearm, 5.2. The forearm, therefore, averaged 16% better than the full arm, and 30% better than the fingers. As a matter of fact, the so-called 'finger movements' of the left hand are not true writing movements. The left hand cannot make those movements without practice. In trying to make movements of the fingers one finds himself using his wrist, flexing and extending it and holding the fingers stiff.

It may, perhaps, occur to the reader as an objection that the extremely rapid side-to-side movement of the forearm is a mere muscle trembling, a sort of clonic contraction, and, therefore, of no use for writing. Inasmuch, however, as the most rapid movements give the same sort of tracings as the more moderate movements of the forearm, inasmuch as the highest rate may be approached without break from the moderate rates, and inasmuch as a certain degree of control can, even without practice, be exercised over the very fastest side-to-side movements, the conclusion must be that these are not muscle trembling, but *bona fide* voluntary contractions, subject to improvement and voluntary control the same as any other rapid movements. It must be admitted, indeed, that the most rapid forearm movements produce fatigue rather quickly. But very much can here be expected of practice. And, besides, this maximum rate is

<sup>1</sup> This result agrees well with that of Bryan, who found that the fastest series of taps on a telegraph key could be made with wrist or elbow, never with shoulder or finger. *Amer. Jour. of Psy.*, V., 123-204.

not much faster than a really comfortable rate which can be kept up for a long time. If one sets out to make movements of the three kinds, not at a maximum speed, but simply at a comfortably fast rate, one generally makes the forearm movement not slower, and very likely even faster than the much more familiar finger movement.

There is then no room for doubt that if the forearm movement should be found feasible in other respects, it would be of decided advantage in the matter of speed.

3. As regards accuracy there are several points to be considered. In uniformity of direction or slant the forearm movement is easily the best of the lot. This may be seen in Fig. 14, or better still in a tracing of the reader's own. There is a smoothness and grace of movement about the work of the forearm that is entirely lacking in the others. The spacing is also fully as uniform as by the other methods. The alignment is, however, inferior to that of the finger movements; the forearm movements do not stick to a straight line very well. And there is more variability in the *lengths* of the single strokes. These two points of inferiority are probably due simply to lack of practice. We can easily remember the difficulty we had as children in both the alignment and the heights of our letters. In order, however, to see how considerable this inferiority was, an experiment was devised in imitation of the simpler parts of learning to write. A series of movements like those of Fig. 14 was made, except that they were required to confine themselves between two parallel lines a centimeter apart (lines of ruled note paper), and to extend just up to the lines. The experiment was tried at different speeds and the errors in extent measured—that is, the distances by which the separate strokes overran or fell short of the boundary lines. The average errors are given in the Table.

It will here be noticed that while the forearm movements show on the whole the greatest errors, yet there are frequent exceptions. The forearm movement *averages* the least accurate in only two of the four subjects. In one the full arm movement gives the least accuracy, and in one the finger movement. In the general average of the four subjects the full arm gives the least error,

TABLE XXXIII.

MM.		50	100	150	200	250	300	350	400
Subj. G.	Fingers,.....	0.24	0.39	0.71	0.84	0.79	0.93	1.09	1.22
	Full arm,.....	0.21	0.46	0.70	0.79	0.93	0.53	1.44	0.71
	Forearm,.....	0.19	0.38	0.86	0.97	1.10	1.35	1.06	1.07
Subj. H.	Fingers,.....	0.14	0.25	0.69	1.54	1.81	2.45	2.19	2.41
	Full arm,.....	0.21	0.61	0.74	1.02	1.63	1.95	1.90	1.74
	Forearm,.....	0.22	0.48	1.02	1.91	1.11	1.74	1.63	2.17
Subj. Sp.	Fingers,.....	0.64	1.04	1.14	1.52	0.83		1.05	0.94
	Full arm,.....	0.43	0.75	0.97	1.79	0.71		0.96	1.49
	Forearm,.....	0.32	0.71	1.03	1.77	1.68		1.25	1.42
Subj. W.	Fingers,.....	0.20	0.39	0.40	0.42	0.55	0.52		
	Full arm,.....	0.41	0.35	0.42	0.45	0.51	0.70		
	Forearm,.....	0.24	0.48	0.48	0.44	0.40	0.61		

Accuracy in extent of three different writing movements. The 'normal' was 1 cm. The average errors are given in mm. Error of each average  $\frac{1}{15}$  thereof, except in case of subject W., where it is  $\frac{1}{25}$ .

the fingers next, the forearm most, in the ratio of 100, 106 and 118 respectively. The forearm gives 18% greater error than the full arm, and 11% greater than the fingers. When we take account of the much greater practice of the fingers in this sort of movement, this result points to the probability that, given equal amounts of practice, either forearm or full arm would surpass the fingers in accuracy. As between the forearm and the full arm, these averages would show that the full arm gives somewhat greater accuracy in extent. This view is confirmed, in case of movements of 15-20 centimeters, by similar experiments on the blackboard, and by the analysis of the error in hitting at a target ('three-target method') into an error of distance or extent and an error of direction. This is done in Table XXXIV.

The result is that the error in distance is less at target No. 1, the movement to which is a full arm movement (a pushing forward of the arm), than at targets Nos. 2 and 3, the movement to which is largely made by the forearm. The error of direction, on the other hand, is greatest at No. 1, being here decidedly in excess of the error of distance, whereas at the other targets it is smaller than the error of distance. The full arm movements are therefore more accurate in extent, the forearm movements in direction. But as far as concerns writing, there can be no



doubt that either movement would with practice attain *sufficient* accuracy for all ordinary purposes.

TABLE XXXIV.

MM....	40	80	120	160	200	240	280	SUM.
1 { dist.	.3	.6	1.3	1.7	2.9	3.2	4.1	14.1
1 { dir.	.3	1.2	2.1	2.2	5.3	4.3	5.6	21.0
2 { dist.	.2	.9	1.5	3.2	3.8	4.3	5.7	19.6
2 { dir.	.1	.5	1.2	2.4	2.4	5.0	4.3	15.9
3 { dist.	.3	.9	2.2	2.9	4.0	5.3	6.1	21.7
3 { dir.	.2	.9	1.7	2.7	2.4	4.5	3.3	15.7

'Three-target experiment.' Subject W. The error due to faulty direction of the hit is separated from that due to faulty extent. This was accomplished by measuring the distance of each hit, not from the target itself, but from two axes passing through the target, one in the normal direction of the movement toward that target, the other perpendicular to the first. Any hit which fell on the first axis was perfect in direction, any which fell on the second was perfect in extent. The distance of each hit from the first axis gave its error in direction, while its distance from the second axis gave its error in extent. The errors recorded in the table are the averages obtained from fifty hits at each of the three targets. The 'error' of each average is one-tenth of that average.

Careful comparison of the three movements available for writing leads then to the discovery of certain points of superiority on the side of the forearm movement. It is easier, made with good-sized muscles, capable of greater rapidity, more uniform in direction, and only slightly inferior in accuracy of extent and of alignment. Some of these points of superiority it shares with the full arm movement, which seems even to be somewhat more accurate in extent. But the great inferiority of the full arm movement in point of ease and rapidity puts it out of comparison with the forearm movement.

It is freely admitted that purely analytical results of this sort are not sufficient to establish the practical superiority of any way of doing a thing. The suggestions gained in the laboratory need to be tested in actual practice before being adopted. I have not had the opportunity of teaching children by the suggested method, and observing their success. That lies beyond the scope of my work. I have, however, tried the suggested mode of writing on myself, not spending time in special practice, but simply using the new method in part of my ordinary writing.

The first difficulty to make its appearance when one who has been brought up to write with the fingers starts to write with the forearm movement is that the paper needs to slant over toward the right rather than to the left, and the unusual appearance of the line to the eye leads to extreme backhandedness. This may be avoided by crooking the arm in closer to the front of the body, and allowing the paper to slant only a trifle, if at all, to the right. This is not the best position for the forearm movement, but it does very well and makes the writing *look* right as it is being written. Undoubtedly one who had never learned to write would experience no difficulty in learning with the slant which to us is unfamiliar. A second difficulty in writing with the forearm movement is that the hand is carried along by a new movement, which at first is awkward. This awkwardness, however, soon passes away.

The first advantage that appears in the new movement is that there is no longer that strong tendency in rapid writing to flatten out the letters until the vertical strokes are mere rudiments of what the copy books teach. This tendency is almost unavoidable in both finger and full arm writing; but it disappears in forearm writing on account of the great ease and freedom of the movement that produces the vertical strokes—that same ease and freedom which make it difficult at first to make the letters of equal height and to keep the alignment. Another advantage which appears in the new method as soon as the first awkwardness has worn off is that rapid writing is easier and less tiring. On the whole, I have found the possession of the new way of writing of advantage to me. A change from one method to the other affords sometimes a very welcome rest.

Besides using the new movement in my right-handed penmanship, I have also practised both it and the finger movement with the left hand. As the left hand had never been used for such purposes, it was somewhat in the condition of the child's right hand when the child is first learning to write. The adult's left hand soon reaps the benefit indeed of the long practice with the right. But at the beginning the left hand is very awkward, and probably gives us an insight into the difficulties

that confront the child in first learning a new movement. On trying with the left hand the different modes of writing, it became at once clear that the finger movement was a hard one to get. At first it is quite impossible to get the proper coördination. The forearm movement, though awkward, is ready from the start. The principal difficulty with it is that the hand is carried along the line by *pushing* it in the direction of the forearm, instead of pulling it as in the right hand; and this pushing of the forearm as it rests on the table is at first very jerky. The finger and the forearm movements were practised exactly equal amounts. Improvement was fairly rapid in both cases. The finger movement came to be the better in uniformity of height and in alignment, but it remained subject to little jerks and angularities due to imperfect coördination. The forearm movement was somewhat hard to restrain, but it was always freer and more rapid. The degree of practice finally attained was not at all high. Specimens of both methods in their present state are given in Fig. 14, which contains also specimens of rapidly written work with the right hand by each method.

The apparent outcome of these practical experiments is that the forearm movement is entirely practicable. And if it be practicable, we may justly infer from our more analytical experiments that it will be in certain important respects an improvement on the modes of writing now in vogue. It will be freer, easier, and less liable to cramp than the finger movement; it will be more rapid; it will not tend to the extreme flattening out of the letters, such as results from rapid writing by either of the other methods; it will be more regular in the direction of the strokes. Whether it will surpass the present methods in the accuracy of height or of alignment is a matter of doubt. It will undoubtedly be perfectly adequate in this respect. And there seems little room for doubt that it will be more readily learned.



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## A Study of Lapses

BY

H. HEATH BAWDEN, A.M.

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## CONTENTS.

I. INTRODUCTION.....	1
II. GENERAL SURVEY OF THE PROBLEMS INVOLVED .....	6
The Conditions of the Lapse .....	6
List of Cacophonous Juxtapositions in the Pronunciation or Writing of which Errors were Made.....	9
In Pronunciation.....	9
In Writing.....	10
Lapses and Sense-illusions.....	16
Coalescence due to the Conflict of two Peripherally Ex- cited Ideas.....	20
Coalescence due to the Conflict of a Centrally, with a Peripherally Excited Idea.....	21
Graphic.....	21
Oral.....	21
Coalescence due to the Conflict of two Centrally Ex- cited Ideas.....	22
Graphic.....	22
Oral.....	22
Principles of Classification.....	25
Errors in Type-writing.....	30
III. THE ARTIFICIAL PRODUCTION AND THE TABULATION OF ERRORS.....	33
Method of Producing Errors.....	33
Errors due to Letters, Word-parts, or Words in Adja- cent Lines.....	36
Errors in Repeating Cacophonous and Alliterative Com- binations .....	37
Cacophonous Juxtapositions.....	37
Alliterative Combinations.....	37
Tabulation of Errors.....	39
IV. LAPSES AS A STUDY IN ASSOCIATION.....	41
General Nature of Association.....	41
The Psychological Unit and its Statement in the Terms of Lapses .....	44
The Factors of Verbal Assimilation .....	58
List of Errors Occurring in 'Internal Speech,' or 'Men- tal Pronunciation.' .....	67
Errors in Writing due to the Influence of Auditory Im- agery .....	70
The Laws of Verbal Assimilation.....	71
List of Errors showing Tendency to form Familiar Words, or Words-parts.....	78
Graphic .....	78
Oral.....	78
List of Oral Errors due to Similarity in Sound of Adja- cent Letters, Word-parts, or Words .....	82



List of Graphic Errors due to Similarity in Appearance in Adjacent Letters, Word-parts, or Words.....	83
Examples of Repetition in Experimental Graphic Er- rors.....	86
Examples of Errors due at once to Persistence and to Anticipation .....	91
Conflict and Coalescence.....	92
Ellipsis due to the Sense of having Written the Letter, Word-part, or Word.....	95
Ellipsis due to Previous Pronunciation of the same Let- ter, Word-part, or Word.....	96
Graphic Ellipsis due to the Anticipation of Letters, Word-parts, or Words.....	97
Oral Ellipsis due to Anticipation of Letters, Word-parts, or Words.....	98
Examples of Graphic Persistent Transposition.....	99
Examples of Oral Persistent Transposition.....	99
Examples of Graphic Anticipatory Transposition.....	100
Examples of Oral Anticipatory Transposition.....	101
Graphic Persistent Substitutions .....	102
Of Letters, or Word-parts.....	102
Of Words .....	102
Oral Persistent Substitutions.....	103
Of Letters, or Word-parts.....	103
Of Words .....	103
Graphic Anticipatory Substitutions.....	104
Of Letters, or Word-parts.....	104
Of Words.....	105
Oral Anticipatory Substitutions.....	105
Of Letters, or Word-parts .....	105
Of Words.....	106
Graphic Examples of Coalescence which involves the Modification of a Vowel or Consonant.....	107
Oral Examples of Coalescence which involves the Modi- fication of a Vowel or Consonant.....	108
Examples of Graphic Exchange.....	109
Within the Word.....	109
Involving Two or More Words .....	109
Examples of Oral Exchange.....	110
Within the Word.....	110
Involving Letters, or Word-parts, in the Case of Two or More Words.....	111
Verbal Exchanges .....	112
V. SUGGESTIONS FOR A PSYCHOLOGY OF THE LUDICROUS .....	113
List of Ludicrous Lapses.....	117
Examples of 'Deformity' Lapses.....	117
Examples of 'Immorality' Lapses .....	119
VI. CONCLUSION.....	120

## A STUDY OF LAPSES.

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### I. INTRODUCTION.

Every one has experienced those unaccountable lapses in thought and expression which form the subject of this study. Ordinarily they come and go, attracting little or no attention; or when unusually striking elicit only a laugh or a passing smile. But occasionally one occurs of such extraordinary character that it excites comment and inquiry as to its causes and probable significance in relation to mental life in general. Professor James mentions the case of a man who said he was going 'to the coal to buy the wharf,' and the writer heard a friend say that he was going out for a walk in order to 'get a bresh of freath air.' Another asked at the druggist's for some 'Phosford's Acid Horsephate,' and inquired for the 'portar and mestle.' Says Baldwin, "We often speak or write words which we do not mean and have not been thinking of."<sup>1</sup> A slip of the tongue or pen has often led to unexpected and unwished for results. Who has not inadvertently said just the thing it was most desired to conceal? Who has not unintentionally distorted words and even sentences into incongruous forms by reason of haste or nervousness? A man who was abruptly obliged to answer an impertinent question, asked by a young man named William concerning the name of another young man to whom reference had been made, confusedly uttered just the name which he wished to keep secret. He answered: "Really, Fred, I cannot." He intended to say: "Really, Will, I cannot tell you who told me." 'Fred' was the very name he wished to conceal. The case is cited as authentic of a "lady who accepted a proposal of marriage when she intended to refuse it, simply through the addition of one or two small words to her letter."<sup>2</sup>

<sup>1</sup> Feeling and Will, p. 283; cf. p. 60, footnote.

<sup>2</sup> F. W. Edridge-Green, *Memory and its Cultivation*, p. 190.

Instances are not infrequent in which the word 'glad' is written in place of the word 'sorry' in letters of condolence.<sup>1</sup>

To cite instances of how words and sentences become distorted, a person accustomed to rapid utterance, intending to say 'original article,' said 'oriticle,' thus dropping the latter part of the first word and hitching on the latter part of the second. Another person said she must 'go out on the corch to pool.' Another said, 'The pastor cut the shermon sort.' Still another caught himself saying, 'My squair' for 'My chair squeaks.' A public speaker said 'Your presence is recorded' for 'Your presence is requested by order of the President.' Another said 'cumbersable' for 'cumbersome or practicable.' Other instances are 'pice-peape' for 'Peace-pipe,' 'clink thearly' for 'think clearly,' and 'that doesn't hofthen' for "that doesn't happen often." A professor in his class-room inadvertently referred to the 'tropic of Cancercorn,' intending to say 'the tropic of Capricorn and of Cancer.' A mother fondly remarked that her child was a 'dear, good girling,' meaning to say that she was a 'dear, good, darling girl.' Another said she must take her 'ba-boyby out for a ride.' A physicist said that he feared he should 'get the instrument out of needle,' when he intended to say that he feared he would 'get the instrument out of level and deflect the needle.' A father told his son to 'put the barn in the cart,' instead of 'put the cart in the barn.' A sister told her brother to keep his 'cleeth tean.' Yet another said, 'Your finger-neels' for 'Your finger-nails need cutting.' A writer in the *Atlantic Monthly*,<sup>2</sup> in an article on 'Heterophemy,' records the case of a young lady who to her own intense mortification often reverses her vowels. "One summer evening she was sauntering with a friend towards the post-office of the little town where they were staying. On the way they encountered an acquaintance with a handful of letters. 'Ah, good evening,' she said, in her peculiarly gracious manner, 'Are you strailing out for your mole?'"<sup>3</sup>

<sup>1</sup> Cf. the same work, pp. 151-152, where such instances are treated under the title of "Revival of Components."

<sup>2</sup> Vol. LXXV, pp. 431-432.

<sup>3</sup> This is, of course, inaccurately reported, even if it actually occurred, as will appear later. It should be reproduced graphically as 'möll.'



Another person spoke of all his misfortunes coming upon him in one 'swell foop.' One who deals with lapses soon becomes familiar with such things as 'beas and peans,' 'wesert dastes,' 'Sophomen and Freshmores,' 'withered lose-reafts,' and other things too numerous or too curious to mention. A sentence composed entirely of lapses probably would read something like this: "Nast light I stook pecial spains to get the quettl sestioned as to how the Saniards were spettled." This meaningless jargon is composed of actual errors perpetrated at one time or another in the course of conversation by different persons. One is reminded of the strange utterances of the aphasic patient. Multitudes of similar errors might be cited.

The lapse always comes as a surprise to the reader or writer himself. It is out before he is aware of it. Frequently it will escape him altogether. A study of lapses is a study of automatic, not of voluntary mental process, since the errors are perpetrated before they are recognized as such. The lapse is as truly an involuntary act as that of the absent-minded man who draws his stocking over his shoe or blows out the gas, or that of the woman who smells her watch to see if it has stopped. A friend relates the following experience: "I was reading along on a double-column page, glancing up now and then, when the word 'tobacco' unaccountably loomed up in consciousness. Looking back and ahead in the same column I could find no trace of it, and gave up the search, thinking that it probably came in from some outside suggestion. I afterward found it, however, in the right-hand column, where it occurred almost exactly opposite the point where it intruded itself in the left-hand column. Doubtless it made its impression at the moment when, in the excursion of the eye while reading the first column, it fell upon the second." The writer had a similar experience in looking up from a book which he was reading (*Nursery Rhymes*) to amuse the baby who was tugging at his chair. I said, "Baby! Oh, Croakety, Croakety, Croak." I was surprised to find myself repeating these words to the child, and wondered what could have made me say so unusual a thing, when my eye fell upon the page of *Children's Rhymes* before me, which contained these words, though I had

not as yet consciously read them. Another instance may be cited. After jotting down a line of thought suggested in reading a paragraph in which the word 'explicative' occurred, I was surprised to find it reverberating in consciousness. Not recalling that I had read the word, I looked to see if it was suggested by the text I had been reading, and, of course, found it, when it immediately ceased its clamor. It is interesting to note that when first conscious of the word I was pronouncing it *sotto voce* with distinct movements of the lips. Lapses are thus what Professor James would call 'fringe' products of the conscious mental process. If we could only reverse the process which the mind goes through in attempting to read an illegible letter, we would find in the motor process of expression just those incongruous errors which flit before the mind in attempting to decipher the signs and symbols which were intended by the writer to convey definite ideas. Lapses begin as semi-conscious inhibitions and overlappings. They may end fully conscious as product, but never as process. Even as mental products they often escape one entirely, so that when confronted with an error that has just fallen from his lips, one is disposed to ask with the Ancient, *Ubi lapsus? Quid feci?*

The foregoing examples, it will be observed, are all oral errors. These are more complex and interesting as a rule, though not always more instructive, simply because the average person speaks more rapidly than he writes. The following are representative errors in writing, taken at random from a large list of lapses. A woman, attempting to write her own name (which was Mrs. H. H. Borgoine), was at the same time holding a conversation with another person concerning a certain Mr. Porter. The result of this division of her attention was that she wrote, instead of her own name, 'Mrs. H. H. Porter.' Of a great variety of lapses these are typical: 'Ard' was written for 'Are distributed'; 'wisher' 'liker' and 'wither' for 'wish her,' 'like her' and 'with her,' respectively; 'John cambe' was written for 'John came to bear'; 'whold' for 'who hold'; 'if it it' for 'if it is'; 'clodium' for 'sodium chloride'; 'ank' for 'any kind'; 'Anothing' for 'Another thing'; 'Hig' for 'His fig.'; 'max' for 'wax model'; 'very soof' for 'very soon

after'; 'beneathe' for 'beneath the'; 'Sea, Dear!' for 'See, Dear!'; 'rember' for 'remember'; 'oppositesides' for 'opposite sides'; 'himsight' for 'himself in the light of'; 'id dit' for 'it did'; 'no dot' for 'do not'; 'scrace' for 'scarce'; 'sacred cat' for 'scared cat'; 'bother' for 'both taken together'; 'mend' for 'men and'; 'wright' for 'right and wrong'; 'growp' for 'growing parts'; 'the forg' for 'the form which an organ takes'; 'tace' for 'taste nice'; 'eath' for 'each other'; 'you make' for 'you may keep'; 'frow' for 'from now'; 'of they' for 'of the eye'; 'each set when they are set' for 'each set when they are sent'; 'What are the neurochord and the neural tube'? for 'What are the notochord and the neural tube'? Here we get the *disjecta membra* of speech, the wraiths of incoming and the ghosts of departing words. Any one who has served as subject in reaction experiments for association in the psychological laboratory knows what the feeling is of catching at the rag-and-tag ends of ideas that are struggling to take on definite word-forms. This is what we get in the case of lapses.

Now just as little irregularities in the road enable one accustomed to it to make his way in the dark, so to the student of human nature little 'inadvertences in expression,' 'aberrations in speech,' 'lapses of thought,' 'confusions of ideas,' 'hitches' or 'slips' in speaking or writing (*lapsus linguæ* and *lapsus calami*) are sometimes most useful and unerring guides in the understanding of mental process. Neglected trifles are sometimes suggestive of most fruitful problems for research. Professor James says: "The great field of new discoveries is always the unclassified residuum." Lapses belong to this category. Such minutiae become valuable chiefly, however, only as they are collected in great numbers and interpreted in the light of inductive generalizations from a wide range of data. A vast body of facts must be accumulated before their connections and dependencies can be made apparent. Accumulation of such data is necessarily slow, since the most instructive errors often occur at times when it is impracticable to record them. Yet many may readily be noted when one's attention has been directed to the subject and their value as psychological material has been pointed out.



This study has a two-fold aim: first, to set forth a comparatively new range of phenomena, of which the above are examples, and, second, to propose general lines of interpretation for these phenomena.

## II. GENERAL SURVEY OF THE PROBLEMS INVOLVED.

*The Conditions of the Lapse.*—On the testimony of the persons who made the errors lapses were variously caused. A lapse is usually accounted for by the person making it in one of three ways. It is due either (1) to the *lack* of sufficient attention, caused by thoughtlessness, hurry or nervousness (cf., *e. g.*, what further on are called persistent transpositions and substitutions); or (2) to *over* attention (cf., *e. g.*, anticipations and insertions); or (3) to a *divided* attention in a case where two objects, either both external, one external and one internal, or both internal, strive for the focal point in the field of consciousness (cf., *e. g.*, modifications of consonants and vowels, and exchanges). The first of these may be regarded as belonging to the general class of fatigue phenomena. The second and third types belong to what Stout has called conflict or competition.

The conditions described by introspective analysis on the part of a person making an error can, of course, be only in the most general terms and relatively inaccurate, since lapses are phenomena of motor automatism. Whatever the conditions may be, they can be discovered only indirectly, since, in addition to the error in its already completed form, we have no facts or introspective data with which to deal. These errors are not the product of artificial, in the sense of voluntary effort, but are purely subconscious in origin. Even in the errors produced under experimental conditions there is no hint of what sort of an error will result, until it is actually seen or heard. The first experience of the person making the error is the auditory or visual perception of a lapse; *e. g.*, a word is recognized as having been spoken or written in a connection where it has no meaning or gives a meaning which was not intended. Collins<sup>1</sup> thus describes this process in the case of oral errors: "If

<sup>1</sup> Faculty of Speech, etc., p. 251-252.

when one is speaking aloud a word is misplaced or a word is not used in its proper sense, if there be made what is called a *lapsus linguae*, the auditory area, which is keenly alive to the slightest misuse of words, quickly detects the error and communicates it to the intelligence or carries it into consciousness. This in turn calls up the articulatory image of the proper term, which is then articulated. The sound of every articulated word acts as a stimulus to the auditory centre for the next." An analogous description holds for graphic errors. The important point to notice in this connection is that the error never is detected *until after it has been made*, and, except indirectly, there is no control over the organs involved which can prevent such errors in the future.<sup>1</sup>

Strictly speaking, of course, the proximate causes of errors are exclusively neither central nor peripheral, but partly both. There is good reason to believe that in *all* verbal association there are so-called sensory (peripherally excited) as well as motor (centrally excited) factors. It is sometimes a question whether the disability results from interference with the formation of the mental content (in the process of the interpretation or construing of the stimuli) or in the emission, in the expression, of what has been rightly interpreted. It is quite impossible to determine this point on purely introspective grounds. Certainly it is impossible to exclude such immediate causes as stiffness of the lips, immobility of the hand or fingers, fatigue, cold, nervousness, preoccupation, distraction, etc., on the side of the organism, and such obstacles as slight irregularities in the dictation or on the printed page (inaccurate articulation, poor type, etc.), on the side of the external stimuli. The influence of the latter, however, has been reduced to the minimum. In lapses, then, we have to deal with phenomena which, even in their simplest form as artificially isolated in the laboratory, form a complex of processes. The peripheral alternations of stimulation are far too complex for any but the roughest measurement. Even less computable are the fluctuations of circulation and respiration which undoubtedly affect the centrally excited pro-

<sup>1</sup> A striking confirmation of the general doctrine that the activity experience is always reported in consciousness in terms of sensation and feeling alone.

cess. The fact that in so-called motor aphasia the faculty of mere mechanical copying (sheer imitation) is frequently retained,<sup>1</sup> while the power of interpreting what is written (intelligent copying—the translation of the symbols into internal language, into meaning) is entirely lost, shows the high degree of complexity of such a process as the ordinary intelligent act of writing.

The specific occasions of errors, mentioned by the persons making them, are as follows: (1) Too great speed or hurry; (2) embarrassment, bashfulness, or reticence; (3) nervousness or worry; (4) fatigue, weariness, exhaustion; (5) preoccupation from within or distraction from without; (6) absent-mindedness; (7) confused ideas or conception of the subject-matter; (8) hesitation in choice of words (especially between synonyms); (9) blank stupidity or mental vacuity; (10) carelessness, inattention, neglect. (11) One fruitful source of errors is a complete or incipient state of reverie. The word or sentence is initiated in good form by the voluntary attention, but, due to a temporary diversion, makeshift combinations ensue. There is just sufficient attention to secure an expression of some sort, but also sufficient disorganization or fluctuation of the attention to make that expression take an incomplete or erroneous form. (12) Many errors arise from difficulty of pronunciation or writing. Lists of such cacophonous juxtapositions of letters and words are appended below.<sup>2</sup> (13) Many curious cases arise, also, out of peculiar methods of abbreviating words and sentences in rapid writing: for example, one writer's usual abbreviation for such an expression as 'fact of being' is 'fact of bg.' In the rush of composition he wrote 'facting.' This is a single example of what may be called condensation-errors or abbreviation-lapses. Others will be found scattered throughout the tabulations. (14) Many errors do not find their way into the tabulation simply because there is no certain mark by which to classify them. An example is the following, which is only a sample of myriad others. A lady wrote as follows: "I was glad to get your letters and delighted y" for 'delighted with.' No reason immediately appeared for the error, and she was sur-

<sup>1</sup> The same is true of mechanical reading.

<sup>2</sup> See Table I, and cf. second column in the other tabulations.



prised to find that she had written the 'y' and thought for a moment that, of course, 'with' began with a 'y.' The only possible explanation of the error was the 'y' of the word 'your,' which was just above it in the preceding line, but the presence of this 'your' did not come into the consciousness of the errorist.

Grouping the above approximate occasions of error according as they belong to the fatigue phenomena or to the phenomena of conflict or competition, they may be stated as follows. (1), (3), (4), (9), (10), (11) may be regarded as due to the effects of fatigue, in the broad sense of that term. Thought runs away from execution. The organism does not respond to the stimulation; as the common expression goes, we 'are not equal to it!' On the other hand, (2), (5), (6), (7), (8), (12), (13) may be regarded as due to conflict or competition. Here the functioning of the organism is altered or brought to a dead-lock by reason of opposing stimulations, as for instance when one attempts to carry on a conversation while composing a letter; as the common expression goes, we 'get all wound up.'

TABLE I.

List of Cacophonous Juxtapositions in the Pronunciation or Writing of which Errors were Made.<sup>1</sup>

*In Pronunciation.*

Nova Scotian coal fields	Mainspring since still strongly
Sinking ship	stirred
Hymonymous hemianopsia	A current propagable purely by
The superhuman energy of a ferocious archangel	hand
Æsthetic satisfaction	Stretched string
Details which lack objective purposiveness	The ignition of such rapid inflammables
Chants a rhythmic catechism	There is still a strong tendency
Indefatigable disinterestedness	That single shore-side street
Wit is an approximately instantaneous revelation	We see in him an image
Development in individual	A Spanish cavalier and an Italian physician
Indissoluble unification	I looked up at the artist's dark intelligent face
Peculiarly difficult complications	Betrayed his strange mastery

<sup>1</sup> These are samples merely.

Irks care the crop-full bird; frets	nuovo's few mean hut-like
doubt the maw-crammed beast	houses huddled together on the
Anything reticulated or decus-	hill-foot bleak
sated at equal intervals	Save as thou teachest us
Has not habitually	Our only anthem and hymn
Shadworth Hodgson	A sharp spear of pain
Maintains the minimum character-	Silk purses are not made from
istics	sows' ears
Strand so strengthened	The grief that gazes at a grave
Incomputable but irrefragably cer-	Bask, ask, task
tain	Various salts and gases
He was rhythmically reeling off	Miles distant
cachinacious retorts	Lashing furiously
Pronouncing ponderous polysyl-	Hawaii and elsewhere
lables	Organism consists
Dainty bits enrich the ribs but	Sixth seal
bankrupt quite the wits	This joint-sense test
I found the wayside inn by Castle-	Statistics

### *In Writing.*

Earliest stages	Catechism
Philadelphia and Egypt	A series of fault lines
Sudden dawning	Regarding the two as homodyna-
An analogous	mous
Who hold	The fossil botanist
An auditory age	Fundamental antithesis
Lest my Lord be	From that of hoofed Herbivora
Development in individual	Denominational college
Feelingful	We see in him
Three hundred acres	It is wonderful what
Absent-minded man at the mo-	Remember
ment	That an organism
Some thick, some thin	Our own resources for research
On the first bed, though in a lions'	work
den	Sends the shivers
Sixth seal	Whom something
Conflict and morbid conscience	Have not habitually
Rapidity with which	Concrete realities
Solved in the initial assumption	Begun on
Two-toed sloth	Quadrigemina
Alliteration	The thought into the thing

In the outward form	Are equally
Know nothing	By proliferation
Another thing	Right and wrong
At different times	Shoulder the burden
Harvest time	Pusillanimous
Graphic characters	The hitherto true tone
Granular layer here	Before fully

To make a statement of the conditions of the lapse on the objective side, that is, in terms of the still embryonic science of cerebral physiology, is much more difficult. The most that can be done is to state plausible hypotheses. The fact that articulate and written language may be produced without errors during unconsciousness (hypnosis, etc.), and the fact that such errors as do occur in conscious life are products of sub-liminal processes, show that the conditions of the phenomena we are here dealing with must await statement in terms of the cortical processes involved, since there are practically no immediate introspective data in terms of which a statement can be made. In speaking or writing I concentrate my attention on one letter or syllable, on one word or sentence at a time, according to my previous training; but there is always a going and a coming marginal part of the sentence or paragraph which is also more or less vaguely in consciousness. It is out of the confusion or conflict of the various elements forming this visual, auditory or kinæsthetic marginal content, that all we know introspectively of oral or graphic lapses arises. The real conditions of these 'marginal' or 'fringe' processes of combination are seldom, if ever, within the scope of introspective observation. They have to be worked out indirectly and in neural terms. When an error, ostensibly in the terms of psychology, is called a fluctuation or aberration of the organic or involuntary attention, this is the same thing as saying, in physiological terms, that there is a 'hitch' in the organic adjustment. The so-called organic attention is nothing but physiological or neural process, viewed as the factor *conditioning* a certain series of conscious experiences. As to the neural relations which produce an error, the most that we can say is that it probably results from an antagonism of dynamic systems. As Leuba says, "The reflex arc processes are com-



bined in two groups, and end respectively in muscles coördinated for contrary actions."<sup>1</sup> The result is primarily only a physical antagonism, but it is reported in consciousness as conflict or competition. Its genesis in the neural coördination, or rather mal-coördination, is, however, the only adequate explanation. Often the hybrid products of such conflict find their way into clear consciousness only after their complete formation beneath the threshold, while yet they are felt to be the resultant of a complicated process of growth. In such cases they are recognized in experience as examples of assimilation, or more specifically as cases of coalescence. The problem of lapses as considered in the following treatment is restricted to its statement in terms of association, which deals with them chiefly in their form as mental products. But certain further questions propose themselves as to the cerebral conditions, which we may pause simply to state, before passing on.

An interesting query arises whether lapses, both oral and graphic, may not, some clearly, others less definitely, be due to an incipient aphasia or agraphia? At least, must we not say that they are due to a momentary mal-coördination in the corresponding cortical areas—and what is aphasia but a similar phenomenon on a larger scale? Certainly, the frequency of errors in an individual sometimes reaches a degree and a constancy which might well be called a transitory or local aphasia, or paraphasia. Such would be what is called functional aphasia, doubtless often simply cases of temporary exhaustion through over-expenditure of nerve force. Since the speaking or writing returns almost immediately to its normal form it is evident that the disturbances in most such cases can be only transitory.<sup>2</sup> It has long been recognized that defects of speech may be due to disease (aphasia). It is now recognized that bad spelling as well as bad pronunciation may be due to cerebral lesion (agraphia). Aphasia (in the broad sense of Asemia) results from lesion of the zone of language, or of the pathways leading to or from it. These may relate to the reception (inter-

<sup>1</sup> *Am. Jour. Psychol.*, July, 1897, p. 531.

<sup>2</sup> Ballet (*Le Langage Intérieur*, p. 78) remarks on the gradual stages which intervene between functional verbal amnesia and aphasia.

pretation) or emission (expression) of language. Lesion in the case of the first constitutes what is called sensory aphasia; lesion in the case of the second constitutes what is called motor aphasia. The literature of aphasia is very large and forms a distinct department of mental pathology. Of this we do not intend here to speak except to call attention to the likeness of these pathological phenomena to the facts under consideration. In certain conditions of the nervous system the agraphic patient will almost invariably write 'ot' for 'to,' 'tub' for 'but,' etc. In another slightly varying form, instead of 'the' the first letter is omitted, while in longer words the letters and syllables are often so distorted that it is difficult or impossible to get the sense. This simple type of misspelling or mispronunciation is not confined to persons who are acknowledged to be agraphic or aphasic; it occurs also in the normal experience. The chief difference seems to be in the frequency and variability with which they occur. The same mistakes will be made by the normal and by the abnormal individual, but in the former case the error is variable, while in the latter case it tends to be constant. For example, one person in rapid writing from dictation frequently writes 'tub' for 'but,' and like the aphasic patient drops the first letter of 'the,' yet this is the exception and not, as in the aphasic patient, the rule. The resemblance between oral errors and aphasic expressions is of too common observation to require illustration.<sup>1</sup> Many cases of real aphasia doubtless pass simply as unusual difficulty in speech, or as what are here called *lapses linguæ*. The following agraphic material illustrates sufficiently the similarity which exists between the normal and the abnormal expression. It is arranged in columns for convenience of comparison. With this may be compared the graphic lapses in the tabulations below.

*Error.*

Drar Ser  
I wont too sells  
three Hudderð acers  
of lande foe actage

*Correct Form.*

Dear Sir,  
I want to sell  
three hundred acres  
of land for a house,

<sup>1</sup> Interesting examples are cited, for example, by Bastian, *Brain as an Organ of Mind*, p. 668 f.

six Rooms rick and  
Saliage Hous Mtht Barnis  
and Shedes and a Poller yard  
close in Denver.

a six-room brick cottage,  
with barns,  
sheds, and a poultry-yard,  
close to Denver.

Roydnendd navendendd ofor  
endendd Belondendd.

Royal naval medical office belong-  
ing to the Admiralty.

The assoil lens a puff piff miss  
corres povety.

The Odessa line is again working  
properly.

omdern  
schrussen  
schreigen  
butter  
Willeer Dotson

modern  
grussen  
schreiben  
mutter  
William Dotson

The peole enjojm evfoves themseds. The people enjoyed themselves.<sup>1</sup>

Does such resemblance confirm the theory that every person is mentally a little unbalanced, and that education from this point of view is simply the attempt to secure and maintain mental equilibrium, which, however, is never actually attained? Sleep, it has recently been suggested, is a periodic lapse of consciousness due to a certain contraction of the nerve elements.<sup>2</sup> Of essentially the same nature may be the approximate cortical conditions of the lapse. One writer has suggested that possibly the central condition for many of the errors made by the normal person in speech and writing, as well as in many other forms of motor coördination, is a momentary playing over against one another of the two cerebral hemispheres (that is, in the case of lapses, the bilaterally situated, though unequally developed, oro-lingual and grapho-manual centres). Another has suggested that possibly an uneven suffusion of the cortical areas concerned, due to some abnormality in the general circulation, occasions errors

<sup>1</sup> The above cases are nearly all taken from Eskridge, *Med. News*, Aug. 1, 1896, pp. 9, 122.

<sup>2</sup> M. Duval, *Théorie histologique du sommeil*, C. R. Soc. de Biol., 1895 and 1898.



in speech and writing, somewhat as minute granules in the humors, or opaque bodies in the lens of the eye obscure vision. But such general suggestions have little value in lack of detailed application and explanation.

Stricker<sup>1</sup> and others have called attention to the insufficiency of any theory which assumes a corresponding multiplicity of cerebral elements for the different words, letters or parts of letters used in speech or writing. He emphasizes the fact that the words which we speak or write do not exist in the brain as heard or seen; we construct each word out of the heard or seen images. He likens the cerebral apparatus in this respect to a modern type-writer in which there is a key for each sound. We begin life with simply a cerebral apparatus for the reception, registration, coördination and expression of sounds, and the meaning-ideas associated with these simple sounds or complexes of sounds (this would be true also of visual symbols) are developed by gradual stages of growth from these simple conditions. This is borne out, he says, by the facts of the development of speech in children. The child first uses only a few simple sounds. Later he unites these sounds into simply constructed syllables. Then these are framed into short words or fragments of longer ones. These fragments are finally united by the simpler connectives with other syllables and word-fragments. Thus develops articulate speech. The growth of the speech and writing faculties is conditioned upon the degree of adaptability and the amount of actual practice; learn by doing is the law. But here again, supposing this to be in general a correct statement of the process, we are lacking in specific details on the neurological side.

On one more point just a word: With regard to the problem of the localization of function in the cortical areas there are two general theories: (1) one which holds that there must be a separate coördinating center; and (2) an opposing theory which denies the necessity of a separate mechanism for coördination. According to the first view there must be at least a kinæsthetic (articulation or inscription) word center, an auditory word center and a visual word center—all sensory. For these to coöperate

<sup>1</sup> Studien über die Sprachvorstellungen, pp. 34, 77.

in the production of speech or writing there must be another, a coördinating center. According to the second view it is necessary only to suppose a certain relation existing between these centers. The latter is at present the view which harmonizes best with the whole range of the facts.<sup>1</sup> This has a bearing upon what will be said later as to the factors of verbal association.

*Lapses and Sense-illusions.*—It has been said that lapses become known first as errors in expression, though they may originate as errors in thought or idea. Even mental pronunciation, in which errors frequently occur, is a form of expression—a motor as well as a sensory process. Yet lapses differ from sense-illusions only in that the emphasis in the first case is on the motor, while in the second case it is on the sensory side of a sensori-motor arc. When the humming of a mosquito in the ear was mistaken for band-music playing at a distance, and when the low whistle of a distant locomotive was mistaken for the humming of a mosquito, we are dealing with sense-illusions. But when a person inadvertently speaks of the ‘dosy rawn,’ ‘hale and dill’ and the ‘coss crat’ instead of the ‘rosy dawn,’ ‘hill and dale’ and the ‘cross cat,’ we are dealing with errors of the motor type, which, though conceivably originating as sensory, become known only as motor errors. It is impossible in the present state of knowledge to draw an absolute line between the purely sensory and the purely motor types of error, since we become aware of the sensory only through the motor end of the process. We know, however, in a general way whether the peripherally excited or the centrally excited factors in an error predominate. In the former case we classify the error as a sense-illusion, or, as treated here, a peripherally excited error (sensori-motor in the restricted sense). In the latter case we classify the error as a lapse proper, or as a centrally-excited error (ideo-motor in the general use of the term).

The problem here arises as to whether lapses are essentially errors in the interpretation of the sensory presentation or errors in the expression of what has been rightly interpreted. Theo-

<sup>1</sup> Cf. R. Sommer, Zur Theorie der cerebralen Schreib- und Lesestörungen, *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorgane* (1893), V., p. 319.

retically there are four possibilities. They may be (1) Errors in interpretation (the sensory end of the process), (a) not followed by a subsequent error in expression, (b) followed by such a subsequent error (a double error); or (2) Errors in expression (the motor end of the process), (a) of what has been rightly interpreted, (b) of what has been excogitated (in which there is no discoverable element of interpretation). The motor expression of a sensory error in interpretation would, accordingly, not be strictly a motor error, for the motor end of the process would be perfect, only it would reproduce what is contrary to reality. To illustrate—A pupil understood his teacher to say ‘Bible,’ and wrote the word thus, what the teacher really said being ‘fibre.’ Similarly, a child understood its teacher to say ‘pound her dominion,’ and so wrote it, what the teacher really said being ‘pawn her jewels for her dominion.’ In these cases the error was obviously in the sense-perception, not in the graphic (*i. e.*, motor) reproduction. But in the case of the boy who said he had ‘jot a golly apple’ and the girl who said she had an ‘awful home-strick seak,’ we are dealing with lapses involving a mal-adjustment of the motor process. It is impossible in many cases, however, to determine whether an error belongs exclusively to the one category or the other.

It will thus be seen that we regard the sensory and motor aspects as simply two ends of the same organic circuit. Every mental state is a complex of peripherally and centrally aroused ideas. An idea aroused solely from without or solely from within probably never occurs. The question then becomes one in which psychology has to wait for physiology and neurology, rather than one which can be settled on the basis of mere introspection. Wundt says<sup>1</sup> that probably the great majority of so-called hallucinations are illusions, and that these illusions in their psychological character are nothing but assimilations. In other words, it is a purely arbitrary matter where you draw the line between the two ends of the process, the truth being that if you start with either end you inevitably run over into the other by a continuous analysis. An error, accordingly, is to be interpreted not only as a passive sense-illusion, but also as a

<sup>1</sup> Outlines of Psychology, p. 269.



sensori-motor, sometimes as an ideo-motor complex, which presents these two aspects according to the point of view from which it is regarded. It is in line with recent investigations of the dynamic character of consciousness to believe that in the speech and writing consciousness every expressive process is conditioned more or less directly upon a receptive process; that every motor involves a partial or complete reinstatement of a corresponding sensory process<sup>1</sup> or, to state it less equivocally, that every motor is but one aspect, or one end, of the same process of which the sensory is the correlative aspect or other end. Applying this principle, the same process, or sensori-motor arc, viewed from the standpoint of its centripetal aspect (from the point of view of interpretation), we call a sense-illusion: viewed from the standpoint of its centrifugal aspect (from the point of view of expression), we call it a lapse.

The distinction which is here made between sensori-motor and ideo-motor errors is based on the more obvious sources of the ideas involved. The sensori-motor are peripherally excited errors which find expression in the motor process. They may be called imitation errors, since they are always such as appear in oral or graphic reproduction. The oral occur in reading aloud or in repeating from dictation; the graphic, in the process of copying either from dictation or from the manuscript or printed page. The ideo-motor are centrally excited errors which find expression in the motor process. Here the sensory process, in the sense of involving the use of any of the ordinary avenues of sense, is absent or reduced to the minimum. These may otherwise be called origination or invention errors, since they occur always in the expression of excogitated thought. They are distinguished from those which occur as immediately and obviously due to a sensory process involving the use of some one or more of the senses. Just because ideo-motor lapses belong to the category of those mental processes which are centrally, rather than peripherally, initiated, they are the more baffling to investigation, because not admitting of experimental control. These might have been called ideational errors, except that they seldom enter consciousness before they

<sup>1</sup> Cf. Baldwin, *Mental Development in Child and Race*, p. 460.

are precipitated, the term 'ideational' rather implying a conscious process. The term 'ideo-motor' was first used by W. B. Carpenter for a muscular movement caused or prompted unconsciously by an idea. It is used here, since an error is usually unconsciously produced, the occasions facilitating the error, which comes into consciousness only after its precipitation, and then is not always recognized, often being entirely beneath the threshold.

Oral ideo-motor errors occur in impromptu speech, either in platform utterances or in conversation. Graphic examples are found in original composition, letter-writing, etc. Some examples will illustrate what is meant by these terms. First, four sensori-motor errors. In one the lapse occurred in the vocal reproduction of what was perceived through the sense of sight. 'Althey though' was read for 'although they.' In another the lapse occurred in the vocal reproduction of what was perceived through the sense of hearing. "Round the rugged rock the *rugged* rascal" was repeated after another person for "Round the rugged rock the *ragged* rascal ran." In two errors the lapses occurred in the manual reproduction (in writing) of what was perceived through the senses of sight and hearing, respectively. 'Dection' was written for 'detection,' and 'eather' for 'each other.' Compare with these, four ideo-motor errors, in which there is no discoverable sensory process (in the sense in which this term is used above), but simply a recombination of ideas or images, already existing in the mind, by the constructive use of the imagination. The first two are oral: the second two are graphic. "Have my tucket pinched" was spoken for "Have my ticket punched." "I fool so feelish" was said for "I feel so foolish." 'Can be asily' was written in a letter for 'Can be easily,' and 'will he' for 'will be his.' In Tables II., III. and IV. errors to which this distinction can be clearly applied have been grouped according as they involve (1) a conflict between two peripherally excited processes, (2) a conflict between a peripherally and a centrally excited process, and (3) a conflict between two centrally excited processes. Further reference to these Tables will be made below, in the discussion of the various types of coalescence.

TABLE II.

Coalescence Due to the Conflict of Two Peripherally  
Excited Ideas.<sup>1</sup>

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
as is carried	is carried .....	'i' visual, 'a' auditory.
away a f from its	away from its.....	'a' visual, 'f' auditory.
leeders	leaders.....	'a' visual, 'e' auditory.
violed a her	violated her part .....	'a' visual, 'a' auditory.
and exg bec	and declarative of.....	{ The <i>sound</i> 'f' coming just before 'g' (the series repeated was a-b-c-d-e-f-g) in the auditory series, changed 'c' to 'x.' The 'd' of 'and' took the place of the 'd' of 'declarative.' The 'b' of 'bec' came also from the auditory series.
on behalf in of othe	on behalf of the nation..	{ 'in' came from the auditory series (which was 'in an end pond'): the 'o' of 'othe' persists from 'of.'
responisnbility fon	responsibility for.....	{ Influence of the 'n's of the auditory series.
i the etabishment	the establishment.....	{ 'i' from the auditory series.
of good gonverment	of good government.....	Influence of the 'n's.
as in Cn his	as in Cuba his.....	{ Influence of the 'n's. Note that 'n' equals 'u' in handwriting.
with whnch	with which the.....	{ Influence of the 'n's. Note that 'n' equals 'u' in handwriting.
at 7 sfive oclock	at five o'clock.....	{ Same, only repeating 1-2-3-4-5-6-7-8: first wrote the figure 7, then started to spell it.
{ and for for lover thirty or veven fewer	and for over thirty or even fewer	{ ... { 'for l' from auditory series.
which f correspond	which correspond.....	'f' from auditory series.
There in sunversal	There universal.....	's' from auditory series.

<sup>1</sup>These are all graphic and experimental: the conflict was between a visual and an auditory stimulus (the subject was copying and at the same time repeating aloud the letters, etc., which appear under 'Remarks').



TABLE III.

Coalescence Due to the Conflict of a Centrally with a Peripherally Excited Idea.<sup>1</sup>

GRAPHIC. <sup>2</sup>		
<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
cover the corn wells	cover the corn well.....	{ Letter-writing by a man whose name is Wells, and who had just been writing his name which was before him on the page.
l	elision .....	
The Good Subject	The Good Shepherd.....	{ Attempting to compose while also trying to keep the drift of a lecture in which occurred the phrase 'the good subject.'
punishman	punishment.....	
power of constraint	power of contrary choice	{ 'man' occurred in the lecture just as the word-part 'ment' was about to be written.
congregational	congregation .....	
{ in the direction of transmission	in the direction of transmission } .....	{ Ditto—in which the phrase 'freedom from external constraint' occurred.
were circapble	were cirpable.....	{ Thinking of 'Congregational Church.'
mom	somites.....	
quari	quadrilateral .....	{ As the word was being copied, its derivation, 'trans-mittere' flashed into mind.
e	I .....	
srack	crack .....	{ In copying an oral error, the word 'capable' being in mind.
s	catfish.....	
woull	will.....	{ 'myosomita' in mind.
		{ 'parallelogram' in mind.
		{ 'eye' in mind.
		{ 'store' in mind.
		{ The words 'shell' and 'Spanish' were in the marginal consciousness.
		{ Experimental: 'would' in mind.

ORAL.<sup>3</sup>

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
That's a perison	That's a period.....	{ In speaking: the word 'comparison' was on the page before the eyes.

<sup>1</sup> None of these are experimental errors except those so indicated under Remarks.

<sup>2</sup> The errors are in copying, unless otherwise indicated.

<sup>3</sup> In reading aloud unless otherwise indicated.

democrat	demagogue.....	{ Preacher reading from manuscript about election time.
Cis	German (His) .....	{ The idea 'Capital' in mind (in reading proof).
Looking for 'Mushroom'	Looking for 'Muscle'...	{ In speaking: 'mushrooms' on the page before the eyes.
possessing	touching .....	{ In speaking: 'possessed' before the eyes on the page.
One two	When two .....	{ Another person was just saying 'One, two, three, etc.'
f-th	v-th .....	{ Reading proof; had just pronounced the word 'fifth.'
Ganglican	Gallican .....	{ 'Anglican' in mind: in reading aloud.

TABLE IV.

## Coalescence Due to the Conflict of Two Centrally Excited Ideas.

GRAPHIC. <sup>1</sup>		
<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
ideati	ideo-motor.....	'ideational' in mind.
idea	ideo-motor.....	'ideational' in mind.
ideomotor	ideo-motor.....	'ideational' in mind.
whether it bet	whether it be.....	{ 'that' in mind: the intention was to change 'it' to 'that.'
8 ft.-8 in.	5 ft.-8 in. ....	{ Due to hesitation whether to use metric or English measure.
journey	journal.....	{ Thinking in general about a certain Journal.
Guadaquerque	Guadaloupe .....	'Albuquerque' in mind.
like a see	like a sea.....	{ 'the' in mind: contemplated changing 'a' to 'the.'
Lima to Cylinder	Lima to Dayton.....	{ Clerk making out pass for employee while engrossed in the shipping of cylinders.
practices	practiced .....	{ The other form of the word ('practice') came to mind.
{ iron alum 4	iron alum formalin } 8 days	{ Confusion of the figure '4' with the word-part 'for': suffering intense pain at the time.

<sup>1</sup> The errors all occurred in 'composition' (as contrasted with copying), 'letter-writing,' etc

sid	see books.....	'vide' in mind.
Ill	Elisions .....	'illustrations' in mind.
we are yo	we are so.....	'you' in mind.
Knowing, Thinking	Knowing, Feeling .....	{ Hesitation in choice between 'Knowing' and 'Thinking.'
William James	William Jackson.....	{ The well-known psychologist's name in mind.
complexicty	complexity .....	'complication' in mind.

ORAL.<sup>1</sup>

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
winter March	winter month.....	'March' in mind.
liquals	liquids .....	'linguals' in mind.
ours would bump	ours would break.....	'our pump' in mind.
North-Weast wind	North-West wind .....	'East wind' in mind.
{ in the back part of the brain	in the back part of the book	{ 'brain' in mind.
barg	larger margin.....	'bigger' in mind.
turn round	turn up.....	'come round' in mind.
polycotyls	polypetalous .....	'dicotyls' in mind.
think	thing .....	'mark' in mind.
ruver-shoes	over-shoes .....	'rubbers' in mind.
degreements	differences.....	'disagreements' in mind.
as soon as I have written	as soon as I have eaten..	{ The idea of writing immediately in mind.
The nights are begetting	{ The nights are becoming shorter	{ 'getting' in mind.
dreeze	breeze .....	'draft' in mind.
It bost	It cost.....	'butter' in mind.
headwards	Edwards.....	'headship' in mind.
perple	persons .....	'people' in mind.
Well, I'll spaddle you	Well, I'll paddle you .....	'spank' in mind.
trying to apploy	trying to employ.....	'apply' in mind.
six cents a quarter	six cents a quart. ....	{ 'five for a quarter' in mind.
foot-sprints	foot-steps.....	'foot-prints' in mind.
symblem	symbol .....	'emblem' in mind.
emricron	omricron .....	'epsilon' in mind.
Look at that little puddle!	Look at that little poodle!..	'pug dog' in mind.
Jenarii	Genarii .....	{ 'J' and 'G' (soft) confused; note the influence of the auditory imagery.
forwarth	forward .....	'forth' in mind.

It is probable that many of the errors which have been classified among those of impromptu speaking and conversation (oral) and original composition (graphic) are really to some

<sup>1</sup> The errors all occurred in 'conversation,' 'lecture' or 'address' (without notes), etc.



extent due to unconscious reproduction of obscured sensory impressions.<sup>1</sup> It is a question, in other words, whether impromptu speaking and spontaneous writing are not essentially such processes as occur in reading or copying from a book before the eyes, except that the copy is mental (cerebro-cerebral) instead of what we call physical (retino-cerebral). In reading, it is very plain that we do not read or even see all the letters or even all the syllables of a word, in order to recognize what it is and properly to reproduce it: a hint, a part of a letter, a loop above or below the line, is sufficient. This is probably the psychological truth in the statement that such persons as Macaulay could read by sentences or paragraphs rather than by mere words or letters. An educated person gets only a schematic perception of the units in a line or sentence or paragraph, the fulness of detail depending on his familiarity with the subject. For example, in the sentence "We swept the swan-down out of the room" we may think on first inspection that we take in most of the letters in reading the sentence; but in the sentence 'A huge complex of contradictories and self-destructive incompatibilities' it is evident to any one who knows the meanings of the words that only prominent or significant letters enter consciously into perception in reading it. These key letters or syllables are taken to stand for the whole. Now in the speaking or writing of what has been excogitated, as distinguished from reading or copying, it is probable that in a similar way the mental images of these and similar words do not include each and every letter or even all the syllables; they are rather outline skeletons. The less significant details are filled in automatically. Only when they are relatively unfamiliar or striking are they filled in consciously. Just as we find errors in reading and copying (the process of reproduction from the external copy), so there are lapses in the process of mental reproduction, from the internal copy, so to speak. The following error illustrates the impossibility of differentiating the factors of external and internal speech. A person in reading and translating from the German said 'through whuse' for 'through whose universal.' The German word ('allgemeine') was nothing like the English,

<sup>1</sup> Memory may be regarded as simply a prolonged after-sensation.

so that the error must have been made like any other ideo-motor error, the word *being expressed* coalescing with a word *about to be expressed*, the peripherally excited stimulus in this case simply serving as the condition for 'setting-off' this series of words (*i. e.*, setting up a different *central* process). From the outside, to any one who was not aware that the speaker was really translating, the error could not have been distinguished from any sensori-motor error peripherally produced. Such an example shows that, psychologically at least, the distinction between the peripherally and the centrally excited errors can not fruitfully be made the ultimate basis of interpretation and of classification. It is, furthermore, often difficult to determine whether to classify an error as sensori-motor or as ideo-motor, because of the memory factor. When an error is made in repeating, *e. g.*, a line of poetry or a passage of prose literature, it may be due either to a falsely interpreted sensory impression (either visual or auditory) at the time of committing the passage to memory, or to an imperfect motor coördination at the time of making the error. For example, in the following case shall we classify the error as sensori-motor (the passage was doubtless learned through either the organs of vision or of audition) or as ideo-motor? A preacher in repeating the familiar verse of Scripture said 'without remission' for "without shedding of blood there is no remission of sins." Here the error may obviously have been due either to an imperfect memorizing of the passage in the first place, or, assuming that these cortical impressions all existed in proper relation, it may have been due to some defect in the central coördination or the peripheral innervation for expression.

*Principle of Classification.*—The problem of the classification of the data of lapses becomes thus a difficult one; first, because of the number and variety of the data to be handled, and second, because of the complexity of the conditions which produce them. A classification based upon the composition of the mental content should be supplemented by a classification based upon the predisposition or functional conditions of the lapse. But before this is possible a greater knowledge of the cerebral conditions must be attained, and meanwhile some

flexible arrangement of the data already in hand must be sought. The classification here employed is a provisional one only, based partly upon the nature of the mental elements entering into combination, partly upon the nature of the attentional process involved, and partly upon the avenue of expression by which the errors become known. Upon this analysis, which is psychological, is grafted another, simply for convenience, which is purely mechanical, grouping the lapses accordingly as they involve sentences, clauses, phrases, words or merely letters. This classification may appear somewhat arbitrary in places, but this must be endured for the present as a necessary limitation due to the embryogeny of the subject. Especially in the more minute subdivisions will the analysis seem somewhat formal, *i. e.*, based upon verbal rather than upon psychological principles; but it must be remembered that convenience of reference is also in such a case a prerequisite, and a somewhat arbitrary classification which is elastic is preferable to a misleading, because premature, attempt to proceed upon purely psychological principles.

The possibility of such minute classification of material depends, of course, upon the certain ascertainment of the details in each instance and the rigorous exclusion of all doubtful cases. This has been done, many most interesting errors not being included in the tabulation because their details were so elusive as to escape accurate registration. Mistakes which are due to any known organic incapacity (chronic aphasia, stammering,<sup>1</sup> 'tied tongue,' etc.), or to any peculiarity of stylus, typographical errors, etc., are not recorded. This study is restricted to the examination of verifiable data, not because there are not multitudes of non-authentic cases at hand which would be very instructive (probably many of them of actual occurrence); but the material which can be verified is so rich and plentiful that it would be quite unnecessary to expose one's self to the charge of an unscientific collation of facts. Suggestions for the inter-

<sup>1</sup>Stammering occurs in writing also, though it is comparatively rare. R. Dodge cites his own experience in his 'Die Motorischen Wortvorstellungen.' He regards this phenomenon as due to the conflict of the articulo-kinæsthetic with the grapho-kinæsthetic imagery. (See further below.)



pretation of many such non-authentic errors for the psychology of the ludicrous are made in a subsequent section.

Many errors belong at once to several categories, illustrating, for example, in a single instance what are called in the tabulation persistence, anticipation, elipsis and transposition. To cite a single case, which shows these four characteristics upon analysis, 'asll' was written for 'well as.' The forms of error involved in this lapse are (1) the anticipation of *as*, (2) the elision of *we*, (3) the confusion resulting in the persistent transposition of *ll*, and (4) the hybridization or uniting of parts of two words into (in this case) an unmeaning compound. There are many errors which can be no better described than as jumbings of letters or words—apparently lawless reconstructions or recombinations of letters or words, often suggested by some insistent idea or by some anticipated form, but never definitely, and usually without any discoverable occasion.<sup>1</sup> In most cases, in the tabulation, at least three subdivisions of the classification are represented by the center-head title. The columns which follow contain, respectively, the error as it occurred, the correct form (*i. e.*, the intended form), and explanatory or commentary remarks (see tabulations). In what here follows two main principles of division will be considered—(1) errors as oral or graphic, (2) errors as verbal and literal. The other principles of division will be developed in the subsequent treatment.<sup>2</sup>

It has seemed useful to classify lapses according to their character, whether oral or graphic—two different cortical areas and peripheral musculatures being concerned. This has the advantage of being a physiological as well as a psychological mark of distinction, and has thus a superiority over the possible, but more arbitrary classification upon the basis of the individual, occasion or subject, language or dialect, or merely verbal characteristics, which if used at all should be reserved for a subordinate category. By visual-vocal and auditory-vocal sensori-

<sup>1</sup> Such have been for the most part reserved for further consideration in the light of the evolution of language (the genetic study of lapses).

<sup>2</sup> The author wishes here to express his gratitude to those who have kindly furnished him with much of the data used, and to add that further material which any readers of this article may desire to put into his hands will be welcomed. Address H. Heath Bawden, Granville, Licking Co., Ohio.

motor errors are meant errors which, occurring in the sensory processes of vision or hearing, are reproduced in a motor process by the organs of speech. In oral errors a digraph made up of vowels is treated the same as if it were a simple vowel; the same is true also of consonantal digraphs. Occasionally oral errors occur, especially when there is a modification of the vowel sound or sounds, which cannot be recorded without explanation. In all such cases the necessary comments are made in the third column under 'Remarks.' Some of the errors tabulated were made in what has been called mental pronunciation or in mental writing (*e. g.*, in dreams).<sup>1</sup> For example, one person in reading to herself ('mentally'), from the newspaper, read 'five thousand walls of roll-paper' for 'five thousand rolls of wall-paper'; another read 'lings' for 'lungs and wings'; another, 'dame, leaf and blind' for 'deaf, lame and blind'; still another read 'to roll untangled records' for 'to unroll tangled records.' Other cases will be found in Table VIII., below. But these errors differ in no significant way from those made in ordinary vocal speech or externally visible writing. Obviously this relative identity of character is just what one would expect from the fact that the same musculatures and the same central processes are employed in the two cases.

By visual-manual and auditory-manual errors are meant errors which, occurring in the sensory processes of vision and hearing, are reproduced in a motor process by the organs used in writing (the arm, hand, fingers, etc.). All the graphic errors here recorded were made in handwriting (long-hand) except such as were made on the key-board of a typewriter, examples of which are grouped together in Table V. Many graphic errors were made in passing from the end of one line to the beginning of the next. Though numerous, but few of these are here recorded. Most such cases of simple reiteration seem to be due to a sudden backward thought to recover the broken thread of the sentence and a failure to note the precise point of previous termination: it is likely to take place in the unimportant words. Examples are 'haste to to' for 'haste to,' 'courses to to pursue' for 'courses to pursue,' 'both both tragedy' for

<sup>1</sup> Taken together these constitute Ballet's 'le langage intérieur.'

'both tragedy,' 'time in time in' for 'time in,' etc. The faulty numbering of pages in which the same number is repeated, belongs here. Cases are frequent also in which the wrong letter is crossed or dotted, as when in writing 'not be too much,' the *b* of 'be' by anticipation was crossed instead of the *t* of 'too.' Everyone is prone in rapid writing to dot the wrong letter. Such cases have not been entered into the tabulation simply because it would have swelled the latter beyond desirable dimensions.

The different forms of amnesic agraphia, in which through a partial weakening or total loss of the memory for graphic images, voluntary writing becomes very difficult or impossible, are illustrated in their incipient forms even in ordinary life in such very frequent cases of the forgetting of graphic symbols of different sorts. Agraphia in its beginnings is often not recognized as abnormal. In so far, however, as the forgetting of the graphic symbols attains a certain degree and frequency, it is attributed to some serious cerebral lesion, which our still crude clinical manipulation attempts to remedy, and the disease is then dubbed with a scientific name, agraphia. The fact is that the most classic and pedantic of us are both aphasic and agraphic at times, and sometimes all the more because fastidiously accurate, and life is little more than an unsuccessful attempt to keep from making either serious or ridiculous mistakes. Amnesic agraphia, says Preyer, can be limited to but a single letter. This is shown by the well-known suggestion experiment,<sup>1</sup> in which the hypnotized individual is persuaded no longer to write this or that letter, such as *e*, and in which after awaking he notices that he has written, for example, 'dn lftn Sptmbr' for 'den elften September.' A case is on record, quoted by Bastian,<sup>2</sup> of an aphasic patient who "perfectly recollected the initial letter of every substantive or proper name for which he had occasion in conversation, though he could not recall to his memory the word itself." "He never was at a loss for the initial of the word he wished to employ." There are cases also of aphasic patients who always exchange certain letters of words in pronounc-

<sup>1</sup> W. Preyer, *Zur Psychologie des Schreibens*, 1895, pp. 211-212.

<sup>2</sup> *Brain as an Organ of Mind*, p. 622.



ing them; thus, endeavoring to say the word 'flute,' one said 'tufle,' 'puc' for 'cup,' 'gum' for 'mug,' etc. Again, there may be an almost invariable substitution of certain letters for others, such as 'z' for 'f' in every word which should contain the latter letter.<sup>1</sup>

TABLE V.

Errors in Type-writing.<sup>2</sup>

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
dise	side .....	{ (s) (d) [Relative position of the keys involved].
willst di du	willst du dich.....	(u) (i).
consciou states	conscious states.....	An example of Elision.
woman s	woman who smells.....	{ (w) (s).
aspect a	aspect over .....	Not near.
from o	{ from some outside } suggestion	.....Not near.
ans	and sentences.....	(s) (d).
my chair q	my chair squeaks.....	{ (q) (s).
of selection	of selecting hybrids.....	Not near.
Professor's	Professor Baldwin's.....	Anticipation.
ot	of that .....	{ (t) (f).
But is	But if this is true.....	(s) ( ) (f).
sen	second .....	Not near.
habil	habitual.....	Not near.
c	occur.....	Not near.
c	economy .....	Not near.
or nal	or analogous.....	Not near.
ul	unlike elements.....	Not near.
ff	feeling .....	Reduplication.
{ cognition stand in in- verse ration	cognition stand in in- verse ratio	} ...Persistence.
not be	not by reference to.....	Not near.
morot	motor .....	(r) (t).
detail	details.....	Not near.
{ pronouncing ponder- ous syllables	pronouncing ponder- ous polysyllables	} ...Elipsis.
fidd	difficult.....	(d) (f)
Philadelphis	Philadelphia .....	(a) (s)

<sup>1</sup> Cf. Bastian, *Brain as an Organ of Mind*, p. 639.

<sup>2</sup> A large proportion of such errors are due to inaccuracy in striking the keys, *e. g.*, when two keys that have to be struck in close succession are near one another (see under Remarks). Otherwise the errors differ in no important feature from other graphic errors. These errors were all made on the key-board of a Smith-Premier and are samples merely.

strans so strengthened	strand so strengthened.....(s) (d)
the c	the important component..Not near.
stt	setting .....(e) ( ) (t)
compen	components .....Not near.
a a a	a b c ..... { Had in mind the idea of underscoring them all alike.
sttod	stood.....Not near.
cu	accustom.....Not near.
neceaaas	necessarily.....(a) (s)
come to come	come to some..... { (s) (c)
himlse f	himself alive.....Not near.

Errors are further classified according as they are verbal or literal. A verbal error is one in which an entire word is concerned. For example, a person said 'Play down' for "Sit down and play me a tune." All parts of words which are also used separately are included under verbal errors. Of course the greater number of verbal errors are oral, and the greater number of these occur in the case of monosyllabic words. As will be seen later, the length of the word is not the only or chief determining condition of the lapse. Rather it is the relation of the word to its 'setting' or context.<sup>1</sup> Literal errors are such as involve alterations or substitutions of letters or unmeaning word-parts only. For example, a person instead of saying 'identity and difference,' said 'idefference.' Another person spoke of the 'ox and the ax' instead of the 'ox and the ass.' Graphic errors are very largely literal; *e. g.*, 'very soof' for 'very soon after,' 'rex' for 'reflex,' 'wright' for 'right and wrong.' A consonant, in the case of oral errors, is regarded as a letter of the alphabet which can not be produced orally except with a vowel. It is but a mouth-form, a muscular attitude, giving 'shape,' 'size' or 'color' to a vowel-sound, thus using to some extent a different musculature from that used in the production of the vowel-sounds, but by themselves having no vocal value. Consonants are not tones but simply breath obstructions. A consonant, in the case of the graphic errors, is a letter of the alphabet graphically produced, which represents such a mouth-form or muscular attitude. The equivalent of a consonant (*e. g.*, kn, wn, gr, pl, st, sm, sn, etc., etc.) is treated here also as a consonant. A vowel, in

<sup>1</sup> Cf. also, on this point, Pillsbury (as quoted below), p. 342.

the case of oral errors, is a sound or tone produced by the vibration of the vocal cords through the aid of the musculature of the larynx, etc. The duplication of single vowels and the combination of vowels in diphthongs are also classified here. A vowel, in the case of graphic errors, is a letter of the alphabet, graphically produced, which represents such a sound. Obviously the above conception of the real nature of a consonant puts the oral errors, in respect of the distinction between the language units, on a different basis from that of the graphic errors, where a consonant differs in no important respect, psychologically, from a vowel.

Here arises the whole problem of the interpretation of lapses in relation to the racial evolution and individual development of language, and in connection with this the problem of the psychogenesis of meaning. This is a problem in genetic psychology which, being too large for consideration here, the writer hopes to develop later as a separate 'Study.' The general method employed promises to be that which Charcot first made popular, the contrasting and comparing of the acquisition of language with the phenomena of its dissociation in disease. This suggests that the fruitful study of the psychology of language (and through this the psychology of meaning) will begin with the pathogenesis of aphasia. The genetic principle maintains that the language processes will 'fall down' or disintegrate in the reverse order in which they were 'built up' or developed. This general principle has been applied with success to the study of memory, where it is found that substantives are forgotten more quickly than verbs and adjectives, and proper names more quickly than common, and concrete terms more quickly than abstract. So, also, in the cases of lapses, other things being equal, those combinations of letters which are least firmly grounded in the organic memory will be the ones first to be involved in error. From such a standpoint it should be possible to work out in serial order the language components of ordinary speech and writing, and by a comparison with the results of philological study to get some light on the racial and individual growth of language. Certainly it is significant that it is the consonantal digraphs which the child learns the last,



that are the most often involved in the phenomena of lapses, *e. g.*, sp, fl, bl, br, st, sm, qu, etc., etc.; that it is the consonants related in origin (so regarded, at least, by modern philologists) that tend to coalesce, *e. g.*, dentals with dentals, labials with labials, etc., and that it is the variable and derivative vocables that chiefly are involved in the errors. In some cases there was a strong tendency to spell words phonetically, or to relapse to the orthographic instincts of childhood, even in the case of words the correct spelling of which was perfectly well known to the subject. Thus 'survival' was spelled 'servival,' and 'receptacle' was spelled 'resceptacle,' 'urgent' was spelled 'ergent,' etc. A determination of the letters which are visually the most easily confused with one another has been worked out by Professor Sanford, and his generalization is that "reproduction is very nearly a function of the ease with which we distinguish the various letters." Pillsbury has shown the relation of defective letters or mutilated letters (incomplete, blurred or absent) to the tendency to 'lapse,' and finds that such defects are most influential when they occur in the earlier part of the word, syllable or sentence.<sup>1</sup> This is a rich field, and much is expected from further investigation.

### III. THE ARTIFICIAL PRODUCTION AND THE TABULATION OF ERRORS.

*Method of Producing Errors.*—The attempt was made to supplement the class of data collated from ordinary experience by some of an experimental nature. The experiments were made with ordinary language forms and modes. Experimentation with numbers and with nonsense syllables affords similar problems; but they do not present so common, and therefore characteristic, phenomena as are found in the types of experience used below. Nonsense syllables were used, but did not yield results in any important respect different from those obtained by the other methods.

The specific *problems* took the following form:

(1) In general, to multiply data from which generalizations might be made. This is in great part successful, but the kinds

<sup>1</sup>*Am. Jour. Psychol.*, VIII., p. 347. Cf. also what he says on the influence of the 'high' and 'low' letters.

of errors are in certain ways limited. For example, there are produced almost no errors of the sort catalogued below as exchanges (cf. Tables XXIX. and XXX.), while errors classified below as Repetitions are very abundant (see Table below).

(2) To get a general statement of the normal scope of anticipatory and so-called retrospective attention in reading and speaking, copying and writing (composing), in different classes of subjects and in different degrees of familiarity with the subject-matter by an examination of the lapses produced under varying conditions. To do this it was necessary to produce errors under differing conditions, and then from an examination of those errors to get a general statement of the psychological unit in the terms of lapses, that is, to determine the unit of which the sentence or word is ordinarily constructed in speaking and writing (whether it is a letter, a part of a letter, a word, word-part, or phrase), and to determine whether such psychological unit follows the meaning of the sentence and how it is dependent on the purely formal or grammatical conditions.<sup>1</sup>

(3) To study the conditions for the consentient memory of spoken and written words, that is, the relations of the different factors, kinæsthetic, visual and auditory, in the concrete verbal consciousness; this to be done by an analysis of what is involved in different types of lapses. The chief reliance was necessarily upon introspective evidence. This intended to be, as it only can be, corroboratory of already well-recognized principles.

(4) Finally, to analyze into its functional elements the ordinary complex stream of speech and writing consciousness in order to determine the laws of verbal assimilation in the terms of lapses; in particular, to study the conditions under which are produced the various types of coalescence classified below as persistence, anticipation, ellipsis, transposition, substitution, exchange, etc. Here, again, the attempt is made to interpret these new data of lapses in terms of the already existing and well-recognized categories of association, assimilation and coalescence.

The following *methods* were employed:

<sup>1</sup> The methods employed and the results secured are taken up more fully below in the discussion of the psychological unit.

(1) For the visual-vocal method, a screen with an opening or slot capable of instantaneous closure. By visual-vocal experiments are meant those in which the stimulation is visual and the expression vocal. The subject reads either to himself or, better, aloud (thus introducing the operator's hearing as a check). In the latter case the subject need not know the object of the experiment. The subject is seated with eyes fixed on a legend so arranged as to be instantaneously covered by a drop shutter. He is asked to read aloud at a normal rate, the shutter being dropped at various points in the sentence. Artfully arranged words, sentences, etc., are used to vary the experiment. The subject is asked immediately upon the dropping of the shutter to continue the sentence or words as far as possible. Unexpected juxtapositions of thought are used to see how far the completion is due to mere association. By various combinations of letters and words it is discovered how far peculiar visual impressions serve to catch the anticipatory attention. This experiment is made with separately printed legends or with the last three or four lines on the page or in the paragraph, and the shutter is dropped just as the reader pronounces the second word in the last line. In this way the disturbing and otherwise uncontrollable influence of the next succeeding lines is excluded.<sup>1</sup> Visual-vocal experiments may be with (*a*) familiar or (*b*) unfamiliar subject-matter. The 'Psalms,' for example, were used for the familiar and Browning's 'The Ring and the Book' for the unfamiliar. Again, they may be made with (relatively) (*a*) undivided or (*b*) divided attention, the diversion in the latter case being through either the same sense or through a different sense. For example, experiments may be varied by introducing some other interest or diversion for the subject, such as requiring the subject to continue reading or writing in the midst of a diverting noise or while listening to the reading of a fascinating story, etc.

(2) For the auditory-vocal method cacophonous juxtapositions and alliterative combinations of letters or words were dictated and required to be repeated from memory. Greater difficulties present themselves here than in the visual-vocal method,

<sup>1</sup> For the disturbing effects of contextual forms in reading see Table VI.



because of the interference of the element of time and the distinctive memory factor. It is doubtful in all forms of the auditory-vocal experiment where no great time has elapsed between the hearing of the dictate and its attempted repetition, how far we are dealing with memory proper, how far we are really controlling the experiment. In addition to the above, the following experiment was made: The subject repeated a familiar snatch of prose or rhyme while silently reading an interesting tale, thus deliberately introducing the memory factor.

(3) Two methods may be employed in experiment with the auditory-manual type of errors. Either a list of familiar or unfamiliar and meaning or unmeaning letters or words are dictated in the ordinary manner, gradually increasing the speed, or some disturbing factor is introduced from without. The latter form of the experiment was varied in several ways: (*a*) dictation took the ordinary form except that interjacularatory remarks were suddenly introduced, irrelevant questions asked, etc.; (*b*) the subject was compelled to write with his eyes closed; (*c*) the subject was compelled to write with his eyes closed and his ears stopped up so that he could not hear the sound of his own writing, but still could hear the dictate.

(4) In the visual-manual method the subject wrote (copied) under the following conditions: (*a*) in the ordinary manner, with increasing rapidity, (*b*) while repeating some familiar series of letters, numbers, or words, or some set phrase, (*c*) while the operator read to him, (*d*) with eyes closed and with ears stopped with cotton, so as to exclude both eye and ear control, (*e*) while dictating alternately to another amanuensis.

TABLE VI.

ERRORS DUE TO LETTERS, WORD-PARTS, OR WORDS IN ADJACENT LINES.<sup>1</sup>

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
fifth door	fifth floor at the door	'door' next line below
fench	hedge	'fence' " " "
Mr. Geoffrey's incident	Mr. Geoffrey's injury	'accident' " "
I conclceech	I conclude	'speech' " "
Over the brummer sea	Over the summer sea	'the briny deep' "
Giovanni smoled	Giovanni smiled	'over' next line "
flacked	flocked	'valiant' " "

<sup>1</sup> Except where otherwise indicated, these were all in reading aloud.

whom I	whom to have seen	The following three lines began with 'I.'
we might not infer	we might not interfere	'infer' just below in next line.
The sun rose in the same length	The sun rose in the same place	'length' next line below.
sheep	speech	'sheep' " "
Instantly the thrightened man	Instantly the frightened man siezed the threaten- ing antlers	The word 'threaten- ing' just below in the next line.
this is in prophecy	this is in parenthesis	A graphic error in copying: the word 'prophecy' was in the next line below.
These al	These illusions	Graphic: 'assimila- tive' next line below

## TABLE VII.

ERRORS IN REPEATING CACOPHONUS AND ALLITERATIVE COMBINATIONS.<sup>1</sup>*Cacophonous Juxtapositions*

Big bug's black blood	Bax kiz fob dap lom bax	She sells sea shells
. . blug's	. . fiz	. . . . she
Bigs bugs	. . . . fab	sea
Big bigs	. . . . . lap	. . . . . sells
. . . . . bug	. . . . . lap dom	. . shell sea shell
. . back	. . . . . lop	. . shells
. . bug bug bug	. . . . foz dab	sea sells
. . blug blug	. . . . fab dop	sea sells sea sells
. . blood's	. . . . . lam box	. . shells she shells
. . . . . bood	. . . . . box	
Grief that gazes at a grave	Round the rugged rock the ragged rascal ran	
. . . . . grazes	. . . . . rags	
. . . . . grazes at a gave	. . . . . rascal	
. . . . . grazes at a grave	. . . . . rugge rocks	
gief	. . . . . rugged	
. . . . . gave	. . . . . ragged rack	
gries		
gies		

*Alliterative Combinations.*

Pyle's Pearline Possesses Peculiar Purifying Powers	Propagable
. . . . . Peculiar	Procagable
. . . . . Pepurifying	Procabable
. . . . . Pessesses	Propabable
. . . Possesses	Propable
Pyers	Propagagable
Compound complex comminuted fracture of the olecranon process	
. . . . . olequenon	
. . . . . communuted	
. . . . . plocess	
. . . . . clomplex	

<sup>1</sup> These are simply samples selected from a large list of similar errors.

The following *results* were obtained:<sup>1</sup>

(1) In the use of the visual-vocal method, the results have been grouped as follows: (a) The subject completing the line, his vision being cut off as he is speaking the second word. Wundt's 'Outlines of Psychology,' and Hodgson's 'Metaphysic of Experience' were used for the experiment, also cacophonous and alliterative combinations. In both cases there was a consciousness of selecting letters and word-parts and piecing them together, forming hybrids, but 'it seemed to do itself.' In the case of cacophonous alliterative combinations, a general break-down is noticeable as compared with ordinary English prose. The little (short) words are left out. The whole thing is very fragmentary. The combinations selected by the subject make less sense. A big or an unfamiliar word 'spreads itself over everything else.' The portion clearly seen and retained by the subject varies from a single word or part of a word, in the case of the less familiar or the unmeaning, to nearly the whole line, when the passage is more familiar or makes more sense. (b) The subject glancing (for about one second) at the line and reporting what is seen clearly. The same material was used as in the experiment just preceding this. The results are sufficiently apparent in the tabulations. (c) The subject filling in blank spaces in ordinary English text, where words have been erased. Commented on below. (d) The subject reading an interesting story, the operator noting the errors due to emotional changes. Commented on below.

(2) In the auditory-vocal method errors were noted in the repetition by the subject of cacophonous and alliterative combinations.<sup>2</sup>

(3) In the auditory-manual method five types of experimentation were used. (a) Dictation to the subject with increasing rapidity, his eyes open. Some subjects constantly multiply strokes (sometimes several times in succession), letters, and even words; but get all the dictate, and learn with practice not to multiply letters so much (the subject in one instance

<sup>1</sup> General statements only are here made: for detailed results, see tabulations and treatment below.

<sup>2</sup> See Table VII., for results in a brief form.



looked over the material with the operator in each case immediately after the completion of the experiment). Other subjects drop letters and whole passages, that is, do not get all the dictate. The influence of both persistent and anticipated forms is very marked, of course, in all alliterative dictate. (*b*) Dictation with subject's eyes shut and ears stopped so that he could hear the dictate plainly, but could not hear the sound of his own pencil. This was about the same as the following experiment as to results. (*c*) Dictation with subject's eyes shut, subject repeating phrase. Here persistence of entire words is frequent. The results found in (*a*) are exaggerated. (*d*) Dictation of English words backwards, letter by letter, subject's eyes shut. Errors involved letters only. T's not crossed and i's not dotted. (*e*) Operator and subject dictating to each other. Ellipsis and persistence of entire words and phrases. Great disturbance.

(4) In the visual-manual method also five types of experimentation were used. (*a*) Subject copying (or writing from memory) a familiar word or phrase as rapidly as possible. Tendency to repetition in some subjects; tendency to ellipsis in others. Tendency to function but once for all alliterative letters, or else rhythm takes it up and we have reduplication of the alliterated letter. (*b*) Subject copying, with eyes shielded from his own manuscript, and repeating a phrase. Fewer mistakes in copying foreign (unfamiliar) language and unmeaning subject-matter; but the subject went forward much more slowly and wrote with greater effort, fatiguing sooner. Repetition of letters and words from copy and substitutions and insertions from the repeated phrase (echolalia) were noticeable in certain subjects, while ellipsis of letters and words from copy and substitutions and insertions from repeated phrase were noticeable in other subjects. Coalescences were numerous and striking. (*c*) Subject copying while operator read to him. Same as preceding. (*d*) Subject copying, eyes closed and ears stopped up. Some subjects much confused. Others not. (*e*) Subject copying while dictating alternately to another amanuensis. Very disturbing.

*Tabulation of Errors.*—Typical errors have been thrown

together into separate tables according as they illustrate different principles as discussed. These tables consist of the following :

I. List of Cacophonous Juxtapositions in the Pronunciation or Writing of which Errors were Made.

II. Coalescence due to the Conflict of Two Peripherally Excited Ideas.

III. Coalescence due to the Conflict of a Centrally, with a Peripherally Excited Idea.

IV. Coalescence due to the Conflict of Two Centrally Excited Ideas.

V. Errors in Type-writing.

VI. Errors due to Letters, Word-parts, or Words in Adjacent Lines.

VII. Errors in Repeating Cacophonous and Alliterative Combinations.

VIII. List of Errors Occurring in 'Internal Speech,' or 'Mental Pronunciation.'

IX. Errors in Writing due to the Influence of Auditory Imagery.

X. List of Errors showing Tendency to form Familiar Words, or Word-parts.

XI. List of Oral Errors due to Similarity in Sound of Adjacent Letters, Word-parts, or Words.

XII. List of Graphic Errors due to Similarity in Appearance in Adjacent Letters, Word-parts, or Words.

XIII. Examples of Repetition in Experimental<sup>1</sup> Graphic Errors.

XIV. Examples of Errors due at once to Persistence and to Anticipation.

XV. Ellipsis due to the Sense of having written the Letter, Word-part, or Word.

XVI. Ellipsis due to Previous Pronunciation of the same Letter, Word-part, or Word.

XVII. Graphic Ellipsis due to the Anticipation of Letters, Word-parts, or Words.

<sup>1</sup> Experimental errors are used in the Tables only when in some way adding to the significance or variety of the data collated from ordinary experience. A larger use of the experimental material will be made in the genetic study alluded to above.

XVIII. Oral Ellipsis Due to Anticipation of Letters, Word-parts, or Words.

XIX. Examples of Graphic Persistent Transposition.

XX. Examples of Oral Persistent Transposition.

XXI. Examples of Graphic Anticipatory Transposition.

XXII. Examples of Oral Anticipatory Transposition.

XXIII. Graphic Persistent Substitutions.

XXIV. Oral Persistent Substitutions.

XXV. Graphic Anticipatory Substitutions.

XXVI. Oral Anticipatory Substitutions.

XXVII. Graphic Examples of Coalescence which involves the Modification of a Vowel or Consonant.

XXVIII. Oral Examples of Coalescence which involves the Modification of a Vowel or Consonant.

XXIX. Examples of Graphic Exchange.

XXX. Examples of Oral Exchange.

XXXI. List of Ludicrous Lapses.

#### IV. LAPSES AS A STUDY IN ASSOCIATION.

*General Nature of Assimilation.*—The most prominent feature of the lapse is the aspect which it presents as an instance of what is rather loosely called mental association or association of ideas. Lapses we have seen to be products of involuntary attention or subliminal association, what G. F. Stout calls *anoetic* or *subconscious* mental life. They are, of course, influenced indirectly by voluntary attention and by habit; but they are products essentially of the associative (relatively passive) rather than of the apperceptive process (which relatively is active). The process involved is best described by the term *assimilation*, as used by the English analytic psychologists.<sup>1</sup> Assimilation is defined by these writers as that *anoetic* mental process in which presentations (or percepts) and images (or ideas) are fused, or coalesce. It is thus the handmaid to association, and analogous to attention and apperception in *noetic* consciousness. We have in the realm of unexpressed thought the coalition of part of a new idea with an old one by substitution or modification. A new element is assimilated to the old

<sup>1</sup> Cf. G. F. Stout, *Analytic Psychology*, II., p. 118 f.



content by exciting the motor associations of that content. This process Herbart called apperception, but it is now more commonly called assimilation, at least in so far as it takes place in the so-called subconscious type of association. It is indifferent for this discussion whether assimilation ultimately be interpreted in terms of association as used by the English psychologists, or in terms of apperception as developed, for example, by Wundt under the influence of German idealism. The category of association is here employed simply because phenomena analogous to lapses have hitherto been chiefly studied under this category. In the present use of the term it may be said, to employ a figure first used by Ward, that assimilation is related to association in the narrower use of the term, somewhat as, politically, an amalgamation or union differs from an association or confederation—that is, it is specific and more intimate. This process of assimilation may or may not, in the case of lapses, find expression in audible speech or in visible character. The rule is that it does, but, as has already been seen in the case of mental pronunciation, this is not an essential feature.

Lapses have been roughly classified into two main groups, according as they are sensorimotor, *i. e.*, peripherally excited, and ideomotor, *i. e.*, centrally excited. But since, relevant to the present investigation, reproduction or memory may be regarded as simply a relatively permanent or prolonged after-sensation, we may in discussing these errors as phenomena of mental association, treat them as in this respect all upon the same basis. That is, the centrally excited or ideomotor errors are no more associative material than are the peripherally excited or sensorimotor. The assimilative process takes place in both cases for the most part beneath the threshold and so far as we can determine in an identical manner. This is what occurs in all cases of what Titchener has called ‘associative supplementing,’<sup>1</sup> such as auditory localization, judgments of distance by size and of size by distance, etc., which is little more than a technical way of referring to the commonly observed fact that ideas or objects (we are not here concerned to distinguish them)

<sup>1</sup> An Outline of Psychology, p. 194.

are not isolated and separated from each other, but are related, that is, have a context. The simplest experience (in the adult at least) has a 'setting' or 'background.' This 'setting' may be entirely disregarded for the time being, yet its influence is indubitable, since if absent or removed the effect is marked at once in the attentive consciousness. That these ultimate constructive elements of our ideas of objects are really lurking in the margin of consciousness, comes out clearly when through some 'hitch' in the thought, these ideas find only partial or defective expression. They are what Pillsbury has called 'dark ideas,' which require certain conditions for clear recognition, yet whose presence is felt by their general influence on the net result in consciousness. Pillsbury gives the following instance and explanation of this. "If, for example, I read 'shocolate' as 'chocolate,' I get first an association of identity between the last letters of the word given and the corresponding letters of the word-idea 'chocolate,' and these, by contiguity, give the 'c' immediately and simultaneously."<sup>1</sup> The general purpose of Pillsbury's investigation was "to determine the amount of change which might be made in an object ordinarily perceived or assimilated in a certain way without change in the character of the resultant perception or assimilation." The result was to show that considerable alteration could be made without being detected in the ordinary process of reading the word. We shall refer to these researches again. Münsterberg has found "that if a word is displayed for a brief time, which presents some slight difference from another word, it is read as though this difference were not visible, provided that a word is previously called out to the observer which stands in intimate association to the other, but has nothing to do with the actual impression. Thus 'part' is read 'past' if 'future' is suggested; 'fright' as 'fruit' if 'vegetable' is given."<sup>2</sup> Carpenter went so far as to maintain that the ordinary process of expressing thought in spoken or written language is for the most part of the nature of a cerebral reflex (he calls it an 'ideo-motor' process). He says, "The attention may be so completely given up to the choice of words and to the composition of the sentences, that the move-

<sup>1</sup> *Am. Jour. Psychol.*, VIII., p. 333.

<sup>2</sup> So stated by Külpe, *Outlines of Psychology*, p. 183.

ments by which the words and sentences already conceived are uttered by the voice or traced on paper, no more partake of the truly volitional character than do those of our limbs when we walk through the streets in a state of abstraction.”<sup>1</sup> A very fruitful field for the observation of the effects of such assimilation is what we have referred to above as sense-illusions. The tendency is very strong to make something with a meaning out of the fragmentary percepts that are actually grasped.<sup>2</sup>

With this statement of the general nature of what we mean by assimilation, we turn to the specific phases which it presents in connection with the study of lapses. We will take up these under the following heads: (1) the light which lapses throw on the nature of the psychological unit in verbal assimilation, (2) the factors of verbal assimilation as brought out by this study of lapses, (3) the laws of verbal assimilation from the standpoint of lapses, and (4) conflict and coalescence.

### *The Psychological Unit and its Statement in the Terms of Lapses.*

The human consciousness in distinction from the consciousness of the lower animals might fairly be characterized as the speech consciousness. In human life speech is *par excellence* the function of expression and the medium of education or progress in intelligence. But this speech arises like everything else, not only in the race evolution, but in individual development, out of chaotic beginnings which, comparatively, are what we call unintelligent, irrational, meaningless. “Articulated sound when informed and interpreted by thought,” says Bowne, “becomes rational speech; but in and by itself it is only noise.”<sup>3</sup> One characteristic of the language of the educated human being, as contrasted with the mere unmeaning noises of inanimate objects and the imperfectly symbolic noises produced by the lower animals, is its aspect as the unification or integration of certain objective symbols of physical expressions into units of meaning, what we have here called psycho-

<sup>1</sup> Mental Physiology, Section 236, p. 280.

<sup>2</sup> Cf. Table XXIV., and the discussion below.

<sup>3</sup> Theory of Thought and Knowledge, p. 47.



logical or mental units. Collins calls attention to the fact that in our language as it stands, ciphers and figures have a much more definite and constant value as symbols than letters and words (thus facilitating the work of arithmetical prodigies).<sup>1</sup> This is probably because the figure like the verb in language represents a condensed judgment. An adequate study of the psychology of meaning we have already said must be genetic and primarily observational, and only in a secondary way and for the purposes of verification, analytic and experimental. Much is to be expected from further researches on the kinæsthetic sensations, muscular memory, etc., but even more from the scientific study of childhood and from anthropology. This genetic study is not undertaken here. But certain facts relevant to the psychological conception of that problem are suggested by a study of lapses, and to these facts attention is here called. To get the psychological unit in the sense of a mathematical statement or psychometric formula is impossible, since it is of the very nature of meaning that it should vary with each shift of the environing conditions, and any relative 'fixing' of the conditions means to that extent the 'fixing' of the meaning. Hence the complete control of the conditions would mean an absolutely static meaning, and thus no phenomena with which to experiment. The only recourse is to what has aptly been called 'Nature's experiments,' pathological phenomena (such even as we have here in lapses), and the phenomena of ordinary experience. Consequently, the most that can be undertaken is the determination of the nature of a unit of meaning in some functional statement of its relation to the rest of experience, especially in the light of the ways in which the expression of such meaning breaks up under the conditions which produce what we have called ellipsis, transposition, substitution, exchange, etc. From this point of view two generalizations may be made: (1) that the psychological unit may vary (according to the context) from a single letter (or even part of a letter) or word, to a whole clause, sentence, or even paragraph; and (2) that the psychological unit ultimately is to be stated in terms of the activity-experience (in terms of kinæsthetic sensations, using that word in a broad sense).

<sup>1</sup> Faculty of Speech, etc., p. 63.

It has for years been held, largely on theoretical grounds, that the psychological unit in reading and speaking may be the entire word or even the sentence, as well as the letter or syllable. In modern logical theory the mere proposition does not become a judgment until the words are grasped, not merely as separate units, but as entering into each other, modifying each other, and thus becoming parts in an ideal whole of meaning. This truth is often expressed, says Bosanquet, by saying that the sentence is the unit of language, *i. e.*, "a word taken by itself cannot have a complete meaning—unless it is a verb, or used with verbal force, for a verb is an unanalyzed sentence."<sup>1</sup> That words may be single presentations as well as single letters seems probable from the fact that the child learns and uses many names of things, and is capable of using them discriminatingly, long before he can spell them. Moreover, it has been found by experimentation that "a familiar word of four letters can be apprehended by the attention as if it were a single letter; it is attended to not as a series of letters, but as one total impression."<sup>2</sup> Reaction experiments by Cattell and others show that the word as a whole may be the unit, and that the separate letters often require as much time for recognition as short words. In paraphasia the patient often loses his speech not by single letters, but whole groups of words drop out, what Stout (after Herbart) would call entire apperceptive systems. Nouns and adjectives are the classes of words which are the first to disappear, often together. Pedagogy is only beginning to learn a useful lesson from such facts. It is significant that the aphasic convalescent who relearns to read, does so in the reverse order to that by which most of us learned to read. He begins, not first with the letter as made up of such and such strokes, then with the syllable, and finally the word and phrase or sentence, but he begins "by first getting the perspective of the word, the outline, then the syllables that constitute the word, and, last of all, the letters."<sup>3</sup> Says Onuf, concerning certain aphasic patients,<sup>4</sup>

<sup>1</sup> Essentials of Logic, pp. 82-85. Cf. also p. 86, where he says, "There are traces in language that indicate the sentence to have been historically prior to the word."

<sup>2</sup> Titchener, An Outline of Psychology, p. 146.

<sup>3</sup> Collins, Faculty of Speech, etc., p. 132.

<sup>4</sup> *Jour. Nerv. and Ment. Diseases*, Mch., 1897, p. 147-148.

“Entire words were read more promptly than letters composing them. Words which were read correctly were spelled wrongly. The patient often spelled *a posteriori* from the sound of the word (*e. g.*, ‘one’ read correctly but spelled ‘won’ and ‘unknown’ read correctly but spelled ‘unwnown’.” He says further, “In arriving at their conclusion that reading always occurs by spelling, Wernicke and Grashey evidently did not consider the peculiarities of the various languages, especially the English. The varying enunciation of the same combination of consonants, or of vowels, or of both, make it impossible to read English only spellingwise. It is enough to call to mind the three-fold enunciation of ‘ow,’ and the three-fold manner of writing the sound ‘n’ (n, kn, wn). It may further be mentioned that a new method of teaching is now in use in many schools, by which the children are taught to read words before learning the single letters. This method seems indeed more rational, as the visual word memories become thus directly associated with the formerly acquired sound memories and psychomotor images of the words. But even if reading was learned purely spellingwise, one would learn to read as a whole those words which occur frequently. Many persons will read a language correctly and with great fluency and yet make numerous orthographical mistakes in writing, for the reason that certain combinations of letters are read as a whole and only the sound of them is remembered. ‘Enough’ may be written ‘enuff,’ and ‘though’ as ‘thow.’ He who has learned a new language by grammar is less apt to make orthographical mistakes in writing than he who has learned it directly from hearing; yet the latter may eventually read it much better than the former.”

A study of lapses confirms the above theory. The fact that so many complete exchanges<sup>1</sup> are made and yet the sentence felt to be correctly expressed, the error not being noticed (that is, the meaning or symbolism of the words, taken as a unit, not being affected), shows that the psychological unit, in reading and speaking at least,<sup>2</sup> may include several words. By going

<sup>1</sup> See Tables XXIX. and XXX.

<sup>2</sup> Few exchanges occur in writing.



through the tabulation and taking clear cases of exchange it would thus be possible to work out in a rough way the span for the speech and reading consciousness, by noting the extreme limits in the sentence where words are involved in error. This would be to measure how far ahead letters or words would be anticipated or persist—and mutually coalesce. These may be taken as the nuclei or nodal points in the sentence which embrace between them the units into which the sentence becomes disordinated under any of the conditions mentioned in a former part of this study. Such an attempt reveals the fact, however, that most of the words which constitute the language used by adults are made up of several component presentation elements or assimilative factors varying with diverse conditions. These conditions remain to be worked out in detail by a genetic study of the psychogenesis of the unit of meaning. We know that in general the earliest acquisitions in childhood are dependent upon such conditions as (what for want of better terms are called) verbal adhesiveness, alliteration, rhythm, euphony, onomatopœia, cadence, accent, etc.

These language elements, whatever they be, which become involved immediately in the lapse are here called the coalescent forms, or simply coalescents. Whether these are to be regarded always as identical with the psychological or meaning units seems to depend upon laws lying deeper than any that have been unearthed by the mere analytic approach to the problem; the rule is, however, that the errors follow the meaning rather than the mere grammatical form. The important point emphasized in this connection, and clearly brought out by the study of the errors tabulated, is the fact that among these language symbols which we call letters and words there are certain ones which seem to stand for the rest. It is a familiar matter of observation that it is not necessary to hear all the words (with the tones and modulations of vowels and consonants) in a spoken sentence, nor to see absolutely all the characters (flourishes, punctuation, dots of 'i's', crosses of 't's', etc.) in a written sentence, to get the meaning.<sup>1</sup> Says Wundt,<sup>2</sup> "The hearing of words is

<sup>1</sup> Cf. Stricker, *Sprachvorstellungen*, p. 63.

<sup>2</sup> *Outlines of Psychology*, p. 228-229.

continually accompanied by assimilations; the sound-impression is incomplete, but it is entirely filled out by earlier impressions, so that we do not notice the incompleteness. So it comes that not the correct hearing of words, but the *misunderstanding* of them, that is, the erroneous filling out of incomplete expressions through incorrect assimilations, is what generally leads us to notice the process. We may find an expression of the same fact in the ease with which any sound whatever, as, for example, the cry of an animal, the noise of water, wind, machinery, etc., can be made to sound like words almost at will." So with the common experience of the 'right hearing of words wrongly spelled.' "Even a practiced proof-reader," as Titchener says, "may overlook mistakes in very familiar words. On the other hand, the misprints in a book which is written in a language not so familiar to us as our own attract our attention at once. We read English by general impression, supplementing what we see as we glance quickly over the printed words; we read German or French more accurately, because more slowly and toilsomely."<sup>1</sup> These elements which we do get are the verbal schemata which, in normal and correct expression, are filled out in full by the entire number of letters and syllables. That these schemata are thus filled out is amply brought out by Pillsbury's experiments and by such cases as the following visual illusions, which have been collated by the writer: 'Causality' was read 'casuality,' 'calvary' 'cavalry,' 'conversation' 'conservation,' 'density' 'destiny,' 'through' 'though,' 'perceptual' 'perpetual,' 'Atlantic' 'Analytic,' 'forfeited' 'fortified,' 'anatomic' 'automatic,' 'unite' 'untie,' 'spilt' 'split.'<sup>2</sup> "The art of reading," says James, "is the art of skipping; that is why an author is usually the poorest proofreader of his own productions, since he is most apt, by association, to supply the missing characters."<sup>3</sup> When in reading, one comes repeatedly upon such a connective as 'and,' say, between two proper names, as 'Jack and Jill,' or 'Philadelphia and New York,' one does not every time have to spell it out, *a-n-d*. Again, in other expressions such as 'San Francisco, California,' one often gets the word

<sup>1</sup> An Outline of Psychology, p. 196.

<sup>2</sup> Cf. also James, Psychology, I., p. 264-265.

<sup>3</sup> Psychology, II., p. 369.

from the context so forcibly as to require the seeing of no more than a letter or two in its usual relations to get the whole. Bain says, we frequently recall entire sentences 'by hitting on catch-words.' "The single word 'phrenzy' uttered with emphasis will recall, in a mind familiar with the passage, 'The poet's eye in a fine phrenzy rolling'; the principal epithet in such a case being enough to reinstate the entire connected train." So 'Duty' may call up Nelson or the Duke of Wellington.<sup>1</sup> The recall of names by things and of things by names furnishes illustrations of cases in which the word-idea is divorced from the meaning-idea; the physical symbols become floated off, so to speak, from their natural 'mental' relations. In proof-reading we get the pure word ideas as the successive foci of consciousness in a way which is not true of the ordinary process of reading to get the sense. These pure word-ideas in such cases are the schemata of which we were just speaking. Still further illustrations of such schemata are found in the use of grammalogues in short-hand writing, where the purely arbitrary symbol stands for a more or less complex meaning-idea.<sup>2</sup>

Lapses throw light on the nature of these schemata. A reference to the tabulation will show the coalescent forms to be largely consonants and consonantal digraphs, and, as we saw above, derivative rather than fundamental language forms. As we shall see later, also, contiguous substantives tend to coalesce when they contain wholly or partially identical meaning elements. On the other hand, contiguous connectives tend to coalesce when they stand in identical or analogous relations to adjacent substantives. A given word or sentence has a certain meaning, and it is indifferent, within certain limits,<sup>3</sup> what subsidiary elements go to make it up, or whence they come, so long as the significant or fundamental elements (the ribs of the skeleton of the word, so to speak) are intact. If the meaning attaches, not to the word, but to the phrase, then you will get such a lapse as 'Phosford's Acid Horsephate' or 'Put the trays on the weights' (exchanges). If the mean-

<sup>1</sup> *Senses and Intellect*, third ed., 1868, p. 469.

<sup>2</sup> Cf. also Bosanquet, *Logic I.*, p 74.

<sup>3</sup> The problem as to just what are these limits remains to be solved. Sanford and Pillsbury have done something on the experimental side.



ing attaches to the word, then the error will rarely go further than to take the form of a substitution, while under still more restrictive conditions we get simply the transposition or ellipsis. In the graphic errors we notice the relative paucity of the former, and the relative abundance of the latter types of error, just because here the word-parts, syllables, letters, etc., constitute important immediate factors in the meaning.

A corroborative statement for the above observations on the psychological unit in reading and writing was sought by the writer in other ways. (1) The subject was required to read aloud from the printed page and at the same time write some familiar phrase or write dictated material. This proved to be at first very difficult. It was varied by using (*a*) combinations of words making sense, and (*b*) unmeaning combinations of words. The manner in which the unit was isolated in this experiment was for the operator to note down the snatches of the text read aloud between the spasmodic spurts of writing, or the passages written in the intervals between the attacks upon the reading. The ability to utilize words in this experiment holds about the same for words of different lengths in the case of the combinations of words which made sense. That is, two long words will be read or written about as readily in one pulse of attention as two short words. But in the case of the unmeaning combinations of words not only is the span of the reading and writing consciousness greatly abbreviated, but it is also very variable, due, no doubt, to the irregular and inconstant degree of possibility of making sense out of the nonsense combinations. In other words, this confirms the general view derived from a study of lapses, that the span or unit of the reading or writing consciousness varies with the meaning of the phrase or clause, not with its absolute length. Any person accustomed to copying on a typewriter knows how much easier it is to remember a passage which makes some sense or is complete in itself as to meaning than it is to remember one which at both ends perhaps shows its fragmentary character. (2) Another method employed was that of using paragraphs of text from which here and there words were erased, the subject being required to fill out the blanks with the first word or words that oc-

curred to him. The words which were inserted, as a rule, bore a definite and constant relation to the immediately preceding or following content. That is, if a single word would complete the sense a single word alone would be substituted. If several words were required to complete the sense these would be substituted, if not orally, at least mentally. The single word does not come in except with such a context of meaning as does not require more than a single word to complete the sense. This experiment supports the conclusion that for the reading consciousness the single word is not the unit, but always carries a context with it (usually to the next idealized punctuation mark), it may be a phrase or even a clause, and it may be in definite or in vague, ill defined symbols. A variation of this experiment was that in which incomplete sentences were written and the subject required to complete them. This showed essentially the same results, the portion supplied to complete the sentence varying from a single word to an entire (and sometimes lengthy) clause, and varying from the greatest readiness to the greatest hesitation in response, on the part of the subject. (3) A third method is that in which there was exposed to the subject for a period of time sufficient for a single perusal, a line of reading matter, and the subject asked immediately to tell what he remembered of it, and to indicate what stood out prominently in this memory of the immediate past. The results tend to support the conclusions of A. Binet and V. Henri,<sup>1</sup> who found (a) that the words which stand at the beginning and at the end of a series are best retained, but that (b) those words whose sense it is more difficult to understand are more easily retained, since they make a greater demand on the attention. That is, here, again, the breaking up, and the reconstruction of the sentence in memory, both follow the meaning.

We have suggested that the word-idea and the meaning-idea are different things. The term 'word-idea' is here employed as Stricker uses it, as "ein leeres, inhaltsloses Wort \* \* \* oder

<sup>1</sup> La Mémoire des mots, *L'année psychol.*, I., pp. 1-23; 1895; reviewed in *Zeitschr. f. Psy. u. Physiol. d. Sinnesorgane*, XII., p. 154-155. Cf. also Pillsbury, *Am. Jour. Psychol.*, VIII., p. 349-350, and Cron. and Kraepelin, *Psychol. Rev.*, March, 1899, p. 230.

eine reine Wortvorstellung.”<sup>1</sup> That is, the pure word-idea is the idea of the symbol as a mere symbol apart from its content of meaning. In the ordinary experience of the person who has not given especial attention to the subject the two are not felt as distinct. But when once attention has been directed to the difference they are readily separable. If I understand English only, I may have a pure or mere word-idea corresponding to the visual or auditory image of the German expression ‘Schlag,’ but I will have an idea of the meaning of this word only if I understand it to mean ‘stroke,’ or ‘beat.’ Or if we take an acrostic or an Egyptian hieroglyph the idea of the symbol as a symbol stands apart from its meaning. How the meaning-idea may transcend the word-idea which is the mere linguistic symbol or statement of it, is seen, also, in the fact that often “when people do not know what they mean, they yet mean something of very great importance,” or in the observation that “what people demand is seldom what would satisfy them if they got it.”<sup>2</sup> The mere word-idea stands for what the word is by and for itself.<sup>3</sup> The meaning-idea stands for what the word is in relation to a context of experiences. The mere word-idea stands for the word as it appears or sounds. The meaning-idea stands for what we can *do* with the word-idea. That is, the pure word-idea is usually in terms simply of visual or auditory imagery. The meaning-idea must be in terms of kinæsthetic imagery (so-called ‘motor’ imagery). It is true that one’s apprehension of a language is not necessarily proportionate to his ability to use it in either speaking or writing. Many persons, for example, can *read* scientific German who can not speak it or understand it when spoken. But this ability to read a language beyond one’s ability to speak or write it, is possible only by borrowing, so to speak, the motor imagery from one’s native tongue. One literally respeaks or rewrites the foreign language, mentally, in his mother tongue, as he reads it, before he understands it. This is borne out by the fact, noted above,

<sup>1</sup> Sprachvorstellungen, p. 18.

<sup>2</sup> Cf. Bosanquet, *The Philosophical Theory of the State*, p. 118.

<sup>3</sup> This, of course, is true only within limits, since there must be some recognition of meaning in order to distinguish the symbol as a symbol and this particular symbol from others.



that the errors in the tabulation follow the dynamogenic or kinæsthetic imagery, which is the imagery of meaning, rather than the sheer auditory or visual imagery, which taken alone corresponds to what we have called the pure word-idea. The same thing is brought out in the experiment which James uses to illustrate his discussion of the principle of association, "Partly open your mouth and then imagine any word with labials or dentals in it, such as 'bubble,' 'toddle,' etc.," or attempt to "think of one vowel while continuously sounding another."<sup>1</sup> It is found to be very difficult, if not quite impossible. According to Stricker,<sup>2</sup> we learn to speak and write, to combine auditory and visual images into symbols and words, through the mediation of the motor or kinæsthetic ideas which are developed in the actual attempt and operation of speaking and writing. Probably through auditory and visual images alone, without these kinæsthetic images, we should never learn either to speak or to read or to write. Or, to state the same thing in other terms, the meaning-ideas which constitute the content of our verbal associations are ultimately kinæsthetic or motor, not visual or auditory. These kinæsthetic images may be associated more closely with this or with that set of sensory experiences, thus giving rise to the different associative types, 'visuals,' 'audiles,' 'motiles' (other kinæsthetic), etc.; but, as Stricker puts it, there must be a 'Mitwirkung des motorischen Sprachcentrums' for the understanding of the 'meaning' of the purely visual or auditory symbols. Why, it may be asked, are all our sensations concentrated with reference to the visual process (in reading) or with reference to the auditory process (in listening)? Simply, it must be answered, to get an adequate stimulus to enable the tongue or hand to do something which *it is starting to do*. There must be this return wave of the kinæsthetic imagery, to select and organize the visual or auditory perceptions, before these processes are really perceptive, before they have any meaning. If it were not for some other organ or organs implicitly involved (such as the musculatures of speaking or writing), the eye would be as well pleased with a blotch of ink or with a patch of color as

<sup>1</sup> Psychology, II., pp. 63-64.

<sup>2</sup> Cf. Sprachvorstellungen, pp. 26-28, 77-78.

with letters and figures. It is because these other organs (which carry with their functioning the distinctive imagery of 'meaning') come in, and it is because these organs, through previous association or use of it, are adapted, as it were, to determine the object, that this object presents itself to the eye as a stimulus, as an object of interest and attention. Edouard Claparède<sup>1</sup> lays emphasis upon the fact that what he calls the absolute abolition of the muscular or kinæsthetic sense results in the utter incapacity for those coördinations requisite for the complex movements involved in the simplest type of attention.

The phenomena of mental blindness and mental deafness, so-called, bear out this same general conclusion. These pathological states may be described as the ability to see and hear objects (perceive them) without being able to put them to any use (to apperceive them).<sup>2</sup> The connections with the center which in experience has represented the chief content of the meaning of the objects (the center for the kinæsthetic sensations which report the use to which the objects are intelligently put) are severed or damaged. It is significant in this connection that we have instances in which the kinæsthetic area functions for the visual, but no cases in which the converse of this is true. Says Collins, "Patients with word blindness are sometimes able to read written or printed words and sentences by tracing the word (which, it is to be remembered, they see with customary acuteness) with the end of the index finger or with a pencil."<sup>3</sup> In an analogous manner, in the case of lapses, we give expression in speaking or writing to a letter or word which does not make sense, and which, as we say, we did not intend; we are quite unconscious of its irrelevancy or incongruity at the time of its utterance (we are mentally deaf or blind for the nonce), and scarcely believe that we have made the error when told of it. A still further corroboration of this principle is the familiar fact that meaning (for English-born persons) in the case of the Anglo-Saxon derivatives is primarily such as suggests objects, situations, actions, etc., as over against the more abstract and

<sup>1</sup> *Du Sens Musculaire*, 1897, p. 134.

<sup>2</sup> Cf. Külpe, *Outlines of Psychology*, pp. 174-175, and Starr, *Familiar Forms of Nervous Disease*, Chap. VI.

<sup>3</sup> *Faculty of Speech*, etc., p. 282.

remote meaning attached to the Latin and Greek derivatives, which tends rather to be in the visual and auditory imagery : this is true, of course, only in a general way.

As has been remarked on a previous page, much confusion in the study of the psychology of speech—as, indeed, of much other psychology—has resulted from a false antithesis of the ‘sensory’ and ‘motor’ processes. There is no more reason to regard the sensations connected with the activity of the sense organs as sensory than those connected with the activity of the muscles : on the other hand the sensations arising from the muscles are no more motor than are the sensations arising from the sense organs. A ‘central’ theory of the origin and nature of the kinæsthetic sensations is responsible for much of this confusion. The truth is, of course, that both and all processes are equally motor and sensory. The terms ‘sensory’ and ‘motor’ properly used are not content, but functional distinctions. This confusion in the use of the term ‘motor’ has led to the neglect of the proper sensory aspect of the kinæsthetic sensations. For example, Ballet says<sup>1</sup> that the meaning-idea arises ordinarily not only apart from, but temporally before the word-idea. For this he finds support in the mental development of the child, in the manner in which it gets, for example, the idea of a bell, or of an orange, through the association of separate sensations.<sup>2</sup> It is certainly true, as Ballet shows,<sup>3</sup> that the child gets first, for example, a simple auditory idea or sensation (usually by imitation, in Ballet’s use of the word), and then later, associated with it, other ideas which, taken together with this, constitute the idea of the object. Finally the auditory or visual image, as the case may be, comes to stand for the entire object or for the ‘meaning’ as a whole, and we have the beginnings of that complex language development in which the word serves as a substitute for the idea or thought. But the trouble with Ballet’s arguments for the independent development of the word-idea and the meaning-idea is this, that in the illustrations which he employs he does not exclude *all* use of symbols, such, for example, as the

<sup>1</sup> *Le langage intérieur*, p. 6.

<sup>2</sup> He adopts Charcot’s schema ; cf. a suggestive summary on p. 13f.

<sup>3</sup> Pp. 10, 11.



kinæsthetic, which, in our opinion, are most significant for the content of the meaning-idea.<sup>1</sup> When, therefore, he insists that the disappearance of the word-idea does not necessarily carry with it the disappearance of the meaning-idea (and that this is essential for the theory of aphasia) he confuses two possible uses of the term 'word-idea.' It is quite true that *a* word-idea (which is a *partial* symbol for a 'meaning') may vanish and yet the meaning, in some sort, remain; but if we could banish the *whole* symbolic complex which stands for the idea, it is questionable whether we would have any meaning-idea left. In other words (at least in adult life in the overwhelming majority of cases), no meaning is confined to a single symbol or to a single sense. Meaning is consentiently acquired. Certainly the word-idea is in a sense separable from the meaning-idea. Words are the tools of thought. They give it not only its flexibility, but also whatever of definiteness it has. But on the other hand the one cannot be floated off in utter detachment from the other. They are significant in experience only in relation to one another. Our internal language is for the most part in auditory verbal images. Where great use has been made of reading or writing the visual imagery may come to play a prominent part. In both cases the kinæsthetic imagery is the determinant factor in the 'meaning.' But any imagery may be predominant according to the constitution and training of the individual.

Egger, as Ballet says, is an *auditif*, and Stricker is a *moteur*; this explains the different accounts which they give of the imagery of internal language. But the auditory or visual verbal imagery never carries a 'meaning' unless it is combined with the articulo- or grapho-kinæsthetic imagery. This has been abundantly shown for the auditory imagery. D. Bernard<sup>2</sup> reports a case of mental blindness in which the patient could understand the meaning of the written word only when going through the motion of forming the letters as in writing. This illustration shows it for the visual imagery also. We may restate Ballet's scheme,<sup>3</sup> then, as follows. In writing (copying)

<sup>1</sup> Cf. p. 9, for instance.

<sup>2</sup> *Progrès médical*, 21, juillet, 1883; noted by Ballet, p. 55.

<sup>3</sup> P. 14.

or reading we have (1) the auditory sensation of the word (the word heard), (2) the visual sensation of the word (the word seen), (3) the articulo-kinæsthetic sensation of the word (the word spoken), (4) the grapho-kinæsthetic sensation of the word (the word written). If (1) or (2) be impaired the meaning of the word may be affected, but (3) or (4) must be impaired to remove entirely the meaning (that is, to render the person incapable of interpreting the meaning of the symbols). It is lesion of the centers corresponding to (1) and (3) or (2) and (4) which constitutes mental (and in this case verbal) deafness or blindness. Ability to interpret 'meaning' is quite compatible with the lesion of (1) or (2) only. It is just because the kinæsthetic is the fundamental imagery (and hence the imagery of 'meaning') that we find in most cases of functional aphasia that there is not a total loss of the apperceptive function, but only a more or less partial effacement of it. It depends altogether upon the degree to which the kinæsthetic imagery is predominant and independent of the auditory and visual imagery, as to how far the loss of the latter will disturb the ability to interpret the 'meaning' of an object or event. In Helen Kellar we have one of Nature's experiments in which the auditory and visual imagery is completely gone, yet where there is a rich experience of 'meaning.'

*The Factors of Verbal Assimilation.*—The complexity of the simplest of our experiences, when analyzed, has long been a matter of comment among psychologists. The exact determination of the number and relative strengths of the different components or elements thus isolated by analysis is just beginning to be studied experimentally. In that mental process, or complex of processes, which we call verbal association, as we have seen, it depends upon very variable conditions whether a single word or a part of word or a group of words shall be regarded as the psychological unit. From one point of view, that of the mere grammatical formation and analysis, the sentence breaks up into letters and words, phrases and clauses, more or less arbitrarily, according to the grammatological principle invoked. But from the point of view of psychological analysis proper (the psychology of the *meaning* of the sentence).

letters and words, and even phrases and clauses, as such, cease to be the significant units, cease to afford the useful lines of cleavage. The psychological units lie rather on the side of serviceability for 'getting on' in the activity-experience; they consist rather in the dynamogenic units of thought as they find expression in linguistic symbols. From this point of view a word may be psychologically very complex, and an entire sentence relatively simple. Instances are fresh in everyone's experience, of single words packed and doubly compacted with meaning, and of whole phrases and clauses containing scarcely sufficient meaning to arrest the attention as they are seen or heard. It is not into the analysis of the psychology of meaning (or, as we have called it, the psychology proper) of the language forms which serve as thought symbols, that we here propose to enter. This, as has already been emphasized, can be profitably approached from the genetic side only. Nor is it into the experimental isolation of the different factors that we here inquire; this also, as requiring a neurological basis, we have set aside as a problem for the present beyond our immediate scope. But the question which we may answer is, What do lapses suggest as to the elements or factors which go to make up that complex experience which we call verbal assimilation?

The first step in this analysis consists in determining what are, introspectively, the important component elements in our ordinary speech and writing consciousness, or, to use the older terminology, in ordinary processes of verbal association. Ordinary speech and writing are dependent upon a variety of factors for their normal maintenance and uninterrupted flow. These factors have customarily been grouped under three heads: (1) the visual, (2) the auditory and (3) the kinæsthetic. A verbal association (or, as he calls it, a 'verbal idea') in the sense of such a complex, according to Professor Titchener, "consists of an auditory complex, a mixture of clang and noise (word heard), a strain complex due to the adjustment of larynx and mouth necessary for the emission of a particular sound (word spoken), a visual complex, a written or printed form (word seen), and the strain complex due to the adjustment of hand and fingers necessary for the production of this form (word written)."<sup>1</sup>

<sup>1</sup> An Outline of Psychology, p. 198-199.



Or, for short, we may adopt Professor Hill's diagram, as given in his 'Genetic Philosophy.'<sup>1</sup>

A word is composed of	a. an <i>auditive</i> image—the word as <i>heard</i> ,	} [kinæsthetic]
	b. a <i>visual</i> image—the word as <i>seen</i> ,	
	c. an <i>articulate</i> image—the word as <i>spoken</i> ,	
	d. a <i>graphic</i> image—the word as <i>written</i> .	

In most of our actual experiences all of these factors will not be present in equal degree. Clearly in certain abnormal cases one or more is entirely absent. But probably in all ordinary or normal verbal consciousness each factor has a more or less influential part to play. It is the determination in a general way of the mutual relations of these factors as brought out by the study of lapses that we are concerned to seek. The 'motor' or kinæsthetic factor is uniformly combined with the auditory or visual. This reduces the four factors in the above analysis to two, which we may call the auditory-motor and the visual-motor, or the auditory-kinæsthetic and the visual-kinæsthetic. By this connection of the two sets of factors it is meant to indicate, what we find to be a constant law of all mental process, that the sensori-motor arc or organic circuit is one process, that all thought and feeling tend to action, that all sensory naturally and inevitably flows over into motor activity and that all such activity is, in turn, reflected into experience as sensation. The first half of this principle has been diligently advocated in this country by Professor Baldwin in instructive researches on his children. According to this conception, the different elements of the speech faculty come under the law of what Professor Baldwin calls sensori-motor association, in the following forms: "auditory, visual, speech-motor, hand-motor (writing) memories," the former two developed in and through the latter two.<sup>2</sup> He, accordingly, distinguishes but two speech types, the auditory and the visual. The former is found most frequently, he says, among unliterary people who have not had large practice in writing and reading. That is, this hypothesis postulates that the units of our articulo-motor are the same as those of our sensory-auditory consciousness, and that the units

<sup>1</sup> P. 188: taken apparently, from Ballet, *Le Langage Intérieur*, p. 14.

<sup>2</sup> *Mental Development in the Child and in the Race*, p. 466.

of our grapho-motor are the same as those of our sensory-visual consciousness.

The fact that confronts us is the wide range of variability in the relative proportions which can exist among these factors. It is their great range of possible combinations that gives our language such flexibility and such adaptability to all shades of meaning. Without doubt we employ, without consciously analyzing the process, many variations in the use of the different sensory factors in our verbal ideas. This is ultimately the psychogenetic source of those grammatical (formal) distinctions which we have introduced into what we call the 'correct' use of language. It is this which makes possible the richness and variety of the language and literature of civilized man, and makes possible and necessary the linguistic sciences and the science of literary criticism. It is the possibility of such variety of union of the elements which come from the different sense modalities which make language the adaptable tool that it is in the hands of a versatile literary genius like a Homer, a Goethe, or a Shakespeare.<sup>1</sup> The usual psychological analysis states that spoken language is chiefly in terms of auditory images, and written language, chiefly in terms of visual images. This, with the modifications made below, is probably the case. Experimental data are constantly coming to the front in our psychological laboratories which tend to confirm the general principle.<sup>2</sup> In the case of lapses we find that the disturbing element or coalescent in the case of graphic errors is usually visual in form; the mere sound of the word, unless very pronounced or coming into direct conflict with the visual image, does not enter to the extent of interfering with the correct expression. Examples where the influence of the auditory imagery is felt are appended below (Table IX.). In oral errors, on the other hand, the disturbing element is usually auditory, *i. e.*, the *sound* of the just entering or just departing word is

<sup>1</sup> As Stricker says (Studien über die Sprachvorstellungen, p. 60), "Unsere gegliederte Sprache ist gleichsam flüssig, wir können sie disgregiren, wir können Silben und Laute verschieben, und darin ist eben ihre enorme Entwicklungsfähigkeit begründet."

<sup>2</sup> Cf. a review of experiments by Schiller on school children, in *Am. Jour. Psychol.*, Jan. 1899.

influential. Baldwin refers to a case of aphasia which brings out these relations clearly. It is the case of a patient "who spelt aloud a word wrongly when he *wrote* it ('candd' for 'cat'), but at the same time pronounced it correctly, as he heard it. This means that his spelling movements, letter by letter, had been learned in association with the making of the letters and the sight of them, while the learning of the word's pronunciation, as a whole, had been in connection with its sound."<sup>1</sup> Assuming this position in general to be the correct one, let us look at these factors a little more in detail.

We take up the speech consciousness first because it is psychogenetically more primitive than the writing consciousness. Probably general expressive reactions and mimic reactions precede articulate speech; but almost certainly writing was developed later than speech. This is made more probable by the facts of mental pathology, which go to show that the writing consciousness is the less stable of the two, suggesting its relative lateness of origin.<sup>2</sup> Written language is not, as Keraval shows,<sup>3</sup> the immediate symbol of the object or of the idea of the object. It is but a representation or sign which stands in most cases for the articulate or spoken symbol for the object. Written language thus only indirectly stands for the object; it is the symbol of a symbol.<sup>4</sup> According to the same author,<sup>5</sup> the Chinese, Egyptians, etc., think more in visual and grapho-kinæsthetic than in auditory and articulo-kinæsthetic images, which is just the reverse of the European languages. Yet, in spite of these peoples, writing has become phonetic, that is, the reproduction or symbolic representation of the spoken symbols rather than the direct representation of the object.

We have said that speech is dependent primarily upon the auditory imagery. There is reason to believe that the motor center for speech never becomes independent of the sensory center which presided over its education. J. Cohn, in his re-

<sup>1</sup> *Mental Development in the Child and in the Race*, p. 470-471.

<sup>2</sup> Cf. Collins, *Faculty of Speech*, etc., p. 68.

<sup>3</sup> *Le langage écrit*, p. 33 f.

<sup>4</sup> "L'écriture est un dessin conventionnel des sons" (p. 160).

<sup>5</sup> P. 40.



searches on the auditory-motor and visual memory,<sup>1</sup> finds (1) that an auditory-motor memory is more profoundly disturbed by auditory-motor stimuli than a visual memory would be by the same stimuli, and (2) that when the auditory-motor memory is thus disturbed greater use is made of visual memory. In other words, the auditory-motor imagery is the important imagery for speech. This is as true of internal speech, or 'mental pronunciation,' as it is of ordinary vocal speech. As P. Keraval says<sup>2</sup> "C'est la parole intérieure qui souffle la parole extérieure, qui dicte l'écriture, et, durant la lecture, c'est elle qui reproduit intérieurement les mots lus, si bien que les images visuelles sont intimement unies aux images auditives dont elles sont, dans une certaine mesure, dépendantes." Says Collins, "The sound of every articulated word acts as a stimulus to the auditory center for the next. If the auditory center has been destroyed there is no such leader in the memorial order of words, and we have the frequent occurrence of *lapsus linguæ*, or paraphasia. \* \* \* In a similar way the auditory center guides the action of the articulatory center in the employment of internal language."<sup>3</sup> As another writer says, "Peripherally aroused sensations from actual articulatory movements are not essential for inner speech; their reproduced images suffice. When present they raise the mental presentation of words to greater clearness."<sup>4</sup> Probably mental articulation employs the same sensori-motor circuit as for audible articulation, but it stops short of sufficient innervation to overcome the neural resistance necessary to call into action so complex a musculature<sup>5</sup> as that necessary for audible articulate speech. It is a lesion that disables or militates against such externalization of language in either spoken or written form that is properly called motor aphasia. The ordinary influence

<sup>1</sup> Experimentelle Untersuchungen über des Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses, Zeitschr. f. Psy. u. Physiol. d. Sinnesorgane, XV., p. 182.

<sup>2</sup> Le Langage Écrit, p. 155.

<sup>3</sup> Faculty of Speech, etc., p. 251-252. Cf. reference, above, to Onuf, p. 139-140.

<sup>4</sup> E. B. Delabarre, in *Psycholog. Rev.*, May, 1897, p. 326-327.

<sup>5</sup> In articulation at least four sets or groups of muscles come into play, (1) the respiratory, (2) laryngeal, (3) lingual, and (4) the buccal. Each has its definite representation in the Rolandic area (cf. Collins, p. 82).

of the auditory imagery, chiefly in speaking (cf. the ideo-motor oral errors in the tabulations), takes the familiar form of echolalia. A table of such errors has been made for another purpose (to be mentioned below), which shows very plainly the influence of the sound of one word upon the oral production of another (Table XI.). A rough measure of the part played by the auditory imagery as a check on the correct enunciation in speech is found in the disturbing effect of total or partial deafness on the speech of one who previously had both normal hearing and normal speech. Onomatopoetic words (such as buzz, hiss, splash, whirr, etc.), and words whose spelling give no hint as to their pronunciation (such as phlegm, *Pall Mall*, Majoribanks, Magdalen College, Bowdoin, etc.), often give trouble here. The confusion of such words as 'two,' 'too,' and 'to,' of 'I,' 'eye,' and 'aye,' etc., especially in writing, is due chiefly to the influence of the auditory imagery.

The visual factor is usually introduced in reading, as distinguished from speaking. Here, as elsewhere, the kinæsthetic imagery is a constant factor. We question the accuracy of the introspection in the case of the subject who is reported by W. B. Secor<sup>1</sup> as not dependent upon the articulatory imagery in reading. There is no doubt that this writer's conclusion is correct when he says that practice brings the visual imagery into greater prominence, but that it is possible to 'grasp the meaning of printed or written matter through the eye alone without the aid of articulatory images' is open to grave question. The probability is that the kinæsthetic imagery has changed from a gross to a finer type, say, from the grapho- or articulo-kinæsthetic to the imagery connected with the finer movements of the eye, but that it vanishes entirely is *apriori* improbable and *aposteriori* undemonstrated. Errors occur chiefly when the visual and the auditory imagery come into conflict. For example, in the following case the auditory word-idea came into conflict with the visual word-idea, when a person, wishing to look up a meaning of the German word 'geneigt,' first looked by mistake under 'ge,' in the lexicon, and then, observing his mistake and ostensibly rectifying it,

<sup>1</sup> *Am. Jour. Psychol.*, Jan., 1900, XI., No. 2, pp. 226, 232.

looked under 'gn,' the sound image of the word triumphing over the visual image. Similar conflict of imagery occurs in writing. An example is found in the following error in writing on the typewriter, where *er* was written for 'urgent,' the auditory image of the first syllable of the word predominating and driving out the visual image. Other similar cases are more or less fully treated throughout the tabulations under Remarks. The effect of the visual imagery on listening is well shown in W. B. Secor's experiments<sup>1</sup> in which, for example, when '*t* was exposed, and *dot* pronounced, the subject saw *tot*.' This was the case when the letters involved belonged to the same class or group. "When the letter was followed by a word whose initial or final letter belonged to a different class, the subject saw the word pronounced with the exposed letter hovering about in space near the incongruous initial or final. When, for example, *d* was exposed and *pelt* was pronounced, the subject saw *pelt*, but somewhere near the *p* he also saw the *d*. The *d* 'seemed to be trying to get into the word.'"

The kinæsthetic factors in speech are very much more important than was formerly recognized. It fact, it is probable that every speech idea is an auditory-kinæsthetic idea; the separate treatment of them is possible only by abstraction. H. Schiller shows in his researches on learning to spell by children that (1) merely hearing words pronounced is the least effective way of learning to spell; (2) that exposing the word-form to the eye reduces the number of errors somewhat, but that (3) writing the word, either on paper or in the air, or soft or loud speaking of the word (that is, the introduction of the kinæsthetic, in addition to the auditory and visual sensations) greatly reduces the errors.<sup>2</sup> This is why, when trying to study in a noisy room, one involuntarily begins to speak aloud the sentences he is reading, in order to concentrate more fully the attention upon them. Thereby one gets a richer complex of sensations (the kinæsthetic and auditory in addition to the visual) which will serve to fix what one is reading, in the mem-

<sup>1</sup> *Op. Cit.*, p. 235.

<sup>2</sup> Cf. *Am. Jour. Psychol.*, Jan., 1899, p. 307.



ory.<sup>1</sup> Stricker, in his 'Studien über die Sprachvorstellungen,' has called attention to the sensations which accompany ordinary vocalization (articulation). For example, in the case of the consonant B we have a sensation in the two lips, in the case of D in the tip of the tongue, in K in the root of the tongue, in F in the under lip, etc. The pronunciation of vowels also is accompanied by sensations of a similar sort but less pronounced and always in conjunction with some consonant. These are the sensations also which accompany mental pronunciation<sup>2</sup>—what he calls the 'Initialgefühlen.' He concludes that (1) with every idea of a tone there is inseparably associated a more or less distinct sensation in the organs of articulation, (2) these sensations are located in the muscles, (3) these sensations are similar to those which accompany the actual expression of tones. He uses the following introspective evidence. If when quiet, with the eyes closed, you let a familiar stanza of poetry pass through your mind, and direct the attention at the same time to the organs of speech, you will find or feel an inward tendency in the mouth, lips and tongue, to speak the words you are thinking ('mitreden'). This is not noticeable if in place of the stanza of poetry you think of some popular air with which you never have associated any words; but now the inward feeling of strain sensations is located rather in the larynx, while in the case of remembered tunes simply which one has never sung, the feeling seems to be situated indefinitely somewhere in the top of the head. Thus he insists that in speech we remember words primarily by what he calls the 'Initialgefühlen,'<sup>3</sup> or as we would say by

<sup>1</sup> Cf. Stricker, p. 47. "Stricker's assertion," as Delabarre says (*Psycholog. Rev.*, May, 1897, p. 326), "that no sensory elements are present in his motor verbal images, and that they consist in innervation-images, is shown [by R. Dodge] to be indefensible." Cf. also R. Dodge, "Die Motorischen Wortvorstellungen," Halle, 1896—reviewed in the *Rev. Philosophique*, XLIII., p. 640. The close connection of the auditory and kinæsthetic imagery is brought out in the cases cited by Stricker (p. 105) from Erdmann and Lewinsky, where simple thinking resulted in the throat becoming dry and the voice husky.

<sup>2</sup> For examples of errors in 'mental pronunciation' see Table VIII.

<sup>3</sup> Cf. p. 44; cf. also p. 45, where he says, "Die gelesenen Worte ohne Mitwirkung des motorischen Sprachcentrums nicht verstehen kann," and p. 42, where he says, "Das Mitsprechen ist im Beginne des Unterrichts ein Mittel um das Lesen zu erlernen." Cf. also Collins, pp. 67, 68, 114, 131. See also H. Davies, 'The Growth of Voluntary Control,' *Psycholog. Rev.*, Nov., 1899, p. 643.

'motor' or kinæsthetic imagery. It is a matter of common occurrence for persons to accompany their reflective moods by words audibly, though unconsciously and involuntarily, expressed. Very often we see persons on the street or engaged in manual labor talking with themselves, *sotto voce*. Thought certainly tends to speech, or at least to expression of some sort (and where speech is a deeply grounded habit, it tends strongly to speech). Children and uncultivated persons move the lips and mumble to themselves when reading. Everyone probably has at times the consciousness of the impulse to speak the words, to articulate the syllables he reads, though in cultured persons this tendency is, as a rule, almost completely inhibited. Where it is a fixed habit the person may be said to be an *articulo-moteur*, just as we speak of the *auditif* and the *visuel*.<sup>1</sup> Here, then, is evidence from the ordinary consciousness that the visual and auditory ('sensory') and kinæsthetic ('motor') speech centers are very closely interrelated, and not only so, but that through these centers, sensory are constantly passing over into motor processes, without the mediation of something wholly not neutral, without (so certain neurological theorists would maintain) the necessity of a separate coördinating mechanism.

TABLE VIII.

List of Errors Occurring in 'Internal' Speech, or 'Mental Pronunciation.'

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
This fixing	This fixed grouping.....	Thinking.
we are led from this task	we are led from this to ask..	Reading.
Prise	rise of Protestantism...	{ In a dream; the person before retiring had been engaged on an address on this subject. In proof-reading; the lips did not perceptibly move, yet there was a distinct 'strain' sensation in the lips.
stell body	cell-body stains .....	
lings	lungs and wings .....	Reading.
fills, gins, etc.	fins, gills, etc .....	Reading.
bread, cie, and pake	bread, pie and cake .....	{ Repeating mentally, a list of things to be gotten at the bakery.

<sup>1</sup> Cf. Stricker, p. 40.

dame, leaf, and blind	lame, deaf, and blind.....	Reading.
on a water-leaf lily	on a water-lily leaf.....	Reading.
entitling nobles	ennobling titles .....	Reading.
to roll untangled records	to unroll tangled records ..	Reading.
machine-stetched him	machine-stitchéd hem.....	Reading.
a roope and a nose	a rope and a noose .....	Thinking.
{ must be taken as bare	must be taken as really }	...Reading.
really	bare	
chapel place	palace chapel .....	Reading.
{ Five Thousand Walls	Five Thousand Rolls }..	{ In reading an advertise-
of Roll-paper	of Wall-paper	ment in a newspaper.
it is even more	it is more even and sedate..	Reading.
{ in modes predetermined	in modes predetermined }	.Reading.
by the previous organ-	by the nervous organ-	
ization	ization	
the rain wept sky	the rain swept sky	Reading.
{ their special furtheriza-	their further speciali- }	...Reading.
tion	zation	

It already has been remarked that the factors of the writing consciousness are primarily in visual or visual-kinæsthetic terms. One becomes conscious of his own speech in one or both of two ways: (1) one hears his own voice, (2) one feels the movements in the larynx, etc., by which speech is produced. In a similar way, in writing, (1) one sees the page on which he is tracing the letters (one also may hear the pen scratching along the manuscript, and can tell roughly how many lines, words and letters, or even strokes, have been written; in some instances the elimination of the auditory control of the writing is very disturbing), (2) one feels the muscular sensations in the arm, hand, fingers, etc., by which the writing is produced. As we have seen in the experimental production of lapses the mere closing of the eyes does not exclude all visual control, since the page may be strongly visualized ('imagined') even when all direct vision is excluded. Of course, the kinæsthetic imagery cannot be separated from the visual process in writing any more than from the auditory process in speech. The process is more complex than upon first view it appears to be. In the case of copying there may be a direct transference from the visual over into the motor process. In the case of writing from dictation we first receive auditory impressions (sound-images), these are then interpreted (and by some process of 'retention' the meaning of a series of such sound-images is utilized



for reproduction in the motor process of expression which we call writing), and translated over into kinæsthetic (possibly also into visual) terms before expression in the form of writing.

It is practically impossible to determine with experimental accuracy the degree to which the kinæsthetic is helped out by the visual imagery in writing. So far as direct visual control is concerned, most people can write nearly as well without as with the eyes open. Table XII. gives examples of errors due to similarity in appearance (visual similarity) and to similarity in formation (kinæsthetic similarity) of adjacent letters, word-parts or words. The profound influence of the kinæsthetic imagery on writing is easily tested in a rough way by (1) writing one's name on the page before him, then (2) over one's left shoulder (if right-handed), then (3) on one's forehead, then (4) over one's right shoulder. The difficulty that is here introduced by the unusual positions is obviously little or not at all visual, but muscular (kinæsthetic). The importance of this grapho-kinæsthetic imagery is seen in the fact that the ability to write may be retained in spite of both verbal deafness and verbal blindness.<sup>1</sup> Many grapho-kinæsthetic errors, thus, are doubtless classified in the above table (XII.), since it is often impossible to determine whether a lapse is due to the similarity visually or kinæsthetically of the contiguous elements. In the following error the writer had a distinct feeling that it was the similarity in the manual formation of the two words that occasioned the lapse: 'if we ould' was written for 'if we only could.' It must be noted here that in handwriting (the writer's, at least) there is essentially no difference in the formation of 'n' and 'u.' The *ou* (or *n*) *l* by assimilation gave the *d*. The following are other examples. 'And undifferent' was written for 'And under different circumstances.' Here the *di* and *de* are formed very much alike. Again, 'or the acid' was written for 'or the action of an acid.' Here the recurrence of the *ac* was the occasion of the error.

The influence of auditory imagery directly upon the formation of letters in writing is as a rule inconspicuous. Persons sometimes repeat words or phrases (or even letters), either aloud

<sup>1</sup> Cf. P. Keraval, *Le Langage Écrit*, p. 167.

or mentally, which they are attempting to copy, write at dictation, or to compose. This is especially true when one is disturbed by some counter stimulation. When spoken aloud, the hearing of the words repeated (as well as seen, or heard from dictation) serves as a distinct help in getting the subject-matter down on paper correctly. Sometimes the two sets of kinæsthetic imagery which are chiefly concerned in the act of speaking and the act of writing, respectively, come into conflict. Raymond Dodge in his 'Die Motorischen Wortvorstellungen' speaks of this experience in writing, where the two groups of kinæsthetic sensations, those representing the reflective process of composition (chiefly articulo-kinæsthetic) and those representing the actual act of writing (grapho-kinæsthetic) come into conflict, though he says that in his case this conflict does not extend as a rule over more than a syllable or very short word. In the cases recorded in Table IX. the persons testified to the distinctly felt influence of the auditory imagery in producing the errors.

TABLE IX.

## Errors in Writing Due to the Influence of Auditory Imagery.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
bego	begun on.....	Letter-writing.
{ because everybody on	because everybody wants to move at once	{ Letter-writing.
But kno	But no, through .....	{ The word 'know' had been used in a previous sentence and was reverberating in consciousness.
ref	reference for .....	{ Note that 'f' and 'v' are phonetically similar.
whistling too	whistling tunes.....	{ Note that as commonly pronounced, 'oo' = 'u.'
shave	share .....	{ Attempting to carry on a conversation while writing. Spoke the word 'half' just as was writing the word 'share' (here again, 'f' = 'v,' phonetically).
{ and how anybody good get into a quarrel with S.	and how anybody could get into a quarrel with S.	{ Letter-writing.
no end	no n around .....	In composition.
ex	x .....	In composition.
artical	article .....	In composition.

hy	hieroglyph.....	{ Note the influence of the sound of the 'y.'
by	bicycle.....	
horce	horse .....	ce = se, phonetically.
psychologisti	psychologist .....	{ This error was made in type-writing. The word 'psychology' was first written 'by mistake for 'psychologist,' and in correcting this error the writer felt the i force (auditory) of the y, and hence left out the i where it should have been, though it asserted itself later out of its normal position.

*The Laws of Verbal Assimilation.*—In the study of the tabulated errors several important principles appear, which we have called laws of verbal assimilation. For the most part these are principles with which we are already familiar in psychological analysis; but they are so clearly reinforced and illustrated in this new field by this fresh collation of data that it is thought useful to restate them in this connection—especially since it involves the reconstruction of some of them. We may very safely assume as a fundamental principle of association, that an idea,<sup>1</sup> whether it be the comparatively simple idea corresponding to a letter or the more complex idea of a word or a phrase, never appears in consciousness nor becomes crystallized into action unless there is a definite reason why this particular idea should appear in preference to all others. This definite reason it is the purpose of generalizations from these data to aid in elucidating. Construed in the terms of the older psychology, such association is said to take place in accordance with some one, or some combination, of the following laws. The law of association by similarity would maintain that similar letters or words occurring with relative frequency tend to conflict and coalition. This is sometimes called the law of similarity and contrast. The law of association by contiguity would hold that letters or words occurring in relatively close succession, or simultaneously (*i. e.*, in the 'specious' present), tend to fuse or coalesce. This is

<sup>1</sup> The term 'idea' is here used in a broad sense (Titchener's use in his 'An Outline of Psychology').



called also the law of repetition. These two laws, as we shall see below, are best stated as one. They embody obvious truth; but, obviously also, they are the product of a sheer content analysis of the associative process. What is more needed and more in line with the present functional mode of psychological analysis, is some additional statement in the general terms of habit and attention. Another law recognized by both the older and the newer psychology alike, and called the law of emotional preference or the law of the emotionally congruous, is an important one from this point of view in determining the character of assimilations. According to this law those letters or words are associated (and in the case of lapses fuse or coalesce) which have some 'personal' reference. This will be taken up further below.

The first law may be stated as follows: From the standpoint of the adjustment, errors occur in the breaking up of an habitual (familiar) association. From the standpoint of the tendency represented by the habit, thus modified, errors tend to take familiar forms, that is, forms which have a meaning for normal experience. These, as we shall see, are two statements of the same principle. That errors occur in the breaking up of an habitual association is shown by the fact that it is chiefly in the case of a difficult (because unusual) juxtaposition of heard or seen symbols (*i. e.*, in a readjustment requiring attention) that the errors occur.<sup>1</sup> This is further borne out by the fact that habit immediately reasserts itself, as shown in the strong tendency, evident in the larger errors, toward the formation of familiar words (cf. Table X.). It is a commonly recognized fact that habit tends to make reactions reflex and automatic, that, as Baldwin says, consciousness tends to 'evaporate' from such reactions. Habit, as he says, 'means loss of oversight, diffusion of attention, subsiding consciousness.' "That which is habitual is accompanied by least consciousness."<sup>2</sup> When in doubt or perplexity or hesitancy, until a decision is reached, it is the habitual thing that is done. Then at the moment or point

<sup>1</sup> Cf. in this connection Table I. which contains a list of cacophonous expressions in the pronunciation or writing of which the errors were made.

<sup>2</sup> Cf. *Feeling and Will*, p. 49, and *PSY. REV.*, I., p. 612.

of readjustment, the attentional process culminates in a new act. The phenomena of aphasia support what we know from common observation, that as a rule we forget those words with which we are least familiar, and consequently the expression of spontaneous thought is always in terms of frequent use. This natural law of habit is strikingly confirmed by a study of lapses. Here also attention seems to enter at the point of the breaking up of a habit, and lapses are, so to speak, the by-products of the transition. The readjustment (and hence the attention) may take place suddenly or gradually, or in an oscillatory way.<sup>1</sup> Alliteration and rhythm, if moderate, seem to aid the attentional process, but when excessive only tend to break it down and aggravate coalescence, as in stammering.<sup>2</sup> On the other hand, the confusion resulting from the recognition of a mistake just made or from the contemplation of a possible mistake, only plunges the attention into deeper distraction. The immediate effect of the consciousness of error is only to make the already incoherent mental states more disordinated. Disturbed states of the attention always verge on chaotic lapses. Often the question arises (as in the following error—in which 'nowe' was written for 'now we') as to whether the one letter receives the attention usually given to the two, or the two letters, on account of their proximity as well as similarity, simply the attention usually given to one, the resulting error forming a hybrid. In some cases we have the idea-of-a-doubled-letter anticipated, but not the particular letter form, or another letter is substituted. For example, 'givess' was written for 'gives off,' and 'inatt' for 'innate.' Here the habit was broken up sufficiently to precipitate a double letter, but not *the* double letter necessary for the correct expression. The following are similar instances: 'Glsaa' was written for 'glass,' 'sudeen' for 'sudden,' 'acid-dentally' for 'accidentally,' 'perisitence' for 'persistence,' 'claaed' for 'called,' 'accosiation' for 'association' (cf. also Table V.). The attempt to change from the English to the American use of the 'h' before certain words furnishes an example of the disin-

<sup>1</sup> Wundt has emphasized the factor of oscillation in attention (*c. g.*, in expectation)—*Physiol. Psychologie*, II., p. 274.

<sup>2</sup> The relations of lapses to stammering merit investigation.

tegrating effect of the breaking up of an old habit, and the errors which accompany the more or less arbitrary establishment of a new habit. In many cases the *lapsus linguæ* or *lapsus calami* shades off into what is called a *lapsus memoriæ*, as when in numbering successive pages at the top, the figure '8' was written on two consecutive pages instead of in the numerical order ('8,' '9'). This is usually called a simple case of forgetting, caused by the passage from one page to another, yet it is described with equal truth as an example of persistent substitution (*i. e.*, marking the point at which the attentional fell back again into the habitual process). A similar instance is the case in which '(1), (2), (2); (2), (3), (3)' were written for '(1), (2), (3); (2), (3), (4).'

The strong tendency of errors to take the form of familiar words or syllables corroborates the view above stated, since it shows the relatively unstable process of attention passing over again into the relatively stable and fixed habitual process. Recently it has been found in experimentation upon memory and association,<sup>1</sup> that in the case of words heard and of words seen, even when the words seemed to have little natural relation to one another, yet the tendency was strong to force the parts of the series into some sort of intelligible combination. This tendency toward the completion of a word is pointed out also by Ziehen in his experiments on association in children, in which he notices that the tendency to word-completion is very strong, as, for example, in 'postal'-card, 'heart'-shaped, 'post'-office, etc.<sup>2</sup> So, again, Pillsbury in connection with his experiments, shows that there is "always a tendency to connect the separate impressions into something with a definite meaning for consciousness."<sup>3</sup> There is, in other words, a felt demand in all cognition, in all thought, for completion, for integration, and

<sup>1</sup> Mary Whiton Calkins, *Short Studies in Memory and in Association*, from the Wellesley College Laboratory, *PSY. REV.*, V., p. 5.

<sup>2</sup> *Amer. Jour. Psychol.*, Jan., 1899, p. 306. This tendency toward the completion of the word or expression is also brought out in what we have called abbreviation errors (see above) where, for example, 'b. begs' was written for 'bed bugs' and 'cons clief' for 'consc life.'

<sup>3</sup> *Amer. Jour. Psychol.*, VIII., p. 372. Cf. what Bosanquet says of the significance of this fact for logical theory (*Essentials of Logic*, 1895, p. 76 f).



if the completion is not mediated from without, it is mediated from within (from habit, from the general stock of knowledge, so to speak). In this way we get the errors which from different points of view we have called illusions or hallucinations, and lapses. A new percept or idea of an object does not enter (the adult) consciousness alone, but always in connection with the perception of other objects. The new element is connected with others also through reproduction, and these likewise, themselves, have previously been connected with other similar elements. The object in all these relations tends to appear in consciousness as a unit. Striking examples are cited by Titchener from everyday life.<sup>1</sup> Pillsbury also cites the overlooking of misprints in proof-reading, the subjective completion of the rough daubs of stage scenery to a landscape, the seeing of figures in fire, in the clouds, seeing ghosts, etc. As he says, "Equally good, though less striking instances, are found in nearly every form of perception. When the process is carried a step farther, *i. e.*, when the associated elements outweigh the original in number and importance, or when the conscious connections do not correspond to the connections of the moment in the external world, we have 'illusion' instead of perception."<sup>2</sup> Table X. presents a list of errors showing the tendency to form familiar words or word-parts. The same tendency was brought out in the experiment in which the subject was required to write as rapidly as possible words and sentences which should make no sense, and in which forms which had some sort of meaning in the ordinary use of language asserted themselves in spite of the attempted inhibition of all such. Again, when the subject was required to copy a foreign language, in the Latin characters, or to write at dictation English sentences backward, the tendency was strong to

<sup>1</sup> An Outline of Psychology, p. 196. Cf. also Edridge-Green, Memory and its Cultivation, p. 233, where attention is called to the fact that the aphasic patient "will often, if the first part of a sentence be repeated to him (it being one with which he is well acquainted), instead of repeating the portion which was orally delivered to him, finish the sentence." The same tendency to use familiar rather than alien forms is found in a comparison, for example, of French and English aphasic utterances.

<sup>2</sup> *Amer. Jour. Psychol.*, VIII., p. 332.

make normal English sense out of the material. Moreover, it is observable that whenever the hybrid resultant form can readily take the character of a familiar word it facilitates the production of the error. The following is an example: A person in reading aloud from a book read 'seemed to correct the firmness of this statement' for 'seemed to confirm the correctness of this statement.' Here, the fact that the error could take the form of good English words with some semblance of sense seemed to facilitate the mistake in this completely carried-out form. In some cases in the experimental production of errors the subject found that he had written several words in a purely automatic way, and only knew that he had done so by recognizing, on opening his eyes, that the errors were in his own handwriting (cf. Table XIII.). Similar cases are related of reporters falling asleep, and still going on taking down a speech. A large proportion of the verbal errors recorded in the tables involve connectives (the, and, to, at, as, if, of, etc.), by reason of the fact that attention is called to them by their comparatively unfamiliar or unusual juxtaposition with certain other words in the sentence. The same is true of letters and word-parts. So long as they are found in the word or sentence in familiar relations, habitual processes assume charge of them; but when such relations are relatively unfamiliar, the habitual process is broken up—attention goes, as we say, to the difficult point in the adjustment, and errors occur. Other things being equal, that factor in a sensation complex will be modified by association which has the least number of felt relations (or significance). It is the nondescript which is kicked about from pillar to post. The most firmly fixed is also the most abundantly associated, and these elements tend least to error. That is, the consentiently acquired verbal factors tend less to error than those acquired (chiefly) through a single sense, because the habit of correct expression has been so firmly fixed in the former case that, unless exceptional circumstances arise, these words or word-parts tend to be produced always in their correct form. To the question, then, whether errors involve letters, word-parts and words which are more frequently used or those less familiar by reason of less frequent use, it must be answered,

that both are involved; the familiar forms are involved in error because placed in unusual relations with the relatively unfamiliar. It is in the process of readjustment, which is the process of attention, that these errors become apparent, and it is with relation to this process that they must be interpreted.

Stated in other terms, this might be called an illustration of the general law of mental economy of effort. According to Stricker, when a series of words beginning with the same consonant follow each other in an alliterative sentence, as "Roland der Riese am Rathhause zu Bremen," only one 'R' is seen at once, whatever the span of the reading consciousness for the other letters; that is, the marginal alliterated forms are minus the initial 'R,' so that these are really seen as *e.g.*, 'iese' and 'athhause." The organism tends to accommodate for a given stimulus but once, unless rhythm takes it up (and then it is often hard to stop). This is why in rapid reading (the writer has noticed it especially in proof-reading) letters, word-parts and even words and phrases are dropped. For example, the word 'very' suffered 'lapse' when preceded by the word 'several,' which contains, as three of its component letters, the same as the first three letters of the word 'very' (viz., 'ver'): the whole passage was as follows: "In several very small nodular enlargements." The following is another instance: 'earlies stages' was written for 'earliest stages.' In this connection are to be noted also those very frequent errors of type-setters, who omit passages because of the occurrence of similar words in close proximity in successive or near lines. Many cases of ellipsis seem to result from the sense of having already pronounced or written the letter, word-part, or word, or its equivalent. 'Ney Y' was written for 'New York.' In writing the capital 'Y' the writer thought she had written the letter 'Y,' but could not understand how or where, until her eye caught the mistake on the page. In many cases the realization of the necessity of writing the recurring word or letter more than once (contrary to the ordinary run of the words in a sentence) seems to elicit an over-dose of the attention, so to speak, and the word or letter is anticipated, precipitated out of its regular order (transposition), substituted

<sup>1</sup> Cf. Sprachvorstellungen, pp. 86, 87.



for another, or repeated more than the sense calls for. The following is a case in point: 'solved in in initial assumption' was written for 'solved in the initial assumption.'

TABLE X.

List of Errors Showing Tendency to Form Familiar Words or Word-parts.

GRAPHIC.		Remarks.
Error.	Correct Form.	
real	reeling off.	
as and	as an end.	
disturbution	distribution.	
our	of your.	
{ each set of questions when they are set	each set of questions when they are sent	} Perhaps an example of persistence.
dormat	dormant.	
some sick, some sin	some thick, some thin.	
descriping	describing.	
journal	journey.	
des	disturb.	
h ad	h at last.	
no dot	do not.	
tub	but.	
ist	its.....	{ Was engaged in translating from the German.
Egypt	Egypt. ....	Experimental.
Aall	All.	
Eexamine	Examine.	
Aage	Age.	
in Aar	in Art.	
Aarter	Arter.	
and daher	und daher .....	Translating.
specificsch	specifisch .....	Translating.
was whiche	was white on.....	Experimental.
Apr. 9, 189	Apr. 9, A.D. 30.....	Influence of habit.
E. g. Herrok	E. G. Herrok.....	Influence of habit.
contract	contradiction .....	Occurred several times.

## ORAL.

Error.	Correct Form.	Remarks.
ware-fell	well-fare	
conversation	conservation	
Your presence is re- corded	{ Your presence is requested by order of the president.	
do	due to.	
Com, ing up !	Come, ring up !	

festival of ether	festival of Easter.	
covered with carbuncles	covered with barnacles.	
celebrated it	celebrate it.	
such witches	such riches have wings.....	Anticipation also
so as to	as to.	
as in the Autumn leaf	{ as in the Autumn time the leaves fall off.	
applauses and peaches	apples and peaches	
in modes predetermined by the previous organ- ization	{ in modes predetermined by the nervous organiza- tion.	
amazed	dismayed and aghast.	
{ staid and sedate as he always was in the state-room	{ staid and sedate as he al- ways was in the recita- tion-room.	{ Example of persistence also
He took at least	He took leave at last.	
blind and mad	mild and bland.	
led you to left it	led you to leave it.	
shut up !	shh.....	{ Intended immediately af- ter uttering the admon- itory 'shh' to say, 'Shut the door !'
sixty years of old	Sixty years of age.....	Influence of habit.
ac (sh)	acts .....	{ The <i>sh</i> is to represent the sound in the second syllable of the word 'ac- tion' which came to consciousness <i>after</i> the error had been made.
{ in relation to the reality	{ in relation to the impres- sion immediately pres- ent.	{ Experimental visual-vo- cal method (see above)
{ removal of the present affective science	{ removal of the present affective state.	do.
{ regarded as an inter- mingled began	{ regarded as an interme- diate stage between.	do.
{ condition one another after	{ condition one another and form.	do.
{ volitional act are actu- ally formed	{ volitional act are usually far from.	do.
{ subordinated under a single submitting	{ subordinated under a sin- gle predominating one.	do.
processes which express	processes which present...	do.
{ as the most complex efforts	{ as the most complex form of affective.	do.
an emotion is an	an emotion in all its.....	do.
change in change	change in convergence.....	do.
to objects from	two points from the subject..	do.
{ exactly in time for	{ very rapidly to its maxi- mum.	do.
ideas have sides	ideas have a decided.....	do.
there obtain	there are certain other.....	do.
{ which had entered a port	{ which had wintered in the island.	do.

{ for example, an assumption }	for example, a succession of a number.	{ do.
{ through which opposite thing }	through which composite feelings.	{ do.
{ are merely secondary aspects }	are always mere secondary qualities.	{ do.
{ by abstraction of sincerity }	by abstraction as if it merely.	{ do.
special providence	special province.	
face wore a peaceful	face where a powerful eye..	do.
{ superhuman forces of energy }	of superhuman energy of a ferocious.	{ Experimental visual-vocal method (see above)
{ eyrie ickery philosophy }	eyrie oury ickery ann }	{ do.
{ philosophy }	phillosy phollosy.	{ do.
six seven	six Severn salmon.....	do.

The second law of verbal assimilation is as follows: adjacent similars tend to conflict and coalescence. Or, more explicitly stated, this law runs thus: contiguous substantives tend to coalescence when they contain wholly or partially identical elements; contiguous connectives tend to coalesce when they stand in identical or analogous relations to adjacent substantives. This is in reality but a restatement in the terms of a single principle of the so-called distinct laws of similarity and contiguity, about the priority of which there has been so much controversy. Various writers have stated the essence of the contention that is here made, though in abstract and logical rather than in concrete and psychological terms, when they merge both principles into what is called the laws of 'partial identity, or 'partial coincidence.' Applied to the phenomena here under consideration, this principle would maintain that it is the contiguity of similar elements, in the two letters or words concerned, that causes them to coalesce. For example, a person said, 'You want the hot so' for 'You want the fat so hot.' Here the two 't's' are the similar elements occasioning by their contiguity the lapse in question. Thus it appears that we may have coalescence due to the succession of similars or due to the simultaneous<sup>1</sup> existence of either similars or contrasts, in analogous relations, one of these being an element in the psychic fringe. The reason that similars tend to conflict and coalescence is, as has been hinted above, that attention tends to function but once for

<sup>1</sup> For our present purpose, successive association may be regarded as but a series of simultaneous associations which arise successively.



each new adjustment or readjustment, and it is the relative under- or over-functioning of the attention for a given ideated adjustment that occasions the fluctuation, or deviation, or lapse. From this point of view unlike elements are involved only by reason of their juxtaposition to, or association with, like elements. The unlike, other things being equal, will hold the attention: it is the like, the uniform, the monotonous, which allows the attention to lag, and thus permits of errors in the transition from this state of comparatively habitual monoideism to a relatively polyideic state of attentive consciousness. Because the two letters, or word-parts, or words, are alike, one mental process serves for the two, and the other is dropped or mutilated. This is the law of economy everywhere in mental life, not to give equal attention to two stimuli which are exactly alike. Uniformity of stimulation means reducing the degree of attention. Identity of stimulation means the 'lapse' of some of its elements from consciousness—or, ultimately, the complete 'lapse' of consciousness as a whole. This is the functional meaning of Stout's principle of 'continuity of interest.'<sup>1</sup> Of course, we never have any such thing as complete identity of the two similar elements associated, but rather the one simply intensified or reduced by the other, the process that we have here called assimilation.

A large percentage of the errors occur under conditions where two or more like-sounding (chiefly in the case of oral errors) or like-appearing (in the case of graphic errors) letters, syllables, or words stand adjacent to one another. That is, many cases of error are facilitated by phonic or graphic resemblances. The former we here have called euphonic; the latter, eugraphic. Euphonic confusions are such as arise from the mistaking and transplacing of letters, word-parts, or words which sound alike. Eugraphic confusions are such as arise from the mistaking and transplacing of forms which appear alike. Euphonic errors are very numerous, especially in the case of anticipatory substitutions. It is noteworthy that this takes place rather between two thin or between two thick vowels than between a thin and a thick vowel, and between consonants of like origin

<sup>1</sup> "Manual of Psychology" (1899), (p. 422).

(*e. g.*, dentals with dentals, labials with labials, linguals with linguals, etc.), rather than between consonants of divergent origin. In the case of such errors as the confusion of 'dis' and 'des,' the error is probably due to the similarity in sound, and to the prepotency of the 'd,' so that the vowel scarcely emerges (*cf.* also *affect* and *effect*). For examples of errors due to such similarity in sound, see Table XI.

TABLE XI.

List of Errors due to Similarity in Sound of Adjacent Letters,  
Word-parts, or Words.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
{ and let not them that are therein the countries enter into }	{ and let not them that are in the countries enter into. }	
philosophiker	philosophical thinker.	'k' = 'c' (hard).
we haven't any wat	we haven't any hot water.	'ot' = 'at.'
{ It will give father a Charles }	{ It will give father a chance to pitch into Charles. }	Note the common 'ch.'
{ pound-wise }	penny-wise and pound- foolish. }	Influence of alliteration. <sup>1</sup>
{ How do you keep your cleeth tean? }	{ How do you keep your teeth clean? }	'ee' = 'ea.'
{ Have you any of prose Poe's tale? }	{ Have you any of Poe's prose tales? }	'oe's' = 'ose.'
{ because everybody on }	{ because everybody wants to move at once. }	'wants' = 'once.' This was a graphic error. <sup>2</sup>
dollars and se	dollars and cents.....	Graphic error: 'c' (here) = 's'. <sup>2</sup>
On every side u	On every side of us.	'o' = 'u' (here).
{ American Tea and Com- pany Coffee }	{ American Tea and Coffee Company. }	Note recurrence of 'Co.'

Eugraphic errors are also very numerous, especially in cases where one consonant or vowel is exchanged for, or continued into, another consonant or vowel which much resembles it in form (*e. g.*, 'a' and 'g,' 'a' and 'd,' 't' and 'd,' 'y' and 'p,' etc.). As has been seen above, in the discussion of the factors of verbal assimilation, graphic errors are also though with relative infrequency in most persons, due to auditory imagery.

<sup>1</sup> Cf. throughout the other tabulations for further examples of the influence of alliteration.

<sup>2</sup> For further graphic errors of this sort see Table IX.

These cases have been sufficiently commented upon. Cases of errors due to similarity of appearance of adjacent forms are as follows: When 'da' was written for 'dictation,' the error was doubtless facilitated by the juxtaposition to the vowels 'a' and 'i' of two letters of similar general formation (cf. the second and third strokes of the 'd' with the first two strokes of the 't'); so also when 'do' was written for 'due to.' For further examples see Table XII. In the formation of many of these abnormal forms the influence of the kinæsthetic imagery is often very strong.

TABLE XII.

List of Errors due to Similarity in Appearance of Adjacent Letters, Word-parts, or Words.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
da	dictation .....	Note the dentals, 'd' and 't.'
jud	justifying judgments.	
hav	habit.....	{ In the hand-writing 'v' and 'b' are formed much alike.
moality	modality .....	
g	agreement .....	Ditto of 'a' and 'd.'
aggre	aggregation.....	
in reding	in reading .....	" " 'a' and 'g.'
availabe	available.....	
oxyden	oxygen.....	" " 'a' and 'g.'
thatt	that all .....	
The Rio Grange	The Rio Grande.....	" " 'e' and 'l.'
embryolody	embryology .....	
orgain	or again.....	" " 'd' and 'g.'
Canda	Canada.....	
digram	diagram .....	" " 'a' and 'd.'
Chicgo	Chicago .....	
peadgogical	pedagogical .....	" " 'a' and 'g.'
if we ould	if we only could.....	
catchism	catechism .....	" " 'a' and 'g.'
by	but they.....	

The third principle is the common one that the most vivid and emotionally exciting forms tend to break up the habitual process and thus afford opportunity for errors. The principle is a familiar one that, other things being equal, that sensation complex will be longest remembered which is most vivid, frequent,



recent and emotionally congruous. The principle stated above is not an exception to this rule, but rather an application of it in the light of the correlative principle that that is most vivid and emotionally exciting which involves the most profound readjustment of existing habits. Hence, since errors tend to occur in the breaking up of such habits, they are the mark also of vivid and emotionally exciting experiences. A separate tabulation for such errors is obviously impossible, since there is no mark by which we are able objectively to indicate the effect of these affective characters, apart from the cognitive characters of which they are but an abstracted aspect. Reference may be made to the list of ludicrous errors tabulated below (Table XXXI.), which in many instances grew out of emotionally complex situations: see also the tabulation of examples of persistence.

On the basis of what is represented above as the tension between the purely automatic and the attentional processes, lapses are classifiable as illustrations either of persistence or of anticipation, according as the one or the other side of this total adjustment-process becomes prominent. By persistent forms are meant such as inhere in consciousness to the exclusion of incoming new ones. These with some reasonableness might be called *Retrospections*, a word which would correspond with *Anticipations*, except that this term would seem to suggest a conscious process—and conscious process moves forward only. Examples of oral literal persistence are as follows: ‘ballot bax’ was said for ‘ballot box,’ and ‘peace-peap’ for ‘peace-pipe,’ ‘Swedish swinger’ for ‘Swedish singer,’ and ‘ice ouce on the bucket’ for ‘ice out on the bucket.’ The following is an example of oral verbal persistence: ‘a relationship not of fortuitous interrelationship’ for ‘a relationship not of fortuitous interdependence.’ Graphic literal examples are ‘ventral v’ for ‘ventral edge,’ ‘hydry’ for ‘hydra,’ ‘olf. fac’ for ‘olf. cavity,’ ‘bearing upon orthographical development in the de’ for ‘bearing upon orthographical development in the individual,’ ‘all concu’ for ‘all consumption of surplus-stored energy,’ and ‘eggs ten cents a dozen this weew only’ for ‘eggs ten cents a dozen this week only.’ The following is an example of a ver-

bal graphic error: 'and Titchener; cf. Dewey's Titchener' for "and Titchener; cf. Dewey's use of the term 'feeling.'" For further examples of persistence see tables, below, of Ellipsis, Transposition, Substitution, etc. All cases of persistent substitution are, of course, examples of repetition also. What is already in consciousness tends to remain, in accordance with what has been called the law of possession (Stout's law of mental inertia).<sup>1</sup> This is essentially what occurs in the 'memory-after-image,' when, for example, "a knock at the door, the hour struck on the clock, the face of a friend whom we have passed unnoticed," is "recognized a few minutes later by means of the persisting image, although the actual impression was entirely disregarded."<sup>2</sup> Repetitions, in the case of lapses, consist simply in the repeating of a letter, word-part, or word (the pathological terms are paliniphrasia and palingraphia). Such errors frequently occur. Repetitions of phrases and words, or part of a compound word, are called reiterations.<sup>3</sup> Repetition of a single letter is called reduplication.<sup>4</sup> Reiteration and reduplication seem to be under purely reflex or automatic control. This is no doubt the reason why stammerers and stutterers, and blunderers (or clutterers), generally are so difficult of cure. Tautology and redundancy are but the rhetorical equivalents of this tendency. The most marked resemblance which appears upon a comparison of the phenomena of lapses and aphasia are the examples of persistence or repetition. For example, Stricker records the case of an aphasic patient who when the two objects, a knife and a book ('Messer' and 'Buch'), were held up before her, correctly identified and named 'Messer,' but called 'Buch' 'Besser.'<sup>5</sup> Binet also points out that the suggestion of certain morbid states to the hypnotic patient leads to an exhaustion of the writing faculty, one of the first characteristics

<sup>1</sup> Anal. Psychol., I., p. 146.

<sup>2</sup> J. Ward, Art. 'Psychol.,' Enc. Brit., XX., p. 59.

<sup>3</sup> The following is an example: "Certain locomotor reflexes occur in the hind legs even after cross-section of the *hind legs*" for ". . . even after cross-section of the *spinal cord*."

<sup>4</sup> Examples of reduplication will be found in Table XIII.

<sup>5</sup> Sprachvorstellungen, p. 37-38; cf. also the case cited on p. 35, where 'Artillerie' was spoken for 'Artillerie.' Cf. also M. A. Starr, Familiar Forms of Nervous Disease, p. 81.

of which is the tendency of the graphic characters to repetition.<sup>1</sup> The most striking cases of repetition in connection with lapses occur in the experimental errors. Table XIII. presents examples of these (all graphic); other examples will be found scattered throughout the other tabulations.

TABLE XIII.  
Examples of Repetition in Graphic<sup>2</sup> Errors.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
temeporal	temporal .....	{ Auditory-manual : See methods of experimen- tation (above).
basiss	basis.....	
attributess <sup>3</sup>	attributes.....	do.
in in	in .....	do.
possiblee <sup>3</sup>	possible .....	do.
certain <sup>4</sup>	certain .....	do.
relationsns	relations .....	do.
conditiom	condition.....	do.
spacee <sup>3</sup>	space.....	do.
essentialley <sup>3</sup>	essentially. ....	do.
fixaation	fixation.....	do.
of the the man	of the man.....	do.
in number number	in number of vibrations....	do.
the bodies of of a	the bodies of a.....	do.
grrave	grave.....	do.
severeral	several weeks.....	do.
flag flag	flag of red.....	do.
expedition	expedition .....	do.
greene <sup>3</sup>	green .....	do.
opinion opinion	opinion will.....	do.
alliterateration	alliteration.....	do.
meental <sup>3</sup>	mental .....	do.
in the the	in the number.....	do.
of like like	of like and opposite.....	do.
whiichi untes	which unites.....	do.
aleways <sup>3</sup>	always.....	do.
reaaches	reaches.....	do.
suiccessive <sup>3</sup>	successive.....	do.
through	through.....	do.
exissts	exists.....	do.

<sup>1</sup> Le fétichisme dans l'amour, etc., *Essay on L'écriture hystérique*, p. 305. Cf. Stout also on the principle of repetition, *Anal. Psychol.*, I., p. 263.

<sup>2</sup> Experimental.

<sup>3</sup> Facilitated by the likeness in the formation of the adjacent letters in hand-writing.

<sup>4</sup> Many errors, involving parts of letters only, cannot be here tabulated.



prccssess	processes .....	{ Auditory-manual : See methods of experimen- tation (above).
asstate	a state.....	do.
fulfilled <sup>1</sup>	fulfilled.....	do.
viiews viespecting <sup>1</sup>	views respecting.....	do.
back to Spa Spai	back to Spain .....	do.
compounds sso	compounds so.....	do.
uually <sup>1</sup>	usually.....	do.
thee samee <sup>1</sup>	the same.....	do.
whose love liove	whose love is richer.....	do.
forgetting <sup>1</sup>	forgetting.....	do.
A compound may may may	A compound may.....	do
{ From the position of o the bodies it is thought tha exposition explos that	{ From the position of the bodies it is thought that the explosion was due.	{ do.
from its its	from its holy.....	do.
is is razed	is razed.....	do.
to Babylon Babbylont	to Babylon and there.....	do.
it is it is	it is without.....	do.
and and wi a	and without a.....	do.
rhythmical seriees	rhythmical series.....	do.
The rose is dead	The rose is red.....	{ In this error and the three which follow the <i>ead</i> had a peculiar insistency, doubtless facilitated by the like formation of <i>a</i> and <i>d</i> . The four errors were made in immediate succession while writing from dictation. In the last error the word 'red' was written correctly but only after consider- able hesitation.
The rose is read	do.	
The rose is read	do.	
The rose is red (?)	do.	

The nature of anticipations is obvious. Frequently, especially in rapid or anxiously hurried writing or speaking, letters or words from the mental content of the as yet unwritten or unspoken sentence either displace others (otherwise yet to be expressed) or are actually inserted over and above what has already been expressed. These forms force their way in upon the page or lips by reason of (*a*) their intrinsic logical importance or interest in the connection, (*b*) their striking appearance in form, whether familiar or unfamiliar, (*c*) their strangeness or newness, the degree of attention required to recognize or ex-

<sup>1</sup> Facilitated by the likeness in the formation of the adjacent letters in hand-writing.

cogitate (originate) them usually being greater in direct proportion to their unfamiliarity, (*d*) their moral or æsthetic weight or congruity in the connection, attention being the slave of the emotions, (*e*) their sheer length, the degree of attention requisite to hold them in consciousness until expressed being greater in proportion to their length, *ceteris paribus*. A given stroke or combination of strokes (in graphic) and a given sound or combination of sounds (in oral errors) often carry with them the idea of the-word-finished-up. When accordingly this stroke or sound is anticipated in consciousness before being precipitated in objective form, the meaning for which the stroke or sound is the symbol inhibits the production of further strokes or sounds, or alters their normal expression (cf. 'susceptibe' for 'susceptible'; this graphic error was made because the formation of the 'e' in handwriting is the same as the formation of 'l,' and the anticipated formation of such a letter-form carried with it the idea of the word as finished-up). Anticipations James explains as due to brain-processes beginning to be excited before "the thoughts attached to them are *due*—due, that is, in substantive and vivid form."<sup>1</sup> He says further, "In these cases one of two things must have happened: either some local accident of nutrition *blocks* the process that is *due*, so that other processes discharge that ought as yet to be but nascently aroused; or some opposite local accident *further*s the *latter processes* and makes them explode before their time."<sup>2</sup> Examples of anticipatory lapses are very numerous. The following are examples of oral literal anticipations: 'To shut's one' for 'to shut one's,' and "How do you expell?" for "How do you spell 'extension?'" The following are graphic literal errors: 'Beaf-steak' for 'beef-steak,' and 'Harves Time' for 'Harvest Time.' The following are oral verbal anticipations: 'My weather' for 'My over-shoes will get all worn out if this weather continues,' and "We put to ship" for "We put to sea in a ship." The following are examples of graphic verbal anticipation: "Hearing is a time sense (space, a space sense)" for "Hearing is a time sense (vision, a space sense)"

<sup>1</sup> Principles of Psychology, I., p. 257.

<sup>2</sup> Ibid., p. 258; cf. also p. 564, 565, 567, 568.

and "The first chapter of the fifty-fifth chapter" for "The first verse of the fifty-fifth chapter." For further examples of anticipation, see tables below giving examples of anticipative ellipsis, transposition, substitution, etc.

Persistence and anticipation, as the terms are here used, are but another statement for habit and attention. Examples of persistence illustrate the tendency of habitual or automatic process to take up the activity of speaking or writing. Examples of anticipation illustrate the tendency of attention (where a readjustment is necessary) to break in upon this automatic process. In the case of persistence it is the unfamiliarity which occasions the error. Where no unfamiliarity occurs, no errors occur except those of anticipation or attention (those due to similarity). In the case of anticipative or attentional errors, it is the similarity of the two or more forms which occasions the errors (the error tending to fuse the similar elements into one). When there are no contiguous similars, no errors occur except those of persistence or habit (those due to unfamiliarity—the habitual process tending to usurp the function of expression). Thus from the side of attention (anticipation), the similar elements in the word or sentence tend to coalesce, and the unlike elements to be given correctly; while from the side of habit (persistence), the unfamiliar elements in the word or sentence tend to coalesce, and the familiar elements to be given correctly. As has been remarked before in another connection, language is the tool or instrument of thought. If in using a chisel or saw in carpentry the point breaks off or the teeth become dulled, the attention is directed from the work in which one is using the tools to the tools themselves; one swears at them, perhaps, or, better, attempts to sharpen them. So when one goes to the drug-store and asks for some 'Phosford's Acid Horsephate' and the clerk laughs at him, his attention is directed for the time being from the primary end he had in view, that of getting the medicine, to the words he has used as the means of procuring it. The experience goes on smoothly enough, in other words, until the means fail to fit the end, until the instrument or tool which we call language fails to do its work properly—when the necessity of some sort of a readjustment becomes apparent. That is



what is meant by saying that the lapse represents or *is* the break in the adjustment or coördinative process of experience. It is the point at which the preëxisting habitual line of activity fails to meet a new situation, or meets it in an inadequate way, this inadequacy finding expression, in the present instance, in the imperfect language forms. The following lapse brings out beautifully this vacillation between habit and attention, between persistence and anticipation, with the consequent error which resulted from the break in the process of readjustment. In attempting to write the passage 'costs a good deal' the person first incorrectly wrote 'god.' In correcting this, *d* was added (making 'godd'). Then, in confusion, the next word was begun with *g* instead of a *d* (making 'godd g'). Then, finally, a *d* was written over this *g* and *cal* was added, though with hesitation and a sense that "all the mental machinery had stopped working for the time being." Here the attentional or anticipated factor is the letter *d*, while the habitual or persisting factor is the letter *g*. The error, as a whole, represents the tension and finally the break in the process of readjustment with the attendant intensification of the subjective side of the situation and the precipitation of the erroneous forms. So-called *lapsus memoriae*, in which the reinstatement or reproduction is partial only, illustrate best, perhaps, how the lapse grows out of an imperfect adjustment. Here, there is not only the production of an erroneous form (that is, one which is only partially correct), but there is, in addition, the consciousness that it is erroneous, which is not always the case with other forms of the lapse. In the following instances the first form gives the word correctly as it was attempted to recall it, while the second form gives the word as it was actually (in these cases partially) recalled. The psychological unit in these instances would be represented by the word-parts which are italicized: *Davenport*, *Dunraven*: Yucca, Poppy (here the idea of the double letter is what is common: *Accordian*, *Concord*; *Brumback*, *B . . . b . . .* (was sure only that there were two 'B's in that relation); *Creosote*, *Croton*; *Penfield*, *Glenhaven*; *Eupatorium*, *Septorium*; *Boneset*, *Stones*; 623, 632; *McNeal*, *McLean*; *VanKirk*, *Hancock*; *Mitchell*, *Simpson*; *Cooper*, *Spooner*;

## TABLE XIV.

Examples of Errors due at once to Persistence and to Anticipation.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
still obliged	still obliged.....	Conversation
fraternal rations	fraternal relations.....	Reading aloud
{ when the war with Fance	when the war with France broke out. }	do.
a loud soud	a loud sound is suddenly heard. }	do.
Chapel, Cobbel	Chapel, Cobb Hall.....	Conversation
continues to exists	continues to exist, its manifestations. }	do.
and de	and ascended into heaven.	Reading aloud
{ It is at least too early to affirm	It is at least too early to affirm that gold mo- nometalism has won. }	do.
{ some half-mile around to my three-fôld	some half-mile around to my three-fold cord. }	do.
{ September, said the tecretary	September, said the sec- retary. }	do.
Mister Briswood	Mister Brithwood is busy...	do.
see Thayer, page thix	{ see Thayer, page six, thirty-six. }	do.
definite outfet	definite outfit of reflexes...	do.
{ lest my lord by	lest my lord be too much troubled by effron- tery. }	do.
{ pleasureable or plain- ful	pleasureable or painful element. }	do.
stored norvous	stored nervous force.....	do.
{ pays more attention to religion	pays more attention to religious instruction. }	do.
{ which he placed in the window nook, and then returned to the book	which he placed in the window nook, and then returned to the bed and took. }	do.
and his houl	and his soul can hardly contain. }	do.
decrepid, decayed	decrepit, decayed, crazy...	do.
{ Ruskin and Morris mem	Ruskin and Morris remedy. }	do.
{ never slowed the shightest sign	never showed the slight- est sign. }	do.
which he whiches	which he wishes.....	{ Graphic (for other cases see Graphic exchanges, below)
looseless	looseness of his own language. }	Graphic
sometime makes	sometimes make mis- takes. }	Conversation
eyes and mouths	eyes and mouth and cheeks. }	do.

Hobhouse, Cobhead; Sartor Resartus; Tartarus; Locke, Clarke; Passey, Sappey; Deyber, Pergens; 125,215. Other examples which bring out the influence both of persistence and of anticipation, in the same error, are grouped in Table XIV. Still further examples will be found in the tabulation, below, of oral and graphic exchanges.

*Conflict and Coalescence.*—Mr. G. F. Stout uses the term ‘coalescence’ (previously used by Hartley and others) to express that process of association in which one percept assimilates to another percept or to a mental predisposition left by previous percepts.<sup>1</sup> According to this conception, the process of coalescence or ‘overlapping’ is the resultant of a process of conflict or competition of part-processes. Coalescence or overlapping may take either of two typical forms according as it involves merely a displacement or omission with a substitution, or a coalition with modification of the elements involved. The simpler is the case in which one element simply drives out another and takes its place; this we have called coalescence by substitution. Of this type of coalescence Mr. Stout says, “It may happen that under conditions which would otherwise give rise to conflict, no appreciable conflict actually ensues, simply because either the old or the new combination is relatively so powerful as to overbear the tendency opposed to it without a struggle. Suppose the components of the one combination are  $a\ b\ c$ , and of the other  $a\ b\ x$ ;  $c$  may be so favored from the outset that it simply displaces  $x$  without any feeling of discrepancy arising, and without any attention to the difference. This process I call *overlapping* or *coalescence*.”<sup>2</sup> He further remarks that when this happens, it may also extend to the corresponding memory-image, inasmuch as the modified perception gives rise to a correspondingly modified idea. It extends also to mental images, apart from their interaction with percepts, as clearly illustrated in his series of illustrations.<sup>3</sup> The most striking examples of coalescence are those in which the overlapping is one-sided. This is shown in the examples cited by Mr. Stout—in ‘our

<sup>1</sup> Analytic Psychology, I., p. 286.

<sup>2</sup> P. 285.

<sup>3</sup> Pp. 286–289.



views of what *ought* to have taken place,' in the case of 'men who come to credit their own lies by frequent repetition of them,' in that fiction which is only *founded* on fact, in that condition of being 'haunted by a general sense of having done something or experienced something before.' These are chiefly examples of coalescence between mental images. Examples of the 'falsification of perception through the blending of an image with a more or less divergent percept' are noted as of 'very common occurrence.' But certainly as numerous are the cases in which two percepts appear to be concerned, as when, for example, two words being pronounced in rapid succession or seen simultaneously, the result is a hybrid or coalescent form. There are thus three types of coalescence—cases in which are involved (1) two (or more) percepts—for the most part peripherally excited, (2) an image and a percept—in part peripherally and in part centrally excited, (3) two (or more) images—for the most part centrally excited. Examples of these three classes are here appended. As examples of the coalescence of two percepts, the following are instructive: 'surval' was read aloud for 'survival value,' 'these penal enlargements' for 'these penal establishments were enlarged,' 'that's a perison' for 'that's a period for comparison.' The following are examples of the coalescence of a percept with an image or idea. While attempting to compose a sentence in which it was desired to use the expression, 'the Good Shepherd,' another person who was delivering a lecture uttered a sentence in which occurred the word 'subject'; this apparently caused the error in writing, which became 'the Good subject.' In a similar way 'punishman' was written for 'punishment,' the word 'man' being uttered just as the word 'punishment' was being written. So, again, intending to write the phrase 'power of contrary choice,' and the word 'constraint' being uttered by another person just as the word 'contrary' was about to be written, the resultant coalescent form became 'power of constraint.' The following are examples of the coalescence of two mental images or ideas. They were caused in almost every case by hesitation in the choice of terms or expressions (especially synonyms). The synonyms, 'differences' and 'disagreements,' being in mind,

and each struggling for utterance, the resulting coalescent form was 'degreements.' In a similar way resulted 'whotailing' from a confusion and fusion of 'retailing' and 'wholesale,' 'carbohydron' from 'hydrocarbon' and 'carbohydrate,' 'rudiculous' from 'ridiculous' and 'ludicrous,' 'symblem' from 'symbol' and 'emblem,' 'atmosphair' from 'atmosphere' and 'air.' A clerk in the mechanical department of a Railroad Company made out a pass for an employee, while his mind was engrossed in the shipping of cylinders; what he wrote was 'Lima to Cylinder' for 'Lima to Dayton.' Replacing a photograph, which had just fallen upon the floor, and speaking of a key which had been lost, one person said, 'I gave the photograph' instead of 'I gave the key.' A professor wrote 'iron alum 4' instead of 'iron alum formalin, 8 days,' (he was suffering intense pain at the time). Another person while studying the general subject of 'sensation,' went to the dictionary to look up the word 'inhibit' and turned the pages of the dictionary to 'sen....' A similar error was made in looking up the word 'substitution,' the person looking under 'b' instead of 's.' So, again, intending to turn to 'abschliessen' in the lexicon, a student turned to 'as' instead of 'ab,' and wondered why he failed to find the word. Another person went to the dictionary for 'scope,' and turned to 'c,' and was perplexed that the dictionary did not contain that word. Instances might be multiplied indefinitely. Further illustrations will be found in Tables II., III., and IV.

Coalescence by substitution may take any one or more of three forms: (1) ellipsis, (2) transposition, (3) substitution proper. Ellipsis and transposition usually go together. An *ellipsis* is the omission or dropping of a letter, word-part, or word (or even of a group of words). When literal, ellipsis is called elision; elision most frequently occurs in the case when a word begins with the same letter with which the preceding word ends.<sup>1</sup> The following is an example of the ellipsis of an entire word: 'Moral and separation' was spoken for 'moral and spiritual separation.' Probably the common initial consonant, 's,' facilitated the error. The following is an example of

<sup>1</sup>Cf. the scanning of Latin verse.

the graphic ellipsis of an entire word: "As I had to wait some, I saw some baby-hoods" for "As I had to wait some ten minutes,

TABLE XV.

Ellipsis due to the Sense of having Written the Letter, Word-part of Word.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
sudden dawning	sudden dawning .....	Letter-writing
has much	has as much.....	Composition
grief that gazes at a gave	grief that gazes at a grave.	Copying <sup>1</sup>
preacher but	preacher here but.....	do.
are lated	are related.....	do.
process shows	processes shows.....	do.
have not abitually	have not habitually.....	Composition
is is	is his.....	do.
forms of nervous nisease	forms of nervous disease...	Dictation
elemets	elements.. .....	Copying <sup>2</sup>
accout	account. ....	do.
Phla	Philadelphia.....	do.
is required	is required.....	do.
are sone	are some.....	do.
may	many.....	do.
sonetimes	sometimes .....	do.
tenple	temple .....	do.
hone	home .....	do.
Phlippines	Philippines.....	do.
tured	turned.....	do.
then	them.....	do.
cone	come.....	do.
infinte	infinite.....	do.
Columbus	Columbus .....	do.
anythng	anything .....	do.
nuber	number.....	do.
Span	Spain .....	do.
qute	quite.....	do.
pont	point.....	do.
sonething	something .....	do.
sustances	substances.....	do.

<sup>1</sup> This and the errors that follow are experimental errors.

<sup>2</sup> In the errors that follow it must be remembered that the formation (in handwriting) of many letters, such as u, n, m, third and fourth strokes of h, first and second strokes of y, the formation of the letter i, and the last two strokes of the letter p, etc., is essentially identical. Many errors involving parts of letters, only, are omitted from this table, simply from lack of typographical representations for them. They will receive attention in the study from the genetic standpoint.



## TABLE XVI.

Ellipsis Due to Previous Pronunciation of the Same Letter,  
Word-part, or Word.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
It takes one breath	It takes one's breath away	Conversation
heights of ate	heights of hate .....	do.
much less pecialized	much less specialized.....	do.
Anemone, sails, etc.	Anemone, snails, etc.	Reading aloud
{ I think I'll strike off that	I think I'll strike off that }	do.
{ edge	{ sedge..... }	
in raising bead	in raising bread.....	do.
trickled the eardrops	trickled the teardrops.....	do.
truly normous	truly enormous.....	do. 'y' = 'e'
{ as water runs off a duck	as water runs off a duck's }	Conversation
{ back	{ back .....	
very good rounds	very good grounds.....	Platform address
{ at the tribunal of pen-	at the tribunal of pen-	Reading aloud
{ ance that the piests	{ ance that the priests endeavored..... }	

I saw some baby-hoods." 'Truly normous' for 'Truly enormous,' and 'my thy' for 'may thy' are oral examples of the ellipsis of single letters (in the one case, of an initial, and in the other case of an intermediate letter); 'has much' for 'has as much' and 'process shows' for 'processes shows' are graphic examples. An interesting case of habitual elision is the following, which is given in the words of the person who reported it. "J. S., an Englishman of, say, nearly thirty years, entered the D. U. Preparatory Department to study for the ministry. By dint of great effort he had conquered his 'h's and almost invariably spoke them correctly. 'Wh,' however, he often pronounced like 'w.' In his Greek he had the greatest trouble with his rough breathings. In his pronunciation he habitually omitted them and at times would write them smooth. This difficulty continued for at least two years study of the language." Here, plainly, a deeply ingrained habit came into conflict with the attempted acquisition of new forms through the attention, and resulted in the frequent making of errors. Many unaccountable cases of ellipsis occur, such as the following: 'Shoud' for 'should that not be,' 'separale' for 'separable,' 'development' for 'development,' 'receivd' for 'received,' 'in Boton' for 'in Boston,' 'pro of physical' for 'product of physical,'

TABLE XVII.

Graphic Ellipsis Due to the Anticipation [of Letters, Word-parts, or Words.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
may bet	may be set.....	Copying
ethica life	ethical life.....	Composition
pround	profound .....	Letter-writing
{ I think it would be a { good i to p	I think it will be a good idea to keep this handy }	Copying
a series f	a series of fault lines.....	do.
antitheptic	antipathetic.....	do.
quote C. L. Here	quote C. L. H. here.....	{ Composition: the per- son's name was C. L. Herrick.
stan outs	stands out.....	Copying
it is onderful	it is wonderful what.....	Composition
w egree	we agree .....	do.
mak out	make out.....	do.
wh	we have.....	do.
samy	same way.....	do.
of they	of the eye.....	Copying
in reding	in reading.....	do.
rember	remember .....	do.
with word	within work .....	Composition
indepent	independent .....	do.
liker	like her .....	do.
opposides	opposite sides.....	do.
rembran	remembrance .....	do.
at leas three	at least thrèe.....	Copying
from this task	from this to ask.....	do.
se-a	sea-anemones.....	do.
what matters	what matter is.....	do.
basc upond	based upon .....	do.
extrado	extraordinary.....	Composition
effor to	effort to.....	do.
wh	when he.....	do.
buth	but that .....	Letter-writing
are qually	are equally .....	Composition
while it	while yet it.....	do.
dication	dictation .....	do.
by an dir	by an indirect.....	do.
no w	now we.....	do.
quari	quadrilateral.....	do.
is obj	is open to objection .....	do.
even isists	even insists.....	do.
in tensity	in intensity.....	do.

'compressive' for 'comprehensive,' 'succesive' for 'successive,' 'consciousnss' for 'consciousness.' In Tables XV., XVI., XVII. and XVIII. examples of ellipsis are classified by two principles of division (1) whether oral or graphic, (2) whether illustrations of persistence or of anticipation.

TABLE XVIII.

Oral Ellipsis due to Anticipation of Letters, Word-parts, or Words.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
{ down where the apple- blossoms blow	{ down where the apple- blossoms grow.....	Conversation
chronic	chronicle political.....	Reading aloud
too ull for utterance.	too full for utterance.....	Conversation
fed	fled for refuge.....	do.
iea	idea .....	do.
only in the pefic.	only in the specific .....	do.
{ well, three or fours of this	{ well, three or four years of this.....	Reading aloud
rentless	relentless.....	do.
rembled	resembled .....	Conversation
Protestism	Protestantism.....	Reading aloud
repress	repress it.....	Conversation
and ed	and end-bud .....	do.
witten	written word.....	do.
call	small cymose.....	do.
		's' = 'c'
{ The chairman rose in his pace	{ The chairman rose in his place, pale and agi- tated .....	Reading aloud
low oesophagus	lower oesophagus.....	do.
proc of	process of .....	do.
		'c' as in
		'cy'
when the formances	when the performances.....	Reading aloud
Linus utas	Linus usitatissimum .....	do.
save as thou teach us	save as thou teachest us ....	do.
and my thy	and may thy.....	do.
each dividual	each individual .....	do.
the 'sgock	the clock's got.....	Conversation
covetness	coveteousness.....	do.
develoment	development .....	do.
I believed her balone	I believed her to be alone..	do.
A public peaker	A public speaker said .....	Reading <i>sotto voce</i>

By a *transposition* is meant the displacement of a letter, word-part, or word without any complementary substitution or



TABLE XIX.

## Examples of Graphic Persistent Transposition.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
classed hered	classed here.....	Copying
glimpse	glimpse of Pike's Peak.....	do.
an auditory nage	an auditory age.....	do.
whol whold	who hold.....	do.
when whe	when we.....	do.
haste to to	haste to.....	do.
courses to to pursue.	courses to pursue.....	do.
as at at	as at.....	do.
to the to the	to the.....	do.
she shays	she says.....	do.
trademans	tradesman.....	do.
h fore	h for it.....	{ 'h' here was an abbreviation for 'have'; the 'e' came from the suppressed 'ave.'
1885	185.....	
to o	to return.....	Copying do.

TABLE XX.

## Examples of Oral Persistent Transposition.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
Hawaii and Helsewhere	Hawaii and elsewhere.....	Conversation
subsidiary	subsidiary.....	do.
{ I must to Paris, he	I must to Paris, he	{ Reading aloud; the 'h' of 'he' is transplaced.
{ gasped	gasped.	
afford a mans	affords a man.....	Reading aloud
mile distants	miles distant.....	do.
{ at flourish of the blade	at flourish of the blade	{ do.
{ the crowd stood black.	the crowd stood back.	
cheek looks	cheeks look.....	do.
sometime makes	sometimes make.....	do.
suddenly risening	suddenly rising.....	do.
changed muched	changed much.....	do.
cost of dresst	cost of dress.....	do.
grin agrain	grin again.....	do.
{ intellect, affections,	intellect, affections, and	{ do.
{ and moral earnest-	moral earnestness in	
{ ness in these respec-	these respects.	
{ tions.		
eternal sherdes	eternal shades.....	do.
appliclation	application.....	do.
blue blood black	{ blue blood back to the	{ do.
	{ Normans	
est-negg	nest-egg.....	do.
{ Give me a spoon out	Give me a spoon out of	{ do.
{ of the tumbler be-	the tumbler before	
{ fore Tubby gets it.	Tubby gets it.	

exchange. A transposition may, indeed, be viewed as an arrested substitution. A transposition may be either persistent (repetition) or anticipatory, and accompanied by an ellipsis or not so accompanied. They are distinguished from what we have called Insertions in that they have always some obvious persistent or anticipative cause. The following is an example of persistent transposition accompanied by an elision: 'Ith or win' was spoken for 'with or in.' 'Alto solto' spoken for 'Alto solo' is an example of persistent transposition which is unaccompanied by elision; this is an example also of repetition. "The juicy" spoken for "The juice is milky" is an example of anticipatory transposition. These are all oral examples.

TABLE XXI.

## Examples of Graphic Anticipatory Transposition.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
I smade	I made a start.....	Copying
anybodyd else	anybody else would.....	do.
Good	God by nature good.....	do.
boilded	boiled dinner.....	do.
borards	boards.....	do.
tell hus	tell us here.....	do.
plublic	public.....	do.
fundamental anthithesis	fundamental antithesis....	do.
regarding the two ha	{ regarding the two as ho- } omdynamous.	do.
laspsed	lapsed.....	do.
maglignant	malignant.....	do.
ase	as are.....	do.
distuing	distinguished.....	do.
one	on the.....	do.
womand	woman and.....	do.
yout can't	you can't.....	do.
mend	men and.....	do.
manisf	manifestation.....	do.
havep been	have been prepared.....	do.
withoubt	without doubt.....	do.
mut	utmost.....	do.
shpheres	spheres.....	do.
fromed	{ from that of hoofed Her- } bivora.	do.
ell cells	air cells.....	do.
frome	from the.....	do.
we see hin	we seen in him.....	do.
nod e	no education.....	do.

Graphic errors of the same sort are abundant. For the tabulation of transpositions see Tables XIX., XX., XXI., and XXII. Obviously, since persistent transpositions are largely repetitions Table XIX. will cover in part the same ground as Table XIII. (see above); but in the latter case the errors tabulated are all

TABLE XXII.

## Examples of Oral Anticipatory Transposition.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
Fetch the box at onced	{ Fetch the box at once } as she has promised....	Reading aloud
subjective universality	{ subjective universal va- } lidity.	do.
brings inward	bring inwards.....	do.
nerver fi	nerve fibers.....	do.
{ and ang as the birds	and sang as the bird sings }	do.
{ sing on its bough	on its bough.	
old mens	old men are always fables..	do.
{ a violent Equinoctial a	a violent Equinoctial }	do.
{ sgale	gale supervened.	
I say the sfear	I say the fear should fall....	do.
gives one	give ones.....	do.
highly	high-priestly.....	do.
the bloods	the blood of bulls and goats	do.
organisism	organism consists.....	do.
larish	lashing furiously.....	do.
as much as impo	{ as much as possible the } grand impression.	do.
setle of idle	set of idle.....	do.
Motherly in Enderly	Mother in Enderly.....	do.
on the divast	on the vast diversity.....	do.
danger's	danger there's.....	Conversation
to shut's one	to shut one's eyes.....	do.
his flace	his face was a play-ground	do.
finely	fine closely.....	do.
planes	panes of glass.....	do.
leather stel	leather telescope.....	do.
a bright flire	a bright fire blazing.....	do.
with tonguer	with tongue asunder.....	do.
a new atroop	a new troop assembles.....	do.
snappering	{ snapping the numerous } bonds.	do.
earnest	earn an honest living.....	do.
There is onet for rent	There is one for rent.....	do.
plain	{ pain, pleasure, and æs- } thetics.	do.
Decorations day	Decoration days.....	do.
The Brible bings	The Bible brings.....	do.



experimental, whereas in the present instance they are errors made in ordinary writing.

*Substitutions proper* are errors in which one letter, word-part or word, by either persistence or anticipation, takes the place of another such letter, word-part or word. In some

## TABLE XXIII.

## Graphic Persistent Substitutions.

## OF LETTERS OR WORD-PARTS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
Mr. W. Blair Clair	Mr. W. Blair Clark.....	Composition
are are	are all.....	do.
{ The leader blind and led by the land	{ The leader blind and led by the hand. }	do.
if it it	if it is.....	do.
to prove to	to prove who so.....	do.
felling	feeling .....	'e' = 'l.'
paradox	paradox .....	
{ I want to sell three hundree	{ I want to sell three hun- dred acres. }	Copying
independent dissee	independent tissue.....	Composition
{ He who runs away may live to see another way	{ He who runs away may live to see another day. }	do.
beef-steaf	beef-steak.....	do.
metaphysical physie	metaphysical psychology..	do.
conception of dution	conception of duty.....	Copying

## OF WORDS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
{ each set of questions when they are set	{ each set of questions when they are sent. }	Composition
intuitions of men of	intuition of men are.....	do.
{ Mrs. S. R. Jones, Mrs. S. R. Beeman	{ Mrs. S. R. Jones, Mrs. H. N. Beeman. }	do.
one of my one	one of my own.....	do.
{ conflict and morbid conflict	{ conflict and morbid con- science. }	Dictation
{ absent-minded man at the mind	{ absent-minded man at the moment. }	do.
two two	two-toed cloth.....	Copying
rapidity with with	rapidity with which.....	Composition
{ how much dusting you much have done	{ how much dusting you must have done. }	do.
{ the room, so we home to get	{ the room, so we hope to get. }	do.
{ make the assertion that self assertion	{ make the assertion that self realization. }	Copying

TABLE XXIV.  
Oral Persistent Substitutions.  
OF LETTERS OR WORD-PARTS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
{ I have kept the faith ; I have fought the good fith }	{ I have kept the faith ; I have fought the good fight. }	Reading aloud
optic tup	optic cup.....	Conversation
six theal	sixth seal.....	do.
tertium tuid	tertium quid.....	do.
{ on the first bed though in a lions ded }	{ on the first bed, though in a lion's den. }	Reading aloud
if we admid	if we admit.....	Conversation
bent on blowing bib	bent on blowing big.....	do.
so he will so	so he will say.....	do.
tidal wive	tidal wave.....	do.
opponents opp	opponents apparently.....	do.
loosley foo	loosley few-flowered.....	do.
{ Those who believe in evolution think that revolution }	{ Those who believe in evoltion think that revelation. }	do.
refinement, gentlement	refinement, gentleness.....	do.
{ as if he had dashed it on hit or mish }	{ as if he had dashed it on hit or miss. }	do.
{ than the nest to the nedgling }	{ than the nest to the fledgling. }	do.
this joint-sent test	this joint-sense test	do.
baked bans	baked beans.....	do.
{ on the necessity of ne- generation. }	{ on the necessity of re- generation. }	do.
secluded retruts	secluded retreats.....	do.
Herbertian	Herbartian.....	do.
indesdribable	indescribable.....	do.

## OF WORDS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
{ How many bushels do you think he lost in eight bushels of coal? }	{ How many bushels do you think he lost in eight wagon-loads of coal. }	Conversation
{ Mr. S. wishes his mail put in his box }	{ M. S. wishes his mail put in my box. }	do.
{ If I only had more milk I would take that other piece of milk }	{ If I only had more milk I would take that other piece of cake. }	do.
{ It usually appears on the seventh or eighth day; yet in some days. }	{ It usually appears on the seventh or eighth day; yet in some cases. }	do.
parenthesis is parenthesis	parenthesis is adhered to...	do.
{ sight represented by sight }	sight represented by one...	do.

cases where the substitution is mutual (exchanges) the whole word or phrase may be considered as the psychological unit, in which case the errors fall within a subsequent category (coalescence by modification). We speak of an elision when there is simply a part of the expression left out. We speak of a transposition when there is something (taken from some other part of the expression) added to the complete word or sentence. We speak of a substitution when something is put in place of part of the word or sentence. All errors are really substitutions, using the word in a broad sense. But the term is restricted here to the sense just defined. It is noticeable that the number of partial or uncompleted errors, such as transpositions

TABLE XXV.  
Graphic Anticipatory Substitutions.

OF LETTERS OR WORD-PARTS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
The whole error of c	{ The whole error of Kant } can be.....	Copying
ar	or as .....	Composition
no	know nothing.....	do.
he head	the head.....	do.
de	adequately.....	do.
I h	I shall have.....	do.
rhape	shape, roughness.....	do.
et	entirely.....	do.
blanch	branch quite black.....	do.
ang	and got.....	do.
all	as well .....	do.
if	it off .....	do.
shes	she has.....	do.
by	be my. ....	do.
cout	cut out.....	do.
proppit	propped it.....	do.
return Eand	return East and.....	do.
searth	search for truth.....	do.
af	as if.....	do.
bug	begun on.....	do.
concrea	concrete realities.....	Copying
pieches	pieces of machinery.....	Dictation
host	historical .....	Copying
in the outf	in the outward form.....	do.
grach	graphic characters .....	Composition
rad	rapidly.....	do.



OF WORDS.		
<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
I stand reason	{ I stand ready to make large sacrifice on be- half of the kingdom, where I see a reason for such sacrifice. }	Composition
means in end	{ means in mind and end in view..... }	Dictation
made of	made up of.....	Composition
{ The first chapter of the fifty-fifth chapter	{ The first verse of the fifty-fifth chapter..... }	do.
{ Hearing is a time-sense (space, a space-sense.)	{ Hearing is a time-sense (vision a space-sense) }	do.
interpretation to	interpretation due to.....	do.

and substitutions, greatly exceeds that of completed errors, such as exchanges. This is due to the fact that they are often recognized before they have been fully carried out, the incorrectness or strangeness of the expression calling attention to them. The following are examples of persistent substitution: 'Müller and H. Müller' was written for 'Müller and H. Weber,' 'superficial superficial' was written for 'superficial appearances.' The following are anticipative substitutions: 'ball and joc' was spoken for 'ball and socket joint,' 'Put plustard' was spoken for 'Put mustard and flour in the plaster.' Further examples will be found in Tables XXIII., XXIV., XXV. and XXVI.

TABLE XXVI.

## Oral Anticipatory Substitutions.

## OF LETTERS OR WORD-PARTS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
external	external organization.....	Conversation
partakin	partaken in.....	do.
the prid	the proud titles.....	Reading aloud
between filling	between feeling and willing	do.
collooding	colliding on the loop.....	Conversation
high-praced	high-priced places.....	do.
ter	tireless energy.....	Lecture
similar	similar incubus.....	Reading aloud
spice	space and time.....	do.
remoun	remain profoundly.....	do.
benoth	beneath those.....	do.
sa	so may.....	do.

divinely appint	divinely appointed advisor. Pulpit utterance
kets	pots and kettles..... Conversation
accustim	accustom him..... do.
spouted	spotted trout..... do.
no occusion	no occasion for a bugle..... do.
applosive	applausive Rome..... Reading aloud
befur	before the bifurcation..... do.
escup	escaped its clutches..... do.
ice-borgs	ice-bergs are formed..... do.
overthrou	overthrow of Louis XIV.. do.
imitet	imitating perfections..... do.
humin	human interest..... do.
a bast	a vast battlefield..... do.
chrimitive	primitive Christians..... do.

## OF WORDS.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
expersons	expert persons.....	Conversation
He shall judge the right	{ He shall judge the people righteously. }	Public reading
to members	{ to remember in such an important connection the members. }	do.
the buseful	the beautiful is as useful....	do.
forge like two	{ beat like two forge hammers. }	do.
{ Jael took her into her private apartments	Jael took him into her private apartments. }	Reading aloud
Put down your mouth	{ Put down your ear to my mouth. }	Conversation
{ It's funny how this present	{ It's funny how this smell is everywhere present. }	do.
{ which he said he would not hear	{ which he said he would not want to hear. }	do.
tropic of Cancercorn	{ tropic of Capricorn and of Cancer. }	do.
cook house	{ cook meals and keep house. }	do.
anywhere	anyone where.....	do.
{ Put my coat in your pocket	{ Put my cup in your coat-pocket. }	do.
I bought three dollars	{ I bought my dress for three dollars. }	do.
even through	even so through.....	do.
{ The strength of the people	{ The strength of the Church is in the Christian people. }	do.
{ Carried his mouth into his bosom	{ carried his hand into his bosom and thence to his mouth. }	do.
Mrs. eyes	Mrs. Bingham's eyes.....	do.

The other form of coalescence is the case in which two elements combine into one by modification and coalition, forming a hybrid. What are called exchanges may be regarded as belonging here, when (as is usually the case) the whole word or passage involved is to be taken as the psychological unit. Of this type of coalescence Mr. Stout seems to give no distinct

TABLE XXVII.

Graphic Examples of Coalescence which Involves the Modification of a Vowel or Consonant.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
and god	and got.....	Crossed the 'd.'
independent dissue	independent tissue.....	do.
nevey	never ceasing.....	{ The last letter of 'nevey' had characters which partook partly of the 'y' and partly of the 'g.'
describing	describing	
loke	love and hate	Ditto of 'p' and 'b.'
multijll	multiplicity.....	{ Formed a hybrid of the 'p' and 'l,' which can be here represented only by 'j.'
Egyj <sup>1</sup>	Egypt.....	
ainder	are under.....	{ The 'a' and 'u' were run together: this can be represented here only by 'i.'
sonee	are some.....	
Gefülle	Gefühle .....	{ The third stroke of the 'm' was made into an 'e.'
say	saw .....	
chiel	child .....	{ The 'w' had a descending loop added to it.
ofjective	objective .....	

treatment. It would seem that he had in mind only, or chiefly, examples of what we have called coalescence by substitution. The word 'coalescence' certainly may be restricted to cases such as these, though it would appear that the word 'overlapping' would have been a better term if a distinction were to be made, reserving the term 'coalescence' strictly for cases of coalition by modification. We prefer, however, to retain Mr. Stout's term, using it in the more general sense as covering all cases in which

<sup>1</sup> This and those which follow are experimental errors.



either substitution or modification takes place. Coalescence would perhaps be equally well described by such terms as fusion, assimilation, coalition, reinforcement, modification; but Professor Stout's able discussion of the general phenomena here under consideration has given a certain definiteness of content to the word 'coalescence' from which it does not seem well to depart by the multiplication of terms. Common examples of modification occur in the learning of a new language, for instance in the imperfect pronunciation of English by foreigners, when, *e. g.*, they say 'zey' for 'they,' 'ze' for 'the,' 'vas' for 'what,' 'vone' for 'one,' 'zis' for 'this,' 'zat' for 'that,' 'zem' for 'them,' etc. Examples of coalescence by modification are found also in those cases where the pure word idea (*e. g.*, in reading a for-

## TABLE XXVIII.

Oral Examples of Coalescence which Involve the Modification of a Vowel or Consonant.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
our corkey	our turkey was cooking....	{ The 'o' in 'corkey' was pronounced as in 'cork.'
The Wêst	The East Indies or West...	{ The 'e' in 'West' was pronounced as in 'fête.'
Bely Attiden	Betty Alden.....	{ The 'd' and the 'tt' were run together.
by-strââts	{ by-streets known only to Simon.....	{ The vowel-sound in 'strââts' was pronounced as in 'rays'
vergant pastoors	verdant pastures.....	{ The coalescence resulted in the modification of the 'u' into an 'oo' sound.
extêrior tintacles	exterior tentacles.....	{ The coalescence resulted in the modification of the 'ê' of 'exterior' into 'ê' (as in 'ten') and of the 'ê' of 'tentacles' into 'i' (as in 'tin.')
ignêma	enigma.....	{ The 'e' was pronounced as in 'anêmic.'
simping	sinking ship.....	{ The 'p' was anticipated, and the 'm' substituted for 'n,' 'np' being unpronounceable.

TABLE XXIX.  
Examples of Graphic Exchange.

## WITHIN THE WORD.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
bibilography	bibliography.....	Composition.
sucidie	suicide .....	do.
ear	era.....	do.
gential	genital .....	do.
viogr	vigor.....	do.
form	from .....	do.
ebliever	believer .....	do.
dulaistic	dualistic .....	do.
scrace	scarce .....	do.
recokning	reckoning .....	do.
exictory	excitory.....	do.
cyldiner	cylinder.....	do.
olfacort	olfactories.....	do.
Charlottwon	Charlottown .....	do.
pead	pedagogy.....	do.
detial	detail.....	do.
roost	roots.....	do.
Montog	Montgomery .....	do.
anidequate	inadequate.....	do.
tup	put.....	do.
spesific	specific.....	do.
regural	regular .....	do.
next spet in develop	next step in development.	do.
slain	snail.....	do.
excitation	excitation .....	do.
helioprotiotprotism	heliotropism.....	do.
dise	side .....	do.
sdie	side.....	do.
Corcondat	Concordat of Worms.....	do.
intellecutal	Intellectual .....	do.
Neadenr	Neander.....	do.
reap	repeating.....	do.
ser	reservation .....	do.
obth	both.....	do.

## INVOLVING TWO OR MORE WORDS.

ate rany	any rate.....	do.
id dit	it did.....	do.
no dot	do not.....	do.
so n	no subconscious.....	do.
dee sict	see dictionary.....	do.
If I no d	If I do not.....	do.
Leo-N	Neo-Lamarckians .....	do.
are so arranged	are arranged so.....	do.

eign language) becomes modified in accord with some meaning-idea, which nevertheless is recognized as irrelevant. This gives rise to a great part of the ludicrous mistakes made by beginners in the study of a foreign language (which uses the same form of language symbols). Examples of vowel and consonant modification in lapses are exhibited in Tables XXVII. and XXVIII.

Then there are the errors alluded to above, *exchanges*, in which the psychological unit of speech or writing is altered in some way from the usual or normal form; this includes errors which vary from what are here called inversions to exchanges which involve even groups of words. An exchange is the reciprocal substitution of letters, word-parts, words, or groups of words. That is, they may occur within the word or take place between two words or groups of words. They are chiefly oral and anticipatory, because, writing being slower than speech with most persons, the error in the latter case is not allowed to reach this relatively complex form. Exchanges of consecutive forms are called inversions. "The hang wires loose," "The air's roomed out," "I don't feel a good bit," "My thins are sole," "They saved the last till the best," "Put the pot on the cover," "Put some stove in the coal," are examples of verbal exchanges. 'The two-sloed toth,' 'five-gollar dold-piece,' 'blush a brue coat,' 'that thy lays may be dong in the land which the Lord thy God giveth thee' are examples of literal exchanges. Tables XXIX. and XXX. furnish further instances.

## TABLE XXX.

## Examples of Oral Exchange.

WITHIN THE WORD.		
<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
transcendent <sup>1</sup>	transcendent .....	Public speaking.
plotoprasm	protoplasm .....	Conversation.
intripidity	intrepidity .....	Reading aloud.
Conolian	Colonial .....	Conversation.
ennaxation	annexation .....	do.
donimoes	dominoes .....	do.
chulfeerness	cheerfulness .....	do.
direlect	derelict .....	Reading aloud.
orren	erroneous .....	do.
knacksap	knapsack .....	do.



wist	wits.....	Conversation
Itanial	Italian.....	Reading aloud
Cenilli	Cellini.....	do.
regural	regular.....	Conversation
revelant	relevant.....	do.
municifent	munificent.....	do.
Colyparp	Polycarp.....	do.
codemy	comedy.....	do.
evelate	elevate.....	do.
homeogenous	homogeneous.....	do.
disintregation	disintegration.....	do.
pom	compact.....	do.
standstone	sandstone.....	do.
pretesdin	predestinates.....	do.
coborrative	corroborative.....	do.
desuctive	seductive.....	do.
palarerrogram	parallelogram.....	do.
flutterby	butterfly.....	do.
Swegenbordian	Swedenborgian.....	do.
hyptonized	Hypnotized.....	do.

INVOLVING LETTERS, OR WORD-PARTS, IN THE CASE OF TWO OR MORE  
WORDS.

thick quinkly <sup>2</sup>	think quickly.....	do.
the clack's trear	the track's clear.....	do.
make a noyful joise	{ make a joyful noise unto the Lord. }	Public reading
beth mesod	best method.....	Conversation
a jog-lammed creek	a log-jammed creek.....	Reading aloud
paly curte	curly pate.....	Conversation
{ What kind of a weel has Will got? }	{ What kind of a wheel has Will got? }	do.
{ This is bad weatter for buther }	{ This is bad weather for butter. }	do.
You'd metter bake <sup>1</sup>	{ You'd better make a business of it }	do.
Baby is stough and trong	Baby is tough and strong..	do.
thight brought	bright thought.....	do.
fauld fount	found fault.....	do.
blings gradness	brings gladness.....	do.
dur-nayses	day-nurses.....	do.
{ The rabbits went hip- ping and skopping }	{ The rabbits went skip- ping and hopping. }	do.
reats are rool	roots are real.....	Reading aloud
slamefully shandered	shamefully slandered.....	do.
growsing and brazing	browsing and grazing.....	do.
Sir, shaid se	Sir, said she.....	Singing
The miller squake, poth he	The miller spake quoth he	do.
wasked brikly	walked briskly.....	Reading aloud
greet beens	beet greens.....	Conversation

unhold horrors	untold horrors.....	Conversation.
docking starnier	stocking danner.....	do.
merryd whistle tunes	merry whistled tunes.....	do.
martlett Baddened	Bartlett maddened.....	Reading aloud.
lave-wength	wave-length .....	Lecture.
hastened in return	returned in haste.....	do.
I had to top stown down	I had to stop down town...	do.
the stun sill shines	the sun still shines. ....	do.
the facred sire burns	the sacred fire burns.....	Singing.
stepply stateings.	stately steppings.....	Reading aloud.
failly wholed	failed wholly.....	do.
Danto's Inferne	Dante's Inferno.....	do.
the ort of ar	the art of oratory.....	do.
dri some buyed beef	buy some dried beef.....	Conversation.
Bibliosacra Theca	Bibliotheca Sacra.....	do.
{ We're quartered in rath-	We're crowded in rather {	do.
{ er close crowders	close quarters. }	
hore bo	bore holes.....	do.
booh, pah	pooh, bah.....	do.
quine a fality	fine a quality.....	do.
chlodium soride	sodium chloride.....	do.
norishment is stoured up	nourishment is stored up..	do.
{ Why don't you wed you	Why don't you wear {	do.
{ rear one?	your red one. }	
{ Where was his cattle-	Where was his cattle- {	do.
{ lard yocated?	yard located ? }	

## VERBAL EXCHANGES.

the water the wetter	the wetter the water.....	Conversation.
may not be	may be not.....	do.
{ Courage in sword and	Courage in heart and a {	Singing.
{ a heart in hand	sword in hand. }	
when he knew of it	when he knew it of.....	Conversation.
In dark's death	In death's dark vale.....	Singing.
{ There goes the hill on	There goes the bell on {	Conversation.
{ the bell	the hill. }	
{ All the cities in the	All the Churches in the {	do.
{ Church	city. }	
{ Perhaps it will settle to	Perhaps it will help to {	do.
{ to help	settle. }	
Oh, Is this a mat-lamp?	Oh, Is this a lamp-mat?	do.
be want to put in water	want to be put in water...	do.
{ I took the tables from	I took the glasses from {	do.
{ three glasses	three tables. }	
{ You haven't got all the	You haven't got all the {	do.
{ pool out of that trout	trout out of that pool. }	
{ These opposite marks	These opposite points {	do.
{ point	mark. }	
He speaks in her of	He speaks of her in terms.	do.
Is equal Frank to it?	Is Frank equal to it?.....	do.
to be belong	to belong, to be....	do.

golden mouth in his spoon    golden spoon in his mouth. Conversation.

Matthew on Broadus    Broadus on Matthew..... do.

{ Maybe I can leave her    Maybe I can leave him } do.  
   { with him                    with her.                    }

Its apriori time, form    Its apriori form, time..... do.

The trays in each weight    The weights in each tray... do.

{ families from different    children from different } do.  
   { children                    families.                    }

yellowed teeth            teeth yellowed..... do.

who is by force wakened    who by force is wakened... do.

{ so that they were all of    so that they all of them } do.  
   { them                        were.                        }

afterwards it behooves    { afterwards behooves it } do.  
                                   { this one.                    }

and make a gift of me to    and make a gift to me of... do.

in the man of the mind    in the mind of the man.... do.

cellar in the water            water in the cellar ..... do.

You will use all up        { You will use up all my } do.  
                                   { matches.                    }

because they not            not because they occur.... do.

{ I'm sorry I've only got    I'm sorry I've got the } do.  
   { the                        only chair.                    }

I thought he said so        I said he thought so..... do.

{ Boys never care para-    Boys never carry para- } do.  
   { sols, neither do they    sols, neither do they }  
   { carry for them            care for them.                }

{ Put your head under    Put your hand under his } do.  
   { his hand                    head.                        }

## V. SUGGESTIONS FOR A PSYCHOLOGY OF THE LUDICROUS.

Several theories of the ludicrous have been current in psychological literature. On analysis, each is found to emphasize an important truth necessary to an adequate explanation of the phenomena. In the following analysis are stated the leading factors which must be recognized in any comprehensive theory. The purpose of giving this analysis in the present connection, is not for the statement of a theory, however, but for the cataloguing in a reasonably accurate way of certain comparatively new data of lapses which may be taken into consideration in the development of such a theory. These features which are essential to all experience of the ludicrous are: (1) *Novelty and abruptness*:—wit, repartee, the jest, the appreciation of the new and the wonderful; (2) *Unexpected contrast* (great expectations unfulfilled):—the descent from the sublime to the ridiculous, the pun or play on words, antithetic wit, Irish bulls, unusual analogies, etc.; (3) *Incongruity* (irrelevancy, or



unexpected relevancy):—(a) the incongruity of imperfection or incompleteness—*e. g.*, naiveté, crudities, absent-mindednesses, certain forms of lapses, etc., (b) the incongruity of deformity or abnormality—*e. g.*, the grotesque (unintentional disproportion), and the burlesque (intentional disproportion), the odd, droll, bizarre, the ludicrous in situation and incident, mannerisms, slang, certain other forms of lapses, etc., (c) incongruity of immorality—*e. g.*, the obscene story or joke, suggestive situation or picture, and some lapses; (4) *Agreeableness or pleasure-tone*:—(a) on the psychological side, this is the consciousness of superiority; it may be antipathetic (wit), or sympathetic (humor)—as examples of the first, cf. the puzzle, the riddle, conundrum, charade, practical joke, ridicule, caricature, cartoon, satire, sarcasm; as examples of the second, cf. pleasantry of any sort, general geniality, etc.; (b) on the physiological side this means summation of sensations with subsequent free irradiation or discharge, which, within certain limits of intensity, is pleasurable.<sup>1</sup>

It is not the purpose here to go into a defense of the above analysis. It is sufficient if it serves the general end for which it is intended. Since the purpose is simply to show the relations of lapses in a psychology of the ludicrous, only those divisions in the analysis will be touched upon which are directly involved. The above outline is given so as to show the point of view from which the subject is approached. Of these divisions the first and most obvious one involved in the case of lapses is what has been called the Deformity Theory of the Ludicrous. Lapses which are ludicrous belong almost wholly within this category. The Deformity Theory in its essence dates back to Aristotle, who connected the effect of comedy with the presentation of meanness or deformity (provided that this did not go so far as to excite painful feelings). It has been further developed by various writers in modern times, notably by Hobbes and Bain. In the case of deformity-lapses which are ludicrous, either the expression is imperfect or incomplete, that is, crude, as when a person begins to say an absurd thing

<sup>1</sup>This is C. L. Herrick's 'Summation-Irradiation' Theory of Pleasure-Pain: cf. *Jour. Compar. Neurology*, March, 1898.

and checks himself; or the expression may be distinctly abnormal or mal-formed in some way; these are, respectively, illustrated by what have been called, above, substitutions and exchanges. Again, the abruptness, unexpectedness and contrast also play their part, though it is not possible, of course, to show cases which illustrate these aspects alone; it is true of most ludicrous lapses. Such cases are very frequent, however, as when a person inadvertently says just the thing which he most wishes to conceal—which, often, as we say, would be ludicrous, if it were not so serious. A slip of the tongue has often plunged a guest or host into momentary despair, only to be followed by an explosion of uncontrollable laughter. Nowhere are such errors felt to be more ridiculous than in the solemn environment of the church or place of religious worship. Table XXXI. is a tabulation of actual errors which caused an outburst of laughter when they were perpetrated. Of course, it is not to be expected that, in this connection, stripped of all the emotional associations which marked their original occurrence, they will all appear ludicrous; but they will serve to show the nature and variety of errors and the forms which they took in eliciting this experience.

The errors in this tabulation are mostly oral, because in the case of graphic errors the mistake is usually detected and inhibited before it attains a sufficiently abnormal form to be ludicrous—a confirmation of the general theory on the physiological side, adopted in the above analysis, that a certain summation or resistance must precede the irradiation or discharge in the (pleasurable) sense of the ludicrous. In the case of oral lapses it is the *sound* of the expression, of course, which is usually the occasion of its ludicrous nature. In the following instance, for example, no one notes the error as especially funny—when a man spoke of a ‘wesert daste’ instead of a ‘desert waste.’ But in the case which follows, which is taken from the Joke Column in a newspaper, it is the incongruous meaning which we get from the *sound* of the word which makes us laugh. If we did not hear the mistake, but simply saw it upon the printed page with the exact letters transplaced which were involved in the error, it would not create any merriment.

But when written as it sounds and is first apprehended, we get the ridiculous effect. It is as follows: "A dear old college gentleman had occasion to reprimand an undergraduate who had wasted two consecutive terms in youthful follies. After lecturing the delinquent severely in his queerly high-pitched voice, the dean finished by saying: 'I am sorry to have to speak so severely to you, but I am credibly informed that you have broken many rules of the college; you have been incorrigibly lazy; and, to cap it all, you have deliberately tasted two worms.'" The newspaper reporters or the originators of the 'quips and quirks' in the Joke Columns of our periodicals, are thus psychologically quite right in spelling the falsely pronounced words and sentences in their incongruous form, as *heard* rather than as seen, since it is just this tendency of the individual to make some sort of meaning out of his errors which is the source of their ludicrous nature. The above mistake would not seem so ludicrous if the exchanged letters were literally transcribed, "tasted two werms." The following examples, which, like the above, may or may not be 'founded on fact,' may be compared with the tabulation of ludicrous lapses. No attempt is made to give the 'setting.' "Occupew my pie" for "Occupy my pew," "Great aches from little toe-corns grow" for "Great oaks from little acorns grow," "From Iceland's Greasy Mountains" for "From Greenland's Icy Mountains," 'Bon the Japtist' for 'John the Baptist,' 'God save the Weasel' and 'Pop Goes the Queen' for 'God save the Queen' and 'Pop Goes the Weasel,' 'A little of that stink puff' for 'A little of that pink stuff,' 'Three miles as the flow cries' for 'Three miles as the crow flies,' 'The froth of Dukeingham' for 'The Duke of Frothingham,' 'Not one tot or jittle' for 'Not one jot or tittle,' 'peedles and nins' for 'needles and pins,' "Twinkle, Twinkle, Little Star, How You wonder what I are," etc., etc.

Various writers have suggested, what may be an application of the Deformity Theory, that there is something distinctly immoral in the ludicrous. This is apparently confirmed by the unrestrainable merriment often caused by some 'slip' in the ordinary civilities of life. Charles Lamb has suggested that a leading element in the enjoyment of certain forms of Comedy



consists in the fact that they free us from the burden of our habitual moral consciousness. Everyone must admit, though he may not be able to explain, the tendency of persons naturally to laugh at what is bad rather than at what is good. In the tabulation (Table XXXI.) will be found cases in which, to a greater or less degree, the more obviously immoral element enters. A few only, and these not the best examples, are given. Many are omitted which might have been cited, simply because they border on indecency or vulgarity, and no useful end could be subserved in their perpetuation. When it is remembered that those which are here appended are ethically and æsthetically the 'cream' of this type of ludicrous lapses it can be understood how real and important, psychologically, is this aspect of the subject. In all these cases there was, on the part of the persons making the errors, a feeling of moral or æsthetic revulsion of greater or less intensity connected with the recognition of the error. They are classifiable in a general way according as they involve the reference irreverently to something sacred, reference to something indecent, or reference to something vulgar (not necessarily falling under the preceding head). Examples of a similar nature from current literature, in which the immoral element is intentionally introduced, are, of course, myriad.

## TABLE XXXI.

## List of Ludicrous Lapses.

## EXAMPLES OF 'DEFORMITY' LAPSES.

<i>Error.</i>	<i>Correct Form.</i>	<i>Remarks.</i>
in raising bead <sup>1</sup>	In raising bread.....	Conversation.
trickled the ear-drops	trickled the tear-drops.....	do.
{ as the water runs off a	{ as the water runs off a }	do.
{ duck back	{ duck's back. }	
various salts and ases	various salts and gases.....	do.
spring ticken	{ spring chicken, ten cents }	do.
	{ a pound. }	
when the formances	{ when the performances }	do.
	{ begin. }	
bother <sup>2</sup>	both taken together.....	do.

<sup>1</sup> Oral ellipsis :<sup>2</sup> Oral transposition.

snapping	{ snapping the numerous bonds. }	Reading aloud.
{ down where the apple-ossoms blow <sup>1</sup> }	{ down where the apple-blossoms grow. }	Conversation.
It is a thad	It is a sad thing.....	do.
an inch and a high	an inch and a half high....	do.
I swell	I swear I will.....	do.
sourth	source of our faith.....	do.
It is idle to theeek	{ It is idle to seek a third } means.	do.
she swooned and swépt	she swooned and slept.....	Reading aloud.
juicey	juice milky.....	do.
Mithter	Mister Brithwood.....	do.
the sunshet	the sunset shone.....	do.
I must to Paris, he gashped	I must to Paris, he gasped	do.
Hawaii and Helsewhere	Hawaii and elsewhere.....	Conversation.
ice ouce on the bucket	ice out on the bucket.....	do.
It's very unArthurdox	{ It's very unorthodox for } Arthur to do so. }	do.
Profether	Professor Seth.....	do.
I bought three dollars	{ I bought my dress for } three dollars. }	do.
My weather	{ My over-shoes will get } all worn out if this weather continues. }	do.
cook house	cook meals and keep house	do.
{ Inspiration may be smelled <sup>2</sup> }	{ Inspiration may be spelled with a small 'i.' }	Lecture.
Put down your mouth	{ Put down your ear to my } mouth.	Conversation.
{ Put my coat in your pocket }	{ Put my cup in your coat- } pocket.	do.
Miss Crust	Miss Crew likes the crust..	do.
I'll feel it with my fewt	{ I'll feel it with my foot } in a few minutes. }	do.
that thame	that same thing.....	do.
hank	heart sank.....	do.
livers	lakes and rivers.....	Reading aloud.
Quiss	Quick, a kiss.....	Conversation.
kets	pots and kettles.....	do.
such terrible suspenders	such terms as conspectus.	Experimental.
Ours would bump	{ Ours would break and } the pump.	Conversation.
I fool so feelish <sup>3</sup>	I feel so foolish.....	do.
feak and weeble	weak and feeble.....	do.
the ox and the ax	the ox and the ass.....	do.
property pie	proper piety.....	do.
these drumb butes	these dumb brutes.....	do.

<sup>1</sup> Oral substitution.<sup>2</sup> Oral substitution.<sup>3</sup> Oral exchange.

{ She went to the Asho- theary's pop to get a cint pup }	{ She went to the Apothe- cary's shop to get a pint cup. }	Conversation.
I don't feel a good bit	I don't feel a bit good.....	do.
Bass the pasket	Pass the basket .....	do.
Sheats and Kelley	Keats and Shelley.....	do.
{ She went out on the corch to pool }	{ She went out on the porch to cool. }	do.
{ He snatched up his ind- stank }	{ He snatched up his ink- stand. }	do.
the Nalley of the vile	The valley of the Nile.....	do.
a dat with a dosh	a dot with a dash.....	do.
beath-ded scenes	death-bed scenes.....	do.
Let the finishment punt	{ Let the punishment fit the crime. }	do.
finf and thumber	thumb and finger.....	do.
bold coiled ham	cold boiled ham.....	do.
the cocking of the crow	the crowing of the cock....	do.
lest my Lordy by <sup>1</sup>	lest my Lord by.....	Composition.
some sick, some sin	dome thick, some thin.....	do.
Five Fooling	Five Foolish Virgins.....	do.
Anothering	Another thing.....	do.
pats	past.....	do.
roost	roots .....	do.
no dot	do not.....	do.
may bet	may be set.....	do.

## EXAMPLES OF 'IMMORALITY' LAPSES.

our gods	our dogs .....	Conversation.
praying on the street	playing on the street.....	do.
sicked little winner	wicked little sinner.....	do.
Oh Glod	Oh God, It makes us glad.	Prayer.
{ The purple fluid within the hells }	{ The purple fluid within the cells had. }	Reading aloud.
Simon Pether	Simon Peter saith.....	do.
Lord, shôw	Lord, how long shall.....	do.
Jab and Dôvid	Job and David.....	do.
spaul	Paul speaking.....	do.
{ that thy lays may be dong }	{ that thy days may be long in the land which the Lord thy God giveth thee. }	do.
in helebrate	in heaven celebrate.....	do.
and god	and got.....	do.
Has the belly	Has the jelly been passed?	Conversation.
The bulhouse	{ The schoolhouse is the bulwark of civilization. }	do.
stomich	to some extent.....	do.
We've adammed	We've abandoned.....	do.

<sup>1</sup> Graphic errors.



## VI. CONCLUSION.

In summarizing the results of this 'Study,' we must repeat what has been said before, that the conclusions arrived at are corroborative of existing principles, rather than the discovery of new laws. Perhaps the most important and striking illustration of this is the functional rather than *merely* analytic interpretation which the phenomena of lapses require. It is at present impossible to carry this functional treatment into the details of specific cases with any degree of certainty because the method itself, at least when applied in psychology, is still undeveloped. But wherever the functional method can be applied, even in a general way, it always illumines the content analysis. Another such corroborative result is the striking confirmation which the analysis of lapses furnishes, of the bipartite analysis of 'conscious elements.' Search how you will, you never find an error, of the sort here treated, reported in consciousness in any other terms than those of sensation-affection (cognition-feeling).

The examination and comparison of lapses and sense-illusions, moreover, have brought out with great clearness the arbitrary nature of the ordinary distinction between the 'sensory' and 'motor' aspects of the organic circuit. These distinctions, as ordinarily employed, give us no psychological *differentia* at all. The lapse shades over into the sense-illusion, according to the point of view from which it is regarded. But viewed simply as the difference in the prominence of the different phases of the same process, this distinction has a positive value. It is preferable to substitute the term 'kinæsthetic' for the term 'motor' wherever this antithesis is not implied or intended in the use of the latter word.

Attention, it has been seen, must be regarded as the psychical correlate, or rather the psychological statement, of organic adjustment and re-adjustment. Attention goes to, or is developed at or in, the point of difficult adjustment. Ease of adjustment, ready adaptation, mean reduction of consciousness and thus of attention. Conscious experience is a constant disturbance of the tendency toward equilibrium between the automatic and the attentional processes, between habit and ideal. Errors, or lapses,

appear in the re-adjustment in the tension between these two processes. They appear in the breaking up of a habit. Experience is thus better described as a series of errors or mistakes or only approximately perfect adjustments than as a series of 'correspondences.' Just as walking is a series of incipient falls forward, checked at the right juncture for the production of the resultant locomotion, so our normal (ordinary) speech and writing experience is simply a perpetual process of gradual improvement on our past mistakes, a mere refinement of the grosser errors of childhood and infancy by the use of the very flexible and variant symbols which make up our language. There is, perhaps, not a person who could write a page, or make a ten-minute speech, without falling into some form of lapse.<sup>1</sup> Whether the perfect life would mean perfect correspondence with the environment, as certain current theories would seem to imply, or whether such perfect correspondence would not be to remove the very condition of consciousness, it is not for us here to inquire. But certainly the phenomena of lapses suggest rather that the higher development of experience will mean, not frictionless adjustment, but wider range in and greater control of such adjustment.

The similarity between the facts of lapses and of aphasia has been pointed out, mainly to show how our ordinary experience borders on what we call the abnormal or pathological at every point. If the Alienists can make the progress in the interpretation and diagnosis of cerebral disease which their past achievements warrant us in anticipating within the next few decades, there is no reason why the phenomena of lapses should not come in for their share in that interpretation.<sup>2</sup> What the bearing of

<sup>1</sup> Cf. the impossibility of making an absolutely accurate scientific measurement. L. F. Barker (*The Nervous System and Its Constituent Neurons*, 1899, p. 219), speaking of the 'memory traces' in the neurones of the cerebral cortex, says, "Far from being surprised that the reproduction of past experiences in consciousness is occasionally unfaithful, we can only wonder how it can reach the degree of accuracy with which we are familiar."

<sup>2</sup> For a critique of the theory of neurone retraction, the only theory that has been advanced which attempts to cope with phenomena such as lapses, from the physiological side, cf. a paper by the author entitled 'A Digest and Criticism of the Data upon which is based the Theory of the Amoeboid Movements of the Neurone,' *Jour. of Compar. Neurol.*, Vol. X, No. 2.

this may be upon economy in our educational methods, especially instruction in the languages, it would be premature to predict. A further genetic study of lapses is calculated to throw much light upon this side of the subject. But that important results must flow, ultimately, from the study of the relation of the language symbols to the psychogenesis of 'meaning,' no one will question. Especially deserving of experimental, and particularly of genetic study, is the proposition that the psychological unit is ultimately to be stated in terms of the activity-experience; that is, that the psychology of 'meaning' is ultimately reducible to a statement in terms of kinæsthetic sensations.

Attention is called also to the restatement and fresh illustration of the so-called laws of similarity and contiguity, here stated in the form of a single law, that contiguous similars tend to coalesce. It is not presumed that the last word has been said on this subject; but certainly a great many words might have been saved the saying, if these laws had been brought to the test of concrete instances such as are here tabulated.

Lapses, further, are treated under the head of Conflict and Coalescence, as illustrations of the general Herbartian-Stout conception of the competition and synthesis of 'mental systems.' The notion of coalescence, it is seen, must be widened to embrace what have here been called examples of coalescence by modification, as well as coalescence by substitution.

And, finally, it is apparent that among current theories of the ludicrous, the 'Deformity' and the 'Immorality' theories find distinct support in the phenomena of lapses.



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## The Mental Life of the Monkeys

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## PREFACE.

The literary form of this monograph is not at all satisfactory to its author. Compelled by practical considerations to present the facts in a limited space, he has found it necessary to omit explanation, illustration and many rhetorical aids to clearness and emphasis. For the same reason detailed accounts of the administration of the experiments have not always been given. In many places theoretical matters are discussed with a curtness that savors of dogmatism. In general when a theoretical point has appeared justified by the evidence given, I have, to economize space, withheld further evidence.

There is however to some extent a real fitness in the lack of clearness, completeness and finish in the monograph. For the behavior of the monkeys by virtue of their inconstant attention, decided variability of performance, and generally aimless, unforetrollable conduct would be falsely represented in any clean-cut, unambiguous, emphatic exposition. The most striking testimony to the mental advance of the monkeys over the dogs and cats is given by the difficulty of making clear emphatic statements about them.



## TABLE OF CONTENTS.

Introduction.....	1
Apparatus.....	5
Learning without Tuition.	
Tests with Mechanisms.....	9
“ “ Signals.....	20
Experiments on the Influence of Tuition.	
Introduction.....	31
Imitation of Human Beings.....	34
“ “ Other Monkeys.....	40
Learning apart from Motor Impulses .....	42
General Mental Development of the Monkeys.....	54

## THE MENTAL LIFE OF THE MONKEYS; AN EXPERIMENTAL STUDY.

### INTRODUCTION.

The work to be described in this paper is a direct continuation of the work done by the author in 1897-98 and described in Monograph Supplement No. 8 of the *PSYCHOLOGICAL REVIEW* under the heading, 'Animal Intelligence; an Experimental Study of the Associative Processes in Animals.' This monograph affords by far the best introduction to the present discussion and I shall therefore assume an acquaintance with it on the part of my readers.

It will be remembered that evidence was there given that ordinary mammals, barring the primates, did not infer or compare, did not imitate in the sense of 'learning to do an act from seeing it done,' did not learn various simple acts from being put through them, showed no signs of having in connection with the bulk of their performances any mental images. Their method of learning seemed to be the gradual selection of certain acts in certain situations by reason of the satisfaction they brought. Quantitative estimates of this gradualness were given for a number of dogs and cats. Nothing has appeared since the 'Experimental Study' to negate any of these conclusions in the author's mind. The work of Kline and Small<sup>1</sup> on rodents shows the same general aspect of mammalian mentality.

Adult human beings who are not notably deficient in mental functions, at least all such as psychologists have observed, possess a large stock of images and memories. The sight of a chair for example may call up in their minds a picture of the person who usually sits in it, or the sound of his name. The sound of a bell may call up the idea of dinner. The outside world also is to them in large part a multitude of definite percepts. They

<sup>1</sup>*American Journal of Psychology*, Vol. X., pp. 256-279; Vol. XI., pp. 80-100, 131-165; Vol. XII., pp. 206-239.

feel the environment as trees, sticks, stones, chairs, tables, letters, words, etc. I have called such definite presentations 'free ideas' to distinguish them from the vague presentations such as atmospheric pressure, the feeling of malaise, of the position of one's body when falling, etc. It is such 'free ideas' which compose the substance of thought and which lead us to perhaps the majority of the different acts we perform, though we do of course react to the vaguer sort as well. I saw definitely in writing the last sentence the words 'majority of the different acts' and thought 'we perform' and so wrote it. I see a bill and so take check-book and pen and write. I think of the cold outside and so put on an overcoat. This mental function 'having free ideas,' gives the possibility of learning to meet situations properly by thinking about them, by being reminded of some property of the fact before us or of some element therein.

We can divide all learning into (1) *learning by trial and accidental success*, by the strengthening of the connections between the sense impressions representing the situation and the acts—or impulses and acts—representing our successful response to it and by the inhibition of similar connections with unsuccessful responses; (2) *learning by imitation*, where the mere performance by another of a certain act in a certain situation leads us to do the same; and (3) *learning by ideas*, where the situation calls up some idea (or ideas) which then arouses the act or in some way modifies it.

The last method of learning has obviously been the means of practically all the advances in civilization. The evidence quoted a paragraph or so back from the Experimental Study shows the typical mammalian mind to be one which rarely or never learns in this fashion. The present study of the primates has been a comparative study with two main questions in view. (1) How do the monkeys vary from the other mammals in the general mental functions revealed by their methods of learning; (2) How do they, on the other hand, vary from adult civilized human beings.

The experiments to be described seem, however, to be of value apart from the possibility of settling crucial questions by means of the evidence they give. To obtain exact accounts of



what animals can learn by their own unaided efforts, by the example of their fellows or by the tuition of a trainer, and of how and how fast they learn in each case, seems highly desirable. I shall present the results in the manner which fits their consideration as arguments for or against some general hypotheses, but the naturalist or psychologist lacking the genetic interest may find an interest in them at their face value. I shall confine myself mainly to questions concerning the method of learning of the primates, and will discuss their sense powers and unlearned reactions or instincts only in so far as is necessary to its comprehension.

It has been impossible for the author to make helpful use of the anecdotes and observations of naturalists and miscellaneous writers concerning monkey intelligence. The objections to such data pointed out in the 'Experimental Study,' pp. 3-5, hold here. Moreover it is not practicable to sift out the true from the false or to interpret these random instances of animal behavior even if assuredly true. In the study of animal life the part is only clear in the light of the whole, and it is wiser to limit conclusions to such as are drawn from the constant and systematic study of a number of animals during a fairly long time. After a large enough body of such evidence has been accumulated we may be able to interpret random observations.

The subjects of the experiments were three South American monkeys of the genus *Cebus*. At the time of beginning the experiment No. 1 was about half grown, No. 2 was about one-fourth full size and No. 3 was about half grown. No. 1 was under observation from November, 1899, to February, 1900; No. 2 and No. 3 from October, 1900, to February, 1901. No. 1 was during the period of experimentation decidedly tame, showing no fear whatever of my presence and little fear at being handled. He would handle and climb over me with no hesitation. No. 2 was timid, did not allow handling, but showed no fear of my presence and no phenomena that would differentiate his behavior in the experiments discussed from that of No. 1, save much greater caution in all respects. No. 3 also showed no fear at my presence. Any special individual traits that are of

importance in connection with any of the observations will be mentioned in their proper places. No. 1 was kept until June, 1900, in my study in a cage 3 by 6 by 6 feet, and was left in the country till October, 1900. From October, 1900, all three were kept in a room 8 by 9 feet, in cages 6 feet tall by 3 long by 2.6 wide for Nos. 1 and 2, 3 feet by 3 feet by 20 inches for No. 3. I studied their behavior in learning to get into boxes, the doors to which could be opened by operating some mechanical contrivance, in learning to obtain food by other simple acts, in learning to discriminate between two signals, that is, to respond to each by a different act, and in their general life.

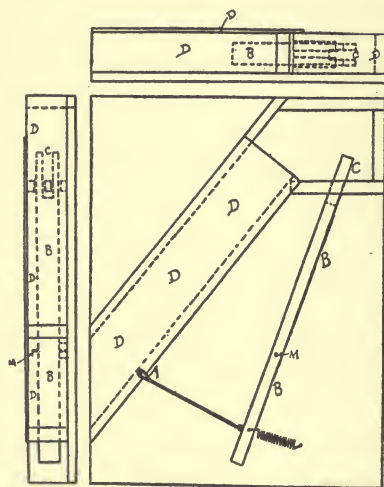


FIG. 1. *A*, loop; *BB*, lever, pivoted at *M*. A bit of food put in front of *C* would be thrown down the chute *DDD* when *A* was released.

Following the order of the 'Animal Intelligence,' I shall first recount the observations of the way the monkeys learned, solely by their own unaided efforts, to operate simple mechanical contrivances.

Besides a number of boxes such as were used with the dogs and cats (see illustration on page 8 of the *Animal Intelligence*), I tried a variety of arrangements which could be set up beside a cage, and which would, when some simple mechanism was set in action, throw a bit of food into the cage. Fig. 1 shows one of these. See description of *QQ* (ff) on page 8.

## APPARATUS.

The different mechanisms which I used were the following :

Box BB (O at back) was about 20 by 14 by 12 inches with a door in the front which was held by a bolt to which was tied a string. This string ran up the front of the box outside, over a pulley, across the top, and over another pulley down into the box, where it ended in a loop of wire.

Box MM (bolt) was the same as BB but with no string and loop attachment to the bolt.

Box CC (single bar) was a box of the same size as BB. The door was held by a bar about 3 by 1 by 5 inches which swung on a nail at the left side.

Box CCC (double bar) was CC with a second similar bar on the right side of the door.

Box NN (hook) was a box about the size of BB with its door held by an ordinary hook on the left side which hooked through an eyelet screwed into the door.

Box NNN was NN with the hook on the right instead of the left side.

Box NNNN was box NN with two hooks, one on each side.

Apparatus OO (string box) consisted of a square box tied to a string, which formed a loop running over a pulley by the cage and a pulley outside, so that pulling on the under string would bring the box to the cage. In each experiment the box was first pulled back to a distance of 2 feet 3 inches from the cage, and a piece of banana put in it. The monkey could, of course, secure the banana by pulling the box near enough.

Apparatus OOO was the same as OO, with the box tied to the upper string, so that the upper string had to be pulled instead of the lower.

Box PP was about the size of BB. Its door was held by a large string securely fastened at the right, passing across the front of the door and ending in a loop which was put over a nail on the box at the left of the door. By pulling the string off the nail the door could be opened.

Box RR (wood plug) was a box about the size of BB. The door was held by a string at its top, which passed up over the



front and top to the rear, where it was fastened to a wooden plug which was inserted in a hole in the top of the box. When the plug was pulled out of the hole the door would fall open.

Box SS (triple; wood-plug, hook and bar) was a box about the size of BB. To open the door a bar had to be pushed around, a hook unhooked, and a plug removed from a hole in the top of the box.

Box TT (nail plug) was 14 by 10 by 10 inches with a door 5.5 by 10 on the right side of the front, the rest of the front being barred up. The door was hinged at the bottom and fastened at its top to a wire which was fastened to a nail 2.5 inches long, which, when inserted in a hole 0.25 inches in diameter at the back of the top of the box, held the door closed. By drawing out this nail and pulling the door the animal could open the door.

Box VV (plug at side) was a box about 18 by 10 by 10 with the door held by a plug passing through a hole in the side of the box. When the plug was pulled out the door could be pushed inward.

Box W (loop) was 17 by 10 by 10 inches with a door 5 by 9 at the left side of its front hinged at the bottom. The door was prevented from falling inward by a wire stretched behind it. It was prevented from falling outward by a wire firmly fastened at the right side and held by a loop over a nail at the left. By pulling the loop outward and to the left it could be freed from the nail. The door could then be pulled open.

Box WW (bar inside) was 16 by 14 by 10 inches with a door 4 by 11 at the left of its front hinged at the bottom. The door could be pushed in or pulled out when a bar on its inside was lifted out of a latch. The bar was accessible from the outside through an opening in the front of the box. It had to be lifted to a height of 1.5 inches (an angle of about  $30^{\circ}$ ).

Box XX (bar outside) was about 13 by 11 by 10 inches with a door 7 by 8 on the left side of the front. The door was held in place by a bar swinging on a nail at the top, with its other end resting in a latch at the left side of the box. By pushing this up through an angle of  $45^{\circ}$  the door could be opened.

Box YY (push bar) was a box 16 by 8 by 12 inches with a door at the left of its front. The door was held by a brass bar

which swung down in front of an L-shaped piece of steel fastened to the inside of the door. This brass bar was hung on a pivot at its center and the other end attached to a bar of wood; the other end of this bar projected through a hole at the right side of the box. By pushing this bar in about an inch the door could be opened.

Box LL (triple; nail plug, hook and bar) was a box 10 by 10 by 13 with a door 3 by 8.5 at the left side. The door could be opened only after (1) a nail plug had been removed from a hole in the back of the top of the box as in TT, (2) a hook in the door had been unhooked, and (3) a bar on the left side had been turned from a horizontal to a vertical position.

Box Alpha (catch at back) was 11 by 10 by 15 with the door (4 by 4) in the left side of its front. The door was held by a bolt, which when let down, held in a catch on the inside of the door. A string fastened to the bolt ran across to the back of the box and through a hole to the outside. There it ended in a piece of wood 2.5 by 1 by .25 inches. When this piece of wood was pulled the bolt went up and the door fell open.

Box Beta was the same as NN except in size. It was 10 by 10 by 13 inches.

Box KK (triple; bolt, side plug, and knob) was a box 16 by 9 by 11 with a door at the left side of the front. The door was held by a bolt on the right side, a wooden plug stuck through a hole in the box on its left side and a nail which held in a catch at its top. This nail was fastened to a wooden knob (1 by 5 by .375) which lay in a depression at the top of the box. Only when the bolt had been drawn and the plug and knob pulled, could the door be opened.

Box Gamma (wind) was 10 by 10 by 13 inches with its door held by a wire fastened at the top and wound three times about a screw eye in the top of the box. By unwinding the wire the door could be opened.

Box Delta (push back) was 12 by 11 by 10 inches. Its door was held by a wooden bar projecting from the right two inches in front of it. This bar was so arranged that it could be pushed or pulled toward the right, allowing the door to fall open. It could not be swung up or down.

Box Epsilon (lever or push down) was 12 by 9 by 5 inches. At the right side of its front was a hole  $\frac{1}{2}$  inch broad by  $1\frac{1}{2}$  inches up and down. Across this hole on the inside of the box was a strip of brass, the end of one bar of a lever. If this strip was depressed  $\frac{1}{8}$  of an inch the door at the extreme left would be opened by a spring.

Box Zeta (side plug) was 12 by 11 by 10 inches. Its door was held by a round bar of wood put through a hoop of steel at the left side of the box. This bar was loose and could easily be pulled out, allowing the door to be opened.

Box Theta was the same as KK except that the door could be opened as soon as the bolt alone was pulled or pushed up.

Box Eta was like Alpha save that the object at the back of the box to be pulled was a brass ring.

Apparatus QQ (chute) consisted of a lever mechanism so arranged that by pushing in a bar of wood  $\frac{1}{4}$  to  $\frac{1}{2}$  an inch, a piece of banana would be thrown down a chute into the cage. The apparatus was placed outside the cage in such a way that it could be easily reached by the monkey's arm through the wire netting.

QQ (a) was of the same general plan. By turning a handle through 270 degrees food could be obtained.

QQ (b) was like QQ (a) except that  $2\frac{1}{2}$  full revolutions of the handle in one direction were necessary to cause the food to a. drop down.

QQ (c) was a chute apparatus so arranged as to work when a nail was pulled out of a hole.

QQ (d) was arranged to work at a sharp pull upon a brass ring hanging to it.

QQ (e) was arranged to work when a hook was unhooked.

QQ (f) was arranged to work when a loop at the end of a string was pulled off from a nail.

QQ (ff) was QQ (f) with a stiff wire loop instead of a loop of string.



EXPERIMENTS ON THE ABILITIES OF THE MONKEYS TO  
LEARN WITHOUT TUITION.

I will describe a few of the experiments with No. 1 as samples and then present the rest in the form of a table. No. 1 was tried first in BB (*o* at back) on January 17, 1900, being *put inside*. He opened the box by pulling up the string just above the bolt. His times were .05, 1.38, 6.00, 1.00, .10, .05, .05. He was not easily handled at this time, so I changed the experiment to the form adopted in future experiments. I put the food inside and left the animal to open the door from the outside. He pulled the string up within 10 seconds each time out of 10 trials.

I then tried him in MM (bolt). He failed in 15. I then (January 18th) tried him in CC (single bar outside). He got in in 36.00 minutes; he did not succeed a second time that night but in the morning the box was open. His times thenceforth were 20, 10, 16, 25 and on January 19th, 40, 5, 12, 8, 5, 5, 5 seconds.

I then tried him (January 21, 1900) in CCC (double bar). He did it at first by pushing the old bar and then pulling at the door until he worked the second bar gradually around. Later he at times pushed the second bar. The times taken are shown in the time curve. I then (January 25th) tried him in NN (hook). See time curves on page 11. I then (January 27th) tried him in NNN (hook on other side). He opened it in 6, 12 and 4 seconds in the first three trials. I then (20 minutes later) tried him with NNNN (double hook). He opened the door in 12, 10, 6 and 6 seconds. I then (January 27th) tried him with PP (string across). He failed in 10. I then (February 21st) tried him with apparatus OO (string box). For his progress as shown by the times taken see the time curve. His progress is also shown in the decrease of the useless pullings at the wrong string. There were none in the 9th trial, 14th, 15th, 16th, 18th, 24th, and following trials.

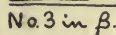
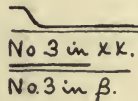
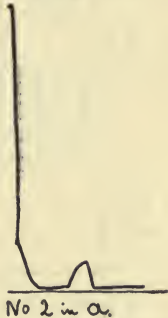
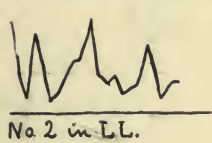
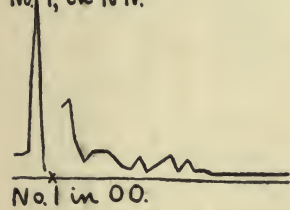
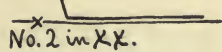
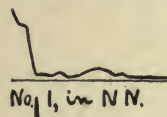
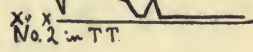
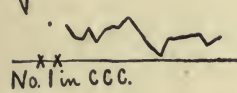
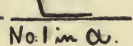
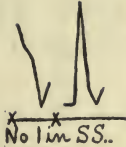
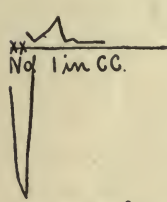
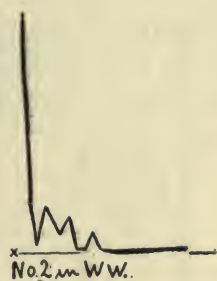
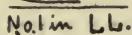
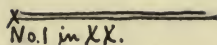
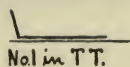
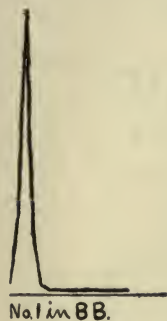
No. 1 was then (February 24th) tried with OOO (string box with box on upper string). No. 1 succeeded in 2.20, then failed in 10.00. The rest of the experiment will be described under imitation.

He was next tried (March 24th) with apparatus QQ (chute). He failed in 10.00, though he played with the apparatus much of the time. Other experiments were with box RR (wood-plug) (April 5th). He failed in 10.00. After he had in a manner to be described later come to succeed with RR he was tried in box SS (triple; wood-plug, hook and bar) (April 18th); see time curve. No more experiments of this nature were tried until October, 1900.

The rest of the experiments with No. 1 and all those with No. 2 and No. 3 may best be enumerated in the form of a table. (Table I., pp. 12 and 13.) It will show briefly the range of performances which the unaided efforts of the animals can cope with. It will also give the order in which each animal experienced them. F means that the animal failed to succeed. The figures are minutes and seconds, and represent the time taken in the first trial or the total time taken without success where there is an F. In cases where the animal failed in say 10 minutes, but in a later trial succeeded, say in 2.40, the record will be 2.40 after 10 F. There are separate columns for all three animals, headed No. 1, No. 2 and No. 3. Im. stands for a practically immediate success.

The curves on page 11 (Fig. 2) show the progress of the formation of the associations in those cases where the animal was given repeated trials with, however, nothing to guide him but his own unaided efforts. Each millimeter on the abscissa represents one trial and each millimeter on the ordinate represents 10 seconds, the ordinates representing the time taken by the animal to open the box. Crosses or figures on the base lines represent cases where the animal failed in 10 minutes or took a very long time to get out.

In discussing these facts we may first of all clear our way of one popular explanation, that this learning was due to 'reasoning.' If we use the word reasoning in its technical psychological meaning as the function of reaching conclusions by the perception of relations, comparison and inference, if we think of the mental content involved as feelings of relation, perceptions of similarity, general and abstract notions and judgments,



240.00	W
67 XX	=
70 TT	=
54 LL	=
67 WW	=
No. 1	No. 3

Memory  
Curves

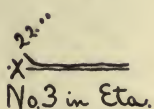
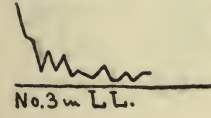
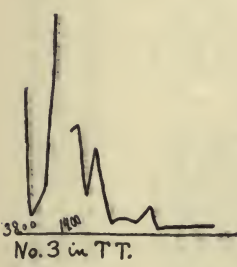
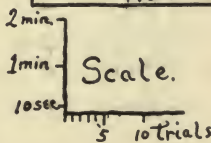


FIG. 2.





TABLE I.—Continued.

	No. 1.		No. 2.		No. 3.	
		Min. Sec.		Min. Sec.	Jan. 4, 1901	Min. Sec.
Box Delta (push back).	Jan. 4, 1901				Jan. 4, 1901	
App. QQ (a) (bar chute).	" 6, "	8.00			" 7, "	2.10
Box Zeta (new side plug).	" 7, "	1.10			" 8, "	.50
App. QQ (b) (2½ revolution chute).	" 9, "	3.00			" 8, "	F 10.00
App. QQ (c) (nail-plug chute).	" 11, "				" 11, "	F 5.00
Box Epsilon (push down).	" 12, "				" 12, "	F 5.00
App. QQ (d) (ring chute).	" 16, "				" 16, "	F 10.00
App. QQ (e) (hook chute).	" 17, "				" 16, "	F 5.00
App. QQ (f) (string chute).	" 17, "	.20			" 19, "	F 5.00
App. QQ (g) (string-wire chute).	" 17, "				" 19, "	F 5.00

<sup>1</sup> Did by pulling door and biting wire.

we find no evidence of reasoning in the behavior of the monkeys toward the mechanisms used. And this fact nullifies the arguments for reasoning in their case as it did in the case of the dogs and cats. The argument that successful dealings with mechanical contrivances imply that the animals reasoned out the properties of the mechanisms, is destroyed when we find mere selection from their general instinctive activities sufficient to cause success with bars, hooks, loops, etc. There is also in the case of the monkeys as in that of the other mammals positive evidence of the absence of any general function of reasoning. We shall find that at least very many simple acts were not learned by the monkeys in spite of their having seen me perform them again and again; that the same holds true of many simple acts which they saw other monkeys do, or were put through by me. We shall find that after having abundant opportunity to realize that one signal meant food at the bottom of the cage and another none, a monkey would not act from the obvious inference and consistently stay up or go down as the case might be, but would make errors such as would be natural if he acted under the growing influence of an association between sense-impression and impulse or sense impression and idea, but quite incomprehensible if he had compared the two signals and made a definite inference. We shall find that, after experience with several pairs of signals, the monkeys yet failed when a new pair was used, to do the obvious thing to a rational mind; viz., to compare the two, think which meant food, and act on the knowledge directly.

The methods one has to take to get them to do anything, their general conduct in becoming tame and in the experiments throughout, confirm these conclusions. The following particular phenomena are samples of the many which are inconsistent with the presence of reasoning as a general function. No. 1 had learned to open a door by pushing a bar around from a horizontal to a vertical position. The same box was then fitted with two bars. He turned the first bar round thirteen times before attempting to push the other bar around. In box LL all three monkeys would in the early trials do one or two of the acts over and over after they had once done them. No. 1, who had



learned to pull a loop of wire off from a nail, failed thereafter to pull off a similar loop made of string. No. 1 and No. 3 had learned to poke their left hands through the cage for me to take and operate a chute with. It was extremely difficult to get either of them to put his right hand through or even to let me take it and pull it through.

A negative answer to the question "Do the monkeys reason?" thus seems inevitable, but I do not attach to the question an importance commensurate with the part it has played historically in animal psychology. For I think it can be shown and I hope in a later monograph to show that reasoning is probably but one secondary result of the general function of having free ideas in great numbers, one product of a type of brain which works in great detail, not in gross associations. The denial of reasoning need not mean, and does not to my mind, any denial of continuity between animal and human mentality or any denial that the monkeys are mentally nearer relatives to man than are the other mammals.

So much for supererogatory explanation. Let us now turn to a more definite and fruitful treatment of these records.

The difference between these records and those of the chicks, cats and dogs given on pages 18-26, 33-34 and 37 of the 'Animal Intelligence' is undeniable. Whereas the latter were practically unanimous, save in the cases of the very easiest performances, in showing a process of gradual learning by a gradual elimination of unsuccessful movements, and a gradual reinforcement of the successful one, these are unanimous, save in the very hardest, in showing a process of sudden acquisition by a rapid, often apparently instantaneous, abandonment of the unsuccessful movements and a selection of the appropriate one which rivals in suddenness the selections made by human beings in similar performances. It is natural to infer that the monkeys who suddenly replace a lot of general pulling and clawing by a single definite pull at a hook or bar have an idea of the hook or bar and of the movement they make. The rate of their progress is so different from that of the cats and dogs that we cannot help imagining as the cause of it a totally different mental function, namely, free ideas instead of vague sense

impressions and impulses. But our interpretation of these results should not be too hasty. We must first consider several other possible explanations of the rapidity of learning by the monkeys before jumping to the conclusion that the forces which bring about the sudden formation of associations in human beings are present.

First of all it might be that the difference was due to the superiority of the monkeys in clear detailed vision. It might be that in given situations where associations were to be formed on the basis of smells, the cats and dogs would show similar rapid learning. There might be, that is, no general difference in type of mental functioning but only a special difference in the field in which the function worked. This question can be answered by an investigation of the process of forming associations in connection with smells by dogs and cats. Such an investigation will I hope soon be carried on in the Columbia Laboratory by Mr. Davis.

Secondly, it might be that the superior mobility and more detailed and definite movements of the monkeys' hands might have caused the difference. The slowness in the case of the dogs and cats might be at least in part the result of difficulty in executing movements, not in intending them. This difficulty in execution is a matter that cannot be readily estimated, but the movements made by the cats and dogs would not on their face value seem to be hard. They were mostly common to the animals' ordinary life. At the same time there were certain movements (*e. g.*, depressing the lever) which were much more quickly associated with their respective situations by the cats than others were, and if we could suppose all the movements learned by the monkeys were comparable to these few it would detract from the necessity of seeking some general mental difference as the explanation of the difference in the results.

In the third place it may be said by some that no comparison of the monkeys with dogs and cats is valid since the former animals got out of boxes while the latter got in. It may be supposed that the instinctive response to confinement includes an agitation which precludes anything save vague unregulated behavior. Prof. Wesley Mills has made such a suggestion

in referring to the 'Animal Intelligence' in the *PSYCHOLOGICAL REVIEW*, May, 1899. In the July number of the same journal I tried to show that there was no solid evidence of such a harmful agitation. Nor can we be at all sure that agitation when present does not rather quicken the wits of animals. It often seems to. However I should of course allow that for purposes of comparison it would be better to have the circumstances identical. And I should welcome any antagonist who should, by making experiments with kittens after the fashion of these with the monkeys, show that they did learn as suddenly as the latter.

Again we know that, whereas the times taken by a cat in a box to get out are inversely proportional to the strength of the association, inasmuch as they represent fairly the amount of its efforts, on the other hand, the times taken by a monkey to get in represent the amounts of his efforts *plus the amount of time in which he is not trying to get in*. It may be said therefore that the time records of the monkeys prove nothing. A record of four minutes may mean thirty seconds of effort and three minutes thirty seconds of sleep, that one of one minute may really represent twice as much effort. As a matter of fact this objection would occasionally hold against some single record. The earliest times and the occasional long times amongst very short ones are likely to be too long. The first fact makes the curves have too great a drop at the start, making them seem cases of too sudden learning, but the second fact makes the learning seem indefinite when it really is not. And in the long run the times taken do represent fairly well the amount of effort. I carefully recorded the amount of actual effort in a number of cases and the story it tells concerning the mental processes involved is the same as that told by the time-curves.

Still another explanation is this: The monkeys learn quickly, it is true, but not quickly enough for us to suppose the presence of ideas, or the formation of associations among them. For if there were such ideas they should in the complex acts do even better than they did. The explanation then is a high degree of facility in the formation of associations of just the same kind as we found in the chicks, dogs and cats.

Such an explanation we could hardly disprove in any case.



No one can from objective evidence set up a standard of speed of learning below which all shall be learning without ideas and above which all shall be learning by ideas. We should not expect any hard and fast demarcation.

This whole matter of the rate of learning should be studied in the light of other facts of behavior. My own judgment, if I had nothing but these time-curves to rely on, would be that there was in them an appearance of learning by ideas which, while possibly explicable by the finer vision and freer movements of the monkey in connection with ordinary mammalian mentality, made it worth while to look farther into their behavior. This we may now do.

What leads the lay mind to attribute superior mental gifts to an animal is not so much the rate of learning as the amount learned. The monkeys obviously form more associations and associations in a greater variety than do the other mammals. The improved rate assists, but another cause of this greater number of associations is the general physical activity of the monkeys, their constant movements of the hands, their instinctive curiosity or tendency to fool with all sorts of objects, to enjoy having sense-impressions, to form associations because of the resulting sound or sight. These mental characteristics are of a high degree of importance from the comparative point of view but they cannot be used to prove that the monkeys have free ideas, for a large number of associations may be acquired after the purely animal fashion.

What is of more importance is the actual behavior of the animals in connection with the boxes. First of all, as has been stated, all the monkey's movements are more definite, he seems not merely to pull but to pull at, not merely to poke but to push at. He seems, even in his general random play, to go here and there, pick up this, examine the other, etc., more from having the idea strike him than from feeling like doing it. He seems more like a man at the breakfast table than like a man in a fight. Still this appearance may be quite specious and I think it is likely to lead us to read ideational life into his behavior if we are not cautious. It may be simply general activity of the same sort as the narrower activities of the cat or dog.

In the second place the monkeys often make special movements with a directness which reminds one unavoidably of human actions guided by ideas. For instance, No. 1 escaped from his cage one day and went directly across the room to a table where lay a half of a banana which was in a very inconspicuous place. It seemed as if he had observed the banana and acted with the idea of its position fully in mind. Again, on failing to pull a hook out, No. 1 immediately applied his teeth, though he had before always pulled it out with his hand. So again with a plug. It may be that there is a special inborn tendency to bite at objects pulled unsuccessfully. If not, the act would seem to show the presence of the idea 'get thing out' or 'thing come out' and associated with it the impulse to use the teeth. We shall see later, however, that in certain other circumstances where we should expect ideas to be present and result in acts they do not.

The fact is that those features in the behavior of the monkeys in forming associations between the sight of a box and the act needed to open it which remind us of learning by ideas may also be possibly explained by general activity and curiosity, the free use of the hand, and superior quickness in forming associations of the animal sort. We must have recourse to more crucial tests or at least seek evidence from a number of different kinds of mental performances. The first of these will naturally be their behavior toward these same mechanisms after a long time interval.

#### THE PERMANENCE OF ASSOCIATIONS IN THE CASE OF MECHANISMS.

My records are too few and in all but one case after too short an interval to be decisive on the point of abrupt transition from failure to success such as would characterize an animal in whose mind arose the idea of a certain part of the mechanism as the thing to be attacked or of a certain movement as the fit one. The animals are all under observation in the Columbia Laboratory, however, and I trust that later satisfactory tests may be made. No. 2 was not included in the tests because he was either unwell or had become very shy of the boxes, entering

them even when the door was left open only after great delay. The time curves for the experiments performed will be found on page 11 among the others. The figures beside each pair represent the number of days without practice.

The records show a decided superiority to those of the cats and dogs. Although the number of trials in the original tests were in general fewer in the case of the monkeys, the retention of the association is complete in 6 cases out of 8 and is practically so in one case where the interval was 8 months.

#### EXPERIMENTS ON THE DISCRIMINATION OF SIGNALS.

My experiments on discrimination were of the following general type: I got the animal into the habit of reacting to a certain signal (a sound, movement, posture, visual presentation or what not) by some well-defined act. In the cases to be described this act was to come down from his customary positions about the top of the cage, to a place at the bottom. I then would give him a bit of food. When this habit was wholly or partly formed, I would begin to mix with that signal another signal enough like it so that the animal would respond in the same manner. In the cases where I gave this signal I would not feed him. I could then determine whether the animal did discriminate or not, and his progress toward perfect discrimination in case he did. If an animal responds indiscriminately to both signals (that is, does not learn to disregard the 'no food' signal) it is well to test him by using two somewhat similar signals, after one of which you feed him at one place and after the other of which you feed him at a different place.

If the animal profits by his training by acquiring ideas of the two signals and associates with them ideas of 'food' and 'no food,' 'go down' and 'stay still,' and uses these ideas to control his conduct he will, we have a right to expect, change suddenly from total failure to differentiate the signals to total success. He will or won't have the ideas, and will behave accordingly. The same result could of course be brought about by very rapid association of the new signal with the act of keeping still, a very



rapid inhibition of the act of going down in response to it by virtue of the lack of any pleasure from doing so.

For convenience I shall call the signals after which food was given *yes* signals and those after which food was not given *no* signals. Signals not described in the text are shown in Fig. 3, page 23. The progress of the monkeys in discriminating is shown by Fig. 4, on page 25. In Fig. 4 every millimeter along the horizontal or base line represents 10 trials with the signal. The heights of the black surface represent the percentages of *wrong* responses, 10 mm. meaning 100 per cent. of incorrect responses. Thus the first figure of the set, left hand, *a*, presents the following record: First 10 trials, all wrong; of next 10, 7 wrong; of next 10, 6 wrong; of next 10, 7; of the next, 9; of the next, 9; of the next, 4; of the next, none; of the next, 3; of the next, 2, and then 70 trials without an error.

I will describe some of the experiments in detail and then discuss the graphic presentation of them all.

#### EXPERIMENTS WITH NO. 1.

Having developed in No. 1 the habit of coming down to the bottom of his cage to get a bit of food when he saw me reach out and take such a bit from my desk, I tested his ability to discriminate by beginning to use now one hand, now the other, feeding him only when I used the left. I also used different sets of words, namely, 'I will give some food' and 'They shall not have any.' It will be seen later that he probably reacted only to the difference of the hands. The experiment is similar to that described on page 89 of the 'Animal Intelligence.' At the beginning, it should be remembered, No. 1 would come down whichever hand was used, no matter what was said, except in the occasional cases where he was so occupied with some other pursuit as to be evidently inattentive. He did come to associate the act of going down with the one signal and the act of staying still or continuing his ordinary movements with the other signal. His progress in learning to do so is best seen in the curves of his errors. To the *yes* signal he responded correctly, except for the occasional lapses which I just mentioned, from the start and throughout. With the *no* signal his errors were as shown in

Fig. 4, *a*. The break in the curve at 110 and 120 is probably not significant of an actual retrograde as the trials concerned followed an eight days' cessation of the experiments.

I next tried No. 1 with an apparatus exposing sometimes a card with a diamond-shaped piece of buff-colored paper on it and sometimes a card with a similar black piece. The black piece was three-fourths of an inch farther behind the opening than the other. The light color was the yes signal. The error curves for both signals are given as No. 1 at the beginning of the experiment did not go down always (Fig. 4, *b* and *b*<sub>1</sub>).

I next tried No. 1 with the same apparatus but exposing cards with YES and N in place of the buff and black diamonds. The record of the errors is given in Fig. 4, *c* and *c*<sup>1</sup>. At the start he came down halfway very often. This I arbitrarily scored as an error no matter which signal it was in response to. It should not be supposed that these curves represent two totally new associations. It seems likely that the monkey reacted to the *position* of the no card in the apparatus (the same as that of the black diamond card) rather than to the shape of the letters. On putting the black diamond in front he was much confused.

I next gave No. 1 the chance to form the habits of coming down when I rapped my pencil against the table twice and of staying where he was when I rapped with it once. He had 90 trials of each signal but failed to give evidence of any different associations in the two cases.

Experiments of this sort were discontinued in the summer. In October I tried No. 1 with the right and left hand experiment, he being in a new room and cage, and I being seated in a different situation. He came down at both signals and failed to make any ascertainable progress with the no signal in 80 trials. (October 20-24.)

I then tried him with the black and buff diamonds, the black being in front (October 25-29). The reaction to the yes signal was perfect from the start. The progress with the no signal is shown in Fig. 4, *d*.

I then tried him with an apparatus externally of different size, shape and color from that so far used, showing as the yes signal a brown card and as the no signal a white and gold card

one-half inch farther back in the apparatus. The yes signal was practically perfect from the start. His progress with the no signal is shown in Fig. 4, *e*.

I then tried a still different arrangement for exposure to which however he did not give uniform attention.

I then tried cards 1 and 101, 101 being in front and 1 in back. 1 was the yes signal. Yes responses were perfect from the start. For no responses see Fig. 4, *f*. I then put the yes signal in front and the no signal behind. Yes responses perfect; for no responses see Fig. 4, *f*, *a*.

From now on I arranged the exposures in such a way that there was no difference between the yes and no signals in distance or surroundings.

The following list shows the dates, signals used and figure on page 25 presenting the results. Where there is only one figure drawn it refers to progress with the no signal, the yes signal being practically perfect from the start.

	Yes Signal.	No Signal.	Figure.
Nov. 13-15, 1900.	2	102	<i>g</i> <i>g</i> <sub>1</sub>
" 14-16, "	3	103	<i>i</i> <i>i</i> <sub>1</sub>
" 16-19, "	4	104	<i>h</i>
" 19, "	5	105	<i>j</i>
" 20, "	6	106	<i>k</i>
" 21, "	7	107	<i>l</i>
" 23 (?) "	8	108	<i>m</i>
" 27-29, "	9	109	<i>n</i>
" 30, "	10	110	<i>o</i>

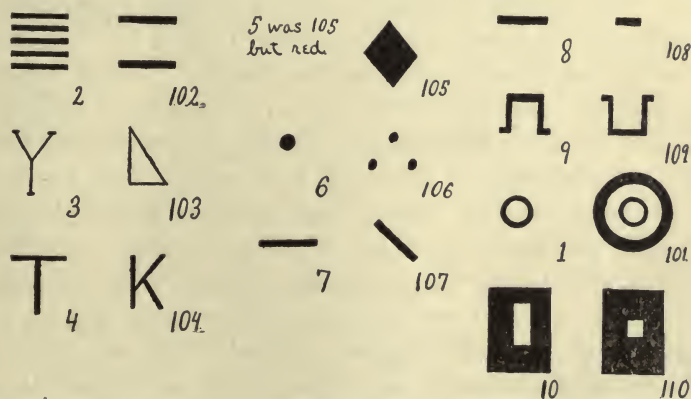


FIG. 3.



Fig. 3 gives fac-similes of the different signals reduced to one-sixth their actual size. The drawing of 101 is not accurate, the outer ring being too thick.

#### EXPERIMENTS WITH NO. 2.

I first secured the partial formation of the habit of coming down when I took a bit of food in my hand. I then used the apparatus for exposing cards, YES, in front being the 'yes' signal and a circle at the back being the 'no' signal. I gave No. 2 25 trials with the yes signal and then began a regular experiment similar to those described. After about 90 trials (November 9-12, 1900) there was no progress toward differentiation of response and it was evident from No. 2's behavior that he was reacting solely to the movements of my hand. So I abandoned the exposing apparatus and used (November 11-13, 1900) as the yes signal the act of taking the food with my left hand from a pile on the front of the box and for the no signal the act of taking food with my right hand from a pile 4 inches behind that just mentioned. No. 2 did come to differentiate these two signals. The record of his progress is given in Fig. 4 by  $A$  and  $A_1$ .

I then made a second attempt with the exposing apparatus, using cards 2 and 102 (November 6, 14-20). No. 2 did react to my movement in pulling the string but in over 100 trials made no progress in the direction of a differential reaction to the no signal. I then tried feeding him at each signal, feeding him at the bottom of the cage as usual when I gave the 'yes' signal and at the top when I gave the 'no' signal. After a hundred trials with the 'no' signal there was no progress.

I then abandoned again the exposing apparatus and used as signals the ordinary act of taking food with my left hand (yes) and the act of moving my left arm from my right side round diagonally (swinging it on my elbow as a center) and holding the hand, after taking the food, *palm up* (no) (November 26, 27, 1900). No. 2 did come to differentiate these signals. His progress is given in the diagram entitled 'palm up' ( $B$ ).

I next used (November 27, 1900) as the yes signal the same act as before and for the no signal the act of holding the food

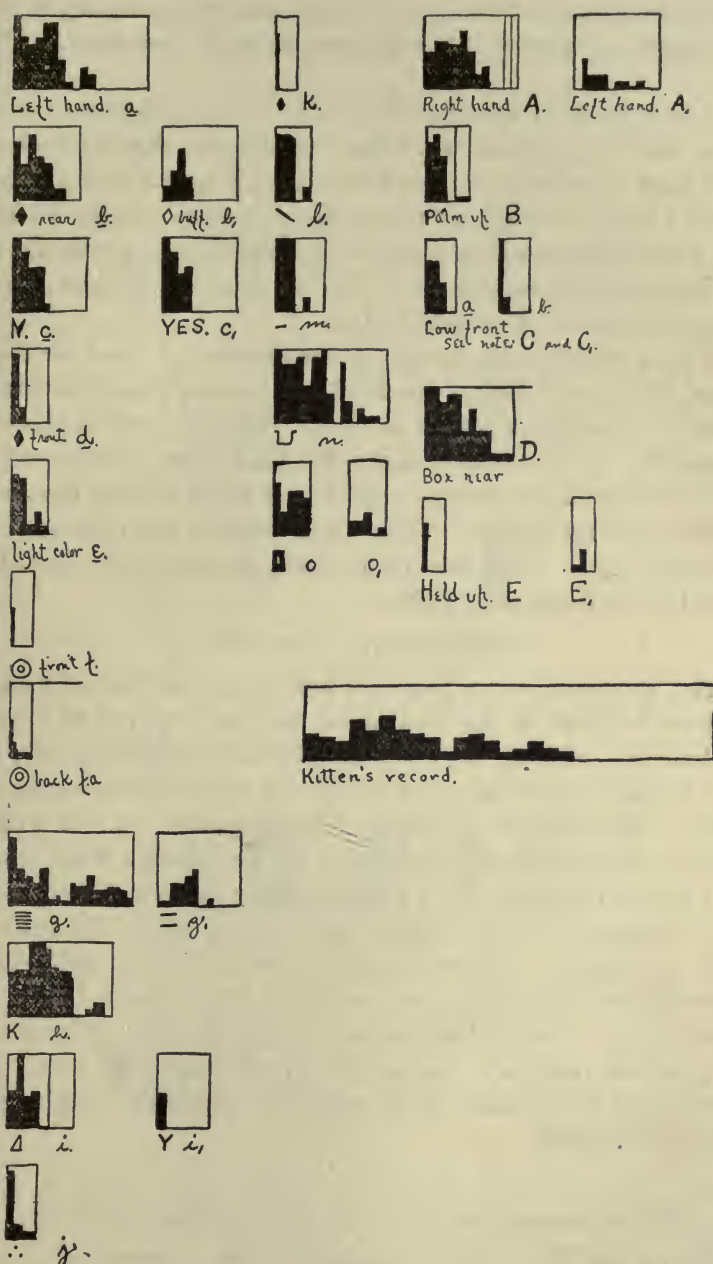


FIG. 4.

just in front of the box about four inches below the edge. No. 2's progress is shown in the diagram entitled 'low front' (*C* and *C*<sub>1</sub>).

I next used (November 27-30) the same movement for both 'yes' and 'no' signals save that as the yes signal I took the food from a brown pasteboard box 3 by 3 by 0.5 and as the no signal I took it from a white crockery cover two inches in diameter and three-eighths of an inch high which was beside the box but three inches nearer me. No. 2's progress is shown in the diagram entitled 'box near' (*D*).

I next used for the yes signal the familiar act and for the no signal the act of holding the food six inches above the box instead of a quarter or a half an inch. The progress is shown in *E* and *E*<sub>1</sub>. I then tried taking the food from a saucer off the front of the box for the yes signal and from a small box at the back for the no signal. 'Yes' was perfect from the start (10 trials given). 'No' was right once, then wrong once, then right for the remaining eight.

#### EXPERIMENTS WITH NO. 3.

No. 3 was kept in a cage not half so big as those of 1 and 2. Perhaps because of the hindrance this fact offered to forming the habit of reacting in some definite way to yes signals, perhaps because of the fact that I did not try hand movements as signals, there was no successful discrimination by No. 3 of the yellow from the black diamond or of a card with YES from a card with a circle on it. I tried climbing up to a particular spot as the response to the yes signal and staying still as the response to the no signal. I also tried instead of the latter a different act in which case the animal was fed after both signals but in different places. In the latter case No. 3 made some progress, but for practical reasons I postponed experiments with him. Circumstances have made it necessary to postpone such experiments indefinitely.

#### PERMANENCE OF THE ABILITY TO DISCRIMINATE.

No. 1 and No. 2 were tried again after intervals of 33 to 48 days. The results of these trials are shown in Fig. 5. Here



every millimeter along the base line represents *one* trial with the no signal (the yes signals were practically perfect) and failure is represented by a column 10 mm. high while success is represented by the absence of any column. Thus the first record reads, "No. 1 with signal 104 after 40 days made 5 failures,

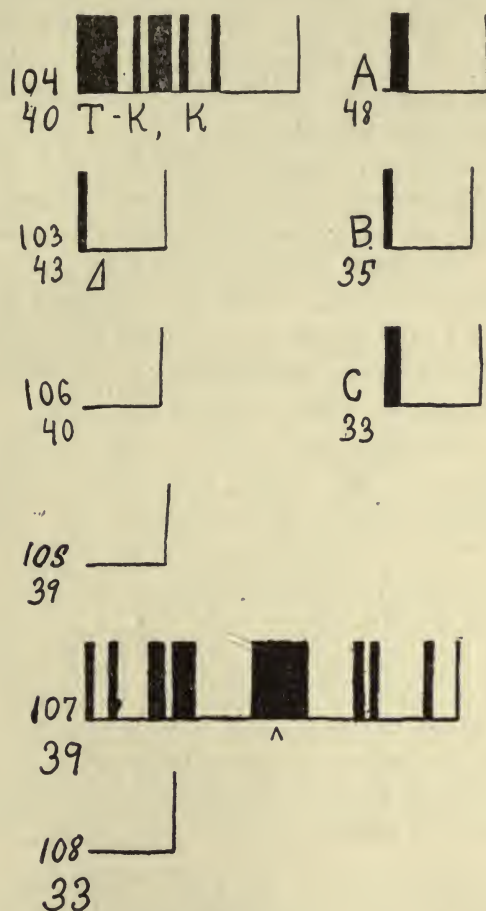


FIG. 5.

then 2 successes, then 1 failure, then 1 success, then 3 failures, then 1 success, then 1 failure, then 3 successes, then 1 failure, then 10 successes." The third record (106; 40 days) reads, 'perfect success in ten trials.'

## DISCUSSION OF RESULTS.

The results of all these discrimination experiments emphasize the rapidity of formation of associations amongst the monkeys which appeared in their behavior toward the mechanisms. The suddenness of the change in many cases is immediately suggestive of human performances. If all the records were like c, f, h, i, j, k, l, m, B, E, and memory trials 103, A, B, and C, one would have to credit the animals with either marvellous rapidity in forming associations of the purely animal sort or concede that from all the objective evidence at hand they were shown to learn as human beings would. One would have to suppose that they had clear ideas of the signals and clean cut associations with those ideas. The other records check such a conclusion.

In studying the figures we should remember that occasional mistakes, say 1 in 10 trials, are probably not significant of incomplete learning but of inattention or of precipitate action before the shutter had fairly exposed the card. We must not expect that a monkey who totally fails to discriminate will *always* respond wrongly to the no signal, or that a monkey who has come to discriminate perfectly will *always* respond rightly. A sudden drop from an average high level of error to an average low level will signify sudden learning. Where the failure was on the first trial of a series a few hours or a day removed from the last series, I have generally represented the fact not by a column 1 mm. high and 1 mm. broad but by a single 10 mm. perpendicular. See i and A. Such cases represent probably the failure of the animal to keep his learning permanent rather than any general inability to discriminate.

K was to some extent a memory trial of d (after over half a year).

The experiment with 10 and 110 is noteworthy. Although, as can be seen from the figures, the difference is obvious to one looking at the white part of the figure, it is not so to one looking at the black part. No. 1 failed to improve appreciably in fifty trials, probably because his previous experience had gotten him into the habit of attending to the black lines.

Before arguing from the suddenness of the change from

failure to success we have to consider one possibility that I have not mentioned and in fact for the sake of clearness in presentation have rather concealed. It is that the sudden change in the records, which report only whether the animal did or did not go down, may represent a more gradual change in the animal's mind, a gradual weakening of the impulse to go down which makes him feel less and less inclined to go down, though still doing so, until this weakening reaches a sort of saturation point and stops the action. There were in their behavior some phenomena which might witness to such a process, but their interpretation is so dependent on the subjective attitude and prepossessions of the observer that I prefer not to draw any conclusions from them. On the other hand records c, g, n, A and D seem to show that gradual changes can be paralleled by changes in the percentage of failures.

In the statement of conclusions I shall represent what would be the effect on our theory of the matter in both cases, (1) taking the records to be fairly perfect parallels of the process and (2) taking them to be the records of the summation points of a process not shown with surety in any measurable objective facts. But I shall leave to future workers the task of determining which case is the true one.

If we judge by the objective records themselves we may still choose between two views. (1) We may say that the monkeys did come to have ideas of the acts of going down to the bottom of the cage and of staying still and that their learning represented the association of the sense-impressions of the two signals, one with each of these ideas, or possibly their association with two other ideas (of being fed and of not being fed) and through them with the acts. Or (2) we may say that the monkeys had no such ideas, but merely by the common animal sort of association came to react in the profitable way to each signal.

If we take the first view we must explain the failure of the animals to change suddenly in some of the experiments, must explain why for instance No. 1 in g should, after he had responded correctly to the no signal for 27 trials out of 30, fail in one trial out of four for a hundred or more trials. If the 27 successes were due to ideas, why was there regression? If the



animal came to respond by staying still on seeing the K (card 104) because that sight was associated with the idea of no food or the idea of staying still, why did he, in his memory trial, act sometimes rightly, sometimes wrongly for eleven trials after his acting rightly twice. If he stayed still because the idea was aroused, why did he not stay still as soon as he had a few trials to remind him of the idea? It is easy, one may say, to see why, with a capacity to select movements and associate them with sense-presentations very quickly, in cases where habit provides only two movements for selection and where the sense presentation is very clear and simple, an animal should practically at once be confirmed in the one act on an occasion when he does it with the sense impression in the focus of attention. It is easy therefore to explain the sudden change in i, l, m, B, C, and E. But our critic may add, "It is very hard to suppose that an animal that learned by connecting the sight of a card with the idea 'stay still' or the idea, 'no food' should be so long in making the connection as was the case in some of these experiments, should take 10, 20 or 40 trials to change from a high percentage of wrong to a high percentage of right reactions."

If we take the second view we have to face the fact that many of the records are nothing like the single one we have for comparison, that of the kitten shown in Fig. 4, and that the appeal to a capacity to form animal associations very quickly seems like a far-fetched refuge from the other view rather than a natural interpretation. If we take the records to be summation points in a more gradual process, this difficulty is relieved.

If further investigation upheld the first view, we should still not have a demonstration that the monkeys habitually did learn by getting percepts and images associated with sense impressions, by having free ideas of the acts they performed; we should only have proved that they could under certain circumstances.

The circumstances in these experiments on discrimination were such as to form a most favorable case. The act of going down had been performed in all sorts of different connections and was likely to gain representation in ideational life; the experience 'bit of banana' had again been attended to as a part

of very many different associations and so would be likely to develop into a definite idea.

These results then do not settle the choice between three theories: 1 (*a*) that they were due to a general capacity for having ideas, 1 (*b*) that they were due to ideas acquired by specially favoring circumstances, 2 that they were due to the common form of association, the association of an impulse to an act with a sense impression rather roughly felt.

It would be of the utmost interest to duplicate these experiments with dogs, cats and other mammals and compare the records. Moreover, since we shall find 1 (*a*) barred out by other experiments, it will be of great interest to test the monkeys with some other type of act than discrimination to see if by giving the animal experience of the act and result involved in a lot of different connections we could get a rate of speed in the formation of a new association comparable to the rates in some of these cases.

Of course here as in our previous section the differences in the sense powers of the monkeys from those of the kitten which I have tested with a similar experiment may have caused the difference in behavior. Focalized vision lends itself to delicate associations. Perhaps if one used the sense of smell, or if the dogs and cats could, preserving their same mental faculties in general, add the capacity for focalized vision, they would do as well as the monkeys.<sup>2</sup>

#### EXPERIMENTS ON THE INFLUENCE OF TUITION.

The general aim of these experiments was to ascertain whether the monkeys' actions were at all determined by the presence of free ideas and if so, to what extent. The question is, "Are the associations which experience leads them to form associations between (1) the idea of an object and (2) the idea of an act or result and (3) the impulses and act itself or are they merely associations between the sense impression of the object and the impulse and act?" Can a monkey learn and does

he commonly learn to do things, not by the mere selection of the act from amongst the acts done by him, but by getting some idea and then himself providing the act because it is associated in his mind with that idea. If a monkey feels an impulse to get into a box, sees his arm push a bar and sees a door fall open immediately thereafter and goes into the box enough times, he has every chance to form the association between the impulse to get into the box and the idea 'arm push bar,' provided he can have such an idea. If his general behavior is due to having ideas connected with and so causing his acts, he has had chance enough to form the association between the idea 'push at' and the act of pushing. If then a monkey forms an association leading to an act by being put through the act, we may expect that he has free ideas. And if he has free ideas in general in connection with his actions, we may expect him to so form associations. So also if a monkey shows a general capability to learn from seeing another monkey or a human being do a thing. A few isolated cases of imitation, however, might witness not to any general mental quality but only to certain instincts or habits differing from others only in that the situation calling forth the act was the same act performed by another.

If the monkeys do not learn in these ways we must, until other evidence appears, suppose them to be in general destitute of a life of free ideas, must regard their somewhat ambiguous behavior in learning by their own unaided efforts as of the same type as that of the dogs and cats, differing only in the respects mentioned on pages 16 and 17.

The general method of experimentation was to give monkeys who had failed of their own efforts to operate some simple mechanism, a chance to see me do it or see another monkey do it or to see and feel themselves do it, and then note any change in their behavior. The chief question is whether they succeed after such tuition when they have failed before it, but the presence of ideas would also be indicated if they attacked, though without success, the vital point in the mechanism when they had not done so before. On the other hand mere success would not prove that the tuition had influenced them, for if they made a different movement or attacked a different spot we could not attribute their behavior to getting ideas of the necessary act.



The results of the experiments as a whole are on their face value a trifle ambiguous, but they surely show that the monkeys in question had no considerable stock of ideas of the objects they dealt with or of the movements they made and were not in general capable of acquiring from seeing me or one of their comrades attack a certain part of a mechanism and make a certain movement any ideas that were at all efficacious in guiding their conduct. They do not acquire or use ideas in anything that approaches the way human adults do. Whether the monkeys may not have some few ideas corresponding to habitual classes of objects and acts is a different question. Such may be present and function as the excitants of acts.

It is likely that this question could have been definitely solved if it had been possible for me to work with a larger number of animals. With enough subjects one could use the method mentioned on page 70 of the 'Experimental Study' of giving the animals tuition in acts which they would eventually do themselves without it and then leaving them to their efforts, noting any differences in the way they learned from that in which other subjects who had no tuition learned the same acts. The chief of such differences to note would be differences in the time of their first trial, in the slope of the time curve and in the number of useless acts.

It would also be possible to extend experiments of the type of the (on chair) experiment, where a subject is given first a certain time (calculated by the experimenter to be somewhat less than would be needed for the animal to hit upon the act) and if he does fail is then given certain tuition and then a second trial. The influence of the tuition is estimated by the presence or absence of cases where after tuition the act is done within the time.

There is nothing necessarily insoluble in the problem. Given ten or twenty monkeys that can be handled without any difficulty and it could be settled in a month.

With this general preface we may turn to the more special questions connected with the experiments on imitation of human acts and of the acts of other monkeys and on the formation of associations apart from the selection of impulses.

## IMITATION OF HUMAN BEINGS.

It has been a common opinion that monkeys learned to do things from seeing them done by human beings. We find anecdotes to that effect in fairly reputable authors.

Of course, such anecdotes might be true and still not prove that the animals learned to do things because they saw them done. The animal may have been taught in other ways to respond to the particular sights in question by the particular acts. Or it may have been in each case a coincidence.

If a monkey did actually form an association between a given situation and act by seeing some one respond to that situation by that act, it would be evidence of considerable importance concerning his general mental status, for it would go to show that he could and often did form associations between sense impressions and ideas and between ideas and acts. Seeing some one turn a key in a lock might thus give him the idea of turning or moving the key and this idea might arouse the act. However, the mere fact that a monkey does something which you have just done in his presence need not demonstrate or even render a bit more probable such a general mental condition. For he perhaps would have acted in just the same manner if you had offered him no model. If you put two toothpicks on a dish, take one and put it in your mouth, a monkey will do the same not because he profits by your example, but because he instinctively puts nearly all small objects in his mouth. Because of their general activity, their instinctive impulses to grab, drop, bite, rub, carry, move about, turn over, etc., any novel object within their reach, their constant movement and assumption of all sorts of postures, the monkeys perform many acts like our own and simulate imitation to a far greater extent than other mammals.

Even if a monkey which has failed of itself to do a certain thing does it after you have shown him the act, there need be no reason to suppose that he is learning by imitation, forming an association between the sight of the object and the act towards it through an idea gained from watching you. You may have caused his act simply by attracting his attention to the object.

Perhaps if you had pointed at it or held it passively in your hand, you would have brought to pass just the same action on his part. There are several cases among my records where an act which an animal failed totally to do of himself was done after I had so attracted his attention to the object concerned.

Throughout all the time that I had my monkeys under observation I never noticed in their general behavior any act which seemed due to genuine imitation of me or the other persons about. I also gave them special opportunities to show such by means of a number of experiments of the following type: where an animal failed by himself to get into some box or operate some mechanism, I would operate it in his presence a number of times and then give him a chance to profit by the tuition. His failure might be due to (1) the absence of instinctive impulses to make the movement in that situation, (2) to lack of precision in the movement, (3) to lack of force, or (4) to failure to notice and attack some special part of the mechanism. An instance of (1) was the failure to push away from them a bar which held a door; an instance of (2) was the failure to pull a wire loop off a nail; an instance of (2) or (3) was the failure to pull up a bolt; an instance of (4) was the failure to pull up an inside bar. Failures due to (3) occur rarely in the case of such mechanisms as were used in my investigations.

The general method of experiment was to make sure that the animal would not of itself perform a certain act in a certain situation, then to make sure that his failure could not be remedied by attracting his attention to the object, then to perform the act for him a number of times, letting him get each time the food which resulted, and finally to see whether, having failed before the tuition, he would succeed after it. This sounds very simple, but such experiments are hard to carry out satisfactorily. If you try the animal enough times by himself to make quite sure that he will not of himself hit upon the act, you are likely to form in him the habit of meeting the particular situation in question with total disregard. His efforts having failed so often may be so inhibited that you could hardly expect any tuition to give them new life. The matter is worse if you add further enough trials to assure you that your attracting his attention to it has



been unavailing. On the other hand, if you take failure in five or ten minutes to mean inability, and from subsequent success after imitation argue that imitation was efficient, you have to face the numerous cases where animals which have failed in ten minutes have succeeded in later unaided trials. With dogs and cats this does not much matter because they are steady performers and their conduct in one short trial tells you what to expect with some probability. But the monkeys are much more variable and are so frequently distracted [that one feels much less confidence in his predictions. Moreover, you cannot be at all sure of having attracted a monkey's attention to an object unless he *does* touch it. Suppose, for example, a monkey has failed to even touch a bar though you have put a bit of food on it repeatedly. It is quite possible that he may look at and take the food and not notice the bar, and the fact that after such tuition he still fails to push or pull the bar may mean simply that it has not caught his notice. I have, therefore, preferred in most cases to give the animals only a brief period of trial to test their ability by their own unaided efforts and to omit the attempts to test the efficacy of attracting their attention to the vital point in the mechanism. This makes the results appear less elegant and definitive but really increases their value for purposes of interpretation.

The thoughtful reader will not expect from my experiments any perfectly rigorous demonstration of either the presence or the absence of imitation of human acts as a means of learning. The general trend of the evidence, it seems to me, is decidedly towards justifying the hypothesis that the monkeys did not learn acts from seeing me do them.

I will first describe a sample experiment and then present a summary of all those made.

On January 12th I put box Epsilon (push down) in No. 3's cage, the door of the box being open. I put a bit of food in the box. No. 3 reached in and took it. This was repeated three times. I then put in a bit of food and closed the door. No. 3 pulled and bit the box, turned it over, fingered and bit at the hole where the lever was, but did not succeed in getting the door open. After ten minutes I took the box out. Later I took No. 3 out and let him sit on my knees (I sitting on the floor with the

box in front of us). I would then put my hand out toward the box and when he was looking at it would insert my finger and depress the lever with as evident a movement as I could. The door, of course, opened, and No. 3 put his arm in and took the bit of food. I then put in another, closed the door and depressed the lever as before. No. 3 watched my hand pretty constantly, as all his experiences with me had made such watching profitable. After ten such trials he was put back in the cage and the box put in with a large piece of food in it and its door closed. No. 3 failed in five minutes and the box was taken out. He was shown fifteen times more and then left to try himself. I tried him for a couple of minutes under just the same circumstances as existed during the tuition, *i. e.*, he on the floor by me, the box in front. In this trial and in a five-minute trial inside his cage he failed to open the door or to differ in any essential respect from his behavior before tuition.

No. 1 saw me do 9 different acts and No. 3 7 which they had failed of themselves to do.<sup>1</sup> After from 1 to 40 chances to imitate me they still failed to operate at all 11 of these mechanisms. In the case of 3 out of 5 that were worked the act was not the same as that taught. No. 1, who saw me pull a nail out by taking the end of it and pulling the nail away from the box, himself put his hand round the nail and wriggled it out by pulling his hand back and forth. No. 3, who saw me pull a bolt up with my fingers, succeeded by jerking and yanking the door until he shook the bolt up. He saw me pull a hook out of an eye, but he succeeded by pulling at a bar to which it was attached. In the case of one of the two remaining acts (No. 3 with *nail chute*) the act was done once and never again, though ample opportunity was

<sup>1</sup>The acts and the number of chances to see me do each and the results were as follows; details can be found on the table on page 47. F = failed after tuition.

No. 1.—MM	21 F	No. 3.—Theta	25 did in 3.00.
Theta	5 F	QQ	40 F
QQ	10 F	Gamma	30 F
RR	4 F		
W	9 did in .22	Epsilon	25 F
Delta	15 F	QQ (ff)	5 F
Epsilon	40 F	QQ (c)	20 F, did in 1.30, F, 5 F, 5 F
QQ (f)	15 F	QQe	5 F did in 2.00
QQ (c)	1 did in 2.20		

given and tuition continued. It could, therefore, hardly have been due to an idea instilled by the tuition. The remaining case, No. 1, with loop, must, I think, be attributed to accident, especially since No. 3 failed to profit by precisely the same sort of tuition with precisely the same act.

Nor is there any evidence to show that although tuition failed to cause successes where unaided effort failed, it yet caused attempts which would not otherwise have occurred. Out of fifteen cases where such might have appeared, there were only three where it is possible to claim that they did. No one of these three is a sure case. With RR (wood plug) No. 1 did seem to pull the plug more definitely after seeing me than before. With QQ (c) (nail chute) and MM (bolt at top) he may possibly have done so.

In 5 cases I tried the influence of seeing me make the movement on animals who had done the act of themselves, the aim being to see whether there would be a marked shortening of the time, a change in their way of operating the mechanism or an attempt at such change. I will give the essential facts from the general table on pages 47-49.

(a) No. 1 had succeeded in pulling in the box by the upper string in OOO (upper string box) in 2.20 and then failed in 3.00. I showed him 4 times. He failed in 10. I showed him 4 more times. He failed in 10. I showed him 4 more times. He succeeded in .20. No change in manner of act or objects attacked, though my manner was different from his.

(b) No. 1 had succeeded in QQ (a) (chute bar) in 8.00. I showed him 20 times. He failed in 10. I showed him 10 more times. He succeeded in 2.00. I showed him 10 more times. He succeeded in 50 seconds. No change in his manner of performance or in the object attacked, though my manner was different from his.

(c) No. 1 had succeeded in 3.00, .25, .07, .25, .20, .06 and .09 with QQ (b) (chute bar double) and then failed in 5.00. I showed him 10 times. He then failed in 5 twice, succeeded in 3.00, and failed in 5 again. No change in manner of performance or in the object attacked, though my manner was different from his.



(d) No. 3 had the following record in box Delta :

2.00 (pushed with head)  
 3.20    "        "        "  
      30 F  
      10 F  
      10 F  
 2.10 (pulled wire and door).

I showed him 20 times by pushing the bar to the right with my finger. He succeeded in 8.00 and 8.00 by pulling the wire and the door. No change in object attacked.

(e) No. 3 had failed twice in 5 with chute QQ (ff) (chute string wire) and succeeded once in 2.00 by a strong pull on the wire itself, not the loop. I showed him 5 times, pulling the loop off the nail. He then failed in 5. There was no change in the objects attacked.

These records show no signs of any influence of the tuition that are not more probably signs of something else. We cannot attribute the rapid decrease in time taken in (b) to the tuition until we know the time curve for the same process without tuition.

The systematic experiments designed to detect the presence of ability to learn from human beings are thus practically unanimous against it. So too was the general behavior of the monkeys, though I do not consider the failure of the animals to imitate common human acts as of much importance save as a rebuke to the story-tellers and casual observers. The following facts are samples: The door of No. 1's cage was closed by an iron hoop with a slit in it through which a staple passed, the door being held by a stick of wood thrust through the staple. No. 1 saw me open the door of his and other cages by taking out sticks hundreds of times, but though he escaped from his cage a dozen times in other ways he never took the stick out and to my knowledge never tried to. I myself and visitors smoked a good deal in the monkeys' presence but a cigar or cigarette given to them was always treated like anything else.

## IMITATION OF OTHER MONKEYS.

It would theoretically seem far more likely that the monkeys should learn from watching each other than from watching human beings, and experimental determinations of such ability are more important than those described in the last section as contributions both to genetic psychology and to natural history. I regret that the work I have been able to do in the study of this phase of the mental life of the monkeys has been very limited and in many ways unsatisfactory.

We should expect to find the tendency to imitation more obvious in the case of young and parents than elsewhere. I have had no chance to observe such cases. We should expect closely associated animals, such as members of a common troop or animals on friendly terms, to manifest it more than others. Unfortunately, two of my monkeys by the time I was ready to make definite experiments were on terms of war. The other had then become so shy that I could not confidently infer inability to do a thing from actual failure to do it. He showed no evidence of learning from his mates. I have, therefore, little evidence of a quantitative objective nature to present and shall have in the end to ask the reader to take some opinions without verifiable proofs.

My reliable experiments, five in number, were of the following nature. A monkey who had failed of himself (and often also after a chance to learn from me or from being put through the act) would be put where he could see another do the act and get a reward (food) for it. He would then be given a chance to do it himself, and note would be taken of his success or failure, and of whether his act was the same as that of his model in case he succeeded, and of whether he tried that act more than before the tuition in case he tried it and failed. The results are given in Table II.

In the fourth experiment No. 1 showed further that the tuition did not cause his successes in that after some successes further tuition did not improve him.

There is clearly no evidence here of any imitation of No. 1 by No. 3. There was also apparently nothing like purposive watching on the part of No. 3. He seemed often to see No. 1

TABLE II.

Subject, Date, Act.	Time tried alone with result.	No. of times attention attracted.	No. of times imitatee did.	Result after chance for imitation.	Similarity or dissimilarity of act.	Similar act attempted, though unsuccessfully in cases where it had not been before training.	General judgment as to influence of training.
No. 3. Dec. 17, 1900. VV (wire loop).	50 F		43	55 F		No.	None.
No. 3. Jan. 15, 1901. QQ (c) (nail chute).	91 F 1.30		75	35 F		No.	None.
No. 3. Jan. 21, 1901. Gamma (wind).	63 F		43	5 F 9.00 6.00	Dissimilar.	No.	None.
No. 3. Jan. 21, 1901. QQ (ff) (string chute with wire).	20 F 2.00		30	1.30 .40 .35 5 F 10 F	Dissimilar.	No.	None.
No. 3. Jan. 23, 1901. QQ (chute).	115 F		40			No.	None.



open the box or work the chute mechanism, but without special interest.

This lack of any special curiosity about the doings of their own species characterized the general behavior of all three of my monkeys and in itself lessens the probability that they learn much from one another. Nor did there appear, in the course of the three months and more the animals were together, any signs of imitation. There were indeed certain notable instances of the lack of it in circumstances which one would suppose would be favorable cases for it.

For instance: No. 2 was very timid. No. 1 was perfectly tame from the first day No. 2 was with me, and No. 3 became tame shortly after. No. 2 saw Nos. 1 and 3 come to me, be played with, fed and put through experiments, yet he never did the same nor did he abate a jot or tittle from his timidity save in so far as I sedulously rewarded any chance advances of his. Conversely No. 1 and No. 3 seemed influenced by the fear and shyness of No. 2. No. 2's cage was between No. 1's and No. 3's, and they were for three weeks incessantly making hostile demonstrations toward each other, jumping, chattering, scowling, etc. No. 2 never did anything of the sort. Again, seeing No. 3 eat meat did not lead No. 1 to take it; nor did seeing No. 1 retreat in fright from a bit of absorbent cotton lead No. 3 to avoid it.

Nothing in my experience with these animals then favors the hypothesis that they have any general ability to learn to do things from seeing others do them. The question is still an open one, however, and a much more extensive study of it should be made, especially of the possible influence of imitation in the case of acts already familiar either as wholes or in their elements.

#### LEARNING APART FROM MOTOR IMPULSES.

The reader of my monograph, 'Animal Intelligence,' will recall that the experiments there reported seemed to show that the chicks, cats and dogs had only slight and sporadic, if any, ability to form associations except such as contained some actual

motor impulse. They failed to form such associations between the sense impressions and ideas of movements as would lead them to make the movements without having themselves previously in those situations given the motor impulses to the movements. They could not, for instance, learn to do a thing from having been put through it by me.

The monkeys Nos. 1 and 3 were tested in a similar way with a number of different acts. The general conclusion from the experiments, the details of which will be given presently, is that the monkeys are not proved to have the power of forming associations of ideas to any greater extent than the other mammals, that they do not demonstrably learn to do things from seeing or feeling themselves make the movement. An adult human being whose hand was taken and made to push in a bar or pull back a bolt would thereby learn to do it for himself. Cats and dogs would not and the monkeys are not proved to do so. On the other hand, it is impossible for me to say, as of the dogs and cats, that the monkeys are proved not to do so. In a few cases the animals did perform acts after having been put through them which they had failed to perform when left to their own trial and success method. In the majority of cases they did not. And in some of these latter cases failure seemed so improbable in case the animal really had the power of getting an idea of the act and proceeding from idea to execution, that one is inevitably led to some explanation for the few successes other than the presence of 'ideas.'

The general manner of making these experiments was like that in the case of the cats and dogs, save that the monkey's paw was used to open the box from the outside instead of from the inside, and that the monkeys were also put through the acts necessary to operate some of the chute mechanisms. Tests parallel to that of comparing the behavior of kittens who had themselves gone into boxes with those who were dropped in by me were made in the following manner. I would carry a monkey from his cage and put him in some conspicuous place (*e. g.*, on the top of a chair) and then give him a bit of food. This I would repeat a number of times. Then I would turn him loose in the room to see whether he had acquired an idea

of being on the chair which would lead him to himself go to the chair. I would, in order to tell whether his act, in case he did so, was the result of random activities or was really due to his tuition, leave him alone for 5 or 10 minutes before the tuition. If he got on the chair afterwards when he had not before, or got on it much sooner, it would tend to show that the idea of getting food on that chair was present and effective. We may call these last the 'on chair' type of experiments.

A sample experiment with a box is the following :

On January 4, 1901, box Delta (push back) was put in No. 1's cage. He failed in 5, though he was active in trying to get in for about 4 minutes of the time and pulled and pushed the bar a great deal, though up and down and out instead of back. In his aimless pushings and pullings he nearly succeeded. He failed in 5 in a second trial also. I then opened the door of the cage, sat down beside it, held out my hand, and when he came to me took his right paw and with it (he being held in front of the box) pushed the bar back (and pulled the door open in those cases when it did not fall open of itself). He reached in and took the food and went back to the top of his cage and ate it. (No. 1 generally did this while No. 3 generally stayed by me.) I then tried him alone; result 10 F; no activity at all. On January 5th I put the box in; result 10 F. He was fairly active. He pulled at the bar but mostly from a position on the top of the box and with his left hand; no attempts like the one I had tried to teach him. Being left alone he failed in 5. Being tried again with the door of the cage open and me sitting as I had done while putting him through the act, he succeeded in 7.00 by pushing the bar with his head in the course of efforts to poke his head in at the door. I then put him through the act 10 times and left him to himself. He failed in 5.00; no activity. I then sat down by the cage as when teaching him. He failed in 5; little activity. Later in the day I put him through the act 10 times and then left him to himself. He failed in 5; little activity. I sat down as before. He failed in five; little activity. On January 6th I put him through the act 10 times and then left him. He failed in 10. This was repeated later in the day with the same result. Record; — By himself, 10 F. Put through 80 times. F 65 (a)



[the (a) refers to a note of his unrepeatable chance success with his head]. No similar act unsuccessfully attempted. Influence of tuition, none.

With the chute mechanisms the record would be of the same nature. With them I put the animal through generally by taking his paw, held out through the wire netting of the cage, and making the movement with it. In one experiment (No. 3 with QQ chute) the first 58 trials were made by taking the monkey outside the cage and holding him instead of having him put his paw through the netting for me to take.

Many of the experiments were with mechanisms which had previously been used in experiments concerning the ability to learn from seeing me operate them. And the following Table (III.) includes the results of experiments of both sorts. The results of experiments of the on chair type are in Table IV. In cases where the same apparatus was used for both purposes the sort of training which was given first is that where an A is placed.

In the first four experiments with No. 1 there was some struggling and agitation on his part while being held and put through the act. After that there was none in his case except occasional playfulness, and there was never any with No. 3 after the first third of the first experiment. The monkeys soon formed the habit of keeping still because it was only when still that I put them through the act and that food resulted. After you once get them so that they can be held and their arms taken without their clinging to, you they quickly learn to adapt themselves to the experiments.

With No. 1, out of 8 cases where he had of himself failed (in five of the cases he had also failed after being shown by me) he succeeded after being put through (13, 21, 51, 10, 7, 80, and 10 times) in two cases (QQ (chute) and RR (wood plug)). The act was unlike the one taught him in the former case.

In only one case (bolt at top) out of eight was there possibly any attempt at the act after he had been put through which had not been made before. The 'yes or?' in the table with RR was a case occurring after the imitation of me but before the putting No. 1 through.

Out of 6 cases where he had himself failed, No. 3 succeeded (after being put through 113, 23, 20, 10, 10, 20 and 10 times) in 3 cases (chute bar, push down and bar inside). The act was dissimilar in all three cases, bearing absolutely no resemblance in one case. There was no unsuccessful attempt at the act taught him in any of the cases. With the chute he did finger the bar after tuition where he had not done so before, but it was probably an accidental result of his holding his hand out toward it for me to take as he had formed the habit of doing. In the case of box Epsilon (push down), with which he succeeded by pushing his hand in above the lever (an act which though unlike that taught him might be by some considered to be due to an idea gained from the tuition), he failed entirely after further tuition (15 times).

Like the dogs and cats then, the monkeys seemed unable to learn to do things from being put through them. We may now examine those which they did do of themselves before tuition and ask whether they learned the more rapidly thereby or modified their behavior in ways which might be due to the tuition. There are too few cases and no chance for comparison on the first point; on the second the records are unanimous in showing no change in the method of operating the mechanisms due to the tuition.

As in Table I. figures followed by F mean that in that length of time the animal failed. Figures without an F denote the time taken by the animal to operate the mechanism.

As a supplement to Table III. I have made a summary of the cases where the animals did succeed after tuition, that shows the nature of the act shown them as compared with the act they made use of.

TABLE III.

Subject. Date. Act.	Times tried alone	Number of times attention attracted.	Result.	Number of times shown by me.	Result in trials after being put shown by me.	Number of times put through the act.	Result in trials after being through the act.	Comparison of act used with act taught.	Similar act attempted though unsuccessfully.	Act done once or more, but not repeated in spite of repetition.
No. 1, Jan. 7, 1900, PP (string across).	10 F			21A	150 F	13	10 F		no.	
" 1, " 17, " MM (bolt at top).	10 F 15 F			4 } 4 } 4 } 4 } 12	10 F 10 F .20	21	10 F	Partly Similar.	(?)	
" 1, Feb. 24, " OOO (upper string).	2.20 3 F			4 } 4 } 4 } 4 }	.22 60 F				no.	
" 1, Mar. 24, " QQ (chute).	120 F			10A	2 F	10	30.00	Dissimilar.	no.	
" 1, Apr. 5, " RR (wood plug).	10 F	2	5 F	1A 1 1 1 4	2 F 2 F 2 F 5 F .22	7 2	2.20 2.00	Similar.	yes(?)	
" 1, Oct. 20, " VV (loop).	10 F 10 F 10 F 10 F			5 15	10 F 10 F	51A 80A	132 F 65 F <sup>(1)</sup>		no. no.	
" 1, Nov. 19, " Theta (new bolt).	10 F			40	10 F 2.00 .50	20	im. im.	Dissimilar.		
" 1, Jan. 4, 1901, Delta (push back).	10 F									
" 1, " 6, " QQ (a) (single wind chute).	8.00									
" 1, " 7, " Zeta (side plug new).	5 F 1.10							?		

<sup>1</sup> He did push it once with his nose.



TABLE III.—Continued.

Subject. Date. Act.	Times tried alone with result.	Number of times attention attracted.	Result.	Number shown by me.	Result in trials after being shown by me.	Number of times put through the act.	Result in trials after being put through the act.	Comparison of act taught.	Similar act attempted though unsuccessfully.	Act done once or more, but not repeated in spite of repeated tuition.
No. 1, Jan. 9, 1901, QQ (b) ( $2\frac{1}{2}$ wind chute)	3.00 to .06 5 F			10	5 F 5 F 3.00 5 F			Dissimilar	no.	yes.
" 1, " 11, " QQ (c) (nail chute).	5 F 5 F 5 F 10 F	5	5 F	1 (1)	2.20			Dissimilar	no.	yes (2)
" 1, " 12, " Epsilon (push down).				25 A 15	10 F 10 F 10 F	10 10	5 F 10 F			
" 1, " 16, " QQ (d) (pull chute).	5 F 5 F 5 F	5	3.30 .10 5 F	15 A	5 F 5 F	10	5 F			
" 1, " 17, " QQ (f) (string chute).	5 F 5 F 5 F	5	im. 60 F	10 A 30	5 F 30 F	113	90 F		(?)	
" 1, " 18, " QQ (e) (hook chute).	5 F 60 F	3				23	20 F		no.	
" 3, Dec. 17, 1900, QQ (chute).	10 F 20 F 10 F 2.10 (by pulling string).	3		20	8.00 <sup>3</sup> 8.00 <sup>3</sup>	5 A 5 15 5	2.00 <sup>4</sup> 3.20 30 F 10 F	Dissimilar	no.	
" 3, " 17, " VV (loop).										
" 3, Jan. 4, 1901, Delta (push back).										

<sup>1</sup> I inadvertently pulled the nail out in one of five cases when I was fingering it to see if attracting his attention to it would lead to the act.

<sup>2</sup> Not significant. Due to inattention. Was temporary.

<sup>3</sup> Pulled wire and door.

<sup>4</sup> Pushed with head by chance.

TABLE III.—Continued.

Subject. Date. Act.	Times tried alone	Number of times attention attracted.	Result.	Number of times shown by me.	Result in trials after being put shown by me.	Number of times through the act.	Result in trials after being put through the act.	Comparison of act used with act taught.	Similar act attempted though unsuccessfully	Act done once or more, but not repeated in spite of repeated tuition.
No. 3, Jan. 4, 1901, Gamma (wind).	10 F			30	10 F	20 A	5 F		no.	
" 3, " 8, " Theta (bolt at top).	10 F				10 F		8 F			
" 3, " 9, " QQ (a) (chute bar).	10 F <sup>(1)</sup>			25	6 F		.40	Dissimilar.	no	
" 3, " 9, " QQ (b) (2½ wind chute).	10 F				3.00 <sup>2</sup>	10	1.00	?	com-	
" 3, " 11, " QQ (c) (nail chute).	5 F	10	5 F	20	8 F	45	1.00	Dissimilar.	plete	yes.
" 3, " 15, 1901, Epsilon (push down).	5 F		12 I <sup>3</sup>	25 A	5 F	10	5 F		circle.	yes.
" 3, " 16, " QQ (e) (hook chute).	10 F				1.30		10 F			
" 3, " 19, " QQ (ff) (string chute with wire).	5 F	5	5 F	25 A	5 F	20	11.00	?	no.	yes.
" 3, " 22, " WW (bar inside).	5 F				5 F		30 F			
	5 F				5 F	15	10 F			
	2.00 <sup>(4)</sup>				2.00	10	.10	Dissimilar.	no.	
	5 F				1.25		.10			
	some 40.00 F				1.20		5 F			
					5 F	7	5 F			
					5 F	8	5 F			
					2.00 <sup>(4)</sup>	12	3.00	Dissimilar.	no.	
					5 F	10	5 F			
					5 F		6.00 <sup>(5)</sup>			
					5 F		7.00 <sup>(5)</sup>	Dissimilar.	no.	

<sup>1</sup> Reached in at 9:30 and took out the banana, which I replaced.<sup>2</sup> Did by constant pulling at the door.<sup>3</sup> Did touch nail four times.<sup>4</sup> Did by pulling hard on wire (not loop); the loop got loose from nail.<sup>5</sup> Did by pulling at the door till the bar was worked around.

## SUPPLEMENT TO TABLE III.

Apparatus.	Model given or act put through.	Act of No. 1.	Act of No. 3.
OOO	To pull upper string.	Pulled both strings alternately, but upper enough more to succeed.	
QQ	To push bar in.	Inserted fingers between bar and its slot and pulled and pushed vaguely.	
RR	To pull plug out with right hand.	Pulled and bit.	
VV	To pull loop off nail with right hand.	<i>Similar.</i>	
QQ (a)	To pull bar around toward him.	Pulled back and forth indiscriminately.	Pulled back and forth indiscriminately.
QQ (b)	To pull bar around toward him in $2\frac{1}{2}$ continuous revolutions.	Pulled back and forth indiscriminately.	
QQ (c)	To take nail and pull directly outward.	Pulled back and forth.	<i>Similar</i> or nearly so.
Delta	To push bar to right with right hand.		Did before tuition by pulling wire; after tuition by chance movement of head.
Theta	To pull bolt up with right hand.		Pulled door and worked bolt loose.
Epsilon	To stand in front, insert fingers of right hand and press lever down.		Inserted arm in general activity while on top of the box.
QQ (e)	To pull hook down.		Pulled at the lever and hook in a general attack on the apparatus.
QQ (ff)	To pull wire loop off nail with right hand.		Pulled outward on the lever which pushed the banana down the chute so hard as to pull it off its pivot.
WW	To stand on top of box, reach right hand down and pull bar up.		Pulled at door until bar worked out of its catch.



I have kept the results of the tests of the 'on chair' type separate from the others because they may be tests of a different thing and surely are subject to different conditions.

They were tests of the animals' ability to form the habit of going to a certain place by reason of having been *carried* there and securing food thereby. I would leave the animal loose in the room, and if he failed in 5 or 10 minutes to go to the place of his own accord would put him back in his cage; if he did go of his own accord I would note the time. Then I would take him, carry him to the place, and feed him. After doing this 10 times I would turn him loose again and see whether the idea of being fed in such and such a place was present and active in making him go to the place. In such tests we are absolutely sure that the animal can without any difficulty perform the necessary movements and would in case the proper stimulus to set them off appeared, if, for instance, a bit of food on one of the places to which he was to go caught his eye. In so far forth the tests were favorable cases for learning. On the other hand, the situation associated with getting food may have been in these cases not the mere 'being on box' but the whole previous experience 'being carried while clinging and being put or let jump on a box.' In this respect the tests may have been less favorable than the acts where getting food was always the direct sequence of the act of going into the box.

The experiments were:

- A. Carrying the animal and putting him on a chair.
- B. Carrying the animal and putting him on a pile of boxes.
- C. Carrying the animal and putting him on the top of a sewing machine.
- D. Carrying the animal and putting him on the middle of a board 6 feet long, stretched horizontally across the room, 3 feet from the floor.
- E. Carrying the animal and putting him on the side of the cage, head down.

The results are given in Table IV., p. 52.

The size of the room in which I worked and other practical difficulties prevented me from extending these experiments. As they stand no stable judgments can be inferred from them. It

should be noted that in the successful cases there were no other signs of the presence of the idea 'food when there' than the mere going to a certain place. The animal did not wait at the place more than a second or two, did not look at me or show any signs of expecting anything.

TABLE IV.

Experiment and date.	Animal.	Results before training.	Number of times put through.	Results after training.
A. Jan. 22, 1901	No. 1.	5 F	10	1.00
" 25, "	No. 1.	5 F	10	3.00 im.
" 23, "	No. 3.	5 F	10	3.30
		5 F		3.30
B. " 26, "	No. 1.	10 F	10 and 5	10 F 5 F
	No. 3.	5 F	10	5 F
			10	5 F
C. " 27, "	No. 1.	5 F	10	3.00
D. " 27, "	No. 1.	3.20	10	5 F
E. " 26, "	No. 3.	5 F	5	5 F

Although, as I noted in the early part of this monograph, there were occasionally phenomena in the general behavior of the monkeys which of themselves impressed one as being suggestive of an ideational life, the general run of their learning apart from the specific experiments described was certainly confined to the association of impulses of their own with certain situations. The following examples will suffice :

In getting them so that they would let themselves be handled it was of almost no service to *take* them and feed them while holding them or otherwise make that state pleasant for them. By far the best way is to wait patiently till they do come near, then feed them; wait patiently till they do take hold of your arm, then feed them. If you do take them and hold them partly by force you must feed them only when they are comparatively still. In short, in taming them one comes unconsciously to adopt the method of rewarding certain of their impulses rather than certain *conditions* which might be associated in their minds with ideas, had they such.

After No. 1 and No. 3 had both reached a point where both could hardly be gotten to leave me and go back into their cages

or down to the floor of the room, where they evidently enjoyed being held by me, they still did not climb upon me. The idea of clinging to me was either absent or impotent to cause them to act. What they did do was, in the case of No. 1, to jump about pawing round in the air until I caught an arm or leg, to which stimulus he had by dint of the typical sort of animal association learned to react by jumping to my arm and clinging there; in the case of No. 3, to stand still until I held my arm right in front of him (if he were in his cage) or to come and stand on his hind legs in front of me (if he were out on the floor). In both cases No. 3's act was one which had been learned by my rewarding his impulses. I often tried, at this period of their intimacy with me, this instructive experiment. The monkey would be clinging to me so that I could hardly tear him away. I would do so, and he would, if dropped loose from me, make no efforts to get back.

I have already mentioned my failure to get the animals to put out their right hands through the netting after they had long done so with their left hands. With No. 3 I tried putting my fingers through and poking the arm out and then making the movement with it. He profited little if any by this tuition. Had I somehow induced him to do it himself, a few trials would have been sufficient to get the habit well under way.

Monkey No. 1 apparently enjoyed scratching himself. Among the stimuli which served to set off this act of scratching was the irritation from tobacco smoke. If any one would blow smoke in No. 1's face he would blink his eyes and scratch himself, principally in the back. After a time he got in the habit of coming to the front of his cage when any one was smoking and making such movements and sounds as in his experience had attracted attention and caused the smoker to blow in his face. He was often given a lighted cigar or cigarette to test him for imitation. He formed the habit of rubbing it on his back. After doing so he would scratch himself with great vigor and zest. He came to do this always when the proper object was given him. I have recounted all this to show that the monkey enjoyed scratching himself. *Yet he apparently never scratched himself except in response to some sensory*



*stimulus*. He was apparently incapable of thinking 'scratch' and so doing. Yet the act was quite capable of association with circumstances with which as a matter of hereditary organization it had no connection. For by taking a certain well-defined position in front of his cage and feeding him whenever he did scratch himself I got him to always scratch within a few seconds after I took that position.

#### GENERAL MENTAL DEVELOPMENT OF THE MONKEYS.

It is to be hoped that the growing recognition of the worth of comparative and genetic studies will lead to investigations of the mental make-up of other species of monkeys, and to the careful overhauling of the work done so far, including these rather fragmentary studies of mine. Work with three monkeys of one species, especially when no general body of phenomena such as one has at hand in the case of domestic animals can be used as a means of comparison, must necessarily be of limited application in all its details and of insecure application even in its general features. What I shall say concerning the advance in the mental development of the monkeys over that of other mammals may then be in strictness true of only my three subjects, and it may be left to the judgment of individuals to extend my conclusions as far as seems to them likely. To me it seems fairly likely that the very general mental traits which the research has demonstrated hold true with little variation in the monkeys in general.

The monkeys represent progress in mental development from the generalized mammalian type toward man :

1. In their sensory equipment, in the presence of focalized vision.
2. In their motor equipment, in the coordinated movements of the hand and the eye.
3. In their instincts or inherited nervous connections, in their general physical and mental activity.
4. In their method of learning or associative processes ; in
  - a. Quicker formation of associations,

- b. Greater number of associations,
- c. Greater delicacy of associations,
- d. Greater complexity of associations,
- e. Greater permanence of associations.

The fact of (1) is well known to comparative anatomists. Its importance in mental development is perhaps not realized, but appears constantly to a systematic student.

(2) is what accounts for much of the specious appearance of human ways of thinking in the monkeys and becomes in its human extension the handy tool for much of our intellectual life. It is in great measure the prerequisite of (4) (c).

(3) accounts for the rest of such specious appearances, is at the basis of much of (4) (b), presages the similar, though extended instincts of the human being, which I believe are the leading efficient causes of human mental capacity, and is thus the great mental bond which would justify the inclusion of monkeys and man in a common group if we were to classify animals on the basis of mental characteristics.

Even the casual observer, if he has any psychological insight, will be struck by the general, aimless, intrinsically valuable (to the animal's feelings) physical activities of a monkey compared with the specialized, definitely aroused, utilitarian activities of a dog or cat. Watch the latter and he does but few things, does them in response to obvious sense presentations, does them with practical consequences of food, sex-indulgence, preparation for adult battles, etc. If nothing that appeals to his special organization comes up, he does nothing. Watch a monkey and you cannot enumerate the things he does, cannot discover the stimuli to which he reacts, cannot conceive the *raison d'être* of his pursuits. Everything appeals to him. He likes to be active for the sake of activity.

The observer who has proper opportunities and takes proper pains will find this intrinsic interest to hold of mental activity as well. No. 1 happened to hit a projecting wire so as to make it vibrate. He repeated this act hundreds of times in the few days following. He did not, could not, eat, make love to or get preliminary practice for the serious battles of life out of that sound. But it did give him mental food, mental exercise.

Monkeys seem to enjoy strange places; they revel, if I may be permitted an anthropomorphism, in novel objects. They like to have feelings as they do to make movements. The fact of mental life is to them its own reward.

It is beyond question rash for any one to venture hypotheses concerning the brain parallel of mental conditions, most of all for the ignoramus in the comparative histology of the nervous system, but one cannot help thinking that the behavior of the monkeys points to a cerebrum that is no longer a conservative machine for making a few well-defined sorts of connections between sense impressions and acts, but is not only fitted to do more delicate work in parts but is also alive, tender all over, functioning throughout, set off in action by anything and everything. And if one adds coordinations allowing a freedom and a differentiation of action of the muscles used in speech comparable to that already present in connection with the monkeys' hand, he may well ask, "What more of a nervous mechanism do you need to parallel the behavior of the year-old child?" However, this is not the place to speculate upon the importance to human development of our instinctive aimless activity, physical and mental, or to describe further its similarity and evident phylogenetic relationship to the instinctive behavior of the monkeys. Elsewhere I shall undertake that task.

4. In their method of learning, the monkeys do not advance far beyond the generalized mammalian type, but in their proficiency in that method they do. They seem at least to form associations very much faster and they form very many more. They also seem superior in the delicacy and in the complexity of the associations formed and the connections seem to be more permanent.

This progress may seem, and doubtless will to the thinker who looks upon the human intellect as a collection of functions of which ideation, judgment and reasoning are chief, to be slight. To my mind it is not so in reality. For it seems to me highly probable that the so-called 'higher' intellectual processes of human beings are but secondary results of the general function of having free ideas and that this general function is the result of the formation after the fashion of the animals of a



very great number of associations. I should therefore say, "Let us not wonder at the comparative absence of free ideas in the monkeys, much less at the absence of inferences or concepts. Let us not wonder that the only demonstrable intellectual advance of the monkeys over the mammals in general is the change from a few, narrowly confined, practical associations to a multitude of all sorts, for that may turn out to be at the bottom the only *demonstrable advance of man*, an advance which in connection with a brain acting with increased delicacy and irritability, brings in its train the functions which mark off human mental faculty from that of all other animals.

The typical process of association described in the experimental study has since been found to exist among reptiles (by Mr. R. M. Yerkes) and among fishes (by myself). It seems fairly likely that not much more characterizes the primates. If such work as that of Lubbock and the Peckhams holds its own against the critical studies of Bethe, this same process exists in the insects. Yerkes and Bosworth think they have demonstrated its presence in the cray-fish. Even if we regard the learning of the invertebrates as problematic still this process is the most comprehensive and important thing in mental life. I have already hinted that we ought to turn our views of human psychology upside down and study what is now casually referred to in a chapter on habit or on the development of the will, as the general psychological law, of which the commonly named processes are derivatives. When this is done we shall not only relieve human mentality from its isolation and see its real relationships with other forms; we may also come to know more about it, may even elevate our psychologies to the explanatory level and connect mental processes with nervous activities without arousing a sneer from the logician or a grin from the neurologist.



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## The Correlation of Mental and Physical Tests

BY

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[Submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy in the Faculty of Philosophy, Columbia University, and being Vol. IX., No. 2, of Columbia University Contributions to Philosophy, Psychology and Education.]

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## CONTENTS.

Introduction .....	1
The Tests Employed .....	4
Class and Sex Differences .....	14
The Correlation of Results.....	23
Tests of Quickness and Accuracy.....	27
Memory Tests .....	32
College Standing.....	34
Physical Tests .....	37
Discussion of Results.....	42
Summary .....	61

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## INTRODUCTION.

This research is occasioned by the fact that psychologists and students of education have proposed certain tests, put them to trial and recorded their results, hoping thereby to find a means by which the fundamental elements of general and specific ability could be isolated and valued. In this they have not been without precedent, since upon the same assumption fitness for the civil and military service, for academic degrees and honors, for professional and technical licenses, etc., is usually determined by arbitrary tests and estimated by numerical averages in grade books. Our work is primarily with the grade book of the psychologist. It is of both theoretical and practical importance to know what relations exist between the results of his tests and those of others. The contention that all tests are arbitrary and futile has no weight in this connection so long as people go on using them. Educational, professional, physical and psychological tests are with us and bid fair to remain, at least until something better is found. Thus the times demand that the results obtained by the various tests be made an object of study. The most obvious line of approach in this problem is through correlation. If a test is general, then its results should correlate with many other special tests, and, in turn, if there is an integral relation between general and special ability, the results of the latter should correlate with the former. Should two tests show no correlation whatever, we can do no more than regard them as defining two entirely independent forms of activity. To determine the relative value of tests with respect to their general or specific significance, we must find some way of estimating the degree of correlation in terms of variability.

To the reader is due some explanation as to the methods of treating data. It is not proper to demonstrate these methods here, because they are not the objects of investigation, but, while they are treated at length in the appropriate literature, the mode of presentation is beyond the comprehension of all save the ex-

pert or those few who can afford to spend their time working up to them. So it seems necessary to give a few words to the methods employed.

The first thing to learn is that when mathematical formulas are resorted to it is only for purposes of convenient and exact statement. Long ago astronomers and engineers discovered that errors of observation are distributed in a certain orderly manner and are consequently susceptible to mathematical statement. By proper treatment they are able to estimate the precision of measurements, determine the number of observations necessary to a given standard of precision, etc. It was soon discovered that biological variation followed similar laws and that such variation could be measured accurately, thus enabling us to determine differences of race, type, species, etc. When psychologists took up their side of the question as to the precision of observations they soon discovered that all human performances, when objectified in units of space, time, etc., seem to follow certain laws of variability, and that these laws are in turn similar to those already worked out. In other words, variability is no longer a barrier to the study of human activity, because we can measure that variability. In more recent times it has been found possible to deal with variability in such a way that the functional and structural relations between phenomena can be accurately estimated, or, in technical terms, a method of correlation has been developed. As may be inferred, these methods are based upon empirical study, they have been attested by use and are the work of some of the best mathematicians and scientists of the past and present centuries. The reader who doubts their validity must look to the authors themselves.

In the following pages only such statements of the method have been offered as will enable the reader to understand the force of the results. In correlation it need only be borne in mind that we are using an accepted method to estimate the relative necessary relations between the phenomena under consideration. To make any such comparison at all we must assign our results a place in a scale of values. This is what the method of correlation does.

It remains for the writer to define his relation to this research. The tests were devised and conducted by Professor J. McKeen Cattell and his associates, but for the methods of compilation, together with all conclusions and opinions respecting the results and the validity of the various tests, the writer is responsible. The conception of the problem and the accumulation of material must be credited to the former, while the latter has only undertaken the compilation of results.

It is scarcely possible to mention the names of all who assisted in making the tests, but special note should be made of the fact that most of the tests on Barnard students were made by Miss S. L. Cody.



## THE TESTS EMPLOYED.

These tests are made yearly upon sixty to seventy freshmen of Columbia College and repeated with those who remain to the end of the senior year. A preliminary report of methods and class averages, published in 1896,<sup>1</sup> gives in detail the methods of recording and preserving data. However, some recent additions and changes make it desirable to repeat them.

The tests now made in the psychological laboratory are as follows: length and breadth of head, strength of hands, fatigue, eyesight, color vision, hearing, perception of pitch, perception of weight, sensation areas, sensitiveness to pain, perception of size, color preference, reaction time, rate of perception, naming colors, rate of movement, accuracy of movement, perception of time, association, imagery, memory (auditory, visual, logical and retrospective). Records of stature, weight, etc., together with data concerning parentage, personal habits and health, are a part of the gymnasium tests required of all students in Columbia College.

At the beginning of the tests in the psychological laboratory a few words are said to the student concerning the object of the tests and the value of the results. He is then told to write his name, class and date of birth on the record blank; also to give information as to any tests already made upon him and necessary information as to how a copy of such measurements may be obtained. While the student is thus engaged the observer fills in the following:

What is his apparent age? (.....), 17 (.....), 18 (.....), 19 (.....), 20 (.....), (.....).

Is his apparent state of health good (.....), above medium (.....), below medium (.....), poor (.....)?

Is he tall (.....), above medium (.....), below (.....), short (.....)?

Is his head large (.....), above medium (.....), below medium (.....), small (.....)?

<sup>1</sup>Physical and Mental Measurements of the Students of Columbia University. Cattell and Farrand. *PSYCHOLOGICAL REVIEW*, Vol. III., p. 618.

Do you think his physical development good (.....), above medium (.....), below medium (.....), poor (.....) ?

Do you think him likely to be as a student good (.....), above medium (.....), below medium (.....), poor (.....) ?

In these mental tests do you think him likely to be good (.....), above medium (.....), below medium (.....), poor (.....) ?

Of this blank the student is kept in ignorance. He is asked whether he is right-handed for all purposes, the observer noting independently the preferred hand in writing and the other movements necessary in the tests.

*Perception of Size.*—This test is made first, in order that retrospective memory may be tested at the end of the series. A sheet of paper bearing a 5 cm. line is placed before the student and a blank sheet of the same size on the right. Without moving them or altering much his point of view he is required to draw a line as nearly as possible the length of the standard. This done he is requested to bisect the line drawn and from the middle draw a perpendicular as long as the whole line and then bisect the right-hand angle. The student is required to do this quickly.

*Size of Head.*—In measuring the head the maximum length and breadth are taken with calipers. For length the directions to observers are, to place one point of the calipers on the most prominent point of the forehead, between the eyebrows. If in this region very prominent bony ridges are felt, take a second measurement from a point just above the ridges and note both. Bring the other point of the calipers down to the posterior part of the head and move it along the middle line until the greatest length of head is found. For breadth of head take the maximum above the ears wherever found. Hold the calipers horizontally and perfectly symmetrically. Make two measurements of each diameter independently and record the results in mm. in the order in which they were made. Repeat until the average variation of the accordant measurements is about 1 mm. Leave a record of all measurements made.

*Strength of Hand.*—This test is made with the oval dynamometer. The student is shown how to hold the instrument and makes the test standing. He is not to see the dial or the record while making the test. The order is right-hand, left, right, left—four trials in all.

*Fatigue.*—The test is made with Cattell's ergometer. The pressure is applied by the ends of the index finger and thumb and the tension of the spring registered on a counter dial. The right hand is used and one trial of 50 efforts in a rhythm of about one a second is taken as the result. The observer records the reading for each ten efforts.

*Eyesight.*—These tests are made with Galton's instrument which gives the distance in cm. at which diamond numerals can be read by each eye singly. The right eye is tested first, beginning at a distance of 44 cm. From this the student proceeds until a card is reached where he can read at least eight out of ten numerals. The test is made in ordinary daylight.

*Color Vision.*—The subject is required to select the green shades from the woolen skeins supplied by the Cambridge Scientific Instrument Company in accordance with Mr. Galton's instructions.

*Hearing.*—The test is made in a quiet room with the ticking of a stop-watch as the standard. 5 m. and above is taken as the normal distance and under 1m. as abnormal.

*Perception of Pitch.*—The test is made with a monochord tuned so that F below middle C is given when the bridge is at 75 cm. The directions for making the test are as follows: "Give tone F twice at an interval of about two seconds to the student, whose back is turned. Then shift the bridge to about 50 and let the student find the tone. He must be warned against humming the tune and must probably be taught in advance how to use the monochord. Record the position of the bridge and then give the original tone twice and shift the bridge to the place where it was left by the student in his first trial, telling him that it is put back to this place. Let him now find the tone and record the position. Ask the student whether he plays a musical instrument or sings and record his answers."

*Perception of Weight or Force of Movement.*—In this test the lift is vertical and the dynamometer gives a pressure of 1 kg. to 10 cm. A mechanical stop is provided at a pressure of 1 kg. to give the student his standard. In making the test he is told to lift the handle to the stop three times and then make ten attempts to lift it to the same height after the operator has re-



moved the stop. Each lift is to be made in about 2 sec., with equal pauses between. A graphic record of the lifts is taken on a kymograph and filed with the other data.

*Sensation Areas.*—The points of the æsthesiometer are 2 cm. apart and the instrument is applied longitudinally to the back of the left hand, between the bones of the second and third fingers. Five tests are made, the student being touched with one or two points in the order, two, two, one, one, two, and being required to decide in each case whether he was touched with one or with two points.

*Sensitiveness to Pain.*—This is determined for the ball of the thumb of the right and left hands. An algometer is used in which the surface applied is of rubber, 1 cm. in diameter and rounded at the corners. The instrument is applied with gradually increasing pressure by the observer and the student is told to say as soon as the pressure becomes disagreeable. If he show signs of discomfort the pressure is stopped. Two tests are made on each hand in alternation, beginning with the right hand.

*Color Preference.*—Rectangles (5 x 3 cm.) of red, orange, yellow, green, blue, violet and white are shown in an irregular group on a black ground and the student asked to specify his likes and dislikes.

*Reaction-time.*—The reaction-time for sound is taken five times in succession with the Hipp chronoscope. After the reactions the student is asked whether he attended to the sound, to the hand or to both.

*Rate of Perception.*—A blank is provided containing 500 11-point capital letters, of which 100 are A's. Each of the other letters occurs 16 times and the whole series is arranged in an order drawn by lot. The student is required to mark as quickly as possible all the A's, the observer taking the time with a stopwatch. The blank is kept as a part of the student's test record and shows the accuracy of the performance as well as its quickness.

*Naming Colors.*—One hundred 1 cm. squares of colored paper (red, orange, yellow, green, blue, violet, pink, gray and black) arranged in chance order on a white ground are to be

named as quickly as possible. The observer takes the time with a watch and notes the errors. This is really a test in rate of reading or naming familiar things. Care is taken to see that all students have a ready name for each color on the card before taking the test.

*Rate of Movement.*—A blank ruled into one hundred 1-cm. squares into each of which the student must put a dot, completing the task as quickly as possible, constitutes this test. The observer records the time with a watch and preserves the record.

*Accuracy of Movement.*—Here a blank with 100 dots arranged in the form of a 10-cm. square is provided, the student being required to strike at each dot in succession, the aim being to hit them as nearly as he can and as quickly as possible. The time is taken by the observer and the blank preserved for the computation of accuracy.

*Rhythm and Perception of Time.*—The present test is of the ability to follow a given rhythm. The student makes with a telegraph key taps in the rhythm of sounds which occur one per second. He is told to continue tapping fifty times at the same rate after the sounder is stopped, which is after ten beats. The standard is given by a telegraph sounder operated by a clock. The student's tapping is recorded on a kymograph with a clock line in parallel.

*Association.*—A blank is provided containing the following words in bold-faced type: house, tree, child, time, art, London, Napoleon, red, enough. The observer explains the test to the student, and, when everything is ready, the blank is handed to the latter, who writes after each word as quickly as possible what it suggests to him, preferably a single word. The observer takes the time and files the blank.

*Imagery.*—The student answers the following questions:

Think of your breakfast table as you sat down to it this morning; call up the appearance of the table, the dishes and food on it, the persons present, etc. Then write answers to the following questions:

1. Are the outlines of the objects distinct and sharp?
2. Are the colors bright and natural?
3. Where does the image seem to be situated? In the head? Before the eyes? At a distance?

4. How does the size of the image compare with the actual size of the scene?

1. Can you call to mind better the face or the voice of a friend?
2. When "violin" is suggested, do you first think of the appearance of the instrument or the sounds made when it is played?
3. (a) Can you call to mind natural scenery so that it gives you pleasure?  
(b) Music? (c) The taste of fruit?
4. Have you ever mistaken an hallucination for a perception, *e. g.*, apparently heard a voice or seen a figure when none was present? If you answer "yes," describe the appearance on the back of this sheet.

*Memory.*—Four different tests are made in the order given here. The observer instructs the student as to what is expected of him in each case.

1. Auditory.—Each series of numerals in the following is read at a rate of about 2 per second, after which the student writes it from memory:

4	8	3	7	1	9	6	2
7	5	9	2	8	6	4	1
3	7	5	2	9	6	4	8

2. Visual.—Corresponding numerals are shown at the same rate.

3	9	5	2	6	8	1	4
8	5	2	7	4	6	3	1
5	1	6	2	7	3	4	8

3. Logical.—The following passage containing 100 words is read to the student, who then writes as much of it as he can. He is directed to give the words wherever possible, but attempt to give the thought completely.

"Tests such as we are now making are of value both for the advancement of science and for the information of the student who is tested. It is of importance for science to learn how people differ and on what factors these differences depend. If we can disentangle the complex influences of heredity and environment we may be able to apply our knowledge to guide human development. Then it is well for each of us to know in what way he differs from others. We may thus in some cases correct defects and develop aptitudes which we might otherwise neglect."

4. Retrospective.—At the beginning of the test a 5-cm. line was shown the student in the size test. At the end of the hour he is reminded of the line and requested to draw it from memory. This was devised as a test of memory for a thing which one had no special object in remembering.



During the test as opportunity may offer the observer fills in the following :

Forehead : straight (.....), rather straight (.....), somewhat sloping (.....), sloping (.....) ?  
 Hair : black (.....), dark brown (.....), light brown (.....), flaxen (.....), red (.....) ?  
 Complexion : dark (.....), rather dark (.....), rather light (.....), light (.....) ? clear (.....), fairly clear (.....), not clear (.....), blotched (.....) ?  
 Eyes : gray (.....), blue (.....), brown (.....) ?  
 Hair : straight (.....), slightly wavy (.....), somewhat curly (.....), curly (.....) ?  
 Nose : convex (.....), slightly convex (.....), slightly concave (.....), concave (.....) ? Elevation : high (.....), above medium (.....), below medium (.....), low (.....) ?  
 Ears : large (.....), above medium (.....), below medium (.....), small (.....); projecting (.....), somewhat projecting (.....), rather close (.....), close (.....) ?  
 Mouth : large (.....), above medium (.....), below medium (.....), small (.....) ?  
 Lips : thick (.....), above medium (.....), below medium (.....), thin (.....) ?  
 Hands (in relation to size of body) : large (.....), above medium (.....), below medium (.....), small (.....) ?  
 Fingers (in relation to width of hand) : long (.....), above medium (.....), below medium (.....), short (.....) ?  
 Face and Head : note asymmetry, also any abnormality, as malformation of ears, squint, etc.

After having discussed the subject the observer fills in the following :

Do you think his state of health good (.....), above medium (.....), below medium (.....), poor (.....) ?  
 Do you think his physical development good (.....), above medium (.....), below medium (.....), poor (.....) ?  
 Do you think him likely to be as a student good (.....), above medium (.....), below medium (.....), poor (.....) ?  
 Do you think that in the mental tests he has done well (.....), above medium (.....), below medium (.....), poorly (.....) ?  
 In understanding what was wanted, was he quick (.....), above medium (.....), below medium (.....), slow (.....) ?  
 Was he talkative (.....), above medium (.....), below medium (.....), quiet (.....) ?  
 Do you judge him to be accurate (.....), above medium (.....), below medium (.....), not accurate (.....) ?  
 Do you judge him to be straightforward (.....), above medium (.....), below medium (.....), not straightforward (.....) ?

- Do you judge him to be intellectual (.....), above medium (.....), below medium (.....), not intellectual (.....)?
- Do you judge his will to be strong (.....), above medium (.....), below medium (.....), weak (.....)?
- Do you judge his emotions to be strong (.....), above medium (.....), below medium (.....), weak (.....)?
- Would you call him well-balanced (.....), above medium (.....), below medium (.....), not well-balanced (.....)?
- Would you call his temperament choleric (.....), sanguine (.....), melancholic (.....), phlegmatic (.....)?

The students are taken through the tests singly. Students and officers of the department act as observers and the average time required to make the test is 45 min. The above enumeration of tests is the order in which they occur on the blank, but as three freshmen are usually taken through during the same period and it being impossible to duplicate all the apparatus the regular order of the blank is not followed, and the tests are arranged in definite groups in separate rooms and each group given in the order of its occurrence in the blank. In all cases the size test is made first and the fatigue and retrospective memory tests at the end of the series. By keeping a record of the order of the three observers for each test period it is possible to divide the students into three groups in each of which all followed the same order.

It is proposed to add tests of attention, apperception, suggestion, etc., to the above series, but so far no very satisfactory method has been evolved. In case of apperception, supplying the missing words in the following was tried:

The manner in which.....of imagery and  
.....follow each other through our.....  
the.....flight of one idea before the next, the  
.....our minds make between things wide as the  
.....asunder, transitions which at first sight  
.....us by their abruptness, but which, when  
.....closely, often reveal.....links  
of.....naturalness and.....all  
this.....streaming has  
from time immemorial excited the.....of all those  
.....happened to be caught by its.....  
mystery.

This, like most mental tests, is in the nature of an experiment. No one can tell offhand what will make a good test for a particular function.

In the observer's blank it will be seen that certain ready estimates of the student are required. The data thus obtained is more for estimating the precision of such methods of observation than for affording information concerning the subject himself. In many cases it is possible to compare the judgment of the observer with actual measurements. By some such procedure it seems possible to determine the general probable error of ordinary judgments.

In connection with the gymnasium measurements the student is required to supply the following information :

Nationality {	Birthplace	Date of birth					
	of father	of mother					
	his father	her father					
	his mother	her mother					

	Father.	Mother.	Paternal Grandfather.	Paternal Grandmother.	Maternal Grandfather.	Maternal Grandmother.
Living? (if so, give age) . . . . .						
Deceased? (if so, give year of death and age at time of death) . . . . .						
Cause of death, if deceased . . . . .						
Most serious diseases from which they have suffered . . . . .						

	1	2	3	4	5	6	etc.
Your mother's { born. children . . . } deceased.							

[Write B for brother and S for sister in the order of age and in the proper column. Include yourself designated by X. After B, S or X write date of birth thus: B. Feb. 10, '84. In case any brothers or sisters have died, write date and cause of death after 'deceased.']

How many brothers did your father have? ( ), how many sisters? ( )  
 was your father his mother's 1st ( ), 2d ( ), 3d ( ), 4th ( ),  
 5th ( ), 6th ( ), or what ( ) child?

How many brothers did your mother have? ( ), how many sisters?  
 ( ), was your mother her mother's 1st ( ), 2d ( ), 3d ( ), 4th  
 ( ), 5th ( ), 6th ( ), or what ( ) child?



Which side of the family do you most resemble in physical build ?

Previous to your birth did your parents live in the country, town or city ?

What portion of your life has been spent in the country (                      ),  
town (                      ), city (                      ) ?

Occupation of father previous to your birth ?                      at present ?

Health previous to your birth of father ?                      of mother ?

How many hours do you sleep (                      ) ?

At what time of day can you study best (                      ) ?

In addition to the above, a series of questions concerning general health are submitted, supplemented by inquiries as to personal habits of eating, drinking, smoking, exercise, work, etc.

The idea of those responsible for these tests was to secure as many data as possible concerning the individual in connection with his college record and his life history. It seems certain that the correlation of such results will give us an insight into the elements that make for success in daily life, as well as point the way to scientific progress. In the following research only a part of these data has been worked over. The standpoint of the writer being largely psychological, more time has been given to the tests made in the psychological laboratory than to those of the gymnasium or class-room.

## CLASS AND SEX DIFFERENCES.

Besides some 250 freshmen and a small number of seniors in Columbia College, we have the results for a number of young women in Barnard College. This enables us to compare the sexes and to estimate the changes among men during their college life. In Table I. are such results as can be easily reduced to averages. Here are the totals for freshmen, seniors and women. The probable distribution of cases around each average is computed according to the formula

$$p = 0.6745 \sqrt{\frac{\sum v^2}{n-1}}$$

In length of head, for example,  $p = 4.3$ , and  $194.0 \pm 4.3$  is understood as defining the limits between which approximately 50% of the cases will fall.  $p$  expresses the probable variation in the group, and the limits of the group may be estimated as average  $\pm 5 p$ .

In estimating the certainty of a difference between two averages the rule is to proceed upon the principle that the combined error of these averages is equal to the square root of the sum of the squares of their respective errors. The relation of the actual difference to this combined error furnishes the basis for estimating the certainty of the result. In Table I. this is expressed in terms of unity under the heading C.<sup>1</sup>

For the sake of clearness the important class and sex differences are shown graphically in Fig. 1. Here the averages for freshmen are taken as a base and the direction and amount of difference plotted accordingly. In reading these curves, *above* means superiority in size, strength, sensitiveness, accuracy, quickness, quality and quantity. The amount of difference is expressed in terms of  $p$  and indicated on the ordi-

<sup>1</sup>For methods of calculation and deduction of formulas see Merriman, A Text-Book of Least Squares; Westergaard, Die Grundzüge der Theorie der Statistik, etc.

TABLE I.

	Sex Differences.						C	Class Differences.						C
	Men.			Women.				Freshmen.			Seniors.			
	Cases.	Av.	± p	Cases.	Av.	± p		Cases.	Av.	± p	Cases.	Av.	± p	
Head—length (mm.).....	267	194.0	4.3	42	181.5	5.8	0.99	34	193.1	4.0	195.3	4.4	0.86	
—breadth “.....	267	152.9	3.8	42	148.1	3.1	.99	34	153.5	2.6	155.5	3.4	.93	
Strength of hand—right (kg.).....	210	36.3	4.9	42	20.0	3.6	.99	35	38.0	5.3	41.2	5.9	.74	
—left “.....	210	33.5	4.7	42	17.5	3.5	.99	35	34.4	4.9	37.0	5.0	.77	
Fatigue (%).....	110	65.1	27.0	39	65.0	25.1	.05	.....	.....	.....	.....	.....	.....	
Perception of weight—error (g.).....	123	144.1	51.7	35	140.	60.3	.16	.....	.....	.....	.....	.....	.....	
Pain—right hand (kg.).....	266	5.9	2.4	42	2.4	1.2	.99	35	6.4	2.8	5.7	1.4	.65	
—left hand (kg.).....	266	5.6	2.2	42	2.4	1.1	.99	35	5.6	2.4	5.5	1.5	.11	
Sensation areas—av. correct in 5.....	262	3.7	0.9	42	3.8	0.8	.41	33	3.4	0.9	2.8	0.7	.97	
Pitch—error on 75 cm. string.....	265	7.2	4.7	42	5.3	2.9	.99	33	7.6	4.2	3.7	2.5	.99	
Perception of size—error (mm.).....	243	2.4	2.0	34	3.4	2.1	.92	35	2.1	1.2	2.7	1.2	.86	
Estimation of 10 sec.—error (sec.).....	127	1.7	0.8	.....	.....	.....	.....	31	1.8	0.6	1.3	0.6	.98	
Reaction-time (σ).....	256	158.7	18.9	42	183.9	27.7	.99	47	164.0	16.0	157.2	20.0	.80	
Marking out A's—time (sec.).....	270	100.2	12.2	42	91.2	8.1	.99	35	105.4	12.8	88.9	10.3	.99	
—A's omitted.....	270	2.2	1.6	42	3.0	1.9	.92	35	4.7	2.0	1.6	0.7	.99	
Naming 100 colors—time (sec.).....	123	84.8	14.2	40	65.1	8.3	.99	33	.....	.....	79.0	18.6	(.61)	
Movement-time (sec.).....	59	34.1	4.3	23	36.0	4.0	.82	.....	.....	.....	28.6	2.5	(.99)	
Accuracy of movement—time (sec.).....	58	49.0	7.0	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	
—error (mm.).....	58	0.8	0.3	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	
Association-time (sec.).....	171	55.4	22.9	39	74.8	26.0	.99	32	53.8	23.1	47.5	21.0	.54	
Auditory memory—numerals recalled.....	266	7.6	0.4	42	7.3	0.5	.97	34	7.6	0.5	7.6	0.4	.00	
Visual “.....	142	6.9	0.5	42	7.3	0.4	.99	33	.....	.....	7.0	0.4	(.58)	
Logical memory (%).....	101	44.5	11.1	37	48.2	13.2	.65	33	.....	.....	58.5	12.0	(.99)	
Retrospective memory—error (mm.).....	256	4.5	2.6	.....	.....	.....	.....	31	6.3	2.7	5.4	3.2	.58	



nates in a scale of 10  $p$ . In this way we are able to express the relative differences for all averages and so have a common unit of measure. In Table I. these differences have been translated into probabilities and we have assumed that when the probability of a difference is 0.90, or 2.5  $p$ , it may be taken as a difference of considerable certainty. This limit we have indicated

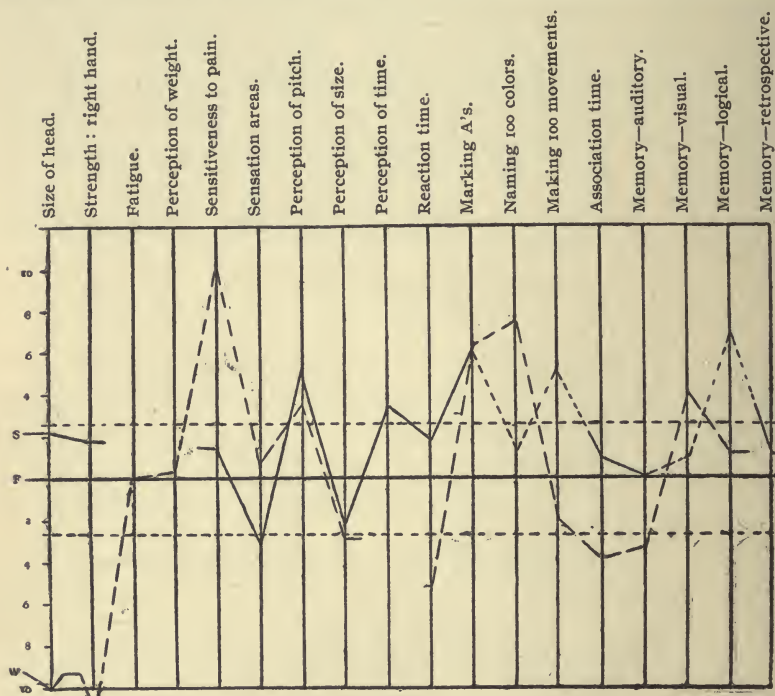


FIG. 1.

by the parallel broken lines. A difference of 5  $p$  has a probability of 0.999 which may be regarded as sufficiently certain for all purposes. In Table I. a certainty of 0.82 may be regarded as indicating a probable difference and 0.50 as equivalent to no difference. In reading the curves the probability of all differences should be estimated according to the above. Readers unfamiliar with such methods of treating data should bear in mind that this is only a way of estimating the allowance that must be made for accidental variation.

According to this we may say that the women are approxi-

mately equal to men in fatigue, perception of weight, sensation areas and logical memory; they surpass the men in sensitiveness to pain, perception of pitch, marking out A's, naming colors and in visual memory; they are inferior to them in size of head, strength of hand, reaction-time and association-time; they are also probably inferior in movement-time, perception of size and auditory memory.

With the seniors and freshmen we have the record of each individual for comparison, and it is these differences that are plotted, except where the line is broken. So far as we have data, the seniors surpass themselves as freshmen in width of head, perception of pitch, estimation of time, marking the A's and are inferior in sensation areas: they are also probably superior in length of head, strength of hand and reaction-time and inferior in the perception of size. As compared with the class results of freshmen they are superior in movement-time and logical memory. It will be observed that in case of pain, association-time, and visual and retrospective memory they are actually above the freshmen averages. The other tests not represented in the plotted differences may pass without comment.

In addition to the results given in Table I. the following seem of sufficient interest to deserve mention.

One and a half per cent. of the freshmen were color blind, but no cases were found among the women. In color preference 42% of the men chose blue and 42% of the women red, these being the preferred colors. The total distribution of likes and dislikes is as follows:

	Freshmen.		Women.	
	Like %.	Dislike %.	Like %.	Dislike %.
Red .....	22	7	42	8
Orange .....	5	25	8	31
Yellow .....	2	32	5	8
Green .....	7	15	9	21
Blue .....	42	12	9	23
Violet .....	19	8	19	9
White .....	3	1	8	0

Of the 28 seniors 20 showed the same preference as when freshmen and of these 8 were for blue. All the senior prefer-

ences fell upon red, blue and violet except two. In brief, the tendency seemed to be a shifting toward the violet end of the spectrum as age increased.

In eyesight there is no certain difference between the sexes unless it be in the variability. The seniors as a class are neither better nor worse than in their freshmen year, though some individuals have lost and some improved. The distribution of cases was as follows :

#### EYESIGHT TESTS.

Cm.	Freshmen.		Women.	
	R %.	L %.	R %.	L %.
85	0	2	0	0
72	15	14	10	5
61	26	26	22	21
52	26	34	40	40
44	13	12	22	19
37	12	7	0	5
31	7	0	2	4
26	0	2	2	2
22	0	0	0	0
19	1	0	0	2
16	0	3	2	2

According to cephalic index freshmen and women were distributed as follows :

Index	70-74	75-79	80-84	85-89
Freshmen	11%	50%	33%	6%
Women	6	50	39	5

So far as these results go it can not be said that there is a real difference between the sexes.

Ellis<sup>1</sup> has brought together certain results which lead to the conclusion that to reach the maximum at the first trial with the hand dynamometer is a sign of weakness and that this is further evidenced by the predominance of such relation among women. The results of these tests furnish data bearing directly upon this point. Among the freshmen in Columbia College 61% reached the maximum with the right hand at the first trial in the test and 63% with the left. The distribution is apparently the same for both hands, but the student who reaches the maximum record on the first trial with the right hand is no more likely to do

<sup>1</sup> Man and Woman, p. 151.



so with the left hand than those who do not. Taking those who reach the maximum on the second trial, we find them not among the strong men but equally distributed among all grades. So it appears that this variation is independent of absolute strength. However, we find that 80% of the women reach the maximum at the first trial for the right hand and 66% for the left. This may be an accidental difference, but should it prove to be a secondary sexual characteristic the explanation cited above is an unwarranted assumption.

The association test furnishes a large number of associations for each word. In the senior and freshman tests we have two sets of associations with an interval sufficient to eliminate memory interference. Of these 34 cases we find the number of identical associations in the two tests as follows: for house, 9; tree, 8; child, 10; time, 4; art, 9; London, 9; Napoleon, 7; red, 10; enough, 5. On the whole this is more of an agreement than we should have supposed, and indicates a persistence of early associations.

A classification of associations shows that 25% of the Barnard tests give particular associations, as Barnard House, 49th St., etc., while the men give but 6% such associations. From reading over all the lists we get the impression that the women excelled the men in particular and personal associations. As to the kinds of association, no particular difference between the sexes appears. The comparative results of freshmen and seniors give no marked changes in the character of associations.

The answers to the questionnaire on imagery show no important sex differences. Questions as to the nature of our mental images are always difficult to answer and are probably especially so for people not psychologically inclined. The seniors who answered the questions after an interval of four years show great variation, the greatest uniformity occurring in the two first questions of each group in the blank. Eighty-five per cent. of the freshmen and 88% of the women found the outlines distinct and sharp, 68 and 69% could image a face better than a voice. With respect to real hallucinations, 20% of the freshmen reported as against 40% of the women. About 65% of such experiences were auditory. More than half the freshmen reporting experienced

repeated persistent hallucinations of being called by name and of these about half were usually recognizable as "mother's voice." As this is a subject of general interest we give a few typical reports :

" Besides having heard the voice of mother when young while she was some 1500 miles away, I now and then hear scraps of conversation (which I have never heard) float before my ears and also voices of friends call me by name. This, however, is seldom, and I think occurs only when I am greatly fatigued in mind or body."

In cases of visual experience the image is usually that of an unknown person, but one often saw faces belonging to persons known to be dead. Here is the one striking case of a familiar figure :

" One night I awakened from a deep sleep and saw my father standing at the foot of the bed. The image was perfectly clear and appeared to my mother a few nights afterward."

From the senior reports we may see what changes have come during four years. Ten seniors reported cases in the freshmen tests. In the senior tests no new cases were reported, one reported no recurrence of the experience, four reported persistence of the same experience and five recurrence with modifications. The last show a slow development of the experience. One reported that when at a preparatory school he sometimes heard his mother's voice when a piano was played. Now when playing the piano away from home usually in the midst of a selection he hears his mother call in a loud voice, always causing a break at that point. This is a constant experience and becoming more and more frequent.

Although the number of cases is small the tendency seems clear; that the majority of such experiences are auditory and of these the hearing of one's own name in a strange or familiar voice is most common; that, whatever the nature of the experience, it is usually persistent and develops along its own beaten paths—those who had visual experiences have them still, etc. All seem to have their origin before college life begins. However, these are to be taken as suggestions rather than definite conclusions.

In the memory tests for numerals a decided interference effect is often observed in that the second series is confused with

the first. This is especially true of seniors and mature students, with whom the probability of the first trial being correct is much greater than in case of the succeeding. In retrospective memory the error may be measured in two ways, according to the deviation from the standard line or according to the deviation from the line actually drawn in the size test. The results show that the average error for the latter is about half as large as for the former. This difference is doubtless due to a constant error in the individual.

The miscellaneous data found in the various blanks do not differ materially from those given in the preliminary report. As a brief summary, we find freshmen in Columbia College about 18 years of age, about 90% are regarded as having brown hair, their eyes are about equally divided between gray, blue and brown, 70% have straight foreheads, 60% thick lips, 75% are of clear complexion and about 2% are left-handed.

Since sex and class differences are incidental to the purpose of this research, no attempt has been made to compare these results with other data published elsewhere. Difference in method of testing rendering such comparison difficult in any event, it appears best to limit this section to a statement of specific results.

It remains to consider the differences between the freshmen tested for each year. Taking length of head as a special case, we find the following :

Class.	Cases.	Av.	$\pm \phi$ .
1900	60	193.5	4.4
1901	66	193.6	4.0
1902	82	194.4	5.0
1903	59	193.9	4.7

The maximum difference is 0.9, which has a probability of 0.54, or equivalent to an accidental difference. As to how far this is true of other tests may be inferred from the following table of probabilities similarly determined.

Reaction-time,	0.98
Marking A's,	0.77
Naming colors,	0.68
Association-time,	0.68
Logical memory,	0.50
Pitch,	0.25
Pain,	0.82
Strength of hand,	0.16



Thus, with the exception of reaction-time, we find all the maximum differences between the freshmen of the various college classes within the limits of accidental variation. This exception, however, is not very important, since it is found to be a difference of but 3.5  $\mu$ .

The import of this result is that with respect to these tests freshmen entering Columbia College from year to year are similar, or represent a type. This is also borne out by the regularity of the distribution for the various tests.

# THE CORRELATION OF RESULTS.

The most important consideration in a series of tests is their correlation. It is desirable to know to what extent ability in one case assures us of ability in another. This is the real question in all tests. But before going into the subject proper some attention must be given to method. Let us take for example the stature and weight of students, as a case in which we should expect some correlation. In Table II. the cases are grouped in differences of 5 kg. and 5 cm. and so arranged that the weight

TABLE II.

Stature in cm.	Weight in kg.								Total.
	45-50	51-55	56-60	61-65	66-70	71-75	76-80	81-85	
155-160	I	I		I					3
161-165	I	2	7	I	2				13
166-170	I	5	7	10	3				26
171-175		2	9	11	8	2	I		33
176-180			4	11	6	3	2	2	28
181-185			I	3	3	4	2	3	16
186-190								I	I
Total.	3	10	28	37	22	9	5	6	120
Av. Stature.	162.5	166.5	169.8	172.8	173.6	178.6	178.5	181.6	172.5

of any group can be compared with its stature. The average stature of all is 172.5 cm., weight 63.4 kg. At the foot of each column is the average stature for the corresponding weight. Here we see that those of average weight are also of average stature and that weight increases with stature. Had there been no relation between weight and stature, the averages for the columns would have been approximately the same.

These relations may be presented in graphic form as in Fig. 2. Let *AB* represent the place of average statures, if there were no correlation; *CD* their place in perfect correlation; the point of intersection, the place of the group having both average stature and average weight; then plotting the actual averages gives the line *EF*.<sup>1</sup>

<sup>1</sup> See Grammar of Science, Pearson.

In estimating the degree of correlation it is customary to proceed by the formula

$$r = \frac{\sum xy}{n\mu_1\mu_2},$$

in which  $x$  and  $y$  represent the variations of an individual from the respective averages of the two distributions to be correlated,  $\mu_1$  and  $\mu_2$  the mean square deviations of the distributions and  $r$  the coefficient of correlation.<sup>1</sup> When  $r$  equals unity correlation

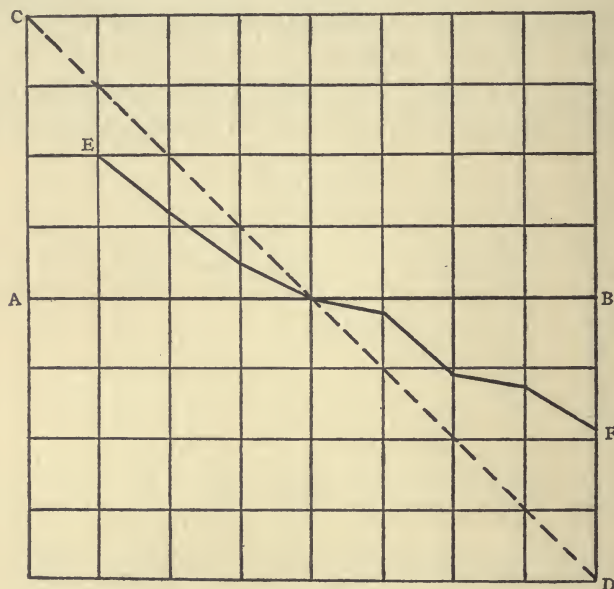


FIG. 2.

is perfect, when equal to zero the correlation is that of chance. Applying this formula to the data for stature and weight (Table II.) we find  $r = 0.66$ .<sup>2</sup> This coefficient expresses the relation between the averages for the successive columns, or arrays, in the table or the place of  $EF$  in Fig. 2. The probable variation of an array may be found from  $p\sqrt{1-r^2}$ . Since  $r = 0.66$ ,

<sup>1</sup>Natural Inheritance, Francis Galton. Mathematical Contributions to the Theory of Evolution, Karl Pearson, *Transactions of the Royal Society of London*; also Grammar of Science, Revised Ed. The Cephalic Index, Franz Boas, *American Anthropologist*, Vol. I., No. 3, N. S.

<sup>2</sup>No account was taken of differences in age.



this expression becomes  $0.75 \bar{p}$ ; calculating  $\bar{p}$  for all statures in the table gives 4.6 cm., from which it follows that the probable variation for an array should be 3.3 cm. Calculating the actual variation in statures for weights of 56-60, 61-65 and 66-70 we obtain 3.5, 3.7 and 3.8. The significance of  $r = 0.66$  is now apparent; if we select students according to weight, their statures will fall within approximately  $\frac{3}{4}$  the limits for students selected at random. In the same way it may be shown that a coefficient of  $0.43 = 0.90 \bar{p}$ ;  $0.66 = 0.75 \bar{p}$ ;  $0.87 = 0.50 \bar{p}$ ;  $0.97 = 0.25 \bar{p}$ ; etc., from which the significance of the various coefficients in the following pages may be estimated.

Since the coefficients of correlation depend upon the mean square deviations, their own probable errors can only be estimated in the same terms. According to the method employed, it appears that for the following data differences greater than 0.10 have considerable certainty.

In order to save time the writer used another form of this method which may be illustrated by an actual case. In reaction the cases may be divided roughly into four groups as

$$(1) \quad 68 + 70 + 55 + 63 = 256$$

and in the A test

$$(2) \quad 57 + 80 + 66 + 53 = 256.$$

Now, if a mere chance relation held between these tests, the 68 cases in reaction should be distributed in series 2 as

$$\frac{57}{256} + \frac{80}{256} + \frac{66}{256} + \frac{53}{256} \text{ of } 68$$

or

$$(15.14 + 21.24 + 17.50 + 14.07 = 67.95.$$

Counting through, we find these 68 cases actually distributed as

$$15 + 20 + 15 + 18 = 68.$$

Taking the 63 in series 1, the chance occurrence should be

$$14.02 + 19.68 + 16.24 + 13.04 = 62.98.$$

The actual order of cases was

$$12 + 21 + 18 + 12 = 63.$$

Thus we find the correlation approximately that of chance. The method can be shortened by calculating the number of cases probably agreeing in the corresponding groups in series 1 and 2.

In the same manner for stature and weight,

$$(1) \text{ Stature } 42 + 33 + 45 = 120$$

$$(2) \text{ Weight } 41 + 37 + 42 = 120.$$

The 42 short men in 1 occur in 2 as

$$25 + 12 + 5 = 42.$$

By chance they should be

$$14.4 + 12.9 + 14.7 = 42.0.$$

The tall men occur

$$5 + 14 + 26 = 45.$$

By chance,

$$15.3 + 13.8 + 15.7 = 44.8.$$

Thus we find evidences of correlation in this case.

In a series of tests like the following, where there are over six hundred possible correlations to consider, a shorter method still will be useful. Correlation may be detected by simply taking account of the distribution of cases. Returning to Table II. we find in regard to stature about the same number above as below the average group. Arranging the 42 short men, the 45 tall and the 33 average men according to weight, we find them distributed as follows :

Short men,	3	....	8	....	14	....	12	....	5	....	0	....	0	....	0	42
Tall men,	0	....	0	....	5	....	14	....	9	....	7	....	4	....	6	45
Av. men,	0	....	2	....	9	....	11	....	8	....	2	....	1	....	0	33
Total,	3	....	10	....	28	....	37	....	22	....	9	....	5	....	6	120

That tall men are heavier than short men is now evident. In practice we need but find the distribution of cases with high and low values, say above  $+p$  and below  $-p$ . A judicious use of this method makes a rough-and-ready estimate of the degree of correlation possible in any case.

The data discussed in the following pages were first arranged according to this last method ; then the actual distributions were compared with the theoretical chance distributions ; finally all the important tests and all giving any evidence of correlation were treated by the Pearson formula and the coefficients taken as the basis of discussion. Thus the treatment has been sufficiently thorough to give a reliable conclusion as to the general degree of correlation between such tests as are considered.

# TESTS OF QUICKNESS AND ACCURACY.

Reaction-time and marking out the A's furnish 252 cases in common. The reaction-time for each individual is the average of five to three valid reactions, observers recording five reactions. In the A test it has been proposed to lengthen the time recorded proportionally to the numbers of A's missed.<sup>1</sup> But this is unnecessary, since  $r = -0.05$  in case of A times so adjusted and  $-0.07$  for them regardless of errors. It appears, then, that the degree of correlation between these tests is approximately zero, or a chance relation. In other words an individual with a quick reaction-time is no more likely to be quick in marking out the A's than one with a slow reaction-time.

The other correlations for tests of quickness may be enumerated as follows :

	Cases.	<i>r</i>
Reaction and naming the colors	118	0.15
Reaction and association	153	0.08
Marking A's and naming the colors	159	0.21
Movement time and naming the colors	97	0.19
Movement time and reaction	90	0.14

The remaining possible correlations in this group of tests are movement time with A's and association, and naming colors and association, but when tabulated the distributions were such as to make a very low degree of correlation certain. Thus it appears that the time required for naming the colors correlates better than any test in this series, yet the coefficients are too low to be of much significance. ( $p\sqrt{1 - (0.20)^2} = 0.98p$ ). The results are given in tabulated form for reaction-time and A's (Table III.) and naming colors and A's (Table IV.).

Though these tests fail to correlate with each other, it may yet be true that the individual who happens to hold the same rank in two tests will tend to hold that rank in all and so divide our subjects into groups one of which correlates and the other not. This question may be answered in the following manner : From the value of  $p$  (Table I.) we may divide the individuals tested into four groups, approximately equal. In reaction-time and the A test there are 68 and 57 cases, respectively, in the first

<sup>1</sup> Cattell and Farrand, PSYCHOLOGICAL REVIEW, Vol. III., p. 618.



TABLE III.

Reaction time.	Marking A's.								Total.
	60-69	70-79	80-89	90-99	100-109	110-119	120-129	130-139	
100				1			1		2
110			4	8	1	1	1	3	18
120	2	5	4	4	4	3	1	2	25
130	1	3	4	11	3	5	1	1	29
140	2	4	6	10	7	5	5	2	41
150	1	3	7	6	4	4	1		26
160		1	6	7	7	3	2	2	28
170		3	5	6	6	5	2		27
180	3		4	6	5			1	19
190	1		6	3	1				11
200		1	2	2	2	1	1	1	10
210	2		3	2	2				9
220			2	2	2	1			7
Total.	12	20	53	68	44	28	15	12	252
Av.	145	143	162	151	160	150	144	140	

group, or upper quarter. Of these 15 are identical, or the number conformable to chance. In the classes of 1902 and 1903 there were 30 cases in the upper quarter for naming colors ;

TABLE IV.

Time for marking A's.	Time for naming colors.								Total.
	50-59	60-69	70-79	80-89	90-99	100-109	110-119	120-129	
60-69		1	3	1					5
70-79	4	3	4	1	3	1	1	1	18
80-89	6	9	11	6	3	1			38
90-99	2	10	9	5	4	1	1	2	34
100-109		3	6	7	4	4	4	1	33
110-119	3	6	2	3	2	1	1	2	22
120-129		1	2			1			4
130-140			2	1	1			1	5
Total.	15	33	39	24	17	9	8	7	159
Av.	90	95	95	98	97	102	99	119	115

this test seemed to correlate in a way with reaction and the A test, yet there is but one case identical in the upper quarters of the three. Theoretically, the chance agreement is 0.4 of a case. The other quarters give corresponding results.

The cases are not sufficiently numerous to warrant giving more space to this correlation. At least the results indicate that, though we find some reasons for assuming double correlations

between certain tests, the rank of the individual in the whole series of time tests seems subject to chance. This conclusion is out of harmony with all general belief, since we usually assume that quickness is a fundamental thing characterizing the individual in every respect. But the discussion of this point must be deferred.

Having now considered tests of quickness, we may take up those whose results are generally expressed in terms of accuracy. In the size test there are three similar requirements: drawing a line, bisecting it and bisecting a right angle. Some correlation appears between the former, 0.38 (123). The errors in all cases are about equally divided between positive and negative, and those who underestimate the line are no more accurate in its bisection than those who overestimate it. Curiously enough, when we come to correlate these results with bisection of the angle, no agreement save that of chance appears.

The length of the perpendicular line involves an illusion and is not so easily correlated. On the average this line is  $\frac{5}{6}$  of the actual base, only 2% drawing it longer than the base.

The tests for accuracy of movement (striking the dots) and the perception of weight (force of movement) correlate neither with each other nor with the test for size. Also, no correlations were found between these tests and the accuracy of estimating time intervals or of following a given rhythm. The A test may be considered here also, since it may be graded according to the number omitted, but this fails to correlate with the above.

It may be well to consider the relation of speed to accuracy. In case of A's they hold a slight inverse relation,  $r = -0.28$ . The time required for naming the colors which seemed favorable to correlation in the group of time tests does not correlate with any of the tests in this group. There is an apparent correlation between the accuracy of movement test and the time taken to do it; however, the writer regards the results of this test as uncertain, because any individual can hit all the dots if he goes slowly enough.

It may be urged that in many cases failure to correlate is due to want of precision in the tests, so we must give some consideration to the validity of the methods employed. In the A

test, for example, we have but one trial, and unless we know something of the limits of variation in repeating it we can give no particular value to the results. It is difficult to repeat this test without modification of some kind; if the same blank is used, the subject remembers where some of the A's were found and thus gains in time; if a new blank is made the conditions vary. We give here the results for two men and two women who made repetitions of the test in succession.

Men	{	94	90	91	91	80
		70	66	67	66	67
Women	{	83	74	71	75	70
		82	76	75	69	71

Two other subjects made one daily trial for six days :

62	60	53	60	57	60
70	68	66	62	59	60

Thus we see what might have been foretold. The first trial is likely to be the longest for want of practice, but it does not appear that the difference between the first and second trial is out of proportion to the succeeding, conforming generally to a practice curve. This satisfies the requirement of an efficient test.

It will be remembered that the seniors repeated this test after an interval of four years; the distribution of cases as freshmen was according to *p*, 7-10-9-9, and of these 5-7-8-5 respectively held their relative positions.

In naming the colors there should be relatively little variation between the various trials since this is essentially a reading time. Five freshmen made two trials of the test at intervals varying from one hour to three days with an average first trial of 61 seconds, second trial 58. We find here proportionally smaller differences than in case of the A test.

With movement-time the difference is relatively large, since the individual variability is about the same as that of the group. In repeated trials with five subjects the average difference between the first and second was 4 seconds, all shorter times; between second and third, 1.9 seconds. The former is approximately the variability of the group. The apparent regularity of gain in the different subjects, however, gives this test some certainty.



In case of the accuracy of movement, we have no data except the daily records of two subjects.

Time,	47	36	36	35	35	32	40
Error,	0.9	1.0	0.8	0.8	0.8	0.8	1.1
Time,	40	37	34	29	30	28	30
Error,	0.8	0.9	1.0	1.0	1.3	1.4	1.2

Here we observe some individual differences. One subject shows a tendency to sacrifice accuracy for speed, the other gains in speed with a fairly constant accuracy. The inference would be that the result of this test will vary according to the attitude the individual assumes. Unfortunately, we have too few cases for precise correlation.

In the association test we have no data for repeated trials except in case of seniors, who gave even more decided agreement in rank than in case of the A test. For this reason we are justified in placing some confidence in the result. Whether the test measures the readiness of association is doubtful, but it seems that the time is indicative of the reaction to the test and that individuality in this respect persists during college life.

When we consider reaction-times we have five trials for each individual and have to do with an average within an average, from which it follows that the precision of the test will depend upon the probable variation of the group. Taking ten cases at random from freshmen of 1900, we find the averages giving an average probable error of  $7\sigma$ , and as seniors the same individuals giving an error of  $12\sigma$ . The extreme case was an error of  $18\sigma$ . The value of  $p$  for the group is  $19\sigma$ . Thus the probable errors of the individual averages are within the limits of the group variation. In the preliminary report it was shown that the tendency was toward quicker reactions in the successive trials. Five freshmen made ten reactions under the conditions of the test and the average of the first five was  $10\sigma$  longer than the average total. The repetition of some cases several days later and an extended series upon two other individuals show a much smaller difference. Here again we have a practice effect and it appears that the tendency is general. The seniors held the relative rank of their freshmen reaction tests as well as in other cases.

In case of the size test we find the distribution of errors for two individuals with a 10-cm. line, 12 daily trials of 10 lines each, 120 lines in all, in every way comparable to the group distribution for the 5-cm. line. In an extended series of trials with the 5-cm. line with three individuals the average errors were  $2.5 \pm 2.2$ ;  $6.0 \pm 0.9$ ;  $2.0 \pm 0.7$ . In the case of seniors we find that they do not keep their relative rank as freshmen in any sense. Though we can not say that this test is very precise, the fact that it correlates in itself seems to indicate sufficient precision to reveal correlations with other tests, if such exist. The other tests of accuracy may pass without comment.

This leads to the conclusion that all the time tests, excepting possibly the rate of movement, are sufficiently precise to bring out correlations if there were decided tendencies in that direction. In short, the failure to give decided correlations can scarcely be attributed to that source. While it is true that variation of the individual is often large, as in case of reaction-time and rate of movement, it is improbable that those above the value of  $p$  should in reality belong to the subgroup below that value. Nor does it seem that the tests of accuracy fail to satisfy the requirement of precision in this respect.

#### MEMORY TESTS.

Any method devised for the gradation of memory tests must be arbitrary. This is especially true in logical memory, and renders comparison of results impossible except when graded by the same individual. In the case of numerals, taking the number correctly written is the most precise, since, if we grade by order, we must make arbitrary estimates. In this study we have graded by the number correctly written and the number in position. It will be said that grading by position is not advisable, since by the omission of one numeral alone a whole series may be counted as out of place; but such combinations were of rare occurrence in these tests, and when they did occur the case was thrown out.

There would be no reason for considering the number correctly written if it were closely correlated with the number correctly placed. But such correlation is not apparent. In the

auditory test half the freshmen (135-266) gave all the numerals correctly and one-seventh (20-142) in the visual test. The former does not give sufficient distribution for precise correlation but, if we take the number correctly placed, the cases correct in all are approximately one-half the above groups. That is to say that the chances are even that an individual who can recall a series of eight numerals will be able to give them in correct order. We can not enter into a discussion of the independence or dependence of ability to recall the individuals in a series and the ability to reproduce the series itself. But if we correlate the number correctly written in the auditory and visual tests we find with 144 cases a coefficient of 0.29 and in the case of number correctly placed 0.39. This is somewhat above a chance relation. If it were safe to put much faith in small differences, we might give some weight to the consistency of these results, but as previously noted the distribution of cases is such that correlations for number correctly written must remain uncertain.

The logical test was graded in a scale of ten and the distribution of cases was found quite regular, affording ample opportunity for correlation. For position in the auditory test a correlation of 0.04 (94 cases) results; for number correctly written 0.05. By use of the short method there seemed no evidence of a marked difference between these and the probable coefficients for visual memory. On the whole, there seems to be a decided absence of correlation between this and the numeral tests.

In the case of the line drawn from memory we find a correlation with logical memory of  $-0.07$  (91 cases). There was no apparent correlation with the numeral test.

The test for pitch is in many respects a test of memory, and may be taken up here. As seen in Table I., the variability is such as to give a wide distribution of cases. From a tabulation of results it appeared that about two-thirds of the cases fell into a typical group with an error of 4 cm. ( $\frac{4}{7\frac{1}{5}}$  of the string), while the remaining cases were widely scattered beyond that limit. This makes the number of available cases small, except in case of auditory memory; yet there were no signs of correlation with the



memory tests. But this method of selecting cases seems uncertain, since a study of variation in individual cases indicates that the present test cannot be considered precise below differences of a half tone. Taking the two trials of the freshmen, we find that the chances that the second error will vary from the first within the limits of a quarter tone are 65 in 100; within the limits of a half tone, 85 in 100. With differences of a half tone the distribution of cases gives a regular asymmetrical curve, but this correlates with the other memory tests no better than the selected group.

The test for force of movement is also largely a memory test, and, though its results gave regular distributions, it failed to show any evidence of correlation with the other tests. We might expect that it would in drawing the line in the size test, but the result was  $-0.08$ .

In general, we may say that these tests fail to show much intercorrelation. The numeral tests, it is true, correlate somewhat, but that would be expected; not necessarily because of a functional relation, but because the individual is likely to mentally repeat the numerals in either test. The data are not at hand for determining the relative value of these tests or even their precision. The whole subject of memory tests needs further consideration before positive conclusions can be reached.

Since it appeared that certain tests for quickness tended to correlate, though with low coefficients, it may be well to try them in the memory tests. We find naming colors gives  $0.03$  (93 cases) with logical memory; reaction-time for logical memory  $0.12$  (96), for position in auditory memory  $0.17$  (112) and for position in visual memory  $0.06$  (104). This seems to indicate that a quick reaction on sound has more weight in memory for things heard than seen, yet the influence is too slight to be of much consequence. Also reaction and pitch gave  $0.01$  (100).

#### COLLEGE STANDING.

In general it appears that correlations in any of the foregoing tests are not of a degree sufficient for practical purposes. We do not learn much of an individual by any one or any group of them. It appears that we are dealing here with special and

quite independent abilities and that the importance attributed to such measurements of elementary processes by many investigators is not justified in this case. However, it seems probable that a basis for correlation will be found somewhere, and it may turn out that though these tests do not show much intercorrelation they may individually correlate with ability in the more complex tasks of life. Research in this direction is obviously difficult for want of adequate standards and satisfactory data, and the only attempt we have made is in respect to college standing.

Students in Columbia College are marked in a scale of five for each semester. The standing for the year is estimated by the sum of the products of the grades and the number of course hours, divided by the total number of such hours, or the average grade per course hour. The distribution of these averages for 240 cases was as follows, it being understood that 1.0 represents the highest possible grade.

Grade,	1.0-1.2	1.3-1.6	1.7-2.0	2.1-2.4	2.5-2.8	2.9-3.2	3.3-3.6
Cases (%),	2.1	7.5	13.7	15.4	19.1	15.8	13.3
Grade,	3.7-4.0	4.1-4.4	4.5-4.8	4.9-5.2	Total		
Cases (%),	6.2	4.5	1.6	0.4	99.6%		

This gives a distribution sufficiently regular for correlation. It may be said in passing that the character of this distribution indicates a high degree of precision in the method of grading.

The following correlations have been calculated for the average standing :

Reaction-time,	227 cases.	- 0.02
Marking A's,	242 "	- 0.09
Association,	160 "	+ 0.08
Naming colors,	112 "	+ 0.02
Logical memory,	86 "	+ 0.19
Auditory memory (position),	121 "	+ 0.16

An application of the short method to the other mental tests gave no hope for correlation.

Here we are face to face with another cold fact: the tests of quickness seem to hold a chance relation to class standing, and ability to do well in the memory tests has but little significance. But it may be well to examine the relative standing in the different courses.

The following correlations were calculated :

Latin and mathematics,	228 cases.	0.58
Mathematics and rhetoric,	222	0.51
Latin and rhetoric,	223	0.55
French and rhetoric,	122	0.30
German and rhetoric,	132	0.61
German and mathematics,	115	0.52
Latin and French,	130	0.60
Latin and German,	129	0.61
Latin and Greek,	121	0.75
Gymnasium and average grade,	119	0.53

Here we find a higher degree of correlation than heretofore. The languages have a correlation of 0.60 to 0.75, or a reduction of variability approximating  $\frac{1}{4}$ . In other cases we have a reduction of about  $\frac{1}{5}$ . The exceptionally low correlation for French and rhetoric is rather puzzling, but is probably due to some accidental cause. On the other hand, the high degree of correlation between Latin and Greek is according to expectation. It is interesting to note that the degree of correlation here is about the same as for stature and weight. From what has gone before it is improbable that a high degree of correlation will be found between particular courses and the separate tests. For example, with logical memory and mathematics the coefficient is 0.11 and with Latin 0.22, no significant variation from 0.19, the correlation for the average class standing.

Whatever it is that makes for correlation in class standing seems to hold generally for all courses. The gymnasium grade, which is based chiefly upon faithfulness in attendance, correlates with the average class standing to about the same degree as one course with another. We have not carried this correlation out to its full possibility, as that would take us too far afield. Yet this serves as a suggestion as to how this method of correlation may aid in the solution of a very important phase of the test question. We might extend it so as to embrace all courses offered, work out the degree of correlation for the different college years or semesters, correlate the successive marks for the students of one department or of a single instructor, etc., all bearing upon interesting and live questions.



## PHYSICAL TESTS.

The strength and fatigue of hand have been objects of much attention for many years, but notwithstanding the mass of published data and the great diversity of mechanical appliances devised, very little advance has been made. Yet it is a general belief that specific individual differences observed in such tests are correlates of mental and physical differences not far removed from the fundamental. It has been claimed that strength of hand is a correlate of mental ability, that civilized men are stronger than uncivilized, and professional men than laborers. In these tests we find no correlation between class standing and strength of hand,  $r = -0.08$ . For reaction-time, marking out A's and naming the colors there is some correlation but with very small coefficients. It might be inferred from this that general physical vigor will have some weight in mental tests such as we have in this series, but this is by no means certain, for we make this inference upon the assumption that strength of hand is an index of general physical vigor. The fatigue test employs the muscles of thumb and forefinger only. If we take the total reading of the ergometer for the fifty efforts we find little correlation with the strength of hand as measured by the dynamometer. Taking the reading for the first ten, so as to avoid as much of the variation due to fatigue as possible, we still have a correlation of but 0.19. On the other hand, there is a close correlation between the two hands in the dynamometer test and also between the first and second trials with the same hand. This shows the difficulty of comparing the results obtained with different instruments. It will be said that we are attempting to correlate a fatigue effect with an unfatigued one; this objection is not of much weight, since the total reading of the first ten efforts in the fatigue test correlates no better with the second trials on the dynamometer than with the first or even the average of the two, which is itself modified by fatigue. Anyway, it does imply that there is little correlation between strength and fatigue tests when made on different muscles.

When we come to the fatigue test itself the question as to

how fatigue shall be estimated arises. If we take the total readings of the ergometer, we have no means of knowing to what degree strength was depleted by the test, as will be seen from the following cases :

	1st.	2d.	3d.	4th.	5th.	Total.
1.	45	40	43	42	40	210
2.	100	80	70	70	60	380
3.	70	65	75	65	67	342
4.	70	55	40	35	40	240

Cases 1 and 3 show very little change in the successive readings, while 2 and 4 show a decided decrease, or, according to the interpretation, the degree of fatigue is much less for 1 and 3. When we correlate the total readings of 144 cases with the first readings we find  $r = 0.91$ . This means that if we estimate by the total reading ten trials is sufficient, and then we should expect to find the total for the first ten closely correlated with the reading for the first trial; thus we fall back to a mere dynamometer test and fatigue drops out of the question.

The difficulty of devising a method by which the relative loss of ability to press on the ergometer may be estimated becomes apparent when we attempt to rank the four cases above. If we estimate according to the variation from the first reading the rank is 3, 1, 4, 2; according to the variability from the average 1, 3, 2, 4, etc. The result wished for in a fatigue test is to ascertain the endurance of individuals under like conditions, and superiority in this respect is assumed to be the degree of approximation to the initial effort. For this reason we have estimated the degree of fatigue as follows: Divide the total reading by five times the first reading. This is on the assumption that without fatigue five times the first reading will approximate the total reading, the difference between this and the actual reading expressing the degree of fatigue. Correlating these fatigue values with class standing we find  $r = +0.23$ , about the same degree as found for naming the colors and marking out the A's, standing in Latin and the logical memory test, and length of head and logical memory. The short method gave no indication of correlation between degree of fatigue and the other mental tests. However, the correlation between the reading for the first ten efforts and the degree of

fatigue is  $-0.34$ ; that is to say, that the tendency is toward a compensation. The most natural explanation of this is that some began the test leisurely and concealed the signs of fatigue by a gradual increase of effort. Upon this assumption the above coefficient is meaningless. The writer is keenly aware of the arbitrariness of the method by which the degree of fatigue was estimated; but, if this method fails, there appears no other way free from the same objections. The enthusiastic designers of ergographs and dynamometers may say that the fault is in the instrument and that by employing their models results will be attained. The writer has used, so far as he is aware, all the types of ergograph in vogue and made a comparative study of them in the laboratory of Columbia University, with the result that when weights were used all gave similar results and likewise when springs were used other similar results. So it seems reasonable to assume that, no matter what the instrument, the result will be the same unless different muscles have different weight in correlation. In a recent study of the question<sup>1</sup> it was shown that certain peculiarities of the muscle in contraction, when reacting against certain peculiarities of a spring, gave results difficult to interpret and exceedingly variable. It thus becomes probable that the foregoing correlation between estimated fatigue and strength is in part an expression of the relation between the muscles and the spring of the ergometer. Thus a problem presents itself that is beyond the limits of this paper. The fatigue test needs an extended comparative study on a large number of individuals.

Since we are mostly concerned with the tests predominantly mental, those of a physical character may be passed over hastily. In case of head measurements we find a correlation of  $0.21$  (99 cases) for logical memory and length of head, but for breadth of head  $-0.05$ . By the short method no appreciable correlations were found between size of head and the other memory tests, for reaction-time, or naming the colors. As previously stated, some correlations appeared between length of head and vital capacity, vital capacity and stature, stature

<sup>1</sup>S. I. Franz. Methods of Estimating the Force of Voluntary Muscular Contraction, *American Journal of Physiology*, Vol. 6, No. 7.



and weight, and between each of these and strength of hand, but the number of cases available will not justify an attempt to determine their relative or combined weights in case of logical memory.

The correlation for memory and length of head seems very unlikely, but it may be of interest to add that by dividing the freshmen into four approximately equal groups, seven men were found to hold the same rank in all the memory tests. The corresponding head measurements were

Rank in Memory.	Length of Head.	Breadth of Head.
Good.	201	152
	202	150
Above Medium.	198	158
	194	158
Below Medium.	198	148
Poor.	190	147
	183	148

These results are rather striking and probably accidental. The writer refrains from drawing any conclusions whatever, leaving the interpretation to the reader. Tracing these seven freshmen through the other tests and college standing gave no corresponding results.

In conclusion, a few of the remaining tests may be noted. No evidences of correlation were found for pain or sensation areas. In case of color-blindness there was decidedly slowness in naming the colors. With eyesight we find a correlation with the number of omissions in the A test; that is, weak eyes result in inaccuracy, but the reverse is not true. Most of those reporting auditory hallucinations were classed as above normal in hearing ability.

Before going on to a general discussion of the subject it must be noted that we have not considered differences in age. It is possible for a close correlation with the age of individuals to neutralize a real correlation. Though we have not sufficient cases to treat each age separately, we can anticipate the result by correlating the various tests with the ages. We find the distribution of freshmen, expressed in per cents., to be as follows:

Age	15	16	17	18	19	20	21	22	23	24	25+
%	1	8	20	24+	26+	11+	4	2+	1	1+	1+

In the case of logical memory there was no correlation, for naming the colors there was a slight inverse one and for marking the A's a slight direct one. Thus it appears that the influence of age is small in the mental tests. Its influence in the physical tests we have not time to estimate, but it is probably greater than in the mental tests.

Also, it may be well to consider the effect of combining the results of the various tests in such a way as to give each individual an average standing, comparable to class standing in college. If it is true that such tests measure little more than special abilities, such an average should possess some significance. The calculation of such averages is by no means easy, because of arbitrariness of method. The writer used several methods and obtained satisfactory distributions of cases, but found no appreciable correlations between them and college standing. This failure may be due to faulty methods, but the probabilities favor its being the true result. Yet we should want a considerable number of separate tests before the status of the individual could be determined with much accuracy.

This does not exhaust the list of possible correlations for these tests, but represents all that could be accomplished in the time allotted. The aim has been to present in detail those of most interest to the psychologist, leaving the others to the future.

## DISCUSSION OF RESULTS.

In summing up the foregoing it appears that all the tests in this series have little interdependence. The idea of correlation implies some structural or functional relation. It is generally assumed that such relation in mental phenomena is functional rather than structural, and the conception of the analysis of mental processes and the search for the fundamental elements of the same imply a close correlation between simple processes. The direction of thought is that such ideas as quickness, accuracy, regularity, etc., represent some fundamental attribute of the individual that characterizes all his acts, and hence, tests giving results in the same category must correlate. On the other hand, when it is said that correlation does not exist, it is understood that the relations are those of chance. In other words, in a given number of tests we can approximate the number of individuals quick, medium and slow in all, as well as the number for any combination of positions in the various distributions. Since this conforms to the general result of the mental tests of this series we must look for the practical value of such tests in an extended series; the superior individuals in all-around quickness, for example, are those few who happen to be quick in the majority of such tests. Thus it becomes evident that the outcome of this research raises questions which throw us back into one of the great problems of psychology, viz., What constitutes mental ability? The significance of this question becomes apparent when we consider its relation to educational practice alone. It is plain that if we accept the conclusions of this research as final, an individual must be regarded as the algebraic sum of a vast array of small abilities of almost equal probability, the resulting combination conforming to the laws of chance. But a number of objections will be raised as to the validity of such assumption. It will, doubtless, be said that such a series of tests cannot give reliable results because the observers will vary the method and the order of presenta-



tion; that practice will so modify the result that we are not sure of any one's rank; that the estimated class standing of students has no value in ranking them as to mental ability and that the differences in grades are unequal and mean nothing; that other observers have found correlations between similar tests; that the result is contradictory to all experience; that such tests are on a different level in relation to the tasks of daily life. As these are the most probable objections not previously considered they will be taken up in turn.

In such a series of tests it is necessary to have many observers. These tests were made by the officers and graduate students in the department of psychology and anthropology. None of these can be regarded as unskilled in the strict sense of the word, yet they will vary greatly in experience and interest. Each year new students are carefully instructed in the methods of procedure and taken through the tests, both as subject and observer. Still, we must consider the equation of the observer as a possible disturbing factor. There are many tests in which it seems safe to assume that any of the observers should possess all the precision necessary, such as perception of size, strength of hand, eyesight, fatigue, pitch, force of movement, rhythm, marking out A's, naming colors and movement time. In reaction, head measurements, sensation-areas, pain, and possibly memory and association, the accuracy, experience and skill of the observers may be of considerable consequence. The only way of approaching this question at present is by a comparison of the results recorded by the different observers. It so happens that the instructors and three or four of the best trained students in the department tested each year approximately three-fourths of the freshmen, the remaining fourth being tested by many observers, presumably of much less skill, each taking from two to eight men during the test period (November and December). Thus the records may be divided into groups convenient for study.

In case of marking A's, naming colors and logical memory the averages for each group of observers show differences so small that when treated by the method employed in Table I. they indicate so low a degree of certainty that they can not be

regarded as possessing any significance whatever. While the number of cases falling to any one individual is small (12 to 30), making it difficult to come to a definite conclusion, yet there seems no good reason to doubt the result. An error of a second or two in starting, stopping or reading the watch can be of little weight in a test where the probable variation of the group ranges from 12 to 15 seconds and the tasks are so easily understood that all observers could give all the directions desirable.

In reaction-time more skill and care are necessary on the part of the observers. Some experience is required to start the chronoscope properly and take the reading accurately. Taking two instructors in psychology as the expert, we find for 29 and 28 cases, respectively, average times of  $147 \pm 16.5$  and  $146 \pm 19.4$ ; the probability of the difference from the total average is expressed by  $11 \pm 3.4$  and  $12 \pm 3.9$ , which is not a very certain difference. Taking a group of 11 observers with little experience, who represent a total of 28 cases, the average is  $159 \pm 16.7$ , almost exactly the total average. Taking two special students of psychology recording the times of 32 cases, the average is  $169 \pm 15.1$ ; certainty of this difference from the total average expressed by  $11 \pm 3.2$ . It appears that so far as these results go the differences between observers count for little, as they vary quite uniformly from the total average. The difference between the instructors and the skilled students, however, is  $22 \pm 3.9$ , which reaches the limits of considerable certainty. Yet this probably represents no more than an extreme variation in subjects, since for a second group of times by two other skilled students the difference was  $8 \pm 3.7$ .

In conclusion it appears that though there is some variation in results the differences between observers can not be very great. The fact that the expert record shorter times is doubtless a sign of a real difference in precision. But to settle this question definitely we should want either a large number of cases for each observer or a long series upon the same subjects by the different observers. After working over these results the writer is of the opinion that the differences between these observers are not sufficient to give the appearance of a low degree of correlation where there is in reality a high one.

In head measurements some significant individual differences may be expected. The writer had the same head measured independently by different observers with the following result:

Observer.	Length.	Breadth.
1	194	162
2	193	155
3	195	158
4	194	159
5	195	161
6	194	163
7	192	157
8	192	158
9	192	158

Of these 1 to 6 are individuals of little experience but having acted as observers in the tests; 7 is an instrument-maker; 8 a designer and draughtsman; 9 an anthropologist of note. The correct dimensions of this head are undoubtedly the result of 7, 8 and 9. The maximum error for length of head is +3 mm., breadth +5 mm. From Table I. we see that the most probable variation of the group is 4 mm., hence these measurements are not very accurate.

There were five trained observers measuring a total of 78 heads; fourteen presumably unskilled measuring 41; and special students of the department measuring 148. The averages and mean square deviations were:

Skilled  $193.8 \pm 5.8$   
 Unskilled  $194.1 \pm 7.2$   
 Medium  $193.9 \pm 6.5$   
 Total Av.  $194.0 \pm 6.7$

Thus, though the averages vary little, the mean square deviations are quite different. The general rule is that the mean square deviation is inversely proportional to the square root of the number of cases, from which it follows that the skilled observers should show a deviation of 12.2. Assuming that the skilled observers give us the most accurate series, the unskilled should show a deviation of 7.9 and the medium expert 4.1. It thus becomes probable that the series is distorted by errors of measurement which are so distributed as to give equal averages but render correlation somewhat uncertain. The fact that no important correlation appeared for dimensions of head makes



it unnecessary to go into a detailed discussion of errors or to attempt to correlate the measurements of the most skillful. The breadth of head being subject to the same errors, no attention need be given it here.

The remaining tests gave differences below the limit of certainty, which means that the observer is not a disturbing factor here. A very obvious way of estimating precision would be the comparison of single observers in a given year, but thus the cases become few and the probable range of differences so great that the accepted methods of estimating their certainty give no results.

As previously stated, the subjects did not take the tests in the same order, but the series was divided into convenient groups so that several men could be taken through at once without duplicating apparatus. In few cases was the regular order of the group varied, so that the tests of each group may be considered as following a definite order. We have, then, a variation in group order; but if the results fail to correlate in other respects, the chief cause is not to be sought here, as there is no evidence that practice or fatigue correlate in such tests.

Regarding the question of practice, it is evident that, if all gained uniformly, there would be no change in rank, and hence no change in correlation; but to assert that by practice the rank of individuals will be so shifted that decided correlations will appear where none existed in case of the initial trial seems to go beyond the limits of probability. Unfortunately, we have no correlatable data for practice. The nearest approach we can make is in the case of reaction times. Correlating the first reaction with the fifth gives 0.31 (136 cases); the fourth with the fifth 0.55. The significance of these coefficients is that, while there is quite a shift in the rank of individuals in the five reactions, each has begun to find his proper level in the fourth and fifth. The effect of this change upon correlations may be inferred from the case of naming the colors: for the first reaction 0.12 (115); fourth reaction 0.20 (112); fifth reaction 0.21 (119). Those who put much faith in the element of practice as the determining factor in correlation may find these coefficients assuring, since the correlation with the average time was 0.15 (118). However, the small variation of these coefficients seems

to indicate considerable precision in correlations for the average times. In an extended study of practice reactions<sup>1</sup> for five subjects the relative rank was practically the same on the last day as on the first. Should this prove generally true, the coefficients of correlation with the average times in this study may be taken as the most probable result to be expected in practiced subjects. On the whole, it seems probable that practice is only one factor among many in determining correlations and so of no special weight.

The correlation for class standing being incidental to this research, no lengthy consideration of the validity of such grading need be taken up here. Whatever may be the shortcomings of the system, it is the criterion of selection from college entrance examinations to graduation, and taking the grades themselves as the point of departure the results of the previous correlations show what is accomplished by this system. In other words, it expresses the probability that a given mark in one subject will be accompanied by a corresponding mark in another. The question as to the validity of the standard and the error in its application presents a different problem, though more fundamental. The consideration of this question alone would require extensive research. The uniformity of correlation between the grades for the different courses leaves no basis for a denial of correlation, but individual assumptions as to the precision of the method in the exact statement of differences in rank will lead to unlike interpretations. The only point aimed at in this research was to show that, while the marks of students correlate with each other to a considerable degree, they show little tendency to do so with the mental tests of the psychologist.

Those familiar with psychological literature know that correlations have been claimed for such tests as have been employed in this series. One of the first extensive studies of this character was by Gilbert,<sup>2</sup> but he concerned himself chiefly with differences found by grouping according to age. However, we are told that height, weight and lung capacity bear no relation

<sup>1</sup> Gilbert and Fracker, *Studies in Psychology*, University of Iowa, Vol. I.

<sup>2</sup> *Mental and Physical Development of School Children*, *Yale Psychological Studies*, Vol. II.

to mental ability, but that reaction-time is slower for "dull children" than for "average and bright." Yet no difference was found between the latter. In case of time memory "it may be said in general that the brighter the child the more accurate his sense of time." This is good enough, but we are given no information as to how such correlation was calculated or its degree. Dr. Scripture has appended a note to this article showing that many of the differences according to age become very uncertain when estimated by the accepted method, and the neglect of such precautions on the part of the author leaves the probability of decided correlations in doubt. But his results are not without value in this connection, since he is able to find no other correlations than these, and his curves of reaction-time show that the differences between bright, average and dull children have fallen below the limits of certainty in the 16th and 17th years, the point at which our series of tests begins. In a second paper<sup>1</sup> Dr. Gilbert confirms the results of the previous research. The only correlations that he claims generally true throughout are class standing with rate and fatigue in tapping. Here, as before, the method is simply a method of difference in mean values, from which the degree of correlation is difficult to estimate. Yet, as before, among students 16 years and over, the differences in rate and fatigue of tapping are below the limits of certainty from which we should expect a very low degree of correlation.

MacDonald<sup>2</sup> made an extensive series of measurements upon school children and concludes that there is a correlation between circumference of head and class standing, an inverse one for length of head and none for strength of hand. Here again we are given no information as to the degree of correlation and, since the author's tables contain averages only, we can not estimate it in any way. His tables fail to show any important evidences of correlation between class standing and sensation areas or sensitiveness to heat, and so in most respects these results agree with those for Columbia freshmen.

<sup>1</sup> Researches upon School Children and College Students, *Studies in Psychology*, University of Iowa, Vol. I.

<sup>2</sup> Experimental Study of Children, Report of the Commissioner of Education, 1897-98.



Dr. Seashore<sup>1</sup> reports no correlation between hearing ability and sensibility to pitch. He also made a series of tests on the same individuals comprising simple reaction to sound, sight and touch; reaction after sight discrimination and choice; rapidity and regularity of action; ability to follow a fixed rhythm, and the estimation of time intervals. There were about thirty cases for each sex and the tests were made with great care. This makes his results especially interesting, since we have opportunity to compare them with the more hastily made tests in our series, even though the number of cases is too small for accurate correlation. He claims a correlation between rate of tapping and reaction-times, though no estimate of its degree is made. Applying our short method for detecting correlations to Seashore's tables, we find approximately a chance agreement for all the reaction-times, and for the tapping test a slight irregularity in case of sound reaction only. From our experience with other data we should expect a small degree of correlation for this case. In general, if Dr. Seashore's 29 cases are at all representative of college men, we have no ground for expecting even a medium degree of correlation for a hundred cases. As his subjects were given some practice in reaction and time to get under way in tapping, the probable agreement of the results with our own is significant. Had Dr. Seashore been able to give a hundred or more cases a direct comparison of correlations could have been made. He also claims a correlation for time estimation and quickness of movement, while the Columbia tests show only a chance relation.

Miss Carman<sup>2</sup> claims that bright children are more sensitive to pain than dull children, but she again gives averages only. Since it is the common experience that the variability is very great in all algometer tests, we can tell little from averages alone. Furthermore, studies of age and class standing by many investigators show that the brightest children in a given grade are often the youngest and, as Miss Carman's results seem to show a decrease in sensitiveness as age advances, it may turn out that

<sup>1</sup> *Studies in Psychology*, University of Iowa, Vol. II.

<sup>2</sup> *Measurements of Pain*, Report of Commissioner of Education, 1897-98. *American Journal of Psychology*, Vol. X., No. 3.

there is no functional relation between mental ability and sensitiveness to pain, but that the differences found by her are due to differences in age.

Kirkpatrick<sup>1</sup> combined the results of several tests, mostly tests of quickness, and correlated them with class standing. We are not told exactly how this was done, but the facts given indicate a medium degree of correlation. As there were about 400 cases, all of the elementary school, the results are rather significant.

Binet and Vaschide<sup>2</sup> present correlations for tests in a definite and detailed way. They have considered physical correlations principally, but present some results of interest in the present connection. The correlation of simple and choice reaction-time, rate of tapping and speed in running gave no significant results. There were some indications of a low degree of correlation between these, but they were so slight that the authors themselves concluded that a decided independence exists between these abilities. Memory for numerals failed to correlate with physical tests. The class standing correlated somewhat with memory for numerals but not with reaction-time. As in some other researches, class standing correlated with physical development. The coefficients for the various tests show greater degrees of correlation for the young children, which is consistent with the results of Gilbert.

Dr. Bolton<sup>3</sup> made a series of memory tests upon school children finding no correlation between the results and class standing, but he found a lengthening of the span with age. Numerals were used in this test.

Dr. Sharp<sup>4</sup> made a series of tests on a few individuals and though concerned principally with method and presuppositions the results are much the same as others. There was a general lack of correspondence in the individual differences of adults even in the memory tests.

<sup>1</sup> Individual Tests of School Children, *PSYCHOLOGICAL REVIEW*, Vol. VII., No. 3.

<sup>2</sup> *L'Année psychologique*, 1897.

<sup>3</sup> Growth of Memory in School Children, *American Journal of Psychology*, Vol. IV., No. 3.

<sup>4</sup> *American Journal of Psychology*, Vol. X., No. 3.

Dr. Bagley<sup>1</sup> worked out the correlation between motor ability and class standing for children and decides that they tend to hold an inverse relation. However, he finds "numerous individual exceptions and varying validity at different periods of development." Also, there was a lack of correlation in reaction time and class standing. Unfortunately, we are not given the variability for the averages, and the degree of correlation can not be estimated, but on the whole the results indicate a general absence of correlation.

In passing we may quote a few minor researches. Delabarre<sup>2</sup> found no agreement between the time and speed of movement in the execution of a reaction. Scripture<sup>3</sup> gives a few cases of simple reaction and quickness of movement in fencing, with a similar disagreement. Miss Lee<sup>4</sup> found no correlation between estimated skull capacity and intellectual ability for 60 men and 30 women; the method of calculating correlation was the same as employed in our tests.

This does not exhaust the literature of the subject, but brings up for comparison the results of some of the most important studies. We find for one thing that where correlations are found they decrease with age. Dr. Boas<sup>5</sup> has proposed that the apparent correlation between stature and mental ability among school children can be explained as a phenomenon of growth; that is, a variation in maturity only. This view will account for the decrease of correlation as we approach the college age. That is to say, the fact that tests similar to those in the Columbia series appear to correlate better with children in the elementary schools than with college students may be explained on the ground that in case of children the relative maturity is the basis of correlation. Thus the bright child will do all things well because relatively more mature, or ahead of his age. It will be remembered that all tests upon children show that ef-

<sup>1</sup> *American Journal of Psychology*, Vol. XII., No. 2.

<sup>2</sup> Force and Rapidity of Reaction Movements, *PSYCHOLOGICAL REVIEW*, Vol. IV., p. 615.

<sup>3</sup> Tests of Mental Ability, *Yale Psychological Studies*, Vol. II.

<sup>4</sup> Study of the Correlation of the Human Skull, *Science*, Vol. XII., p. 1312.

<sup>5</sup> *Science*, March 1, 1895. Also discussed before the American Association of Physical Education, 1901.



iciency increases with age, which means a growth upward toward the adult level. This seems the most probable explanation of the facts at hand. Should this be true, seniors should show somewhat less correlation than freshmen, since we find differences between them analogous to those among children of different grades. Unfortunately, we have too few cases for this comparison. There is some reason to believe that college seniors have about reached the highest level of efficiency in such tests. Professor Cattell recorded results for some of the tests of the Columbia series made upon members of the American Association for the Advancement of Science. In no case were they superior to seniors and in memory tests slightly inferior to freshmen. The whole series of results suggests the probability that tests of this character are in a large sense measures of maturity.

In general, so far as the writer can see, the results of other investigators agree with his own. Correlations have been found to correspond in degree, in so far as it is possible to estimate them from the data given, when calculated for students of approximately the same advancement as college freshmen, but in no case is there reason to believe that decisive correlations have been found for mental tests.

We come now to the apparent contradiction between the results of this research and the experience of life. It is often claimed that quickness, for example, will show itself in all our acts, and that therefore there must be a correlation in tests of quickness. Experience is able to produce cases of all-around quickness, dexterity or mental ability, and these are set up as proofs of functional relations. But this comes from a misconception. A chance correlation does not mean that no one will manifest such efficiency, but that the number of such individuals is governed by accident. That is to say, given a number of directions of activity, the number of individuals excelling in all can be closely approximated according to the laws of chance. In this instance one exception does not break the rule. Some authors have made this very error in estimating correlations for their data. The trouble is that experience keeps no record of numbers but deals in isolated cases. It is only by

such methods as have been employed in this research that any idea of the relative number of cases can be obtained. And lastly, the claim that such tests have nothing in common with the tasks of life is really not an objection at all, but an assumed explanation of the result of this research. As a theory it is good enough, yet it wants proof. It remains to be shown what elements of activity contribute to the results of our daily efforts as well as to the results obtained from the tests. This is an analytical problem which falls beyond the limits of this work. To simply assume that there is a gulf between the two and to proceed to rule such tests out of court on the basis of such assumption, will not dispose of the case.

In summarizing this discussion, it appears that these tests have been made with a fair degree of precision and that the results show what may be expected from such a series, however elaborate. More than that can scarcely be claimed. The writer wishes to be understood as not claiming finality as to degree of correlation in any case. Each test presents a problem in its distribution of results worthy of considerable study, and the effect of various types of distribution upon the coefficients of correlation is as yet undetermined, but it is scarcely conceivable that such disturbing factors could effectually conceal correlations in all the methods employed. However, it must not be overlooked that correlation always proceeds upon the assumption that a given series is homogeneous. Such a series would be obtained by measuring the heads of adult males of a given race; should measurements for females be included, the series would not be homogeneous. Yet this is but a relative term and means that because there is a constant difference between the heads in question they must be treated separately when correlations are calculated for other organs similarly variable. The question in these tests is whether the students who come to Columbia College year after year can be considered as a homogeneous group with respect to the correlations attempted. In the physical measurements some allowance must be made for age, since there will be some growth on the part of the younger men, but in mental ability it seems probable that the method of selection is such as to secure a homogeneous group, passing the

entrance examinations being the criterion of selection. The distribution of ages previously considered indicates that 50% of the cases fall between 17.5 and 19.5 years, 70% between 16.5 and 19.5. As these limits fall beyond the period of greatest acceleration in rate of growth in height and weight, the error in treating the physical distributions as homogeneous can not be very great, probably insufficient to greatly disturb correlations. By calculating correlations for the mental tests and age, as previously stated, the influence of age appeared slight and hence not a factor of much relative importance. With a few exceptions comparison of averages for the different college classes shows them to be constant within the limits of accidental variation; this of itself makes it quite probable that the series is a homogeneous one with respect to these tests. Should decided differences have appeared between the different college classes or between those of different ages the assumption upon which this research has proceeded would be unwarranted. Yet it must be understood that absolute proof of the homogeneity of a series is always difficult. But granting that the series is not mixed, we have still to consider the probable error of the coefficient of correlation. In the discussion of the effects of practice the coefficient of the fourth and fifth reactions with naming the colors was found approximately the same. A nearer approach to the question would be to correlate in groups of 100 and compare the coefficients, but since the mean square deviation is an important factor in calculating the degree of correlation it is only necessary to compare such groups with respect to their variability. As previously shown, there were few important differences in this respect. It is true, however, that correlation becomes more precise as the number of cases increases and several hundred cases are necessary for the comparison of results. In these tests the number of cases is far from satisfactory.

In view of the above, it is not assumed that these results can give a satisfactory answer to the question of functional relations in the processes involved, nor is it our purpose to afford support to any theory in the case. We have found low degrees of correlation in many cases, which seems to imply that, if func-



tional relations hold in these tests, they are exceedingly complex, even more so than many psychologists have assumed. This promises little for such tests from a practical point of view. While the tests do seem to have some value when applied to children in the lower schools, they tell us nothing as to the general individual worth of college students or of adults. Indeed, they lead us to doubt the existence of such a thing as general ability. This general negative statement is likely to impress the reader in such a way that he will feel disposed to declare that psychological tests are of no value, and that time spent in making them is mere waste. The writer can not share in such a feeling. He believes that a little reflection will make it clear that because of the problems raised and the suggestions offered for new hypotheses, the making of tests is more desirable than ever before. While the outcome of this research tends to negate the immediate practical value of such tests, it suggests the possibility of a solution that will be of great importance in itself.

In a preceding section it was suggested that we may assume every act measured by the tests to be special and unrelated to every other act, and that the summation of these capacities for all our acts is a combination conforming to the laws of chance. The reluctance felt in accepting such a view as even probable emanates from a deep conviction that we ourselves are otherwise constituted. The learned world has struggled long and hard with the problem of the individual and in the attempted classification by temperament we find a fundamental assumption which seems entirely contradictory to the result of this research and especially out of all harmony with the above hypothesis. In the theory of the temperaments it is always assumed that individuals can be classified with respect to quickness of mental processes and also with respect to their intensity. Generally there is a tendency to deal with each in terms of the two extremes, as, the quick and the slow, the intense and the weak. The four possible combinations of these give the four classical temperaments. The theory grows out of a deep-rooted traditional belief that rate of action on the one hand and intensity of feeling on the other are gauged in some mysterious way so that they set

the pace for every moment of our lives. This, like all other popular views, is based upon facts of some sort, but they may be facts of subjective as well as objective experience. Or, in better terms, the belief may arise from observed attributes of individuals or from a feeling of a logical or æsthetical necessity in the nature of things. We have already noted the illogical use of the observed facts; but, however that may be, there is another popular belief that stands in seeming contradiction to the former; viz., that each individual has some point of difference that enables him to do some one thing better than any other individual. Brought up to the terms of the preceding classification this means that individuals vary not in universal quickness but in the rate of particular processes. It seems within the line of legitimate speculation to regard this belief as the result of observed facts in the affairs of men where each is measured according to the attributes of some very specific and often trivial act, and that the belief in a general attribute, property, or skill of equal weight in every moment of life is the expression of a desire or an ideal. So long as psychological thought was qualitative and advanced by discovering the common qualities of mental facts it was necessary to attach a general significance to one fact, but when the quantities in mental facts were sought, or when they were expressed in terms of quantity, if you like, the same attitude was assumed. The measuring psychologist set down reaction-time, perception time, accuracy in movement, etc., as characteristics of the individual in every moment of life, or as characteristics common to all acts. To exaggerate for sake of clearness, he was tempted to hope that from a few figures in his laboratory notebook he could estimate the general worth of the man; in other words, assign him a place in some general scheme, such, for example, as a classification according to good, medium, poor. This is dealing with the quantitative difference in the same terms as the qualitative. A reaction is regarded as a simple movement, and all motor processes are complexes of a vast number of simple movements; then why should not the quantitative values of the parts be a function of the total? If a man is slow to react why should not all his acts and thoughts take on the same? If the classification by temperaments means

anything some such relation must hold. Led on in this way even the most conservative psychologists have entertained views assuming an identity of relation between mental processes estimated according to quality and those estimated in terms of quantity. The writer feels safe in asserting, then, that the reluctance felt in accepting the results of this research is not due to knowledge of contradictory observed facts but to old points of view and presuppositions. While the popular beliefs are contradictory, the psychologist has attempted to harmonize the result of his analysis by assuming a close correlation between all the characteristics of mental processes regardless of the category in which they are placed.

Now it does not appear that the want of correlation between mental tests is fatal to the conception and results of analytical psychology so long as the latter deals with sensations as elements in a complex. Indeed, we seem to have come back to a much-discussed question; viz., whether analytical psychology has anything in common with the problems of practical life. Psychologists readily admit that, when we have to do with men, women and children in the affairs of life, we do not regard them as mere things but always as creatures actuated by ideas, as exercising volitions, experiencing emotions, as subject to habit, etc. The tricks of trade and politics, social functions and administration all proceed upon the assumption of minds, and regulate their methods according to their knowledge of the working of these minds. In all organization the differences between the results attained by individuals become important, since the success of the whole depends upon each finding the work he can do best. The leaders of our industries, complex or simple, can only maintain themselves by so parceling out the tasks that each laborer will reach his maximum. This adaptation and study of the individual never loses sight of the assumption of his mentality. But the psychologist as known in academic life turns his attention in another direction. He tells us that psychology is the science of mind and that mind is a name for an activity conceived in its entirety, in just the same way that we speak of life oblivious of the many organic structures that live their separate lives. At first thought there seems no reason why these



two points of view should not have much in common. It seems that the way to study life would be to give our attention to some separate lives and that likewise the way to study mind would be to study some of the minds as we know them. But while this is true, the psychologist informs us that he has no place in his science for the study of the individual. He wishes to ignore such variations and treat mind in its universal aspect. In this he is consistent. So long as we are searching for general laws we can ignore many variations, but when we are concerned with problems of the individual we must take into account the differences between individuals. This is why some analytical psychologists assert that their science has nothing for life or those who operate on its level. But though experimental psychology has been the pursuit of those concerned with analytical problems only, it has been forced to consider individuals in relation to points of difference, and such considerations have excited a hope and created an interest on the part of those operating on the level of practical life. This is the secret of the "rush to experimental psychology" against which Professor Münsterberg cried out some time ago. But no amount of "trespass" or "danger" placarding will conceal the possibilities which experimental methods suggest in the discovery and practical application of facts of individual difference. No one struggling with persons can help seeing that such facts must be of service even if it be in a negative sense.

It seems, then, that experimental psychology, while appropriated by those interested in analytical problems only, has also driven the wedge for some of the problems of practical life. The latter concern themselves with the individual in terms of quantity, and though a pure science may ignore such variations, an applied science can not. The outcome of the preceding research is not to be regarded as a proof of the futility of the method or as a valid basis for assuming experimental methods impotent in social practice, because the two points of view have little in common. There is apparently no necessary presupposition antagonistic to the utter functional independence of mental processes when estimated in terms of quantity. And, moreover, the value of the method in one case is not to be determined by the assumptions underlying its operation in the other.

It will be remembered that in a few tests we found a slight correlation, which suggests the importance of an exhaustive canvass of the whole field of human activity to see if some test may not yet be found that will correlate to a high degree with other lines of activity. If from such we find still no appreciable correlation, the fact will have immediate practical value in establishing certain practices in education and in abolishing others. The present status of research shows us that mental and physical tests have some value when applied to children. If the assumption of Dr. Boas prove true, a few tests will indicate the general relative maturity of the child with a fair degree of precision. This is not saying that such tests can be put into practice immediately, 'for we need more data, much more. Anthropometry and psychology must go on with the work until the assumption of Dr. Boas is proven or rejected. When we once know the truth as to the functional relation between physical and mental development, as well as that between the various acts of life, then such tests will be of value. If Dr. Boas is correct, we can tell at once about where a given child is in respect to his absolute age. Even as it is, a teacher may find such tests of aid in judging a child when the results are considered from this point of view. Whatever may be the case, it seems safe to assume that when a teacher finds a child in the elementary school superior to his class in stature, weight, etc., and also younger than the average, he can feel reasonably sure that this child is a superior pupil. The teacher's judgment thus facilitated may soon verify itself in other ways. These few suggestions will impress upon the student of education the need of a thorough correlation of tests of all kinds. Strange to say, no one seems to have felt the need of a rigid examination of actual results in respect to the correlations existing between academic tests. Every definite school task is susceptible of estimate in terms of quantity and is usually so estimated. Then why should not students of education examine them? If they should not correlate, then so far as their quantitative aspect is concerned there is no functional relation between them, and all such are special activities varying according to practice, etc.

To repeat, then, there seems no reason why the making of mental tests is not more important than ever before.

It seems worth while to call attention to the possibilities of correlation in psychological investigation. In studies of practice the correlations between the successive trials will answer many of the puzzling questions and many that can be answered in no other way. The precision of a test may be estimated by correlating the successive trials; if the individual's own results do not correlate to a high degree, we have a test in which individual differences are relatively small as well as uncertain. The method is such that different observers may compare their results with ease and accuracy. For the fatigue question it offers opportunity to try all methods and apparatus on the same persons and correlate the results of one with the other. The question as to the influence of physical training, exercise, time of day, etc., is also difficult to solve except by correlation and by a method that enables us to estimate its degree. Of course, the whole of psychology is not to be found in a mere process, but the method we have used is promising as one of its tools.



# SUMMARY.

By way of a general summary all the coefficients of correlation as calculated by the Pearson formula are here enumerated:

## QUICKNESS AND ACCURACY.

	Cases	<i>r</i>
Reaction and marking out A's,	252	— 0.05
Reaction and naming the colors,	118	+ 0.15
Reaction and association time,	153	+ 0.08
Reaction and movement time,	90	+ 0.14
Naming the colors and marking out A's,	159	+ 0.21
Naming the colors and movement time,	97	+ 0.19
Drawing and bisecting a line,	123	+ 0.38
Accuracy and speed in marking out A's,	252	— 0.28

## MEMORY.

Auditory and visual—correctly written,	144	+ 0.29
Auditory and visual—correctly placed,	144	+ 0.39
Logical and auditory—correctly written,	94	+ 0.05
Logical and auditory—correctly placed,	94	+ 0.04
Logical and retrospective,	91	— 0.07
Force of movement and drawing line,	123	— 0.08
Logical memory and naming the colors,	93	+ 0.03
Logical memory and reaction time,	96	+ 0.12
Auditory memory and reaction time,	112	+ 0.17
Visual memory and reaction time,	104	+ 0.06
Pitch memory and reaction time,	100	+ 0.01

## PHYSICAL TESTS.

Strength of hand and class standing,	204	— 0.08
Fatigue and class standing,	132	+ 0.23
Fatigue and strength,	140	— 0.34
Length of head and logical memory,	99	+ 0.21
Breadth of head and logical memory,	99	— 0.05

## CLASS STANDING.

Class standing and reaction time,	227	— 0.02
Class standing and marking out A's,	242	— 0.09
Class standing and association time,	160	+ 0.08
Class standing and naming the colors,	112	+ 0.02

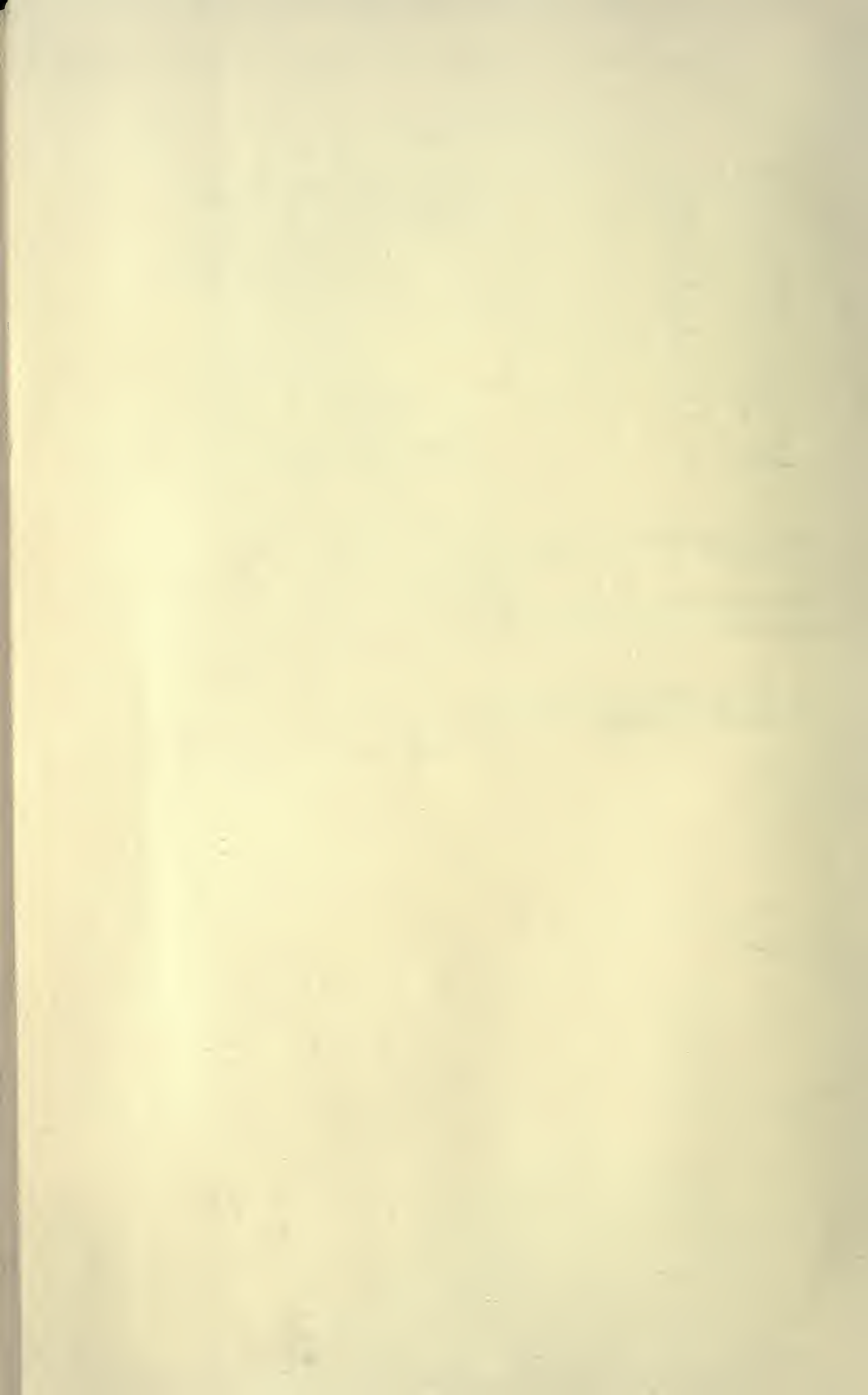
Class standing and logical memory,	86	+ 0.19
Class standing and auditory memory,	121	+ 0.16
Class standing and gymnasium,	119	+ 0.53
Latin and mathematics,	228	+ 0.58
Latin and rhetoric,	223	+ 0.55
Latin and French,	130	+ 0.60
Latin and German,	129	+ 0.61
Latin and Greek,	121	+ 0.75
Rhetoric and French,	122	+ 0.30
Rhetoric and German,	132	+ 0.61
Rhetoric and mathematics,	222	+ 0.51
German and mathematics,	115	+ 0.52
Mathematics and logical memory,	90	+ 0.11
Latin and logical memory,	90	+ 0.22

The general conclusions are :

1. That the laboratory mental tests show little intercorrelation in the case of college students.
2. That the physical tests show a general tendency to correlate among themselves but only to a very slight degree with the mental tests.
3. That the markings of students in college classes correlate with themselves to a considerable degree but not with the tests made in the laboratory.













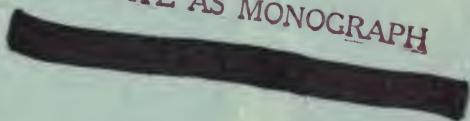
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