THE ACCURACY OF VOLUNTARY

MOVEMENT

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

NEW YORK
JULY, 1899

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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 hun-
dred 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 cor-
responding 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 centi-
meters. 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.[1]

(2) For any one joint, the least perceptible movement was the same in any direction.[2]

(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.[3]

(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.[4] Nor can the muscle sense, in the strict meaning of that term, be supposed to give us information regarding these minute *passive* movements.[5] The sensation which arises in the muscle itself is, except in case of fatigue and pain, a dull, dif-

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[1] *Archiv f. Anat. u. Physiol., physiol. Abth.*, 1889, 486, 487.

[2] *Ibid.*, 480.

[3] *Ibid.*, 1889, Suppl. Band, 207.

[4] *Ibid.*, 1889, 491.

[5] *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.[1]

(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.[2]

Hall and Hartwell,[3] 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,[4] 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.[5] If a manual worker, he exaggerated the left hand; if not, the right.

[1] *Zeitschr. f. klin. Med.*, 1889, XV., 108–111.

[2] *Arch. f. Anat. u. Physiol., physiol. Abth.*, 1889, 498 ff.

[3] *Mind*, 1884, IX., 93–109.

[4] *Pflüger's Archiv für die ges. Physiologie*, XLI., 107–127; XLIV., 101–114; XLVI., 1–46.

[5] 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[1] comes to the opposite conclusion from Loeb as

[1] 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[1] have made the first and only thorough test of Weber's Law as applied to the perception and re-production 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

[1] 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[1] 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.'

[1] 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*[1] 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*[2] 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[3] 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-

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[1] Münsterberg, *Beiträge zur experimentellen Psychologie*, 1892, IV., 81–88.
[2] *Ibid.*, pp. 218 ff.
[3] 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 ⅔ 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.[1]

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-

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 ex-
periment,' 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 determ-
ination of the per cent. of errors. Its advantage is that the sub-
sequent calculation of results requires only counting instead of
measuring. This is a point of considerable importance in study-
ing fatigue, since, in order to get fatigue, an immense number
of hits is necessary, and some device for shortening the calcu-
lation 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 shalts" or "thou shalt nots."

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 con-
tain 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 princi-
pal 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 in-
accurate 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 psy-
chology 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 'sub-
ject' 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, ap-
plied 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 be-
tween them. The figure in italics gives *the error of the aver-
age*—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 ac-
curacy 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

ERROR
mm.

4 -

2 ·

20   40   60   80   100   120   140   160   180   200
SPEED
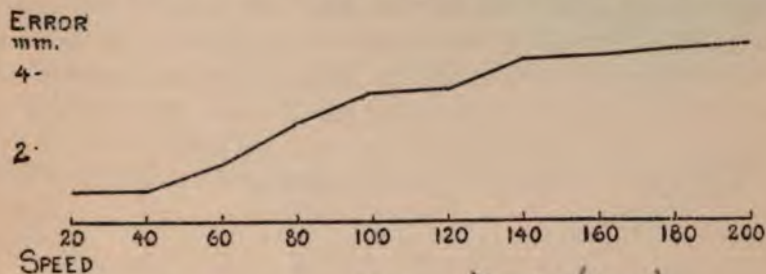
FIG. 2.   MOVTS/MIN.

reduce the result to so simple a formula as that the error is pro-
portional 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 por-
tion 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 inter-
val 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 accu-
racy 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
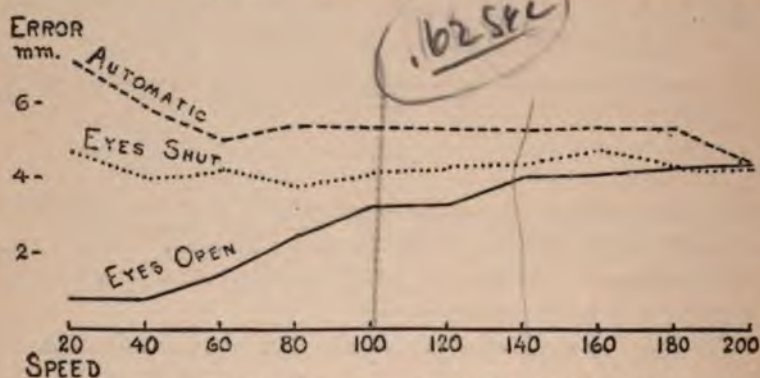
FIG. 3.   Relation of accuracy to speed.   Right hand.

eyes are closed, and also when the movement is careless or 'au-
tomatic.' (For explanation of the ' automatic' movements con-
sult the text to Tables I.–IV.) We see that the automatic move-
ments 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 ac-
curate than this, is still decidedly better than the careless move-
ment. But though this is true at low speeds, it is less and less
true as the speed is increased. The gradual decrease in ac-
curacy 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,
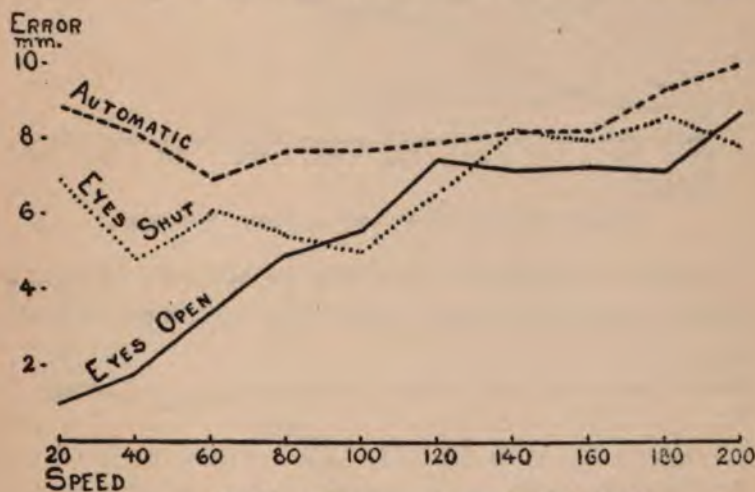
FIG. 4. Relation of accuracy to speed. Left hand.

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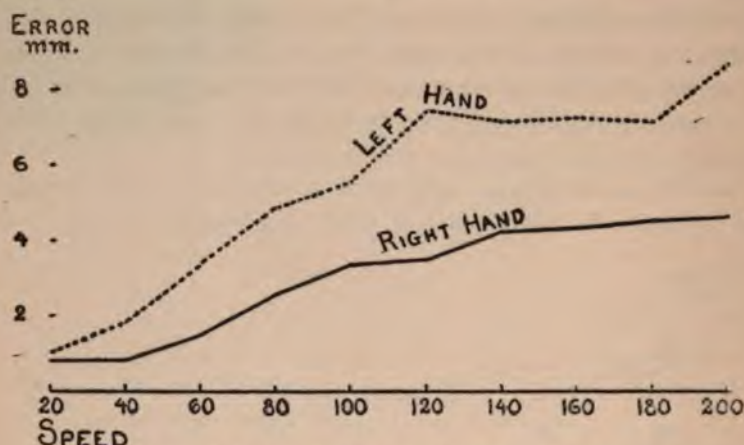
FIG. 5.   Relation of accuracy to speed.   Eyes open.

In order to compare more easily the accuracy of right and left hands, and thus to contribute a little to the study of bilateral
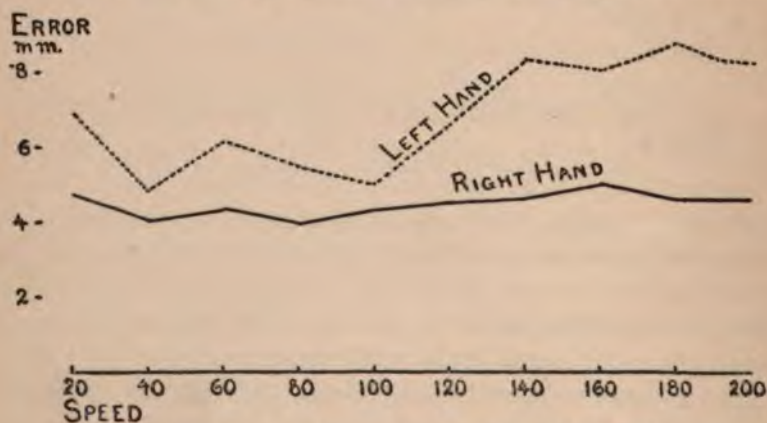
FIG. 6.   Relation of accuracy to speed.   Eyes closed.

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 ex-
tremely 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
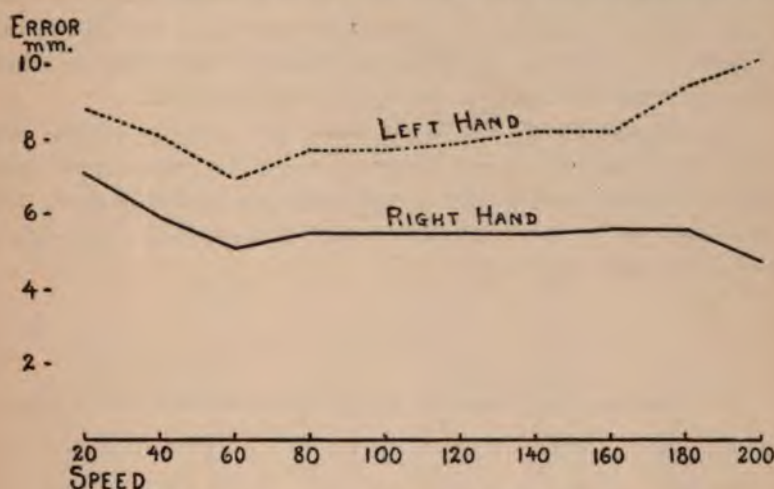
FIG. 7.   Relation of accuracy to speed.   Automatic movements.

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
Alternately { 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,

	Average Error.	Constant Error.
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,[1] 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
$\frac{8}{200}$	5.0	.4	6.0	.6	5.3	.5	4.8	.5	5.4	.6						
$\frac{10}{200}$	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.

[1] 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.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	140	160	180	200
$\frac{1}{20}$	0.5 .1	0.6 .1	1.4 .1	4.3 .4	4.4 .5	5.5 .6	8.6 .7	6.3 .6	4.6 .5	5.7 .6
$\frac{2}{20}$		0,2		3.1 .3		6.1 .7		4.3 .4		6.9 .7
$\frac{3}{20}$			0.3			3.9 .4			3.8 .4	
$\frac{4}{20}$				1.4 .1				4.5 .5		7.5 .6
$\frac{1}{100}$					2.8 .3					
$\frac{1}{120}$						2.8 .3				
$\frac{1}{140}$							4.2 .4			
$\frac{1}{160}$								3.1 .3		
$\frac{1}{180}$									3.5 .3	
$\frac{1}{200}$										3.7 .4

TABLE VIII.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	140	160	180	200
$\frac{1}{20}$	0.9 .1	1.8 .2	2.4 .3	3.0 .3	3.6 .4	3.5 .4	3.6 .4	5.8 .6	7.5 .7	5.5 .6
$\frac{2}{20}$		1.0 .1		3.1 .3		3.8 .3		5.4 .4		9.2 .9
$\frac{3}{20}$			1.6 .1			3.7 .3			5.3 .5	
$\frac{4}{20}$				1.6 .1				3.8 .3		3.9 .4
$\frac{1}{100}$					2.9 .2					
$\frac{1}{120}$						2.8 .2				
$\frac{1}{140}$							3.2 .2			
$\frac{1}{160}$								4.2 .3		
$\frac{1}{180}$									4.2 .3	
$\frac{1}{200}$										4.4 .3

TABLE IX.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	140	160	180	200
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
1/40		1.3 .1								
1/60			2.2 .2							
1/80				3.8 .2						
1/100					4.0 .3					4.4 .5
1/120						3.3 .2				
1/140							5.4 .4			
1/160								6.1 .3		
1/180									5.8 .4	
1/200										5.5 .6

TABLE X.

INTERVAL. MIN.	SPEED PROPORTIONAL TO									
	20	40	60	80	100	120	140	160	180	200
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
1/40		0.5 .1				4.4 .4		6.3 .6		6.6 .7
1/60			1.5 .2	3.9 .4		3.9 .4			4.8 .4	
1/80				2.3 .2				3.7 .3		
1/100					3.8 .3					5.0 .5
1/120						3.3 .3				
1/140							2.9 .2			
1/160								3.1 .2		
1/180									4.5 .3	
1/200										2.8 .2

TABLE XI.

INTERVAL. MIN.	SPEED PROPORTIONAL TO							
	20	40	60	80	100	120	160	200
1/20	0.1	0.2	3.2 .3	3.2 .3	3.3 .3	3.7 .4	3.0 .3	6.5 .7
1/40		0.1						6.6 .6
1/60			0.6 .1					
1/80				2.8 .3				
1/100					4.4 .4			5.1 .5
1/120						3.5 .4		
1/140								
1/160							3.2 .3	
1/180								
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 sug-
gested by the fact, already established, that a fair degree of uni-
formity 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 be-
tween 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 accu-
racy. But beyond a certain point this decrease scarcely con-
tinued; shortness of interval balanced speed.

The bad effect of speed consists in rendering impossible a
delicate *current control*, in preventing those later and finer ad-
justments by means of which a movement is enabled to ap-
proximate 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 inter-
val 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 de-
pends 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 move-
ment 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 sub-
stantiated 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. ($\frac{2}{200}$ 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.*
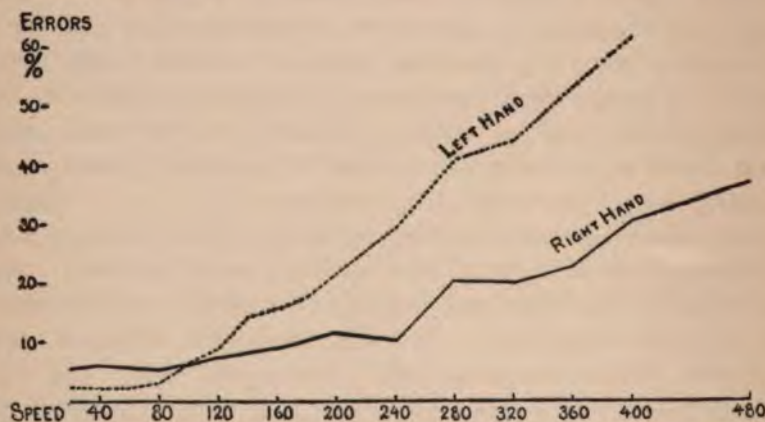
FIG. 8.—Relation of accuracy to speed, in the 'coördinate paper experiment.'

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[1],　　　　　　　　6.2　　　　　　　　6.6
Eyes shut,　　D,　　　　　　　　　3.3　　　　　　　　3.1
　　　　　　　P,　　　　　　　　　6.2　　　　　　　　9.2
　　　　　　　P[1],　　　　　　　　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.

[1] 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.[1] 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

[1] Fullerton and Cattell, p. 108

the duration of a movement, it follows that the speed of a move-
ment 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 ac-
curacy 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 pro-
portional 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 move-
ment 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 in-
creasing 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 accu-
rate 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 in-
terfere within the initial adjustment; and that its great disad-
vantage lies in its prevention of the later and finer adjustments.

PART IV. ACCURACY AS DIVIDED BETWEEN THE INITIAL AD-
JUSTMENT 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 them-
selves. 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 can-
not 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 ap-
pears and gives us an insight into the character of the first ad-
justment. 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 :
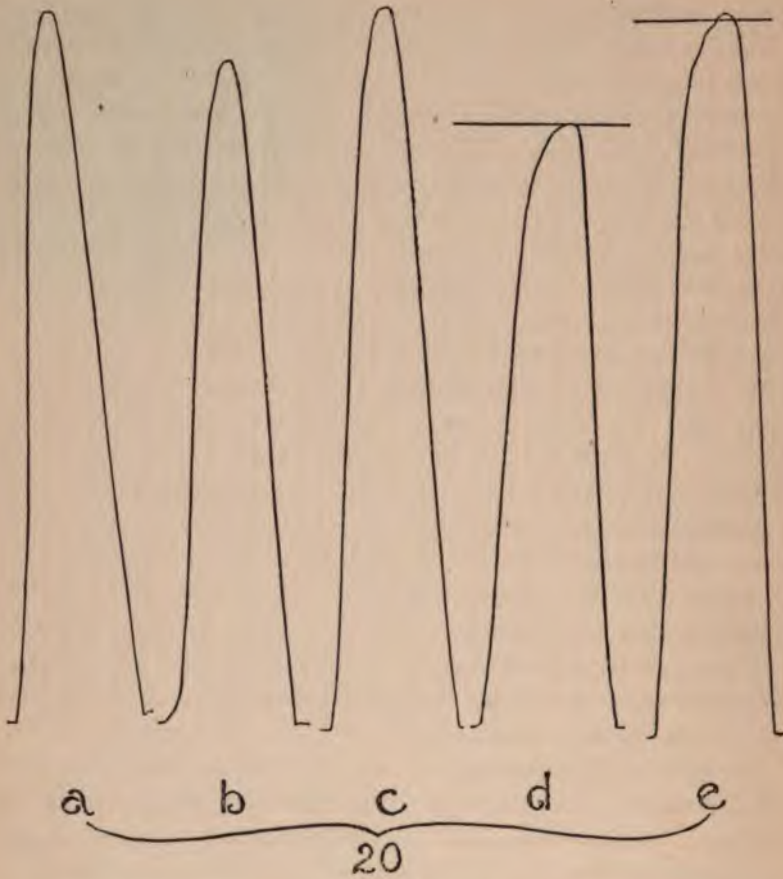
FIG. 9.

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 ⅔ 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.
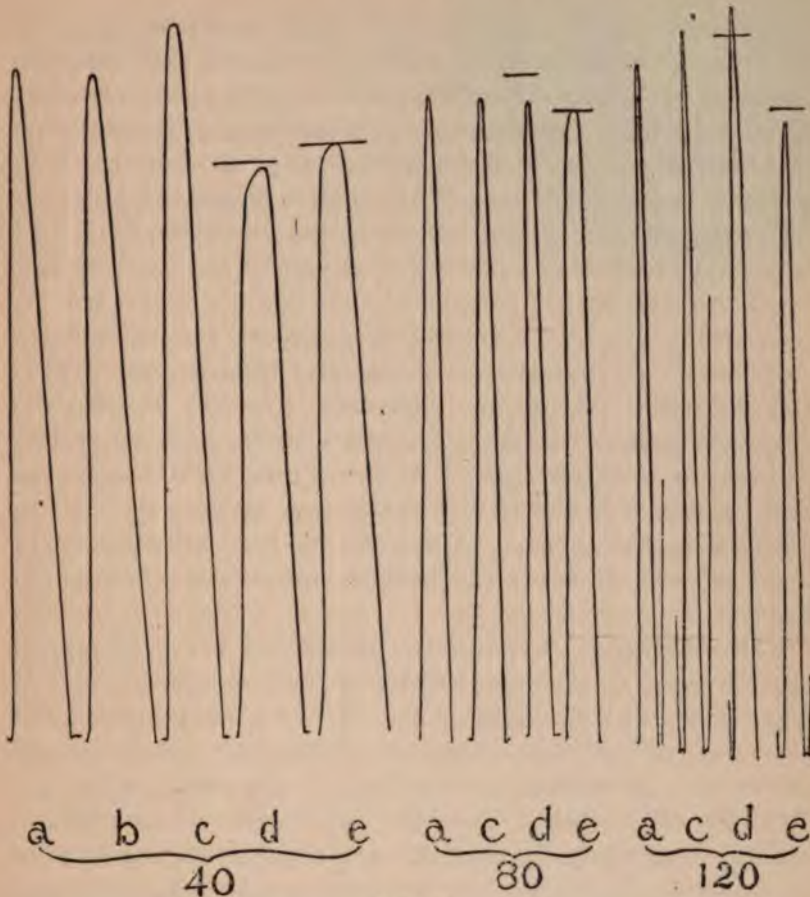
FIG. 10.

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 move-
ment 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 irregulari-
ties 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 move-
ments 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 move-
ment is a repetition of the one before.

The error of initial adjustment can be more readily calcu-
lated 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 be-
tween such changes and those which record a correction. And
this sort of confusion is comparatively absent in slow move-
ments.

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 .1	+0.3	0.4 .1	0.8 .1	0. .1	0.8 .1
80	0.5 .1	+0.4 .1	0.5 .1	1.0 .1	+0.3 .1	1.0 .1	1.5 .1	+0.5 .1	1.5 .1	4.4 .3	0. .5	4.4 .3
120	1.3 .1	+0.4 .1	1.3 .1	2.9 .2	+1.5 .4	2.8 .2	3.4 .2	+1.1 .3	3.4 .2	4.1 .3	—0.1 .5	4.1 .3
160	1.8 .1	+0.7 .3	1.7 .1	3.9 .3	+2.6 .6	3.2 .3	5.5 .5	+2.1 .9	4.9 .4	3.6 .3	—0.4 .5	3.6 .3
200	2.4 .2	+1.2 .3	1.9 .2	3.4 .3	+1.8 .5	3.3 .3	4.6 .5	—1.8 .8	4.4 .4	5.6 .6	—1.7 .9	5.5 .6

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 .1	+0.5 .1	0.6 .1	0.3	+0.2	0.3	0.3	+0.1	0.3	0.1	0	0.1
60	0.4 .1	+0.4 .1	0.4 .1	0.7 .1	+0.5 .1	0.7 .1	0.6 .1	+0.4 .1	0.6 .1	0.7 .1	+0.1 .1	0.7 .1
80	0.6 .1	+0.5 .1	0 6 .1	1.4 .2	+0.1 .3	1.4 .2	1.7 .2	—1.0 .3	1.6 .2	1.4 .2	—0.2 .3	1 4 .2
120	1.9 .2	—0.3 .4	1.9 .2	2.9 .3	—1.9 .6	2.4 .3	3.1 .3	—2.0 .5	2.9 .3	5.3 .6	—3.7 .8	4.5 .5
160	1.7 .2	—0.9 .4	1.5 .2	3.7 .4	—2.3 .6	3.1 .3	5.9 .5	—5.3 .6	3.9 .4	4.4 .5	—1.4 .9	4.5 .5
200	2.8 .4	—2.1 .4	1.9 .2	3.5 .4	—1.2 .8	3.2 .4	7.3 .7	—5.5 1.3	5.1 .5	4.1 .6	—1.7 1.3	4.5 .7

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 .1	+0.3 .1	0.6	0.6	+0.4 .1	0.6	0.6	+0.3 .1	0.6
40	0.2	+0.2	0.2	0.5 .1	+0.2 .1	0.5	0.6	0 .1	0.6	0.6	+0.5 .1	0.6
60	0.8 .1	+0.5 .1	0.8 .1	1.2 .1	+0.6 .2	1.2 .1	1.6 .1	+0.6 .2	1.5 .1	1.7 .1	+0.2 .2	1.7 .1
80	1.1 .1	+0.2 .1	1.1 .1	1.7 .1	+0.1 .2	1.6 .1	2.2 .2	+0.1 .3	2.1 .2	2.6 .2	+0.1 .3	2.6 .2
100	1.2 .1	+0.3 .2	1.2 .1	1.7 .1	0 .2	1.6 .1	2.3 .2	+0.3 .3	2.4 .2	3.0 .2	+0.1 .4	3.0 .2
120	1.6 .1	+0.9 .2	1.5 .1	1.6 .1	—0.5 .2	1.6 .1	2.4 .2	+0.3 .3	2.5 .2	2.8 .2	—0.1 .4	2.8 .2
140	1.4 .1	+0.6 .2	1.5 .1	2.3 .2	—0.7 .3	2.3 .2	3.3 .2	+0.5 .4	3.3 .2	3.4 .2	—0.5 .4	3.2 .2
160	1.6 .1	+0.8 .2	1.2 .1	2.7 .2	+0.8 .3	2.6 .2	3.0 .2	—0.4 .3	2.7 .2	4.2 .3	—0.4 .5	4.3 .3
180	1.5 .1	—0.3 .2	1.5 .1	3.1 .2	—0.6 .4	3.0 .2	3.9 .3	—0.7 .5	3.7 .3	4.0 .3	—2.6 .4	3.4 .2
200	2.0 .1	—1.3 .2	1.8 .1	2.5 .2	—0.2 .3	2.5 .2	5.2 .4	—2.1 .6	4.6 .3	6.4 .5	—1.1 .8	6.2 .4

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. 66⅔.	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.[1] These ex-

[1] 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 mis-
spelling it. In forming the letters, therefore, we come to de-
pend mostly on the muscular and tactile sensibility.

In urging that the muscle (tactile, etc.) sense is disre-
garded 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 sensa-
tion 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 an-
other 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 resi-
dent 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 ex-
erted, 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 possibili-
ties in turn, I shall illustrate by experimental results the follow-
ing general statement:

The extent of movement is not judged exclusively in terms

either of the time occupied or of the force employed, or of the limiting positions, since any or all of these may be different in the normal line and the reproduction. But every such change in the conditions between the normal and the reproduction, though it does not entirely destroy the approximate equality of the movements, does diminish the subjective confidence in their equality, and is almost certain to introduce a constant error, and so impair the accuracy of the reproduction.

The constant error introduced by changing any one element, such as force, time, or position, need not be in the direction of compensating for that change, and is never of sufficient size to compensate.

The movement may be changed in all sorts of ways—in position, direction, time, speed, force and even in the member that makes it—without an entire loss of the power of gauging one movement by the other. The method used is open to one objection, that the subject was aware of the change in conditions, and so could make allowances. I see no way of changing the conditions without the knowledge of the subject, unless indeed it is done very gradually. This the subject will often himself accomplish, if required to make a long series of free reproductions. He will gradually alter the speed or the force used, or the position. I have attempted to measure such results only when the change was in position. In the cases of changes in force and time there was another way out of the difficulty.

The following list gives the accuracy found for different modes of reproducing a given extent of movement. The accuracy is indicated (inversely) by the average error, expressed as a fraction (per cent.) of the normal length :

1. Eyes open, right hand, ruling lines on kymograph, slowly..... 0.6
2. Same, rapidly.. 2.9
3. Same with the left hand, slowly................................... 0.6
4. Same, rapidly......... .... 6.5

The remainder are without the use of the eyes, and, unless otherwise noted, with the right hand.

5. Ruling on kymograph.. 2.6
6. Münsterberg's apparatus.. 2.4
7. Free hand, on paper placed on a table........................... 4
8. Free hand, with crayon on blackboard............................ 5

In connection with No. 12, the results of a more complete experiment may be given. The subject stood before a blackboard, with his eyes closed, and, beginning to his left, drew a series of lines from left to right, one beyond another about as far as he could reach. Short spaces were left between the lines. The lines were required to be equal, and of such length that four or five could be got on the board. The average lengths in millimeters obtained for the two subjects

were, beginning at the left, as follows (the numbers in italics giving, as before, the limits of the reliability of the average) :

	1	2	3	4	5
Subject Dx.	163 ± 5	190 ± 5	203 ± 5	196 ± 4	184 ± 4
Subject W.	156 ± 4	177 ± 4	177 ± 4	167 ± 4	

It will be noticed that the longest lines are those at mid-arm. As this is also the portion of the arm's swing that feels freest and easiest, the latter fact is very probably the cause of the former. This result is only in partial accord with that of Loeb, and with his formula that of two movements, designedly equal, and made by contraction of the same muscle, that will be the longer at the beginning of which the muscle is in the less contracted condition. But I do not find that Loeb's experiments covered the cases where the initial condition of the muscle was one of almost complete absence of contraction. Make a series of movements in extension of the elbow, starting from the position of extreme flexion, or a series of movements in flexion, starting from extreme extension, and the result is that the first movements are not so long as the second and third. Here, for instance, are the results of a series of pairs of lines (flexions) on the blackboard, the first line being started from the extreme extension of the arm, and the second following after toward the median plane :

First line...........................205 ± 6
Second line ....250 ± 5

The freest and easiest, and consequently the longest, movements are the same as the most frequently used, namely those made midway between the extremes of extension and flexion.

The reason why the easier movement is underestimated is undoubtedly, as Delabarre held, that it *feels* easier, *i. e.*, because less sensation is produced by it.

Returning now to the comparative list of accuracies, I have freely to admit that I do not consider the exact values of most of the determinations to be well established. Most of them— all in which no decimals are given—were obtained from short series. They represent accordingly the accuracy attained in the various movements without preliminary practice. Although some of these results have been tested and confirmed in a general way on other subjects, the values given here were all obtained on myself. Singly, therefore, they are perhaps suggestive, but not demonstrative. Collectively, however, they seem sufficient to substantiate a few general statements :

Any difference in the way the two movements are made reduces the accuracy of reproduction.

If there are differences in two respects, the accuracy is still more reduced. Cf. Nos. 16, 24 and 25 ; Nos. 14 and 26.

The greater the difference, the less is the accuracy. Cf. Nos. 20, 21 and 22 ; Nos. 33 and 34.

The more unusual the movements, the less the accuracy. Cf. Nos. 5, 9, 11 and 20.

Notwithstanding all these sources of inaccuracy, the greatest average error obtained is less than one-third of the normal. Hence we cannot say that the ability to gauge one movement by another is *destroyed* by any of these changes in the manner of making the movement, but only that it is *impaired*.

Since a change in speed is a change in both time and force, we have in this list some instances in which all three of the proposed bases for the judgment of extent of movement, viz., force, time and position, are simultaneously changed, and yet the judgment is not entirely at sea. This must mean that the judgment has some basis other than the three proposed. There is, to be sure, the chance that *allowance* may have been made for these changes. But in order to make such an allowance in force, for instance, we must have something to rely on besides force alone. In order to make simultaneous allowances for force, time and position, we must have some fourth element to use. When we cannot gauge the movement by its force, or by its time, or by the sensations of its position, we must have some other basis. Otherwise we should either make a constant error just sufficient to compensate for the change in one of the elements, or we should be entirely at sea and have no control whatever over the movement.

Considerations like these drive us to the conclusion that *there must be a sense of the extent of movement, a sense which is not reducible to a sense either of its force or of its duration, or of its initial and terminal positions.* This sense of its extent is the real basis on which the control is based. The others may be used in certain situations to help out. This conclusion will be strengthened by an examination of each of the other proposed bases separately.

In regard to force and time, the neatest proof that neither of them is the basis for judging the extent of movement has already been furnished by the work of Fullerton and Cattell, although I do not know that their results have ever been used for this purpose. The results to which I refer are briefly summed up on page 158 of their paper, in the following words:

" Within the limits investigated the extent of movements can be judged better than the force, and the force better than the time." Now if the judgment of extent is better than that of either force or time, it certainly does not depend upon them.

In the same way, the result obtained by the same authors, that a movement can be judged with more accuracy than it can be made, disposes of another hypothesis that has been put forward, namely, that the movement is judged entirely in terms of its intention or innervation. If we judged movements entirely by the innervation, we should never know we had made a false move.

This demonstration is surely sufficient as against the views that the extent of our movements is judged by their force, time or innervation. What I have to add from my own results will be much less neat. But it may be worth while to let the evidence accumulate.

A good piece of evidence is afforded by the experiment recorded in No. 29 of the list above. Standing with eyes closed before a blackboard, the subject drew a line with a crayon, and then attempted to put a dot at each end. The error was comparatively small, being 6 per cent., as against 5 per cent. when both lines were drawn alike (No. 8).

It is easy to see that in this case the second movement is so completely unlike the first in time and in force, is made in so different a way, that its length could not have been gauged by the force or by the time of the preceding movement. A force- or time-compensating mechanism would certainly have been thrown out of gear. That the judgment might here have been based on sensations of the terminal *positions* would seem much more probable. But this was not the truth, or at any rate the whole truth, for the errors made at the two ends tended on the whole to compensate for each other. When the first dot was too far to the left the second was generally too far to the left also. From this it is clear that the extent of the second movement was gauged by the extent of the first. The accuracy of extent is in fact superior to that of position.

The fact that a rapid movement tends to exceed a slow—a result noted long ago by Camerer,[1] and exemplified above in Nos. 24 and 26—shows that the force

[1] Quoted by Vierordt, der Zeitsinn, 1868, p. 148.

of the movement does not serve as the basis for judging its extent. As the faster movement has the greater *vis viva*, it should, in compensation, have the smaller extent.

Similarly, the movements of the larger members, though possessing a greater *vis viva*, tend to be longer than the movements of the smaller members. See No. 34.

But perhaps no one would claim that we had a means of perceiving the true physical energy of our moving members, and judged the extent by that means. As far as actual feeling goes, the crooking of a finger or the rolling of the eyeballs is a more forceful act than the swinging of a leg. We perceive, of course, only the *sensations* of resistance or tension that come from the moving member, But it is further true that we are able to distinguish between the sensations that indicate resistance and those that indicate movement. Lifting a small weight a long distance is introspectively a very different thing from lifting a heavy weight a short distance. And moving a small weight rapidly is very different from moving a heavy weight slowly. It is very clear that our perceptions of the extent and of the force of a movement are quite distinct.

Now as regards the time of the movement. Although the constant error when one movement is faster than the other is in the direction of compensation, yet it is not sufficient to compensate.

Again, try the experiment of making a series of lines with eyes shut, every second one to be half as long as the others. As may be easily observed, and as Binet and Courtier[1] have scientifically established, the shorter the movement the slower it is made, and that without and even contrary to our intention. In fact, in making a series of movements such as I have described, we almost surely fall into the way of making them all of equal time, and that without observing the fact. Now, it is quite true that the shorter lines are apt to be greater than one-half. But it is distinctly not true that, made in equal times, they are equal to the whole lines. They are nearer halves than wholes. We must gauge them, therefore, in terms of something else than time.

When we consider such experiments as the last, it seems a waste of time to discuss the matter at all. Of course, we can distinguish between the time of a movement and its extent. If we judged by time alone, the difference between long and short movements would have no spatial value for us: unless, perhaps, in visual terms. But it is certain that we perceive a spatial difference between long and short movements without the use of visual elements. Again, if we judged the extent by the time alone, we should know no difference between fast and slow movements—unless, perhaps, in terms of force. But it is clear that we distinguish between speed and resistance. The perception of the time of a movement, is, if anything, more derivative than that of its extent.

We have now the further question whether the extent of the movement be not judged in terms of sensations of position. Is not one movement made equal to another by making the sensations of its beginning and of its ending like those of that other? Something has already been said in answer to this question.

[1] Sur la vitesse des mouvements graphiques; *Revue Phil.*, 1893, XXXV., 664–671.

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:[1]

> "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

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[1] 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[1] 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.

[1] Elements, II., 171.

After the masterly way in which James has thrown into re-
lief 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 ex-
tent 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 move-
ment 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 compara-
tively slight.] Five thousand, or even twelve thousand repeti-
tions 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	**1.54**	.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	**1.69**	.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	19.4	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, ½-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 1½ 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
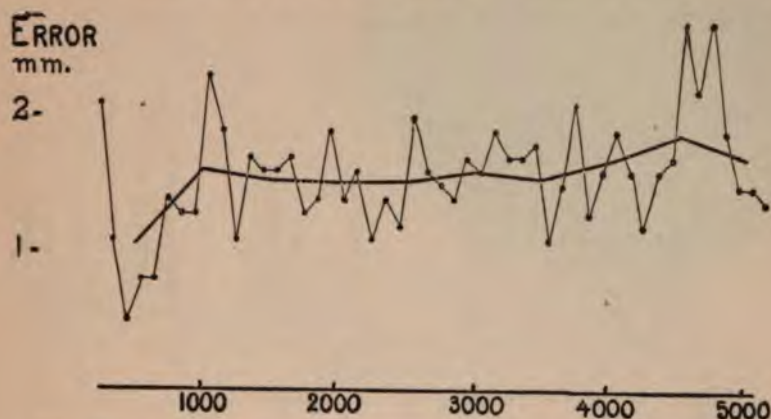
Fɪɢ. 11.—Fatigue. Lines ruled on drum, each equal to the preceding. Subject D. See Table XXII. The heavier line connects the points whose ordinates represent the average error of successive groups of 500 (next to last column in Table XXII.); the light line is similarly the curve by hundreds. The dots 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.
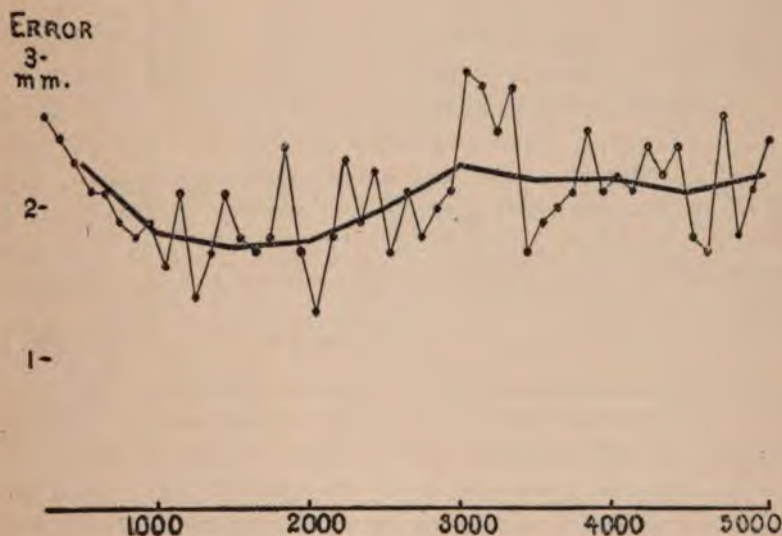
Fɪɢ. 12.—Fatigue. Lines ruled on drum, each equal to the preceding, 60 per minute. Eyes shut. Taken from Table XXIII.
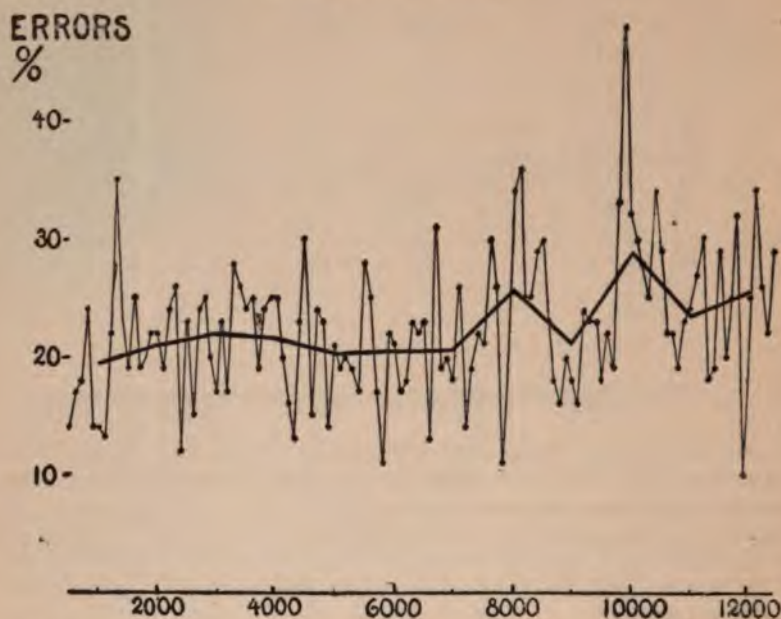
FIG. 13.—Fatigue. 'Coördinate paper experiment.' 200 hits per minute. The heavy line is plotted by thousands, the light line by hundreds. From Table XXIV.

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 ¼ 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[1] 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[2] 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[3] 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,[4] accommodation time,[5] monocular and binocular estimation of depth[6]) 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,

[1] Wundt's *Phil. Stud.*, 1886, III., 489–492.
[2] Kraepelin's *Arbeiten*, I., 156.
[3] *Arch. Ital. de Biol.*, 1894, XXII., 189–196.
[4] *Studies from the Yale Psych. Lab.*, IV., 15–19.
[5] *Ibid.*, III., 87–90.
[6] *Ibid.*, III., 68–87.

bearing the name of Henry E. McDermott,[1] 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

[1] *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	10	14	20	46	32	46	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, ¼ 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 sub-
ject to variation, and introduce variability on its own account.
A high variability may, therefore, be a symptom either of pos-
sible 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, some-
times 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 ap-
pears 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 com-
paring 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 $\frac{1}{60}$. 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	4.2	Av. for 5	8.3	Av. for 5
13.	3.3	days.	3.5	days.	7.5	days.
14.	2.5		3.5		6.8	
15.	2.4		3.3		6.6	
16.	1.0	2.54	3.1	3.52		7.30
17.	0.7		3.0		7.0	
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 investiga-
tions 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 prof-
itably used in writing, a more complete study was made of the
ease, speed and accuracy in extent and direction of this move-
ment, 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
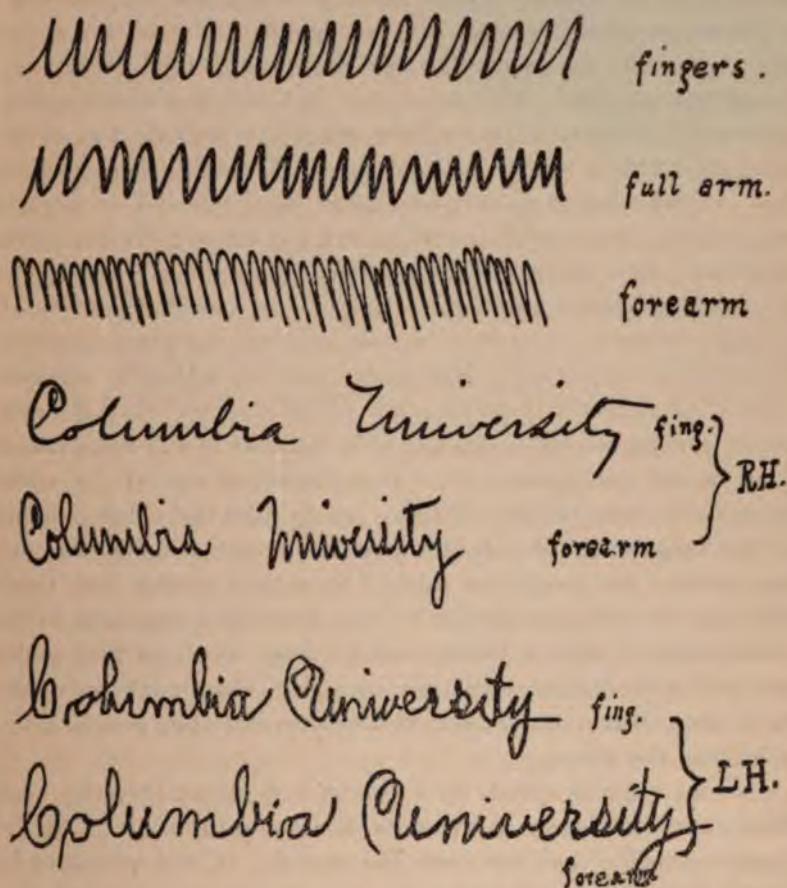

FIG. 14. Different movements in writing. 'R.H.' = right hand, 'L.H' = left hand. Reduced to $\frac{7}{10}$ original size.

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 move-
ment. 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 coör-
dination than the finger-thumb movement. The latter, as has
been shown by the researches of Duchenne[1] and of Obici,[2] 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 move-
ment has over the finger movement the advantage of being made
with comparatively large muscles. The ordinary writing move-
ment is made largely with the little muscles in the hand itself[3]
(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.[4] 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, there-
fore, 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

[1] G. B. Duchenne, Physiologie des mouvements, 1869, pp. 173–175.
[2] Recerche sulla Fisiologia della Scrittura ; *Rivista di Freniatria*, XXXII.,
pp. 625–643, 870–893.
[3] See Duchenne, loc. cit.
[4] 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.[1]

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

[1] 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 com-
fortably 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 move-
ment 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 con-
sidered.   In uniformity of direction or slant the forearm move-
ment 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 fore-
arm 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 fore-
arm 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 $\frac{1}{13}$ thereof, except in case of subject W., where it is $\frac{1}{15}$.

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.

VITA.

The candidate was born at Belchertown, Mass., October 17, 1869. He prepared for college in the Newton (Mass.) High School; was graduated from Amherst College in 1891, with the degree of A.B.; taught in the Watertown (N. Y.) High School from 1891 to 1893, and in Washburn College at Topeka, Kansas, from 1893 to 1895; studied at Harvard University from 1895 to 1897, obtaining the degrees of A.B. in 1896 and of A.M. in 1897; was assistant in physiology at the Harvard Medical School during the year 1897–8; and fellow in psychology at Columbia University during 1898–9. His teachers in psychology and allied subjects, to each of whom he owes some definite portion of his philosophical and scientific training, were Garman of Amherst; James, Royce, Bowditch, Porter, Delabarre, Santayana and Bakewell of Harvard; and Cattell, Butler, Farrand and Boas of Columbia.