RECORDS OF THE EDUCATION SOCIETY.
No. 40.

MENTAL FATIGUE

BY

GILBERT E. PHILLIPS, M.A., D.Sc.

Thesis approved for the Degree of Doctor of Science in the University of London

SYDNEY
WILLIAM APPLEGATE GULLICK, GOVERNMENT PRINTER

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PREFACE.

The investigations into the problem of General Intelligence by Spearman and his pupils have suggested new aspects to the problem of fatigue, and new methods with which to attempt its solution. The following little work is the outcome of an attempt to investigate the existence or otherwise of general fatigue, and the relation between it and the specific variety. The existence of general fatigue has been assumed in most of the experimental work on this subject, while the existence of a form of fatigue which is highly specific has been demonstrated by McDougall and Flügel.

To the Director of the University College Laboratory, Professor Spearman, I owe more than I can express. For his kindly help and friendly criticism, I cannot be too grateful. I am also indebted to Mr. J. C. Flügel for encouragement and assistance, readily given at all times. I must also thank my wife for material assistance in preparing the manuscript.

G.E.P.

Teachers' College, Sydney, Australia.

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HISTORICAL SURVEY OF RECENT WORK.

In the field of psychological inquiry in which our investigation lies, there have been many workers; but the most striking fact which assails a reader of the literature of the subject is the divergences of opinion and result which are there met with. We find contrary opinions held concerning the nature of mental fatigue, the methods and tests used to investigate it, as well as concerning the interpretation of the results obtained by these methods.

Many of the problems connected with this subject are of the utmost importance, not only to the experimental psychologist, but especially to the educationalist. Does a change of work, for example, from one subject to another really result in an increase of mental energy, or is the result purely subjective in character and due to the revival of interest and the temporary abolition of boredom? The psychologist looks at the same question from his own point of view, and asks whether mental fatigue is specific or general, and whether the fatigue incurred in performing one mental task can be transferred to another different task.

In the following article an attempt will be made to throw some light upon one or two of these problems, particularly the problem of general fatigue, which has been assumed in many investigations, and the shapes of the work-curve, and of the curve of recovery from fatigue. In this connection it may be well to remark that throughout the following pages we shall define fatigue objectively, with Thorndike, as "that diminution in efficiency which rest can cure."

Tests of Mental Fatigue.

Concerning the tests that one can adopt for detecting the presence and degree of mental fatigue, we again find great differences of opinion. We will here attempt to give a very brief summary of the principal work done with sensory and muscular tests, as some slight justification of the methods we have used in our own research. A somewhat more comprehensive account of such recent work, as bears upon the methods and aims of this investigation, will follow in the section on Mental Tests. The latter account, however, in no wise claims to be exhaustive.

A.—Sensory and Muscular Tests.

The influence of mental fatigue upon the sensibility of the skin, upon muscular efficiency, and upon the length of reaction time, have each been advocated by different writers as adequate tests of the state of fatigue.

1. *Æsthesiometric Tests.*

Griesbach, Schuyten, Binet, and Abelson, among others, advocate the use of the æsthesiometer as the most reliable test of fatigue.

Griesbach[1] finds a distinct correlation between the amount of work done and the results of his æsthesiometric tests; and Wagner,[2] Vannod,[3] Binet,[4] and Schuyten,[5] have to a greater or less extent corroborated his results. Abelson,[6] Sakaki,[7] and Michotte,[8] also obtained marked fatigue effects with this instrument. On the other side, we have Kraepelin,[9] Bolton,[10] Germann,[11] and especially Leuba.[12]

Kraepelin regards the æsthesiometer as useless as a measure of the state of fatigue, and thinks that the results obtained by it are simply the effect of suggestion.

Leuba repeated Griesbach's test on three psychologists and six women students. This work is characterised by great care and thoroughness. The tests extended over fourteen days, and the usual controls were given, and arrangements made, to prevent changes of temperature. The subjects were engaged upon hard mental work from 9 a.m. until 9.15 p.m., and were tested at regular intervals. The women subjects, however, worked only from 8.15 a.m. until 1.15 p.m. The results obtained absolutely contradict those obtained by Griesbach.

Bolton found that two hours severe mental work made no appreciable change in the æsthesiometric index.

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[1] Griesbach, H. Archiv. f. Hygiene. 1895. V. 24.

[2] Wagner, L. Unterricht und Ermüdung. Berlin. Ruther and Reichard. 1898.

[3] Vannod, T. La Fatigue intellectuelle et son influence sur la Sensibilité cutanée. Geneva. 1896.

[4] Binet, A., and Henri, V. La Fatigue intellectuelle. Paris. 1898, p. 338.

[5] Schuyten, M. C. Comment doit-on mesurer la Fatigue des Ecoliers. Arch. de Psychol. 1904.

[6] Abelson, A. R. Mental Fatigue and its measurement by the Aesthesiometer. Leipzig. Engelmann. 1908.

[7] Sakaki. Ermüdungsmessungen in vier japanischen Schulen. Inter. Archiv. f. Schulhygiene. 1905. Bd. 1.

[8] Michotte. Les Signes Regionaux. Travaux du Laboratorie de Psychologie experimentale de l'Université de Louvain. 1905.

[9] Kraepelin, E. Ueber Ermüdungsmessungen. Archiv. f. ges. Psychol. V. 1. 1903, p 9.

[10] Bolton, T. L. Ueber die Beziehungen zwischen Ermüdüng, Raumsinn der Haut und Muskelleistung. Psych. Arb. V. 4. 1902, p. 175.

[11] Germann, G. B. On the validity of the Æsthesiometric Method as a measure of Mental Fatigue. Psych. Rev. V. 6. 1899, p. 599.

[12] Leuba J. H. On the validity of the Griesbach Method of determining Fatigue Psych. Rev. V. 6. 1899, p. 573.

Germann carried out an investigation of a similar kind to that of Leuba, upon one subject, and obtained similar results. A comparison of the methods used, and of the devices employed to obviate the influence of extraneous factors, as well as of the care taken in the carrying out of the work, as shown in the two opposing groups of investigators, tend to bias one in favour of the latter group. On the other hand, the care taken in making the æsthesiometric measurements may have the effect of allowing the subject to recover somewhat from the effects of the fatigue. For increased care means an increase in the time of performing the test. Those who advocate the use of the æsthesiometer have all used tests that can be quickly applied, and this may account for the lack of uniformity in the results given by these two groups of investigators.

2. *The Ergograph.*

Mosso[1] was the first to devise and use the instrument which he has called the ergograph. The original instrument, however, has been modified and improved by Kraepelin, Lombard, and Bergstrom. Mosso found that the amount of work done with the ergograph is affected by the duration and severity of the preceding mental work, and that the general shape of the ergograms obtained is fairly constant for the individual.

Kemsies[2] tested school children at various times in the school day, and found a definite correlation between the amount of mental work done and the ergographic record.

Claviere[3] found that two hours of severe mental work causes a proportional loss in the amount of muscular work performed; but mental work of only moderate intensity has no appreciable effect on the ergograms.

On the other hand, Larguier[4] and Bolton[5] have found that mental work increases the output of muscular work, as shown by the ergograms. Bolton tested his subjects at intervals of two hours, during which they added, rested, and walked, on consecutive days. The whole experiment lasted fifteen days. The result obtained was that the ergograms exhibited no deterioration after the two hours mental or muscular work as compared with the ergograms taken after two hours rest. Instead, as increase of work was found to be the case.

Thorndike[6] used Cattell's Spring Ergograph. The subjects made from 100 to 300 contractions at a constant rate of one contraction a second, with a rest of one minute after each hundred contractions. The tests were made before and after mental work, and the results showed that the amount of mental work done had no adverse effect upon the

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[1] Mosso, A. Fatigue (trans. M. and W. B. Drummond.) New York. Putnams 1904.

[2] Kemsies, F. Arbeits Hygiene der Schule auf Grund von Ermüdungsmessungen. Berlin. 1898.

[3] Claviere, J. Le travail intellectuel dans ses rapports avec la force musculaire mesurée au dynamometre. An. Psych. V. 7. 1900, p. 206.

[4] Larguier, J. Essai de comparison sur les différentes méthods proposées pour la mesure de la fatigue intellectuelle. An. Psych. V. 5. 1898, p. 190.

[5] Bolton, T. L., and Miller, E. F. Validity of the Ergograph as a measurer of work capacity. Univ. of Nebr. Stud. 1904. V. 4. p. 79.

[6] Thorndike, E. L. Mental Fatigue. Psych. Rev. V. 7. p. 576.

output of muscular work. Thorndike concludes that "the difference between a mind before and after it has worked for six or eight hours cannot be detected by a record of physical work."

Oseretzkowsky and Kraepelin[1] found that mental work had no constant effect upon the ergographic record; sometimes the latter was increased and sometimes decreased, but that very severe mental work undoubtedly decreased the output of muscular work.

Ellis and Shipe[2] found no correlation between the results of the ergograph and the reaction time tests after more than twelve hours of mental work.

Miss Martin[3] observed no constant deterioration in muscular capacity due to the fatigue of a school day.

3. *Length of Reaction Time.*

The length of reaction time to various stimuli has also been used as a measure of fatigue.

Bettman[4] found that prolonged mental work lengthens the reaction time, but diminishes the percentage of false reactions, while muscular fatigue shortens the reaction time, but increases the percentage of false reactions.

Trautscholdt[5] also finds the reaction time a good fatigue measure and concludes that the fatigue effects are most marked in those subjects whose reaction times were fastest.

Arai[6] does not find any difference between the reaction time in the morning and in the afternoon after a hard day's mental work.

Ellis and Shipe[7] found no correlation between the reaction time and mental tests performed after some hours of mental work.

It is evident that the experimental investigation of fatigue by means of sensory and muscular tests has given no definite or constant results which would enable us to conclude that any of these methods gives an adequate measure of fatigue. Before any one test can be established as a fatigue measure, the depreciation in it due to other mental or muscular work must be proved to be highly correlated with the depreciation in the mental or muscular work itself, and, in order that this correlation may be high and positive, fatigue must be general, not specific. Until it is definitely demonstrated that fatigue is transferred from one task to another, independently of the nature of either task, as well as independently of the subjects who do the task, no one test can be considered as an adequate measure of the fatigue incurred with every kind of task. It seems to be beyond question that mental and muscular work do

[1] Osereztkowsky, A., and Kraepelin, E. Ueber die Beeinflussung der Muskelleistung durch verschiedene Arbeitsbedingungen. Psych. Arb. V. 3, 1901, p. 587.

[2] Ellis and Shipe. A Study of the Accuracy of present Methods of testing Fatigue. A. J. P. 1903. Vol. 14, p. 223.

[3] Martin, G. The Evidence of Mental Fatigue during school hours. Jour. Ex. Ped. V. 1, 1911, p. 137.

[4] Bettman, S. Ueber die Beeinflussung einfacher psychol. Vorgänge durch körporliche und geistige Arbeit. Psych. Arb. V. 1, 1894, p. 152.

[5] Trautscholdt, M. Experimentelle Untersuchungen ueber die Association der Vorstellungen. Philos. Stud. 1883, V. 1, p. 235.

[6] Arai, T. Mental Fatigue. Teachers' College, Columbia Univ. Contribs. to Ed., No. 54. New York, 1912, p. 9.

[7] Ellis and Shipe. A Study of the Accuracy of Present Methods of testing Fatigue. A. J. P., 1903, p. 223.

influence the output of muscular work, as well as the æsthesiometric index, and the length of reaction time; but it is far from being established that the loss of efficiency in the mental or muscular work is proportional to the change in any of these three tests; and, until this is established, no work in our estimation can be considered unexceptionable which measures the fatigue incurred in performing one task by its effects upon a different task.

B.—Loss of Efficiency in performing a so-called Mental Test as a Measure of Fatigue.

1. *Work of the Kraepelin School.*

The great majority of investigators have used "mental tests" of various kinds in endeavouring to discover the presence of fatigue. The experimental study of this subject by means of mental tests began with Kraepelin and his students, who were particularly concerned with the shape of the work-curve, and the interpretation of its changes.

In estimating the value of the results obtained by this school, one must consider the assumption upon which much of its work is based. Before the experiment begins, a control test is given, which determines "the general disposition of the day." If the result of the control test is below the average, it is assumed that any other test given during the day will be below the average to a corresponding extent; and, conversely, if the control test is above the average, any other test given during the day will be above the average. In other words, it is assumed that the causes, whatever they may be, which determine the relative degree of excellence of a test, remain constant throughout the day, although they may vary from day to day. There is no evidence whatever to support this assumption. Another point worthy of note about this school is that the work is done mainly on few subjects, but is very thorough. Sometimes the experimenter himself is the chief or only subject as in the work of Bettman, Weygandt, and Miesemer. The employment of the experimenter as sole subject opens the way to the subtle influence of suggestion, and it seems quite possible that some of the work at least is biassed in this way. We can form some judgment upon the influence of suggestion and interest from a comparison of the results gained by workers of this school upon the influence of doses of alcohol on mental performance, and the results of Rivers'[1] experiments. The Kraepelin school has experimented largely upon the influence of various drugs, and especially of alcohol, upon mental work, but has omitted to give control doses and to disguise the taste of the substance administered. Rivers, by disguising the liquid which contained the alcohol so that it was indistinguishable from a control liquid which contained no alcohol, obtained results which directly contradicted those obtained in Kraepelin's laboratory.

Rivers ascribes the results obtained by other investigators to the lack of control doses and control days. When whisky was administered without any disguise, there was a marked increase of work done, but when the alcohol was disguised and control doses given, there was no such increase.

Oehrn[2] in 1889 seems to have been the first investigator to make an attempt to analyse and interpret the work-curve. His tests included

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[1] Rivers, W. H. R. Influence of Alcohol and other Drugs on Fatigue. London. 1908.

[2] Oehrn, A. Experimentelle Studien fur Individualpsychologie. Psych. Arb. V. 1, 1894, p. 92.

counting and picking out letters, memory for nonsense syllables, addition, and motor ability judged by reading and writing. The work was divided into five-minute periods. His main conclusions were that the predominant factors which influence the work-curve are practice and fatigue, and that the curve of work at first rises and then gradually falls.

Bettman[1] in 1895 followed with an investigation which sought to find out whether there were qualitative differences in the performance of mental work after prolonged mental and muscular work.

The fatigue-producing work consisted of two hours, 6 a.m. to 8 a.m., hard walking, or one hour, 7 a.m. to 8 a.m., addition. The tests of fatigue were, in the first experiment, fifty choice reactions, fifty word reactions, learning by heart, and addition; and in the second experiment, 300 choice reactions.

On the first day the subject was tested after doing addition for one hour. The second day was a control day on which no fatiguing work was done, but the subjects were tested at the usual time. On the third day the tests were performed after two hours hard walking. This series was repeated four times, or twelve days in all.

In the second experiment, the fatiguing-producing work was the same as before, but the test was 300 choice reactions. This was repeated three times, or nine days in all.

For the first experiment Bettman himself was the sole subject, and for the second only one other subject was used. This paucity of subjects is the least satisfactory part of the whole work. Bettman himself sees the danger of suggestion, and to obviate it, the results were not calculated until the whole experiment was over. This would do away with any suggestive influence caused by a knowledge of the results, but would in no wise counteract the suggestive influence of the subject's own convictions and opinions.

The results obtained were as follows: Both mental and muscular work had a decided effect upon the length of reaction time. Mental work lengthened the time considerably, but muscular work shortened it. On the other hand, the number of false reactions was not affected to any great extent by mental work, while the number was greatly increased by muscular work.

In the memorising test Bettman found a decided adverse effect upon the work days as compared with the rest days, but here the effects of practice were so large as to make it very difficult to estimate the fatigue effect. However, it can be said that the practice effect was much less on those days when fatiguing work was done than on the rest days. Both adding and walking were found to have an adverse effect upon the addition test.

Bettman concludes that—

1. The mental work involved in one hours' addition as well as the muscular work involved in two hours' walking causes a reduction of efficiency.
2. The decrease of mental capacity after both mental and muscular work is shown in the lengthening of the times necessary for memorising, and for choice and word reactions, in the weakening

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[1] Bettman, S. Ueber die Beeinflussung einfacher psychol. Vorgange durch körporliche und geistige Arbeit. Psych Arb. V. 1, 1894, p. 152.

of memory, and in the reduction of the practice effect. The decrease in efficiency is more pronounced after muscular than after mental work.

3. After mental work, we can notice a decrease of motor ability, but after bodily work, the motor centres are excited, and we find a proneness for these motor centres to discharge before their time.

In 1895 Amberg[1] investigated the effect upon mental work of rest periods of different lengths. He found that the influence of the rest period depended upon the duration of the mental work which preceded it. A rest of five minutes after five minutes work was not so favourable as after fifteen minutes work. This seems to show that the "warming-up" effect due to five minutes work is greatly reduced by a rest of five minutes, while the fatigue effect is not reduced to the same extent. The warming-up due to fifteen minutes work is either largely retained or else the fatigue due to the same cause is diminished.

Amberg also found that the effect of rest was also dependent upon the kind of mental work done.

The number of subjects was very small, in some cases only one, therefore the results cannot be considered as general.

Rivers and Kraepelin[2] in 1896 investigated the effects of "warming-up," spurts, fatigue, and practice, by inserting rests of varying lengths between two periods of work. In one series of experiments, half an hour's rest was given after half an hour's work, and, in the second, an hour's rest was inserted between two half-hours of work. On some days there were four work periods with their intervening rests, and, on alternate days, only one work period. When the amount of work done in the last half-hour of one period was compared with that done in the first half-hour of the next period, it was found that in the series with the half-hour rests there was a greater gain than in the series with the longer rest, showing that a rest of an hour causes a greater loss of "warming-up" effect than the rest of half an hour.

Also the rest of half an hour was approximately sufficient to abolish the fatigue due to half an hour's addition, but was not sufficient to abolish the effects of an hour's addition.

Weygandt[3] in 1897 studied the effects of changes of work upon a subject already fatigued by mental work.

The subjects were the experimenter himself, and also to a certain extent five students. The subject worked at one kind of work for half an hour, then worked for the same length of time at another kind, and, finally, for fifteen minutes at the original work. On some days the whole seventy-five minutes was occupied by the same kind of work, while on further days the second half was replaced by rest.

Weygandt concludes that changes of work are not always favourable, and their effects are almost always small. The relative difficulty of the work is the sole determinant of the result. Work interrupted by more

[1] Amberg, E. Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit. Psych. Arb. V. 1, 1895, p. 300.

[2] Rivers, W. H. R., and Kraepelin, E. Ueber Ermüdung und Erholung. Psych. Arb. Vol. 1, 1896, p. 627.

[3] Weygandt, W. Ueber den Einfluss des Arbeitswechsels auf forlaufende geistige Arbeit. Psych. Arb. Vol. 2, 1897, p. 118.

difficult work shows a decline, and work interrupted by easier work shows an increase.

In 1900 Lindley[1] published an account of his research on fatigue. His investigation sought to determine the factors affecting the work-curve, such as practice, fatigue, and "warming-up." The subjects were three in number, including the experimenter; and the work chosen, addition. The work period was one hour each day, divided into two half-hour periods.

On the first day, no rest was given between these two periods; on the second day five minutes rest was given; on the third fifteen minutes; on the fourth thirty minutes; and on the fifth sixty minutes. This series was repeated five times, and then another day was added, on which no rests were given. Between each series of five days, one day, on which no work was done, was interposed. Thus there were twenty-six days on which work was done.

Lindley concluded that rest causes a loss of "warming-up," fatigue, and also practice. The most advantageous rest varies with the individual, and lies between fifteen and sixty minutes. Practice-gain correlates with fatiguability; and inversely with persistency of practice effect. Almost the whole of the practice effect disappears after a rest of twenty-four hours.

In 1902 Hylan and Kraepelin[2] gave the result of their investigation. Their aim was to ascertain the effects of practice and fatigue, and, instead of the thirty- and sixty-minute periods of Amberg and Lindley, used five-minute periods. The task was to add one-place numbers. Four work-periods were given each day; the first and second, and third and fourth being separated by rests varying from zero to twenty minutes, while thirty minutes separated the second and third.

The subjects were three in number, including the experimenters.

It was found that the amount of practice and fatigue incurred with such a short period as five minutes was quite appreciable, and that the practice effect outlasted the effects of fatigue. Also, some rests had a favourable influence on succeeding work, while the effect of others was unfavourable. The most advantageous rest was five minutes. Rests a little longer than this were not so advantageous, but if the rests were made much longer, *i.e.*, twenty to thirty minutes, they again became favourable.

Miesemer's[3] aim was to study the effect of mental and muscular work upon various performances, *e.g.*, apprehension, memory, and writing figures.

Here again the experimenter was the only subject. The fatigue-producing work consisted of walking or adding for one hour. The tests of fatigue were Finzi's test for apprehension, the same test with an interval of thirty seconds to test memory, and writing figures. The fatigue-producing work was inserted between two series of tests.

He finds that muscular and especially intellectual work injures apprehension and memory, and that muscular work increases the pressure,

[1] Lindley, E. H. Ueber Arbeit und Ruhe. Psych Arb. Vol. 3, 1900, p. 482.

[2] Hylan and Kraepelin. Ueber die Wirking kurzer Arbeitszeiten. Psych. Arb. Vol. 4, 1902, p. 454.

[3] Miesemer. Ueber psychische Wirkungen körperlicher und geistiger Arbeit. Psych. Arb. Vol. 4, 1902, p. 375.

extent, duration, and speed of writing figures, while mental work has the opposite effect. Bettman[1] found that the muscular work had a more injurious effect, but the amount of fatiguing work done in the two cases is not the same.

The experimenter being the sole subject, there is the danger of suggestion, and his figures also lend colour to the suspicion.

Miesemer has placed his results in three columns. The first column gives the results of the test on the muscular-work days, the second gives the results when no fatiguing work was done, and the third the results when the fatigue-producing work was mental in character. The results of the tests done *before* any fatiguing work are almost always the inverse to what might be expected *after* the fatiguing work. This is noticeable in the apprehension test, the memory test, and particularly so in the pressure of writing. The details of the correct answers given in the apprehension test *before* the mental or muscular work are as follow :—

	Muscular work.	Rest.	Mental work.
	33	34	33
	33	39	43
	44	49	51
	45	44	47
	44	44	53
	51	50	57
Average ...	41·7	43·3	47·3

It is remarkable that the scores on the mental-work days should be, in every case except the first, the highest of the three. It should be remembered here that this school assumes that these tests give the general disposition of the day. But why should it so happen that on the mental-work days the general disposition is at a higher level than on other days ? Of course it is obvious that the greater the scores before the fatigue-producing work, the less will be, by comparison, the scores after the work, and *vice versa*.

Again, take the figures for memory :—

Muscular work.	Rest.	Mental work.
24	19	21
33	27	21
26	25	30
39	31	34
28	31	28
30	33	38
180	166	172

Here again we find a tendency for the scores on the rest days to be lower than on the work days.

[1]Bettman, S. Ueber die Beeinflussung einfacher psychol. Vorgange durch körporliche und geistige Arbeit. Psych. Arb. Vol. 1, 1894, p. 152.

If we take the figures for pressure of writing, we find this characteristic still more marked :—

Muscular work.	Rest.	Mental work.
3,810	4,110	5,010
3,290	3,520	6,780
7,140	7,150	6,170
3,690	6,220	5,620
4,280	4,880	8,670
4,460	3,700	5,540
4,180	5,180	7,470
30,850	34,760	45,260

The author himself notices this last result. It is obvious that when the mental-work scores have a general tendency, before any fatiguing work is done, to be higher than the rest-day scores, even if the scores after the fatiguing work were normal, there would be a general loss between the first testing and the second, and consequently an apparent general fatigue effect.

Heüman[1] also has, in 1904, made a study of the effects of pauses and work-periods of varying lengths. The work-periods used were, in length, one minute, five minutes, ten minutes, fifteen minutes, thirty minutes, and sixty minutes respectively. After each work-period rests were given, continuing for one minute, five minutes, or ten minutes, on different days. Following the rest a further work-period of five minutes was given. The task was addition of one-place numbers. The subjects were six in number, including the experimenter.

The effect of a pause of one minute was not always favourable. After sixty minutes work it was always most favourable, but after five, ten, and thirty minutes work it was sometimes unfavourable. The effect of fifteen minutes rest had a similar effect. It, too, was most favourable after sixty minutes work. Fifteen minutes rest was almost always advantageous, especially after sixty minutes work.

Heüman's general conclusion concerning this part of his work was that the favourable effect of a pause is greater the longer it is, and the longer the previous work.

2. *Work done outside Kraepelin's Laboratory.*

Outside the Kraepelin laboratory, we find of late years an increasing interest in the subject of mental fatigue, and a corresponding increase in the number of researches upon this question. Thorndike[2], in 1900, undertook an investigation to see which of the two theories of mental fatigue, the mechanical theory or the "by-product" theory, agreed the better with the facts gained by experimentation. The mechanical theory states that manual work causes a diminution of mental efficiency, and that this diminution is in proportion to the amount of mental work.

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[1] Heüman. Ueber die Beziehungen zwischen Arbeitsdauer und Pausenwirkung Psych. Arb. Vol. 4, 1904, p. 538.

[2] Thorndike, E. L. Mental Fatigue. I. Psych. Rev. Vol. 7, 1900, p. 466.

done. The "by-product" theory is that mental fatigue is not a simple but a complex phenomenon. The mind in its activity has various by-products, the accumulation of which, when sufficiently large, causes a decrease in mental efficiency. Therefore, if the latter theory be true, fatigue ought to appear suddenly. Thorndike's results showed that the mental fatigue is not in any regular proportion to the work done, and frequently there is no loss of efficiency after seven or eight hours of mental work. Introspection did not discover any pure feeling of mental fatigue. His results distinctly favour the "by-product" theory.

Unfortunately, Thorndike's procedure leaves many loopholes by which errors can come in. He does not take into account the possibility of a daily rhythm, or whether the subjects were morning or afternoon workers. Neither was account taken of the practice effect except in the first half of the work. Thorndike himself notes this defect. In any work such as this it is imperative that either the plan of the experiment should be so arranged as to obviate the practice-effect altogether, in which case the subjects must be in an advanced state of practice, so that its curve is linear or approximately linear, or else the subjects must be at the limit of practice, in which case there will be no practice-effect for which to allow.

Thorndike's[1] next research was made on school children. Its aim was to determine the influence of school work upon mental efficiency. He gives a list of some of the dangers to be guarded against, viz., the measurement of the willingness and not the ability to work, the effects of practice, emotional effects, and the influence of the experimenter's instructions. He then gives his methods of eliminating these effects. The instructions to the children were not given by the regular teachers, but by himself and an assistant, neither of whom the children knew. Also the instructions were given in a set form of words, and the same test was never given to the same child twice. The children were divided into sets, approximately equal in numbers and ability. The latter was judged by their performance in a preliminary test. One set would do a test early, and the other set would do the same test late in the school day. Thus any practice effect was therefore avoided. The tests included multiplication, correcting errors in spelling, writing figures, and counting letters exposed on a chart for a few seconds. When the tests done early in the day were compared with those done late, and no deterioration was observed, Thorndike concludes that the mental ability of school children does not diminish as the day goes on.

The whole investigation is too gross to measure adequately anything so delicate as mental fatigue. One could hardly expect any other result than the one obtained. The problem of fatigue calls for the greatest refinement of methods, and the elimination of every source of error possible. It would be a remarkable coincidence if the two sets of children were equal in any one of the tests, and even if such a division were possible it would be no criterion that the two groups would be equal with respect to any other of the tests.

Again, the substituting of a stranger for the usual teacher of the class will not guarantee that the subject's willingness to work will be at a maximum. The emotional effect of a stranger giving the instructions will vary every time that person visits the class, and, indeed, it is a very

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[1] Thorndike, E. L. Mental Fatigue. 2. Psych. Rev. Vol. 7, 1900, p. 547.

debatable point whether the emotional effect caused by the advent of a stranger would not affect the work done in a very arbitrary manner. Wimms[1] says in the conclusion of his article, speaking of the earnestness of his pupils: "With *their own teachers* such children form ideal subjects, and enter into the spirit of the experiment with a zest that is surprising. But it is exceedingly doubtful whether any stranger visiting a school purely for experimental purposes can achieve results with the regularity and utility of those given in this paper. Even the old school children are exceedingly sensitive to variations of the normal routine, and even the slightest change has been found by the author to produce striking and otherwise unaccountable abnormalities."

The second series of experiments dealt with the fatigue of a particular function—specific fatigue. The tests used were cancellation, estimating the areas of small parallelograms, memorising numbers, and correcting examination papers.

The results showed no depreciation in the work at the end of the tests, which lasted from three to eight hours. Evidently the effects of practice outweighed the effects of fatigue.

The same author[2] has in 1912 also investigated the work-curve. He severely criticises the Kraepelin school regarding the shape of the work-curve, and the interpretation of its changes. He concludes that "two hours or less of continuous exercise of a function at maximum efficiency, so far as the worker can make it so, produce a temporary negative effect, curable by rest, of not over 10 per cent. Fluctuations of considerable amount occur in any one work-period for any one subject, but except for a rise in efficiency of approximately 4 per cent. near the end, when the date of the end is known, no regularity in them has been proved for any one subject in any one sort of work, much less for any one subject in all sorts of work or for all subjects in any one sort of work." (p. 187).

We shall have to return to this article later, when we are discussing the results of our own research.

In 1914 Thorndike[3] published a further article dealing with this subject. The work here consisted in writing lines to complete couplets whose first lines were given, taken from Pope and Byron. There were 108 lines divided into nine sets of twelve lines each, and the subjects were divided into nine groups. Eight sets were done without rest in the afternoon or evening of a given day, and the ninth set after rest in the morning or afternoon of the following day. Each group commenced with a different set, so that the whole nine sets were done by different groups on the rest day. The work of each group was judged as to its quality by at least four different judges and the results averaged.

Thorndike draws the conclusion that the speed improves, while the quality of the work remains about the same.

No allowance was made for the effects of practice, and, as we see, it completely masks the fatigue effect, if indeed there was any such effect.

In 1903 Ellis and Shipe[4] published the results of their investigation into the accuracy of the different methods used for testing fatigue. The

[1] Wimms, J. H. The Relative Effects of Fatigue and Practice produced by Different Kinds of Mental Work. Brit. Jour. Psych. Vol. 2, 1907, p. 153.

[2] Thorndike, E. L. The Curve of Work. Psych. Rev. 1912, p. 165.

[3] Thorndike, E. L. Fatigue in a Complex Function. Psych. Rev. 1914, p. 402.

[4] Ellis and Shipe. A Study of the Accuracy of Present Methods of Testing Fatigue. A. J. P. 1903, Vol. 14, p. 223.

particular tests used were reaction time, ergograph, addition, cubing numbers, memory for nonsense syllables, and the Ebbinghaus combination test. The subjects were, in the first investigation, a college professor and five advanced students, and in the second, ten school children.

In the first investigation, the tests used were the ergograph and reaction time, and the subjects were tested at 8·30 a.m. and then about 12·30 p.m., after hard mental work. The results showed no agreement between the two tests. In the second investigation, reaction time, addition, cubing, and nonsense syllables were the tests used. The subjects were tested at 8·30 a.m., 12·30 p.m., and 5·30 p.m. each day for four days, during which they were engaged upon hard mental work. Again the results showed no agreement. From this the writers conclude that the methods used were unreliable. But this is not the only inference that can be drawn.

Granting that their results are perfectly reliable, which is not likely to be the case, as no account has been taken of the practice effect, nor was any allowance made for fluctuations due to time of day, it does not necessarily follow that the methods used are unreliable. The different results may each measure the fatigue in one particular function. The writers assume all through their article that fatigue is perfectly general and is therefore transferred from one activity to every other activity in an equal degree. But it has not been proved so. Certainly it has been generally assumed to be so, and it has been expressly stated by Kraepelin that fatigue is general. Is it not possible for mental fatigue to be strictly specific? If so, one would not expect the results to agree. Each activity would be affected in proportion to the amount of exercise it had had during the mental work which had induced the fatigue. Or, again, it may still more probably be the case that fatigue is both general and specific, and, in this case, the results would not necessarily agree.

Squire[1] has suggested a new method of measuring fatigue. The test suggested is that of tapping a pattern. A series of numbers is committed to memory, and then afterwards tapped with the finger, which is connected to a tambour and smoked drum. The errors can afterwards be counted. The finger tapping the pattern was so arranged that the muscles used in tapping would be those used with the ergograph. The amount of muscular fatigue was afterwards estimated by the tracing taken before and after the tapping test, and found to be negligible.

The subjects were three in number, and the pattern was occasionally altered to avoid the tapping becoming automatic. The subject who worked longest worked for one and a half hours.

No decrease in the ergograms after mental work was noticed, nor was the rate of tapping decreased, but there was a considerable increase in the number of errors. The writer recognises both general and specific fatigue, and, in discussing the work of Thorndike, says that the latter errs in failing to recognise specific fatigue. Squire does not give us the actual figures upon which the conclusions are based, so that much of their value for others is lost. Neither is there given the method of determining the number of errors. The test itself seems to be a useful

[1] Squire, C. R. Fatigue; Suggestions for a new Method of Investigation. Psych. Rev. 1903, p. 248.

one for determining the specific fatigue induced by working with it for a considerable time. Yoakum[1] has made use of it in his research.

Pillsbury[2] has investigated the use of the attention waves as a method of measuring fatigue. The method used was that of fixating a spot upon a just perceptible grey ring on a swiftly rotating disc. The times during which the ring was (*a*) visible and (*b*) invisible were taken. With five subjects the waves showed regular variations for different periods of the day. Towards evening, the ratio of the time during which the ring was visible, to that time during which it was invisible, became smaller.

Only one subject out of six had totally irregular fluctuations. It was also noticeable that on certain days, when little mental work was done, the ratio of the time visible to the time invisible was much greater than on those days when some hours of arduous mental work were done.

In 1904 Seashore[3] published an article in which he discusses some errors which have crept into psychological thought regarding fatigue, and then proceeds to point the lines along which progress can be made.

The first error he mentions is that of regarding fatigue as a concrete homogeneous quantity of something which may be measured in terms of fluctuations of efficiency of some particular work. He emphasises the multiplicity of factors which contribute to the fatigue effect. Few psychologists would venture to define fatigue, at this time, in any other than in an objective way, yet Seashore holds that this error has crept into most investigations up to date. Secondly, he deplores as an error the idea that fatigue is general, and blames Kraepelin for its prevalence. Kraepelin says: "Fatigue through mental work, is, as far as we know, a general fatigue. As especially Weygandt's study on the effect of change in mental work has shown, the fatigue through a particular activity also reduces the capacity. Only the difficulty and not the kind of mental work is significant for the general extent of fatigue."[4]

Seashore holds that the mere fact that change of work is not always beneficial does not prove that fatigue is general. It must first be proved that the changed work has nothing in common with the work substituted for it. Moreover, the work done by the Kraepelin[1] school has been made on so small a number of subjects that one must hesitate before accepting their results as general. The whole question of general and specific fatigue, or the existence of both, is far from being settled. The third error is widespread, and has vitiated a great deal of the work already done in this field. It is the hope of retaining results of wide practical value by gross measurements without a preliminary critique of method. More time and energy have been wasted under the delusion of this error than, perhaps, on any other question in psychology.

Seashore then gives a list of the lines along which we can expect to make progress:—

1. Development of the methods of measuring fatigue.

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[1] Yoakum, C. S. An Experimental Study of Fatigue. Psych. Rev. Mon. Supp Vol. 11, 1909.

[2] Pillsbury, W. B. The Attention Waves as a Means of measuring Fatigue. A. J. P. Vol. 14, 1903, p. 277.

[3] Seashore, C. E. The Experimental Study of Mental Fatigue. Psych. Bull. 1904, p. 97.

[4] Kraepelin, E. Die Arbeitscurve. Phil. Stud. 1902, p. 479 (quoted by Seashore).

2. Analysis of the work-curve under controllable conditions.
3. Detailed examination of such factors as are necessarily interrelated with fatigue.
4. Detailed examination of qualitative, intensive, extensive, and temporal attributes of mental work.
5. Correlation of the psychological with the underlying factors, *e.g.*, such as the physical, chemical, histological, and electrical phenomena.
6. Analysis of individual fatigue resistance.

In 1906 Wright[1] published the results of his work upon the effect of incentives upon work and fatigue. For testing fatigue and the output of work, Cattell's Spring Ergograph was used. His conclusions were that (1) the subject accomplishes more work when working under the mental stimulus of having a set task to be performed; (2) a known impossibility to accomplish the required conditions tends to decrease the subject's total results; and (3) incentives lessen the feeling of fatigue.

In 1907 Wimms[2] gave an account of the relations discernible between output of work, improvability, retentivity of practice, and fatiguability. The work was carried out upon twelve boys, 14 to 16 years of age. Rewards were given for consistency. Wimms finds no constant relation between improvability and retentivity of practice, although there was found a slight tendency towards an inverse relationship between improvability and fatiguability. A very considerable inverse relationship was apparent throughout between retentivity of practice and fatiguability.

In 1909 Yoakum[3] published his monograph on fatigue. In his investigation he used the pattern-tapping test as proposed and described by Squire, with a slight modification. Instead of tapping with the finger, a lip-key was used. The subjects were nine adults—four men and five women—and the tests were of varying lengths, the longest being twenty-seven minutes.

He concludes that "the work of this paper suggests that the rhythm of certain highly automatic processes modifies materially the character of a short and circumscribed experiment."

In 1911, Winch[4] published the results of his inquiry concerning mental fatigue in school children. The subjects consisted of a class of 54 boys, one of 54 infants, one of 52 girls, and finally, a senior class of 48 boys, the highest class in an elementary school. The procedure followed closely resembled that of Thorndike. Each class was divided into two groups, whose average ability was equal. The test of fatigue was a number of "problems" in arithmetic. On the results of five of these tests on five different days was based the division of each class into two groups, so that each pair should be approximately equal in ability. Then, on five different days, one group was given a number of these problems to solve in the morning, while the other group had exactly the

[1] Wright, W. R. Some Effects of Incentives on Work and Fatigue. Psych. Rev. 1906, p. 23.

[2] Wimms, J. H. The Relative Effects of Fatigue and Practice produced by Different Kinds of Mental Work. Brit. Jour. Psych. Vol. 2, 1907, p. 153.

[3] Yoakum, C. S. An Experimental Study of Fatigue. Psych. Rev. Mon. Supp. Vol. 11, 1909.

[4] Winch, W. H. Mental Fatigue in Day School Children. Brit. Jour. Psych. Vol. 4, 1911, p. 315.

same work to do late in the afternoon. The results showed that in the class of boys, the morning group had a superiority of about 7 per cent. over the afternoon group. Tested in the same way, the class in the Infant School showed a difference of 12 per cent. in favour of the morning workers. The results from the class of girls was similar. Finally the senior boys were tested. Here the difference between morning and afternoon workers was not nearly so large, only 3 per cent.

Winch concludes that "mental work involving reasoning of this kind appears to be less and less affected by the fatigue of the school day as the children rise in age and capacity. For the older and more proficient children, the fatigue effects are very small indeed."

The whole experiment depended upon the subjects' willingness to work, and it may be questioned whether, in the tests done, especially with the younger children, the willingness to work was always constant. Do the tests indicate the differences between the ability to work in the morning and the ability to work late in the afternoon; or do they merely show the differences in the willingness to work during the two tests? Again, it may be that the afternoon hours are not so favourable for mental work as the morning ones, even if no school work is done. It seems possible that the depreciation in the quality of the work may be due to the accumulated boredom, and not to mental fatigue at all, as no incentives were offered to encourage the children to do their best under all subjective conditions. It is only to be expected that the younger the child, the more prone he will be to give way to the sensations of boredom.

An attempt was made in 1911, by Norris, Twiss, and Washburn,[1] to estimate the effects of fatigue upon the affective value of colours. A series of ninety coloured squares was presented to thirty-five subjects, who, after looking at each colour in turn for ten seconds, gave their judgments upon its affective value. The presentation of the series took three-quarters of an hour to an hour, and then the first six colours were presented again, and the fatigue effect was gauged by the difference in the affective values of these six colours, which had been presented twice. The values were given numerically, seven being very pleasant, and one very unpleasant. With seven subjects the affective values rose, with three they remained constant, and with the remaining twenty-three the values decreased. This loss in affective value was found to be much less marked in the case of saturated colours than in that of shades and tints.

One of the most painstaking researches done outside the Kraepelin laboratory has been that of Arai[2], published in 1912. This writer has studied the influence of fatigue on certain physiological processes, on the feeling of fatigue, and upon intellectual efficiency.

The first part of the research deals with the fatigue incurred in the continued exercise of a special mental function, and the amount of this fatigue which is transferred to other functions. The mental function tested was that used in multiplying mentally pairs of four-figure numbers. The only subject was the writer herself, who had daily practice in this work until the practice effect was very slight. To increase the difficulty

[1] Norris, Twiss, and Washburn. An Effect of Fatigue on Judgments of the Affective Value of Colours. A. J. P. Vol. 22, 1911, p. 112.

[2] Arai, T. Mental Fatigue. Teachers' College, Columbia Univ., Contribs. to Ed. No. 54. New York. 1912.

of the work the figures to be multiplied were first memorised, and then the actual multiplication carried on with closed eyes. The time for the multiplication of each pair of numbers was taken, and also the number of errors calculated. The work was carried out on four successive days, and lasted from 11 a.m. to 11 p.m. The writer does not say how the errors were calculated. It seems as if it must necessarily be the number of incorrect digits in the answer. This is not altogether satisfactory, as one mistake in working might make two figures in the answer incorrect, and two errors in working might result in only one incorrect figure in the answer. In dealing with these errors, the writer followed Thorndike, and added ten seconds to the total time for each wrong digit. This is to be regretted, as the errors are not evenly distributed throughout the records. One would like to know by what method of calculation either Thorndike or the writer concluded that a certain number of seconds added to the time of multiplying two numbers was equivalent to an error in that multiplication. In any test such as this there are two things to be considered—speed and accuracy; and neither has been proved to be a function of the other. Decrease in the amount of work done in a given time or increase in the time for doing a given work means decrease of speed; and increase in the number of errors means decrease of accuracy, and neither is reducible to the other.

The curve of work shows a continuous increase of fatigue, and there is no warming-up effect noticeable. The number of mistakes increases with the length of the work, but accuracy does not seem to decrease in proportion to the length of time worked (p. 9).

To investigate the amount of fatigue transferred from one kind of mental activity to another the time was taken to memorise German words, after some hours of mental multiplication, and the table following gives the results for the writer herself (p. 106) :—

TABLE XXVIII.

Date.	Hours of Mental Multiplication.	Number of Examples done.	Time taken for Memorising Forty German Words before and after Multiplication.		Coefficient of Fatigue.
			Before.	After.	
			m. s.	m. s.	
25 Feb. ...	4	24	10 10	10 40	5·0
26 „ ...	5	30	8 45	10 20	18·1
28 „ ...	9	51	10 30	11 40	11·1
3 Mar.... ...	12	67	6 45	11 20	72·9
4 „	12	67	8 15	7 35	− 8·1
5 „	12	67	5 40	8 40	52·9
6 „	12	67	7 40	7 30	− 2·2
Average ...	9·2	53	...	...	21·4

It will be noticed that there is a considerable practice effect; and also, the two days, 3rd March and 5th March, which show the greatest fatigue coefficients, are those on which the times for memorising forty German words *before* the multiplication fatigue work were considerably lower than those on other days. It also seems significant that 4th March and 6th March, on which twelve hours multiplication were done, actually show a negative fatigue coefficient; nor were the times *before* multiplication on these days any higher than usual.

With such large variations as are shown in this table there can be no certain deductions drawn concerning the transferability of fatigue from one mental process to another.

On pages 107 and 108 further tables are given of a similar research carried out upon eleven subjects. The tests used were memory for nonsense syllables, "opposites" tests, and addition. These tests were given before and after two hours of mental multiplication. Two of the tables, viz., those for addition and opposites, show a negative average fatigue coefficient, while in the memorising nonsense syllables six subjects showed a decrease after the multiplication, three showed an increase, and two did not alter. No allowance was made for any practice effect in these tables, nor for the influence of the time of day. Arai has not calculated the probable error of the average coefficient of fatigue. The formula for this probable error is $\frac{\cdot 6745\sigma}{\sqrt{n}}$. The values of these probable errors are shown below for the two tables, which give a positive fatigue coefficient:—

Test.	Fatigue Cofficient.	Probable Error.
Memorising German words ...	21·4	7·12
Memorising nonsense syllables ...	11·7[1]	4·1

It will be seen that the former of these coefficients is just three times the size of its probable error, and the latter is less than three times its probable error. Therefore neither result supplies indubitable evidence of the transference of fatigue from one mental function to another.

The purpose of the second experiment was to investigate the influence of general mental work upon special mental and physiological functions. The writer herself was the sole subject, and the investigation continued for seventy-eight days. The subject was tested morning and evening in memorising the German equivalents of English words, and in mental multiplication. The morning test was taken at 10 a.m. before any mental work was done, while the evening test was done after the day's work. On page 110 is given a table showing the fatigue coefficients in the case of memorising German words, according to the hours of mental work done on the different days. The table shows a gradual rise in this coefficient, which amounts to 49 8 per cent. after 9–10 hours of work.

On the next page is given a similar table for the mental multiplication. In this case the increase of fatigue is not parallel to the increase in the number of hours worked. The rise in the fatigue coefficient does not become definitely marked until 7–10 hours of mental work are done.

The results obtained here, even if unexceptionable, do not prove that fatigue may be transferred from one mental function to another having nothing in common with it. Arai has given us no information concerning the work done between the morning test and the evening test. We can draw no conclusions as to whether it might or might not have anything

[1] Arai gives 13·5 for this average, but it would seem to be 11·7 calculated from the figures given on p. 107.

in common with the two tests. If there were common factors, the transferred fatigue would be a case of specific fatigue due to the common factor in both.

Arai has also studied the relations of fatigue in different mental functions having the same cause by means of the correlation between the fatigue coefficients. She finds a correlation of —·19, in the case of herself between the fatigue incurred in multiplication and the fatigue transferred from it to the function of memorising. With ten subjects the correlation coefficient is ·21. She finds no correlation between the fatigue incurred by multiplication and the fatigue transferred from it to the "opposites" test, and that between multiplication and adding to be ·088.

Concerning the fatigue transferred to two functions, from the same fatiguing work, she finds a correlation of ·13 between the decrease in ability to memorise and that in ability to multiply mentally. In the group of ten subjects she finds a correlation of ·77 between the fatigue in the "opposites" test and the fatigue in addition due to the same fatiguing work. However, the probable errors of these correlation coefficients are not given. We calculate them to be as follows:—

Fatigue Transferred.	Correlation Coefficient.	Probable Error.	Number of Subjects.
From multiplication to memorising ...	– ·19	·25	1
From multiplication to memorising ...	·21	·20	10
From multiplication to "opposites" ...	·003	·22	10
From multiplication to addition	·088	·22	10
From general work to memorising and multiplication	·13	·095	1
From general work to addition and "opposites"	·77	·09	9

It will be seen that the only coefficient that is at all significant is that between the fatigue coefficients for the addition and "opposites" tests induced by general mental work. The number of subjects who did both these tests was only nine, and the work was done only once.

As we mentioned before, Arai has given us no clue to enable us to judge the kind of mental work, which is designated "general work." If the general work was in nature akin to the work done in the tests, then the result can be explained as the effect of specific fatigue. However, since none of the subjects were near the limit of practice, the correlation cannot be rightly ascribed to the equal transfer of fatigue. It might, with more reason, be ascribed to the transfer of practice effect, since in the results given in Tables XXX and XXXI (pages 107 and 108) practice is more marked than fatigue. In fact, in both tables the average fatigue coefficient is negative. In Table XXX five of the nine subjects, who did both tests, show a negative fatigue coefficient, *i.e.*, the practice gain outweighed the effects of fatigue, due to two hours multiplication, and in Table XXXI, four have a negative coefficient of fatigue, while two others neither increase nor decrease. It seems to us absolutely necessary, if any definite results are to be gained in the study of the phenomena of fatigue, that the effects of practice should be either abolished by having thoroughly practised subjects, or nullified by the method of experimentation.

It is worthy of notice that in the case of the subject, T.A., who had had a large amount of practice, the correlation between the fatigue coefficients in the cases of memorising and multiplying, due to many hours of mental work, was only ·13, with a probable error of ·095, a result without significance.

Regarding the question of transference of fatigue, Arai concludes on page 115: "On the whole, then, we are led to conclude that fatigue in a special mental function as well as in a general is slightly transferable to other functions, and that the greater the fatigue, the greater the transferred fatigue."

We have seen that the evidence upon which this conclusion is based is statistically quite unsound. In our opinion, no such conclusion can be drawn.

In 1914 Hollingworth[1] published the results of his investigation into the variations of efficiency during a day's mental work. The experiment is marked by great care and thoroughness. The subjects were fifteen adults, who worked for seventeen days. The results of the first seven days were discarded to allow of the practice curve becoming approximately linear before the experiment proper was commenced. The tests used were tapping, muscular co-ordination, colour naming, opposites, addition, steadiness, and reaction time. The subjects were under the eyes of the experimenter the whole time, and were paid for their work. They were further stimulated to do their best by the award of prizes. The testing, which occupied approximately forty-five minutes, was carried out at 7·45 a.m., 10 a.m., 12·15 p.m., 3·10 p.m., and 5·30 p.m.

Addition and opposites show a gradual decrease in efficiency throughout the day; but tapping and muscular co-ordination increase rather than decrease. The latter usually increased until midday, and then slightly decreased as the afternoon wore on.

The second part of the investigation consisted of twelve hours continuous testing for ten subjects on two separate days. In this twelve hours each of the six tests was done fifteen times. As before, the more strictly mental tests, addition, opposites, and colour naming, all show gradually decreasing efficiency as the day wore on. On the other hand, tapping, steadiness, and co-ordination increased in efficiency, taking the first performance as the normal.

The third part of the investigation consisted of ten hours typewriting for two days, by a subject already proficient in typewriting. Ten trials of thirty minutes each were interpolated in the ten hours work. On both days the time for writing four pages of approximately equivalent material decreases to a point of maximum speed between 3 p.m. and 4 p.m. After this the speed is reduced, until at 9 p.m. the speed is nearly the same as that at the beginning of the day's work. This increase in speed is not gained at the expense of accuracy, for the hours of faster speed are also the hours in which fewest mistakes were made.

In 1914 was published a research by Martin[2] concerning mental fatigue in school children incurred during school hours. The subjects, seven in

[1] Hollingworth, H. L. Variations in Efficiency during the Working Day Psych. Rev. 1914, p. 473.

[2] Martin, G. W. Evidence of Mental Fatigue during School Hours. J. of Ex. Ped. Vol. 1, pp. 39 and 137, and Vol. 3, p. 61.

number, were tested at 10 a.m., 12 noon, and 4 p.m., with a dynamometer and ergograph, as well as in multiplication, and a test similar to McDougall's spot pattern test.

The conclusion drawn was that, although some of the tests showed a decrease at 4 p.m., none were significant.

In a later article (J. of Ex. Ped. Vol. 3) this conclusion has been amended. In calculating the probable errors of the differences between the means of the work done during the daily testings, the writer used in error the formula $P.E. = \frac{\cdot 67449 \sqrt{\sigma_1^2 + \sigma_2^2}}{\sqrt{n}}$ instead of the formula

$$P.E. = \frac{\cdot 67449 \sqrt{\sigma_1^2 + \sigma_2^2 - 2r\,\sigma_1\,\sigma_2}}{\sqrt{n}}$$

When corrected for this error, the results are as below. The probable errors are given in brackets. The negative signs signify a fall in efficiency, while the positive signs signify a rise.

Tests.	Difference between the Mean Scores at 10 a.m. and 12.		Difference between the Mean Scores at 12 and 4 p.m.		Difference between the Mean Scores at 10 a.m. and 4 p.m.	
Æsthesiometer	+ ·00	(·016)	− ·04	(·12)	− ·04	(·006)
Dynamometer	+ ·1	(·144)	+ ·2	(·154)	+ ·2	(·059)
Multiplication	+ 12·	(1·8)	− 16·5	(2·0)	− 4·5	(2·8)
Spot Pattern	+ 15·	(2·8)	− 16·	(4·0)	− 1·	(3·4)

From this table it can be seen that several of these differences are now significant, notably in the multiplication and spot pattern tests. The most striking result is that in both these tests, we find a rise between 10 a.m. and 12 noon, of about six times the probable error in each case. This increase is ascribed to the effect of adaptation. Between 12 and 4 there is a fall just as well marked as the increase from 10 to 12. This fall is ascribed to fatigue. But between 10 and 4, although both tests show a decrease, neither fall is significant.

The results in these two tests seem to indicate, in the subjects tested, well-marked fluctuations in efficiency during the day, but the absence of any control days on which no school work was done deprives us of any opportunity of ascertaining whether these fluctuations are due to fatigue or to a normal daily rhythm. The writer also does not say what steps, if any, were taken to counteract the effects of boredom, and to keep the subjects working at maximum pressure each day.

In 1916, Smith[1] published her interesting study of the effects of fatigue induced by curtailing the normal number of hours of sleep. The tests used were McDougall's dotting test, memory for associated words, the illusion of reversible perspective as exemplified in McDougall's windmill, learning and relearning nonsense syllables, and tapping. The method of inducing fatigue was to curtail the normal amount of sleep for three successive nights. The normal performance of each test was gauged

[1] Smith, M. A Contribution to the Study of Fatigue. Brit. Jour. of Psych., 1916.

by a large number of readings on days succeeding the normal rest. The experiment proper was not begun until the influence of practice had disappeared.

The immediate result of loss of sleep is an increase of efficiency, which then falls below normal when the usual number of hours rest is taken. A further curtailment of the normal amount of rest immediately causes a rise in efficiency, up to or above the normal, which again falls when the regular amount of sleep is taken.

A striking result obtained in this experiment is the long time that elapses before the efficiency, impaired by the loss of sleep, again becomes normal. If, after reducing the hours of sleep by more than 50 per cent. for three successive nights, the usual hours of rest are taken until the efficiency again reaches the normal, we find that this point is not reached until about the sixteenth day from the beginning of the experiment.

The writer concludes that:—

1. Fatigue as estimated objectively involves two distinct phases:
    (*a*) A phase when fatigue acts apparently as a stimulant, and increases efficiency.
    (*b*) A phase of longer duration when the body is attempting to make good its losses.
2. The time taken to return to a normal condition after the loss of a few hours sleep is disproportionally great; and this return is gradual but irregular.

OUR OWN EXPERIMENTS.

Part I.—Alternated Work.

1. Conditions of Experiment.
- (*a*) The Tests.
- (*b*) The Subjects.
- (*c*) Incentives to work.
- (*d*) General Conditions.
- (*e*) Preliminary Practice.

2. Plan and Description of Experiment.
- (*a*) Section 1. Multiplication.
- (*b*) Section 2. Cancellation.
- (*c*) Section 3. Multiplication and Cancellation alternated.

3. Results.
- (*a*) Section 1. Multiplication.
- (*b*) Section 2. Cancellation.
- (*c*) Section 3. Multiplication and Cancellation alternated.

1. Conditions of Experiment.

The first part of this investigation was carried out in the Psychological Laboratory of University College during the academic year 1915–1916. We have divided this part into three sections. Section 1 continued for forty-eight days, with a further supplementary period of fourteen days. The days were consecutive, excepting Sundays and one other day, on which the subjects were unable to come to the laboratory. Section 2 lasted for thirty-four consecutive days, excepting Sundays, and Section 3 for thirty consecutive days, excepting Sundays and one other day, on which the subjects were unable to attend the laboratory. The work was commenced between 5 p.m. and 5.15 p.m. each day.

(*a*) *The Tests.*

In Section 1 the test used was the multiplication of one-place numbers by a one-place number. The whole arrangement of figures was the same for each day, so as to obviate any errors due to unequal difficulty of task on different days. To prevent, if possible, any memorisation of the figures in the product, the arrangement of the work given in the initial 15 seconds' practice was varied each day. In marking, one mark was given for each correct figure. The number of errors was noted. Only the unit's figure of the answer was required to be written down.

In Section 2 the test was cancellation. The sheet containing the matter to be cancelled consisted of thirty lines of fifty capital letters. In each line the letters to be cancelled—T, A, X, O—were repeated six times. The same work was done each day, with 15 seconds' practice, as in the previous section. In marking, one mark was given for each letter correctly cancelled, and one mark deducted for each letter wrongly cancelled.

In Section 3 the same two tests were used, given alternately. That is to say, a period of multiplication was first given, and then followed a period of cancellation and so on. Fifteen seconds' practice at multiplication was given each day, as in Section 1. Both the multiplication and the cancellation were the same every day. Marking was the same as in the two previous sections.

In each section the subjects did not look at their work until the signal to commence was given, and were instructed to work as fast as was consistent with correct working.

(b) The Subjects.

The subjects were three boys from a public elementary school in one of the poorer districts of London, and one from a higher elementary school in the same district. Their ages ranged from 12 to 14½ years. All four subjects had previously done a large amount of work in the laboratory, extending over three months, similar to the work they were now asked to do.

(c) Incentives to work.

In order to eliminate, if possible, the otherwise variable factor of the subjects' "willingness to work," and to ensure a maximum output of work, a system of monetary rewards was devised. Each boy was engaged by the week, and was paid a wage higher than was usually received by boys of this class when leaving school. It was agreed that this wage could be withheld, wholly or partially, if, in the experimenter's estimation, any subject did not at any time do his utmost.

As the preliminary practice work lasted three weeks, the experimenter had a very good idea of each subject's capability in each of the tests. The aim before the boys was to beat their previous best performance. Each time a subject succeeded in breaking his record he received one mark, and each time he failed to do so one mark was deducted from his score. Monetary rewards were distributed at the end of each week according to the marks so obtained. So far as we could judge, this device seemed most successful, and the work was done with great eagerness, and even enthusiasm.

(d) General Conditions.

The whole of the work was done in the presence of the experimenter and of no one else. The same room was always used, and the subjects occupied the same places in the room every day. The same instructions were given every day, and were given in a set form of words. During the periods of rest the subjects occupied themselves in reading stories, looking at pictures, or talking to the experimenter. In the preliminary practice work the cancellation scores were found to vary markedly, according to the sharpness or bluntness of the pencil-point. To guard against this, the lead of the pencils was never sharpened to a point. When the pencils required sharpening, the wood alone was cut away. The same pencil was used by each subject every day.

(e) Preliminary Practice.

Although the subjects had all done a large amount of work in this laboratory similar to the work they were now required to do, it was considered necessary to have a practice period before the experiment proper began, so that the curve of practice would be horizontal or almost horizontal. For this purpose practice periods of three weeks were taken at the beginning of Sections 1 and 2, and a practice period of one week before Section 3.

In each of these weeks of practice 20 minutes' work was done daily for six days at maximum effort. This amount of practice was sufficient to ensure that the curve of practice was approximately linear, to enable us to determine its course, and to eliminate its effects. A good deal of practice was also given in starting and stopping work promptly.

The practice curves of the work done before Sections 1 and 2 are given in Figs. 1 and 2. It will be seen that the practice curves rise abruptly

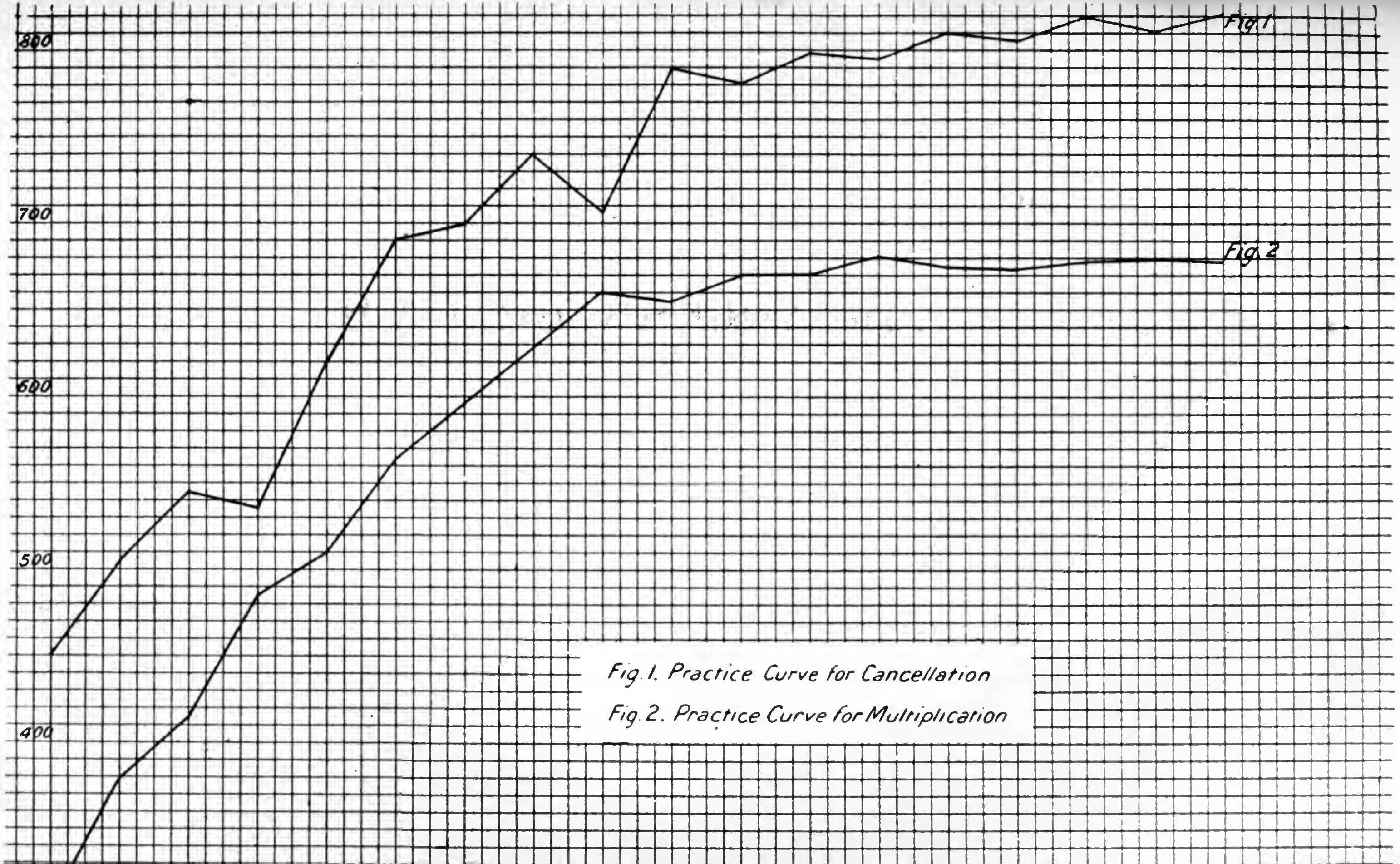


Fig. 1. Practice Curve for Cancellation

Fig. 2. Practice Curve for Multiplication

at first, but the daily rate of increase gradually declines, until, towards the end of the practice period, and, therefore, when the experiment proper was commenced, the curves had almost assumed a horizontal position.

2. Plan and Description of the Experiment.

(*a*) *Section* 1.—*Multiplication.*

The work in this section was divided into four parts (*see* Table 1). Each part occupied six days. In the first part the work-period was 1 minute, in the second part 2 minutes, and in the third and fourth 3 and 4 minutes respectively. Between each work-period, rests, varying from zero on the first day to 4 minutes on the sixth, were interpolated between each work-period. The total amount of work done each day was always 12 minutes—*i.e.*, twelve 1-minute, six 2-minute, four 3-minute, or three 4-minute work-periods. Thus, when rests were given, there were eleven rests in the first part, five rests in the second, three in the third, and two in the fourth. The total amount of rest given on any one day varied from zero to 44 minutes—*i.e.*, eleven rests of 4 minutes each. At the beginning of each work-period the subjects commenced a new line. The whole series was completed in twenty-four days. It was then repeated in reverse order to cancel any practice effort.

Later on it was found desirable to have still longer rests, and a further experiment, lasting fourteen days, was carried out, extending the lengths of the rests to 16 minutes for the part consisting of 1-minute work-periods, and to 32 minutes for the part consisting of 4-minute work-periods. In a similar way the rests with the work-periods of 2 minutes were extended to 8 minutes.

The plan of this supplementary work is given in Table 2.

TABLE 1.—*Plan of Section* 1.—Multiplication. Total time of work constant = 12 minutes.

Day.	Length of Work-period.	Length of each Rest.	Day.	Length of Work-period.	Length of each Rest.
1	1 minute ...	....	25	4 minutes ...	4 minutes.
2	1 ,, ...	15 seconds.	26	4 ,, ...	2 ,,
3	1 ,, ...	30 ,,	27	4 ,, ...	1 ,,
4	1 ,, ...	1 minute.	28	4 ,, ...	30 seconds.
5	1 ,, ...	2 minutes.	29	4 ,, ...	15 ,,
6	1 ,, ...	4 ,,	30	4 ,, ...	....
7	2 minutes ...	....	31	3 ,, ...	4 minutes.
8	2 ,, ...	15 seconds..	32	3 ,, ...	2 ,,
9	2 ,, ...	30 ,,	33	3 ,, ...	1 minute.
10	2 ,, ...	1 minute.	34	3 ,, ...	30 seconds.
11	2 ,, ...	2 minutes.	35	3 ,, ...	15 ,,
12	2 ,, ...	4 ,,	36	3 ,, ...	....
13	3 ,, ...	....	37	2 ,, ...	4 minutes.
14	3 ,, ...	15 seconds.	38	2 ,, ...	2 ,,
15	3 ,, ...	30 ,,	39	2 ,, ...	1 minute.
16	3 ,, ...	1 minute.	40	2 ,, ...	30 seconds.
17	3 ,, ...	2 minutes.	41	2 ,, ...	15 ,,
18	3 ,, ...	4 ,,	42	2 ,, ...	....
19	4 ,, ...	....	43	1 minute ...	4 minutes.
20	4 ,, ...	15 seconds.	44	1 ,, ...	2 ,,
21	4 ,, ...	30 ,,	45	1 ,, ...	1 minute.
22	4 ,, ...	1 minute.	46	1 ,, ...	30 seconds.
23	4 ,, ...	2 minutes.	47	1 ,, ...	15 ,,
24	4 ,, ...	4 ,,	48	1 ,, ...	....

TABLE 2.—Plan of supplementary experiment for longer rests.

Date.	Length of Work-period.	Length of each Rest.	Day.	Length of Work-period.	Length of each Rest.
1	1 minute ...	4 minutes.	8	4 minutes ...	32 minutes.
2	1 ,, ...	8 ,,	9	4 ,, ...	16 ,,
3	1 ,, ...	16 ,,	10	4 ,, ...	8 ,,
4	4 ,, ...	4 ,,	11	4 ,, ...	4 ,,
5	4 ,, ...	8 ,,	12	1 ,, ...	16 ,,
6	4 ,, ...	16 ,,	13	1 ,, ...	8 ,,
7	4 ,, ...	32 ,,	14	1 ,, ...	4 ,,

Rests of 4 minutes were given to enable us to ascertain, by a comparison with the corresponding day of the original experiment, the increment due to practice. The results of this supplementary work, which are given in Table 4, were then reduced in such a proportion as would make them comparable with the results already gained.

(*b*) *Section* 2.—*Cancellation.*

The plan of this section was similar to that of Section 1, except that only 1-minute and 4-minute work-periods were used. The rests were of the same length as in the preceding section.

(*c*) *Section* 3.—*Alternate Multiplication and Cancellation.*

In this section the work-periods and rests were the same as in Sections 1 and 2, but a period of multiplication was alternated with a period of cancellation. Thus when the work-period was 1 minute, there would be six periods of multiplication and six of cancellation, and when the work period was 4 minutes there would be two periods of multiplication, and only one of cancellation. The first period every day was occupied by multiplication.

3. Results of Experiments.

Section 1—*Multiplication.*

In Table 3 are given the scores for each of the forty-eight days. The gross scores represent the total number of figures multiplied, while the net scores represent the number of figures multiplied correctly. A comparison of the two columns of scores shows that the number of errors bears a fairly constant ratio to the number of figures multiplied, and that this ratio is very small. Little is therefore to be gained by the separate treatment of the gross and net scores. Unless otherwise indicated, we shall deal exclusively with the net scores. The "total" scores show the number of figures multiplied, after eliminating the practice effect, and are the sums of the scores of each two days on which the same work-periods and rests were given. In describing the plan of this experiment, the procedure adopted for abolishing the effects of practice was given.

For purposes of comparison it will be convenient to express the results of this work in a graphical form. These are accordingly given in Fig. 3. The ordinates give the average net scores, while the abscissæ represent the length of rest between each work-period.

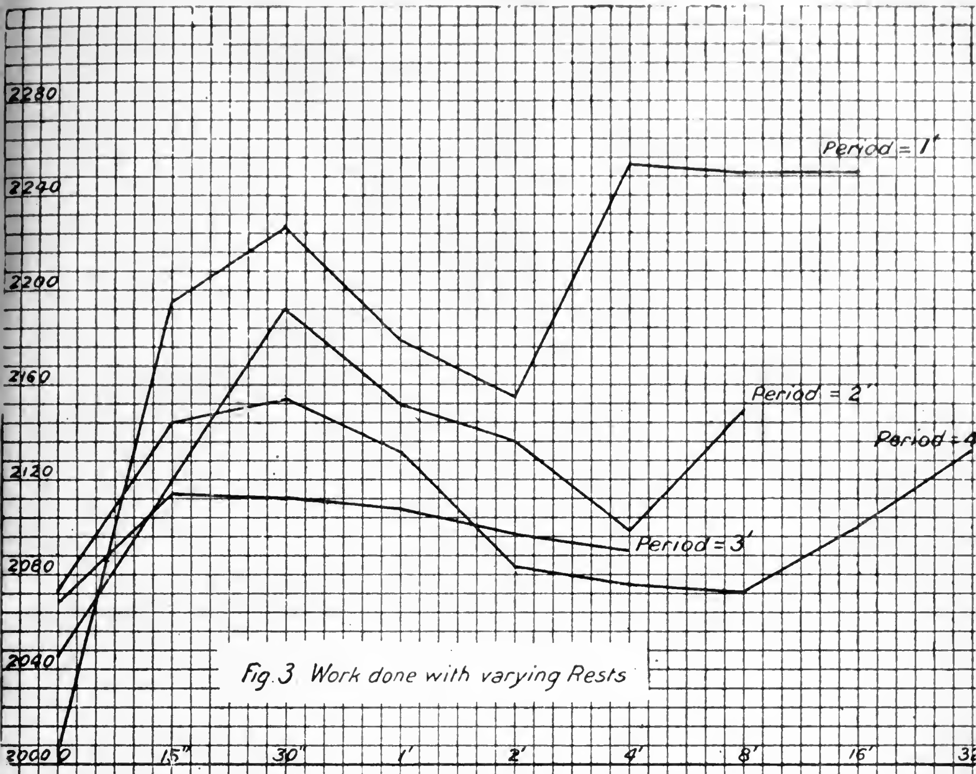


Fig. 3. Work done with varying Rests

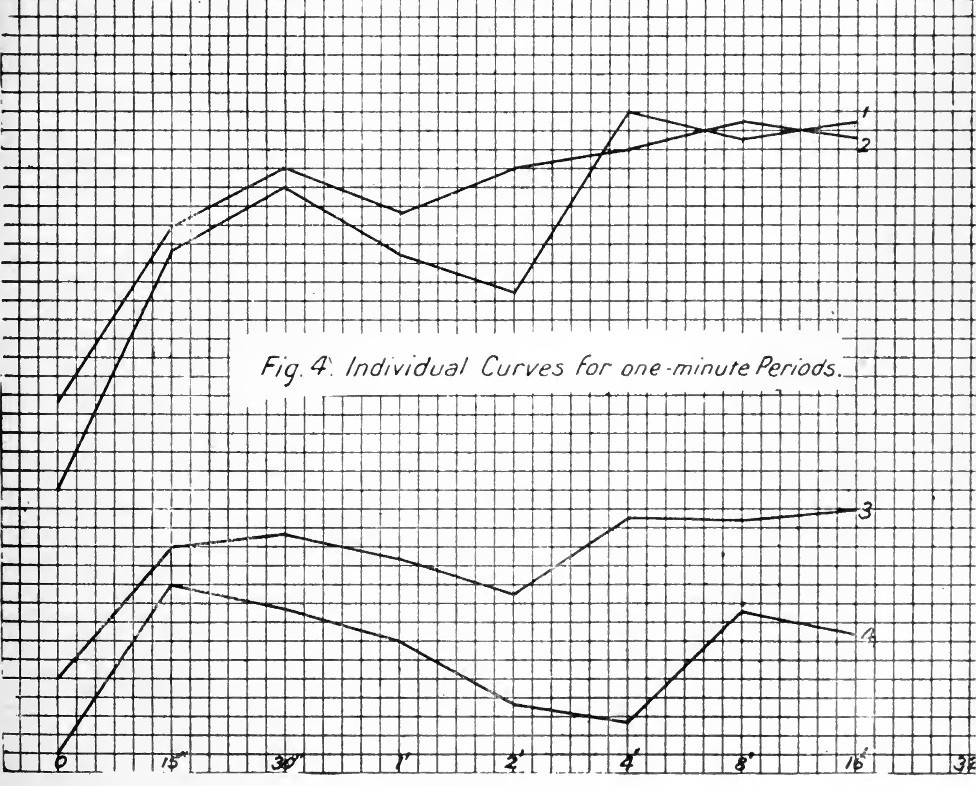


Fig. 4. Individual Curves for one-minute Periods.

TABLE 3.—Section 1. Results. Scores in multiplication. Gross score minus the net score gives the daily number of errors.

Length of Work-period.	Length of Rests.	Gross Scores.					Net Scores.				
		Day.	Score.	Day.	Score.	Total.	Day.	Score.	Day.	Score.	Total.
1 min. ...	......	1	3,054	48	5,132	8,186	1	3,015	48	4,996	8,011
1 ,, ...	15 sec. ...	2	3,351	47	5,593	8,944	2	3,292	47	5,486	8,778
1 ,, ...	30 ,,	3	3,535	46	5,491	9,026	3	3,494	46	5,410	8,904
1 ,, ...	1 min.	4	3,480	45	5,374	8,854	4	3,416	45	5,296	8,712
1 ,, ...	2 mins.	5	3,550	44	5,183	8,738	5	3,502	44	5,118	8,620
1 ,, ...	4 ,,	6	3,862	43	5,296	9,158	6	3,812	43	5,200	9,012
2 mins. ...	......	7	3,455	42	4,885	8,340	7	3,392	42	4,791	8,183
2 ,, ...	15 sec.	8	3,590	41	5,050	8,640	8	3,518	41	4,968	8,486
2 ,, ...	30 ,,	9	3,902	40	5,017	8,919	9	3,824	40	4,919	8,743
2 ,, ...	1 min.	10	3,924	39	4,860	8,784	10	3,848	39	4,765	8,613
2 ,, ...	2 mins.	11	3,847	38	4,838	8,685	11	3,775	38	4,772	8,547
2 ,, ...	4 ,,	12	3,842	37	4,673	8,515	12	3,779	37	4,613	8,392
3 ,, ...	......	13	3,913	36	4,555	8,468	13	3,815	36	4,453	8,268
3 ,, ...	15 sec.	14	3,894	35	4,728	8,622	14	3,817	35	4,645	8,462
3 ,, ...	30 ,,	15	3,981	34	4,627	8,608	15	3,923	34	4,533	8,456
3 ,, ...	1 min.	16	3,978	33	4,599	8,577	16	3,914	33	4,520	8,434
3 ,, ...	2 mins.	17	4,057	32	4,497	8,554	17	4,001	32	4,403	8,404
3 ,, ...	4 ,,	18	4,006	31	4,520	8,526	18	3,914	31	4,444	8,358
4 ,, ...	......	19	4,103	30	4,362	8,465	19	4,020	30	4,271	8,291
4 ,, ...	15 sec.	20	4,120	29	4,606	8,726	20	4,035	29	4,545	8,580
4 ,, ...	30 ,,	21	4,201	28	4,587	8,788	21	4,139	28	4,465	8,604
4 ,, ...	1 min.	22	4,209	27	4,490	8,699	22	4,119	27	4,410	8,529
4 ,, ...	2 mins.	23	4,320	26	4,206	8,526	23	4,230	26	4,111	8,341
4 ,, ...	4 ,,	24	4,248	25	4,234	8,482	24	4,135	25	4,175	8,310

TABLE 4.—Supplementary multiplication scores for rests longer than 4 minutes, reduced to same stage of practice as results in Table 3.

Period.	Rest.	Score.	Period.	Rest.	Score.
1 minute ...	8 minutes	8,980	4 minutes ...	16 minutes	8,351
1 ,, ...	16 ,,	8,984	4 ,, ...	32 ,,	8,533
4 ,, ...	8 ,,	8,301			

Before attempting to give some interpretation of the course of these curves, one apparent irregularity must be dealt with—that is, the differences between the initial points of the four graphs. If the practice effect were wholly eliminated, one would expect all the curves to start from the same point, as each is the score for 12 minutes' work. The differences are caused by the method of experimentation. In order to estimate the amount of work done in each period when no rests were allowed, the subjects were required to commence a new line at the end of each period. As the work lasted for 12 minutes, excluding rests, this would necessitate eleven changes to a new line when the work-period was 1 minute, and five, three, and two changes when the work-periods were 2 minutes, 3 minutes, and 4 minutes respectively. The total time lost in changing from one line to another would consequently be greatest when the work period was 1 minute, and least when the work-period was 4 minutes. Thus we find the work done, when no rests were given, to vary inversely as the number of changes to be made. Of course, these variations would not be present when rests were allowed. By calculation it is possible to remove this discrepancy in the initial scores. If we call "α" the amount of work that would be done, if no time was lost in changing from line to line, and "β" the loss in efficiency caused by each interruption, we get the following equations, where the right-hand side of the equation gives the net scores :—

$\alpha - 11\beta = 8{,}011$ when the work period is 1 minute.
$\alpha - 5\beta = 8{,}183$,, ,, 2 minutes.
$\alpha - 3\beta = 8{,}268$,, ,, 3 ,,
$\alpha - 2\beta = 8{,}291$,, ,, 4 ,,

Taking these four equations in pairs, and solving, we get the following values for β, the loss in efficiency due to one interruption of work : 29, 32, 31, 42, 26, 23, which give an average of 32. For α the corresponding values are 8,326, 8,364, 8,353, 8,395, 8,363, and 8,337, giving an average for the four subjects of 8,356, or an average for each subject of 2,089, which is the initial point of each curve. Now, taking the average loss of efficiency for each interruption of work as 32, and the average score, if there were not interruptions, as 8,356, we can compare the measured and calculated values of the *apparent* (*i.e.*, with the interruption effect not eliminated) initial point of each curve. The figures are given in Table 5.

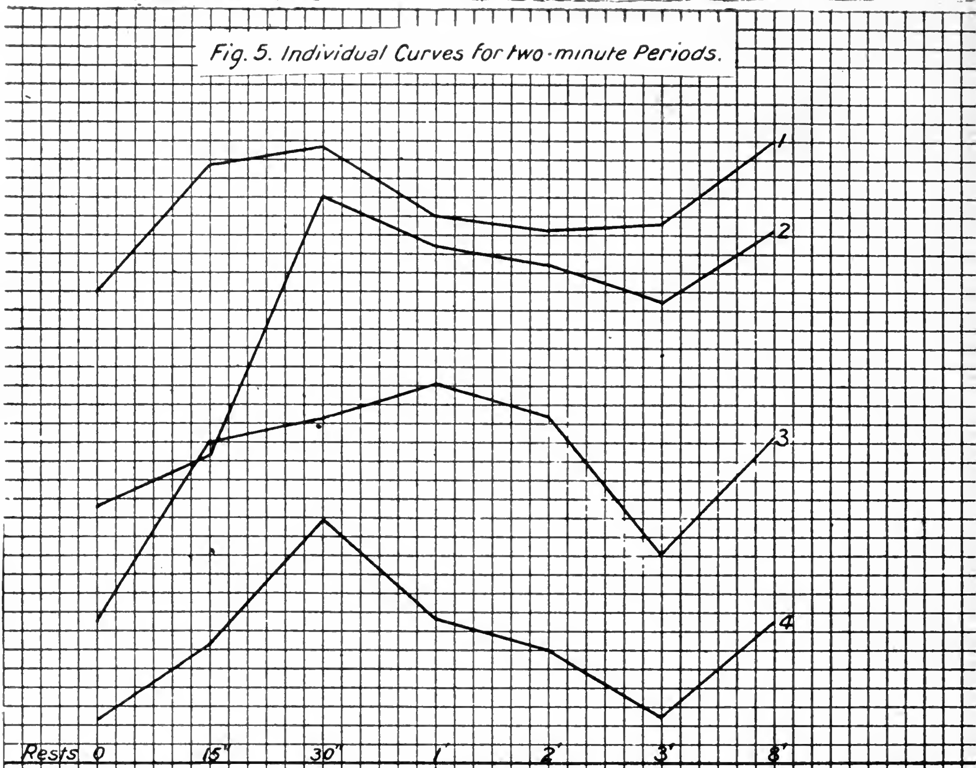
TABLE 5.—Comparison of the Initial Points of Curves as given by Measurement and by Calculation.

Length of Work-period.	Measured Score.	Calculated Score.
1 minute	8,011	8,004
2 ,, ...	8,183	8,196
3 ,, ...	8,268	8,260
4 ,, ...	8,291	8,292

The measured scores do not vary from the calculated scores in any constant manner. They are not all larger or all smaller than the latter. If this were the case there would be a suspicion that some influence besides the interruption effect was present which gave the measured results a bias in one direction or the other. However, this is not so. The measured scores for the 1-minute and 3-minute periods are above, and those for the 2-minute and 4-minute periods are below the calculated scores. This result must be due to random variations in the measurements.

Let us now compare the curves given in Fig. 3. It is clear from what precedes, that the initial points would coincide at the ordinate marked 2,089, if the effects of changing from line to line were eliminated. It will be seen that the four curves exhibit a marked similarity of shape. All rise sharply when rests of 15 seconds' duration are given, and a maximum point occurs on all of them with a rest of between 15 and 30 seconds. For rests of between 30 seconds and 2 minutes all the curves fall, and the shorter the work-period the steeper is their descent, until a minimum point is reached, which does not occur at the same place on each curve. With 1-minute periods this minimum point occurs with a rest of between 1 and 2 minutes, with 2-minute periods at a point which represents a rest of nearly 4 minutes, and with 4-minute periods, with a rest of about 8 minutes, With 3-minute work-periods this minimum point is not determined,

Fig. 5. Individual Curves for two-minute Periods.



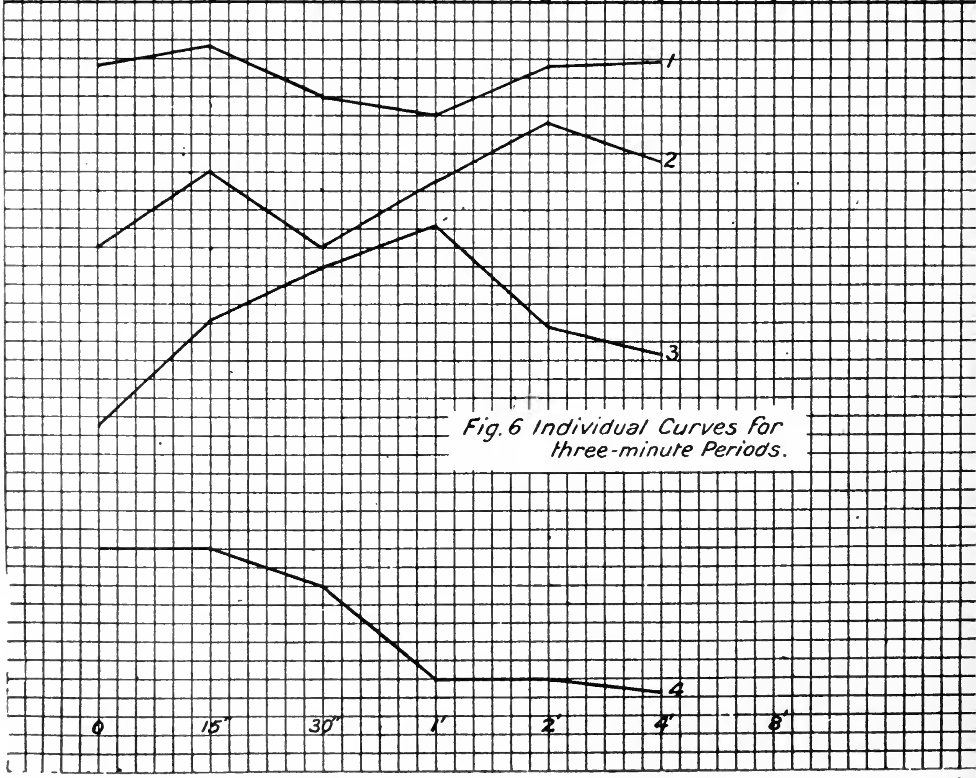


Fig. 6 Individual Curves for three-minute Periods.

but judging from the shape of the graph, and from the position of this point on the other curves, it would probably occur with a rest of between 4 and 8 minutes. Thence the three curves, whose course we can still trace, rise at varying angles, the ascent being steepest with 1-minute work-periods and most gradual with the longest work-period. In the graph representing the work done with 1-minute periods, an important point occurs with a rest of 4 minutes. Here the curve reaches its maximum height. Longer rests do not result in an increase of efficiency, and the graph assumes a horizontal line, notwithstanding the fact that the rests are lengthened to 16 minutes. For longer work-periods the amount of rest was evidently insufficient to allow them to assume this constant position. It is plainly evident that these four curves belong to the same family. Although the maximum and minimum points do not all occur with the same length of rest, yet the general similarity of shape is obvious for all four lengths of work-periods.

Before coming to a conclusion concerning the shapes of these curves, it will be well to examine the corresponding curves for each subject separately, and compare them with the average curves as given in Fig. 3. The individual curves are given in Figs. 4, 5, 6, and 7. The same numbers correspond to the same subjects in all four graphs. There is a general agreement between all these individual curves, and therefore with the average curves, though individual variations seem to have most influence when the work-period is 3 minutes. Out of the sixteen individual curves twelve have a maximum point at or between 15 seconds and 30 seconds' rest, while in three of the others this point occurs with a rest of between 15 seconds and 1 minute. Only one curve fails to show this maximum point. The minimum point is also clearly present in all the curves, while all show a tendency to rise if the rests are made long enough. The horizontal period is visible in three out of the four graphs when the work-period is 1 minute.

In these curves we find the shape constant for each of the four subjects with each of the four lengths of work-periods. It seems safe then to draw the general conclusion that, *in doing such work as continuous multiplication, there are general influences at work of such a nature as would give a constant characteristic shape to the curves representing the amount of work done with varying rests.*

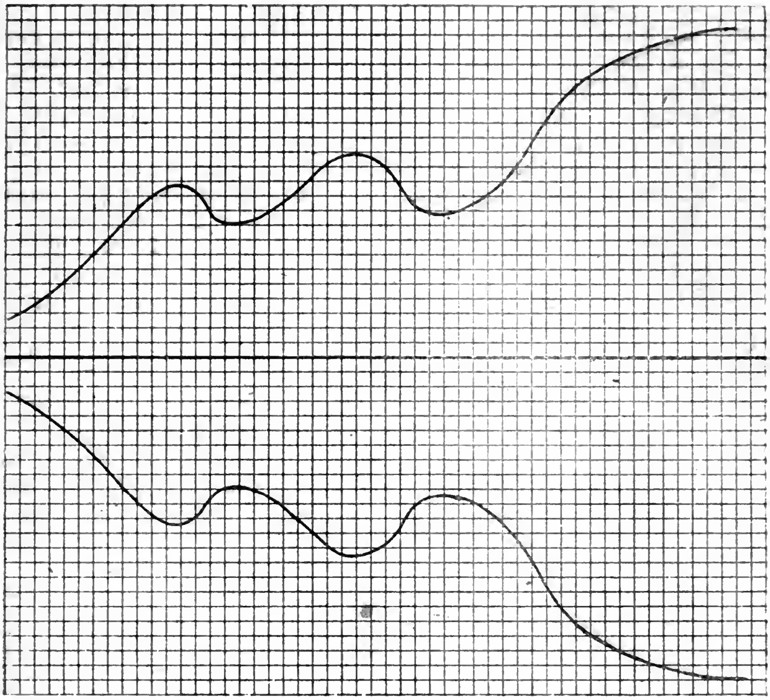
Upon a casual consideration of the effect of the length of rest upon the efficiency of the worker, one might suppose that the longer the rest up to a certain point, the more beneficial its result would be. But it seems this is not always so. This statement would be true of certain parts of our curves, but not of the curves taken as a whole. Take, for example, the curve for 4-minute periods in Fig. 3. With rests of between 8 and 32 minutes, the beneficial effect varies as the length of rest; but the reverse of this is the case if we consider rests between 30 seconds and 8 minutes in length. Here a rest of 30 seconds is much more beneficial than one sixteen times as long. In fact, a rest of 8 minutes' duration is, with these subjects, less beneficial than no rest at all. The reason of this becomes evident when we consider the influences which cause the curves to assume such a characteristic shape.

The Kraepelin school have found various influences at work in the performance of a mental task, namely, practice, fatigue, warming-up, or incitement, adaptation, and spurt. By our method of experimentation we have eliminated the effect of practice, and are left with two factors or groups of factors which affect the work done in opposite ways.

First, we have the factor or group of factors included in the term "fatigue"—that is, all those influences which have for their effect the depreciation of the work done, either in quality or quantity, or both. On the other hand, we have the group of influences which tend to increase the quality or quantity of the mental output, and which, for the sake of simplicity, we have included under the term "incitement." These two opposing influences are inseparable, and their combined effect is positive or negative, according as the incitement or the fatigue predominates.

When the work is continuous and no rests are given, we may assume that the whole of the positive and negative effects are conserved. When a rest is given we get a double effect. Firstly, the accumulated fatigue is dissipated, either wholly, if the rest is long enough, or only in part, if it is short. Secondly, as the fatigue effect dies away, so does the incitement effect, either in part, if the rest is short, or wholly, if it is long enough. Therefore the ordinates of the curves are the result of these two opposite effects. If the rests were made long enough, we should come to a point at which the curve would be approximately a straight line parallel to the horizontal axis, for then both effects would die completely away, and lengthening the time of rest would have no further effect. This point seems to have been attained in the curve for 1-minute periods, but not in any of the others. Rests of 4, 8, and 16 minutes respectively have approximately the same effect on the output of work. This is what might be expected, since the total amount of rest, when the work-period is 1 minute, is much greater than that of any other period. The total amount of rest given for the different lengths of work-periods can be expressed in the ratios 11 : 5 : 3 : 2.

The curves in Fig. 3 represent, then, the resultant of two sets of opposing factors, and the rates at which these factors vary will determine the shape of the curve. If the fatigue effect disappears under the influence of rest at a greater rate than the incitement effect, the resultant curve will rise; and, conversely, if the incitement effect is dissipated more rapidly than the fatigue effect, the curve will fall. In our examination of the rates at which fatigue and incitement are dissipated, we will take as typical the curve for 1-minute periods in Fig. 3, because this is the only complete curve of the four. For rests less than 30 seconds in length the curve rises. Evidently for rests of this length the amount of fatigue dissipated exceeds the loss of incitement, or, to be more precise, the loss of fatigue has a greater positive effect upon efficiency than the negative effect of the loss of incitement. Therefore, at this part of the curve the loss of fatigue proceeds at a greater rate than the loss of incitement. With rests greater than 30 seconds, and not greater than 2 minutes, the reverse is the case. The curve here falls, showing that the loss of fatigue is less than the loss of incitement with rests of this duration. With rests of more than 2 minutes and not more than 4 minutes, the curve again rises rapidly, showing that the loss of fatigue must be here much greater than the loss of incitement. For rests of 4 minutes and longer, the curve becomes a straight line parallel to the horizontal axis. At this stage the effects of both fatigue and incitement are at a minimum, and the amount of work done is at a maximum. The rests are long enough to dissipate both the fatigue and incitement effects due to one minute of work. Either the subject's ability to perform the task is here normal, *i.e.*, not under the influence of the fatigue induced by the preceding period of work at the beginning of each work-period, or else from this point the loss of incitement exactly balances the loss of fatigue, *and continues to do so.* If the latter were the true account, the separate curves of incitement and fatigue, which combine to form the curve we are discussing, would each be the same shape as the reflection of the other in a mirror, which result we illustrate below.



On the ground of improbability, this latter possibility may be ruled out of account. If, then, with rests of 4 minutes or longer the fatigue and incitement effects are at a minimum, both fatigue and incitement must be wholly abolished. The amount of work done with 1-minute periods and 4-minute rests is then the normal maximum, uninfluenced by either fatigue or incitement, except that generated within each separate work-period. There is no transference to another period of the fatigue and incitement of the preceding period. *The difference between this normal maximum amount of work and the amount of work done with any length of rest will give us the real effect of combined loss of fatigue and incitement due to rest.* These differences are given in Table 6.

TABLE 6.—Amount of fatigue and incitement with rests of different lengths.

	Length of Rest.							
	0	15″	30″	1′	2′	4′	8′	16′
Work done ...	1,001	234	108	300	392	0	32	28

As this maximum output of work is not determined for work-periods other than that of 1-minute, the corresponding curves for the other work-periods cannot be given. The curve corresponding to these results is given in Fig. 8. It is the time curve of the loss of incitement and fatigue due to rest, or *the curve of recovery from fatigue.*

We can obtain this curve by another method, which is not so reliable, but will give evidence to corroborate the result already obtained, and, further, will also give us the corresponding curves for the other work-periods. If we consider the amount of work done in the first period each day as the normal performance, and subtract from this score the amount done in the last period, we shall find the depreciation in efficiency for each length of rest. On the days when the work is continuous, the difference between the work done in the first and last periods on each day will give the absolute effect of fatigue and incitement, which we assume to be completely conserved, due to 12 minutes work; and similarly on the days when 15 seconds rest is given after each work-period, the difference between the scores of the first and last periods will give the fatigue and incitement effect with rests of this length. If we call "A" the difference between the scores of the first and last periods on the continuous work days, and "B" the corresponding difference when 15-second rests are interpolated, then (A—B) is the amount of fatigue and incitement abolished by the interpolated rests of 15 seconds. In the same way we can find the effect of rests of any length. There will be eleven rests when the work-period is 1-minute, and five, three, and two when the periods are 2, 3, and 4 minutes respectively. The average effect of each rest can be found by dividing the total effect by these numbers. The disadvantage of the method is obvious. Only part of the results is used, namely, the first and last periods, and their accuracy will be diminished by the influence of random errors and spurts. In Table 7 are given the scores in the first and last periods, and in Table 8 the absolute differences, and the differences relative to the score of the first period. It will be noticed in Table 8 that the differences with rests of 30 seconds are all negative—that is, the scores for the last period in every case were greater than the scores of the first period. This cannot be due to a loss of fatigue, as the subjects were presumably unfatigued at the beginning of the day's work. It must therefore be due to the increased effect of incitement. Evidently a rest of this duration is not sufficient to abolish much of the incitement effect, for of all the shorter rests this is the most beneficial.

TABLE 7.—Loss of incitement and fatigue found by comparing the scores in the first and last periods.—Multiplication.[1]

Rests.	1-minute Periods.		2-minute Periods.		3-minute Periods.		4-minute Periods.	
	First Period.	Last Period.	First Period.	Last Period.	First Period.	Last Period.	First Period.	Last Period.
...	728	663	1,439	1,344	2,147	2,104	2,820	2,723
15 secs.	763	743	1,434	1,404	2,139	2,128	2,901	2,910
30 ,,	756	760	1,401	1,452	2,110	2,146	2,883	2,908
1 min.	754	728	1,467	1,434	2,173	2,092	2,807	2,897
2 mins.	756	705	1,469	1,371	2,149	2,063	2,791	2,799
4 ,,	796	747	1,448	1,367	2,098	2,081	2,847	2,742
8 ,,	773	719	......	......	......	......	2,785	2,736
16 ,,	704	726	......	......	......	......	2,803	2,869
32 ,,	......	......	......	......	......	......	2,827	2,900

[1] See p. 43 for the results, omitting the scores of the first work-period.

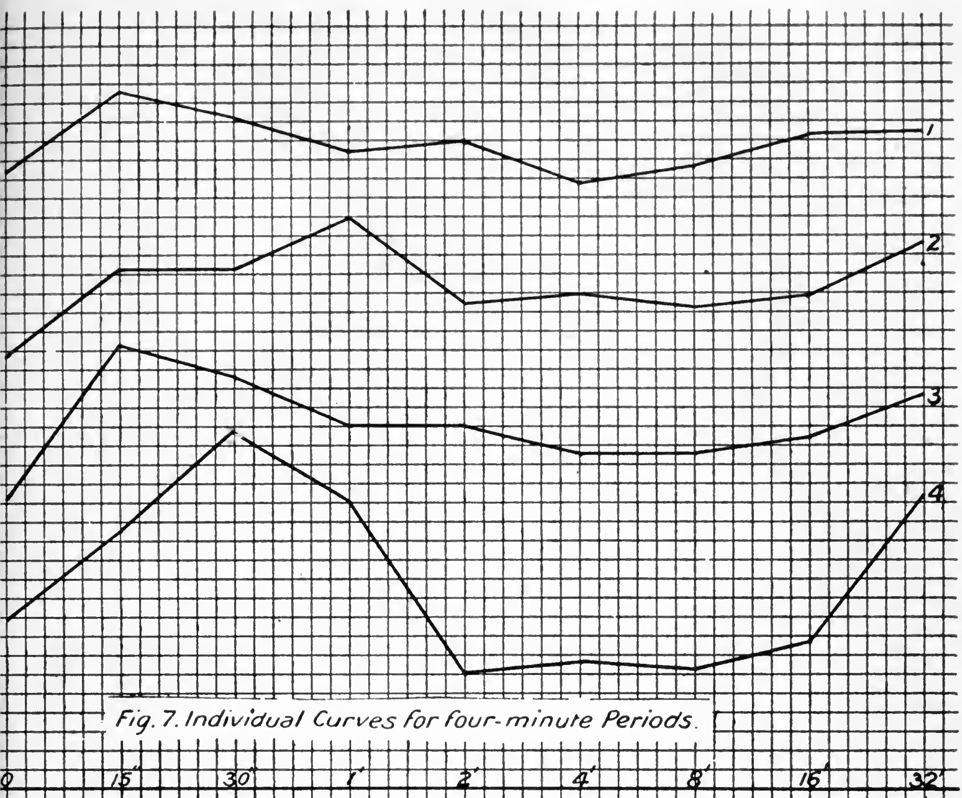


Fig. 7. Individual Curves for four-minute Periods.

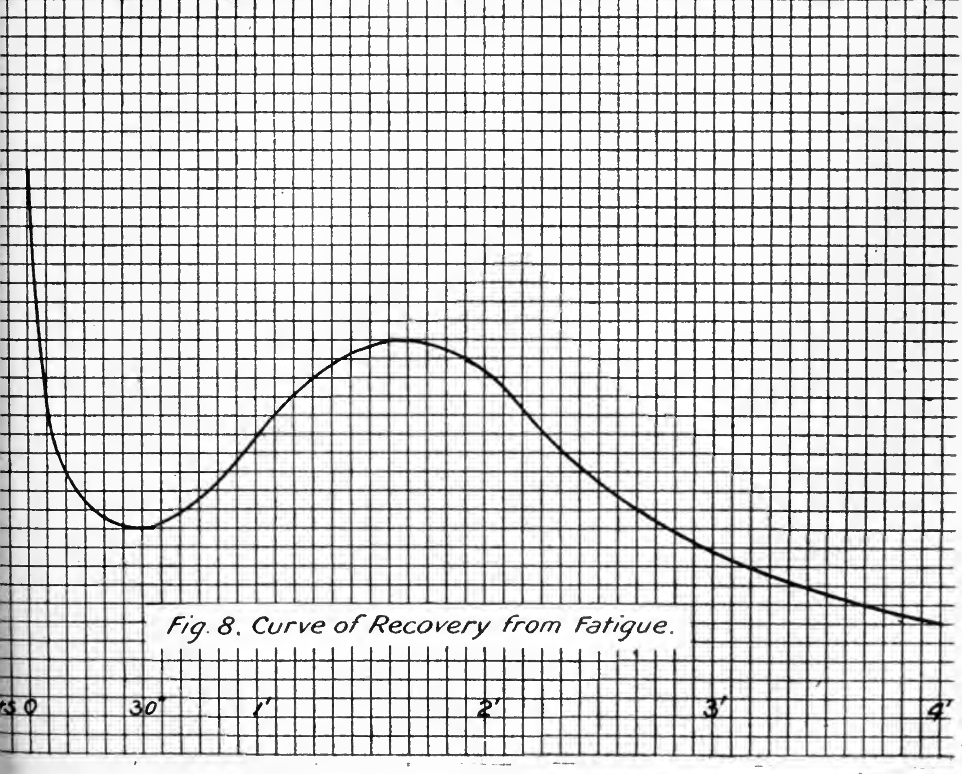


Fig. 8. Curve of Recovery from Fatigue.

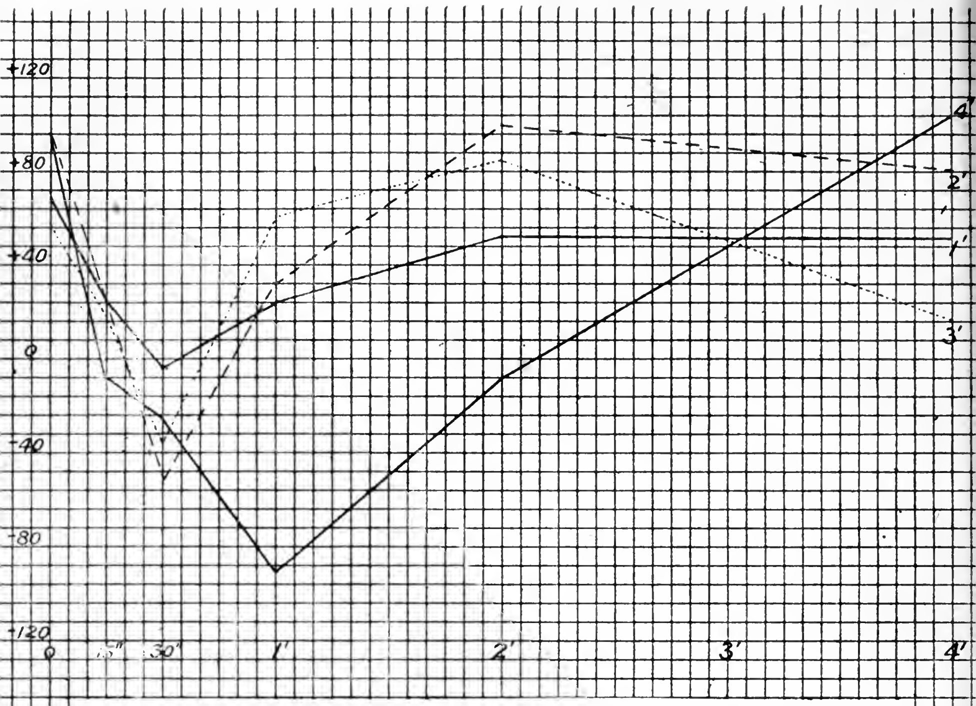


Fig. 9. *Curve of Recovery from Fatigue, obtained by comparing work done in the first period with work done in the last. (Absolute values)*

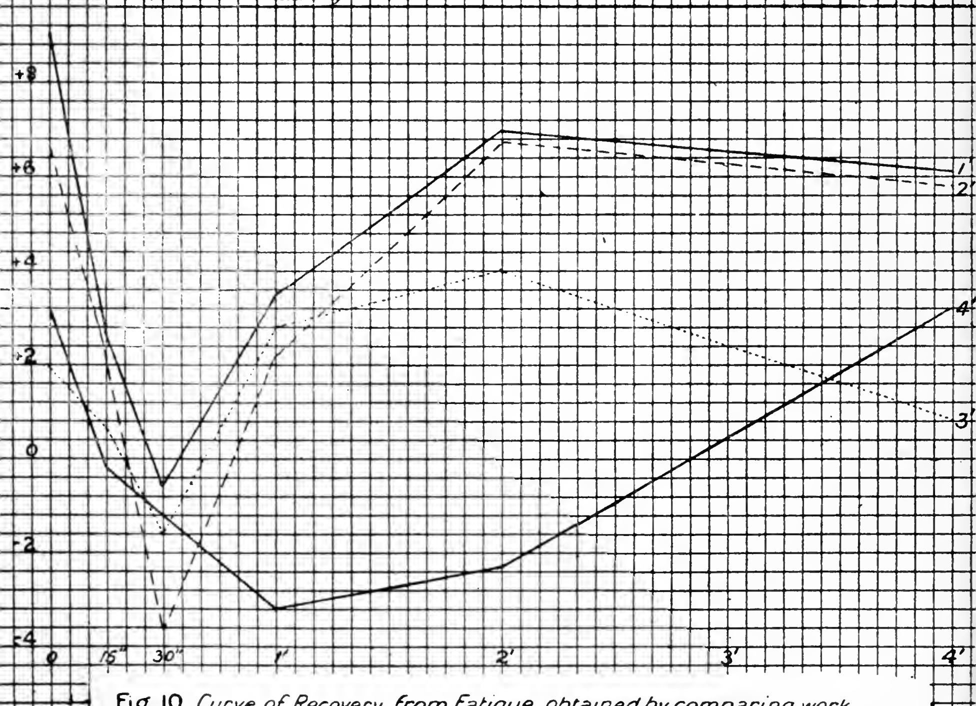


Fig. 10. *Curve of Recovery from Fatigue, obtained by comparing work done in the first period with work done in the last. (Relative Values)*

TABLE 8.—Absolute and relative differences between first and last periods. Multiplication.

Rests.	1-minute Periods.		2-minute Periods.		3-minute Periods.		4-minute Periods.	
	Absolute.	Relative.	Absolute.	Relative.	Absolute.	Relative.	Absolute.	Relative.
...	65	8·93	95	6·60	43	2·01	97	3·44
15 secs.	20	2·62	30	2·09	11	·51	— 9	— ·31
30 ,,	— 4	— ·53	—51	—3·64	—36	—1·71	—25	— ·87
1 min.	26	3·45	33	2·25	61	2·81	—90	—3·23
2 mins.	51	6·75	98	6·67	86	4·00	— 8	— ·29
4 ,,	49	6·16	79	5·77	17	·81	105	3·69
8 ,,	54	6·9	......	......	......	......	49	1·76
16 ,,	—22	—3·1	......	......	......	......	—66	—2·35
32 ,,	......	......	......	......	......	......	—73	—2·58

The curves corresponding to Tables 7 and 8 are given in Fig. 9, and in Fig. 10 the same curves, with the differences between the scores in the first and last periods, relative to the score of the first period. These curves also show a general similarity of shape, though all do not agree with Fig. 8 to the same degree. The curve which departs most from the general shape is that for 4-minute periods; but the method used is a very rough one, and it is rather surprising that the curves exhibit such an agreement as they do. We should expect the results with 4-minute periods to be the least satisfactory, for the amount of fatigue generated *within* each period would be considerably greater than the corresponding amount with 1-minute periods; and, further, the difference between the scores of the first and the last periods would give the effect of only two rests, while with 1-minute periods the corresponding result would be the effect of eleven rests. Therefore the probable error of the mean of the former would be much greater than the probable error of the mean of the latter. The formula for the probable error of a mean is $\frac{\cdot 6745\,\sigma}{\sqrt{n}}$. With 4-minute periods, the denominator of this factor is $\sqrt{2}$, while with 1-minute periods it is $\sqrt{11}$. In other words, random errors will have a much greater influence when there are only two measurements than when there are eleven. Nevertheless, the 4-minute graph shows an obvious resemblance to the other three, as well as to the curve in Fig. 8. Comparing in detail the curves for 1-minute periods given in Figs. 9 and 10 with Fig. 8, we see that in both there is a minimum point at or about the ordinate representing 30 seconds rest, while the succeeding maximum occurs at or about 2 minutes rest. So far the two sets of curves are in complete agreement. Thence in Figs. 9 and 10 there is a general downward trend, but there is no evidence here of the curve becoming horizontal, as we found to be the case with Fig. 8. However, generally speaking, the curves given in Figs. 9 and 10 corroborate the results given in Fig. 8. We have therefore definite evidence for our belief that Fig. 8 represents the true account of the course of recovery from fatigue.

Errors in Multiplication.

Although the number of errors bears such a ratio to the amount of work done as to make unprofitable a separate treatment of the curves for the total amount of work done, as well as those for the amount of work done correctly, yet a short treatment of the errors themselves will perhaps be worth the time spent.

We will first deal with the absolute value of the errors, and then with the number of errors relative to the amount of work done. The total number of errors is given in Table 9.

TABLE 9.—The absolute number of errors with different lengths of rests.

Length of Rest.

Length of Period.	—	15 Secs.	30 Secs.	1 Min.	2 Mins.	4 Mins.	Total.
1 minute ...	175	166	122	142	118	146	869
2 minutes ...	157	154	176	171	138	123	919
3 " ...	200	160	152	143	150	168	973
4 " ...	174	146	184	170	185	172	1,031
Totals ...	706	626	634	626	591	609	......

In Fig. 11 are given the graphs corresponding to the results of Table 9. From the shape of these curves it seems that there is apparently no general regularity in the distribution of these errors for the different lengths of rests. However, the four graphs agree in that they all fall at first, and there is a tendency for the initial point to be the highest point of the curve.

The number of errors seems to have some dependence upon the length of the work-period, for the height of each curve above the horizontal axis is roughly in proportion to the length of the work-period. The curve of error for 1-minute periods is the lowest, while that for 4 minutes is the highest.

If we calculate the ratios of errors to figures correctly multiplied, we get the results given in Table 10, graphs corresponding to which are given in Fig. 12. These curves vary very little in shape from those given in Fig. 11, and therefore a special treatment of them will be unnecessary.

TABLE 10.—Correctness of work with different rests, as shown by the ratio of errors to correct figures.

Length of Rest.

Length of Period.	—	15 Secs.	30 Secs.	1 Min.	2 Mins.	4 Mins.	Average.
	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.
1 minute ...	2·18	1·89	1·37	1·63	1·37	1·62	1·68
2 minutes ...	1·91	1·81	2·01	1·98	1·61	1·47	1·80
3 " ...	2·42	1·89	1·79	1·69	1·78	2·01	1·93
4 " ...	2·10	1·70	2·13	1·99	2·22	2·07	2·04
Totals ...	8·61	7·29	7·30	7·29	6·98	7·17	......

Examining the figures in Table 9, it will be seen that there is a definite progression in the number of errors corresponding to an increase of length in the work-period. The increases are surprisingly uniform, viz., 50, 54, 58, for an increase of 1 minute in the length of the work-period. Expressed as a percentage of the total number of errors, these increments are 5·8 per cent., 5·9 per cent., and 6·0 per cent. We find a similar

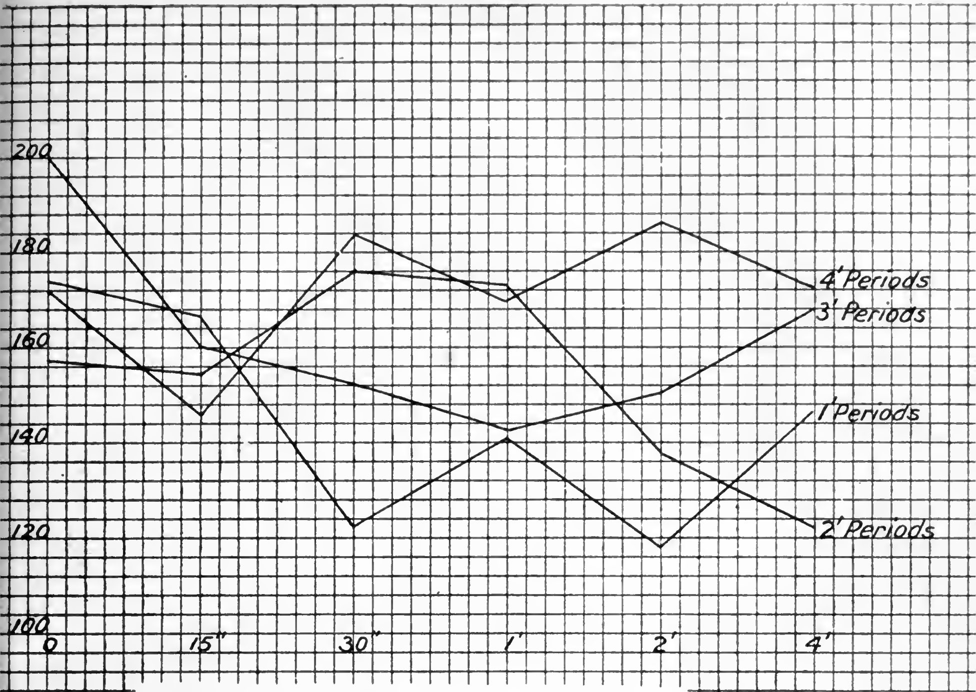


Fig. 11. Absolute number of Errors in Multiplication.

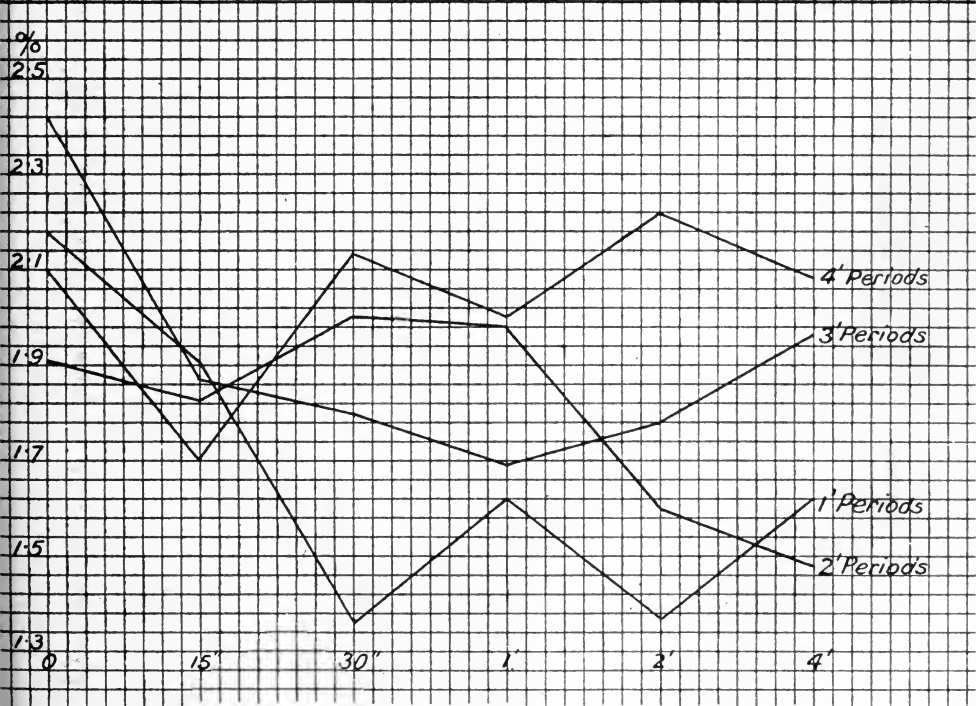


Fig. 12. Errors in Multiplication as percentage of number of correct figures.

result when the number of errors is given relatively to the number of correct figures. In Table 10 the increases in the relative number of errors, for an increase of 1 minute in the length of the work-period, are 1·12, 1·13, and 1·11. To see whether this difference between the number of errors for the different lengths of work-periods is a result of random variations or not, we have calculated the probable errors of the differences between the number of errors for 1-minute periods and each of the remaining periods. The formula for this P.E. is

$$\frac{\cdot 6745\sqrt{\sigma_1^2 + \sigma_2^2 - 2r\,\sigma_1\sigma_2}}{\sqrt{n}}.$$

The results are given in Table 11.

TABLE 11.—Difference between the mean number of errors of each work-period compared with their P.E.'s.

	Difference in Mean Number of Errors.	P.E. of this Difference.
Between one-minute and two-minute periods	8·3	8·4
Between one-minute and three minute periods	17·3	5·6
Between one-minute and four-minute periods	27·0	9·1

These figures show that the difference in the mean number of errors between the 1 and 3-minute work-periods, and the difference between the 1 and 4-minute work-periods are, roughly, three times the P.E. There are about sixteen chances to one against such a result happening from pure chance. For the difference between the means for 1 and 2-minute periods, the P.E. is as large as the difference itself, and this could happen by chance as often as not. Therefore there is a certain degree of probability that the number of errors has some dependence upon the length of the work-period. This probability is augmented by the fact that the number of errors increases so regularly when the work-period is lengthened by constant increments. But this increase in the number of errors is not the result simply of one variable, viz., the length of the work-period. Lengthening the work-period means decreasing the total amount of rest given. For, as only 12 minutes actual work was done each day, lengthening the work-period means decreasing the number of work-periods, and therefore the number of rests. We have shown above that the total amounts of rest with different work-periods, starting with 1-minute, are in the ratio 11 : 5 : 3 : 2 (p. 34). Therefore this result is probably due to the decrease in the total amount of rest as the work-period lengthens, although the mere increase in the length of the work-period may also have some effect upon the number of errors.

Section 2.—Cancellation.

The results given in the preceding section show that the characteristic shape of the curves given there is constant for all four subjects when the task was multiplication. This section of the experiment was devised to determine whether the curves retained the same shape when the nature of the task was changed. Cancellation instead of multiplication was here employed. We have seen that our results are independent of the individual who does the work, but, so far, have not determined whether they are independent of the nature of the task. The plan and results of this section are given in Table 12. It will be noticed that only work-periods of 1 and 4 minutes are here used.

TABLE 12.—Scores for cancellation.

Length of Work-period.	Length of Rest.	Day.	Score.	Day.	Score.	Total Score.
4 minutes ...	....	1	4,391	18	5,611	10,002
4 ,, ...	15 seconds ...	2	4,687	17	5,544	10,231
4 ,, ...	30 ,, ...	3	4,652	16	5,444	10,096
4 ,, ...	1 minute ...	4	4,706	15	5,294	10,000
4 ,, ...	2 minutes ...	5	4,771	14	5,224	9,995
4 ,, ...	4 ,, ...	6	4,944	13	5,191	10,135
4 ,, ...	8 ,, ...	7	4,953	12	5,197	10,150
4 ,, ...	16 ,, ...	8	4,960	11	5,227	10,187
4 ,, ...	32 ,, ...	9	5,086	10	5,180	10,266
1 minute... ...	....	26	5,816	27	5,752	11,568
1 ,, ...	15 seconds ...	25	6,077	28	6,222	12,299
1 ,, ...	30 ,, ...	24	5,833	29	6,163	11,996
1 ,, ...	1 minute ...	23	5,925	30	6,263	12,188
1 ,, ...	2 minutes ...	22	6,018	31	6,105	12,123
1 ,, ...	4 ,, ...	21	5,852	32	6,168	12,020
1 ,, ...	8 ,, ...	20	5,816	33	6,493	12,309
1 ,, ...	16 ,, ...	19	5,858	34	6,585	12,443

On comparing the plan of the work done in this section with that done in Section II, it will be seen that here the subjects were not at the same stage of practice when working with 4-minute periods as when working with 1-minute periods. Thus the scores with the latter periods are considerably higher than with the former. This difference in the stage of practice was caused by the fact that the practice effect with cancellation was found to be more enduring than with multiplication. Originally, the plan of this section was similar to that of Section II, so that the work done with both work-periods would be at the same practice stage. But when the work of the first nine days was corrected, it was found that the practice effect was so marked that it seemed desirable to discard these results. Consequently the work discarded had to be repeated at the end of the original plan, with the result that the work was not all at the same stage of practice. Thus for cancellation, four and a half weeks were devoted to practice before the experiment proper commenced.

To obtain data to enable us to reduce the scores for the 1-minute periods so as to make them comparable with the results for the 4-minute periods, it was necessary to have supplementary work lasting four days. On the first day, continuous work was given with 1-minute periods; on the second day, continuous work with 4-minute periods; and on the third and fourth days, the same work was given in reverse order.

The scores of this supplementary work are given in Table 13.

TABLE 13.—Scores in supplementary work to give data to enable us to reduce the results with 1-minute and 4-minute periods to the same stage of practice.

Day.	Length of Work-period.	Score.	Total Score for 1-minute Work-period.	Total Score for 4-minute Work-period.
1	1 minute	6,023	12,177	12,551
2	4 minutes	6,201		
3	4 ,, ...	6,350		
4	1 minute	6,154		

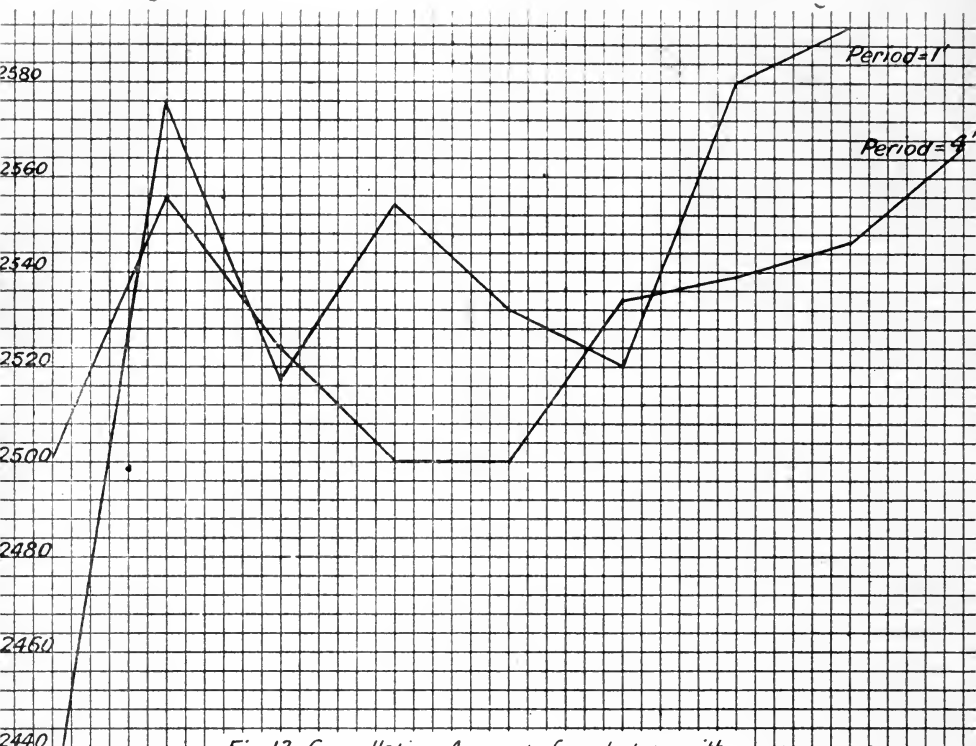


Fig. 13. Cancellation. Amount of work done with varying Rests.

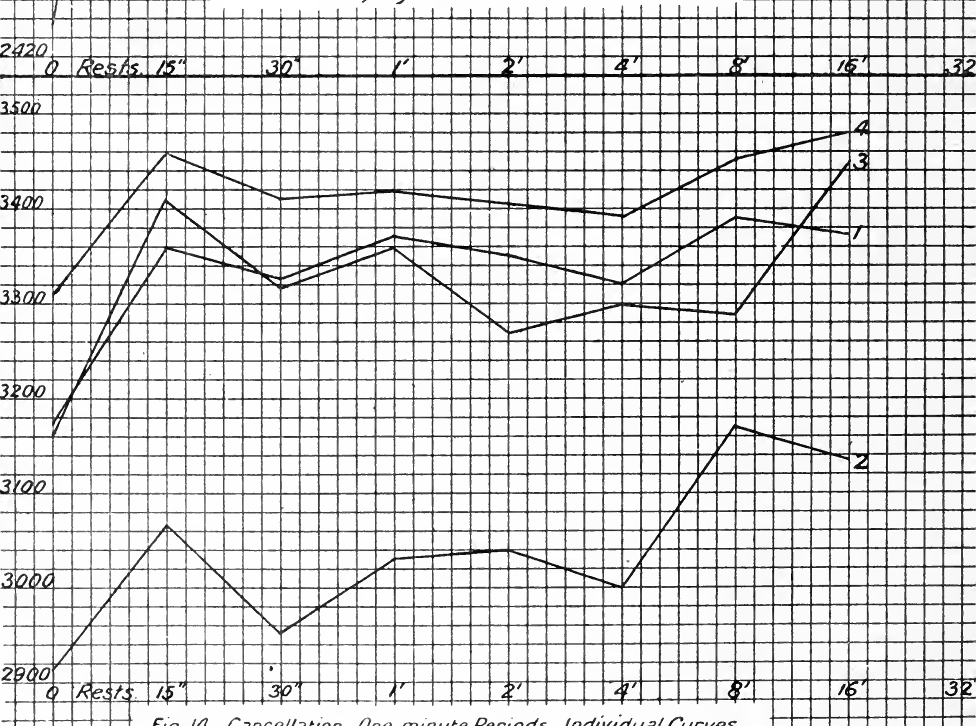


Fig. 14. Cancellation. One-minute Periods. Individual Curves.

Knowing that the scores for 1-minute periods and for 4-minute periods when no rests are given, and the subjects are at the same stage of practice, are in the ratio of 12177 to 12551, we can find what score with 1-minute periods corresponds to 10002, which is the score with 4-minute periods when no rests were given. (*See* Table 12.) On working out, we find this to be 9705. Then, reducing the scores for the other rests proportionately, we get the results given in Table 14.

TABLE 14.—Scores for 1-minute periods, reduced to the same stage of practice as those for 4-minute periods, given in Table 12.

Length of Work-period.	Length of Rest.	Scores.	Length of Work-period.	Length of Rest.	Scores.
1 minute ...	....	9,705	1 minute ...	2 minutes...	10,160
1 ,, ...	15 seconds ...	10,310	1 ,, ...	4 ,, ...	10,080
1 ,, ...	30 ,, ...	10,070	1 ,, ...	8 ,, ...	10,320
1 ,, ...	1 minute ...	10,220	1 ,, ...	16 ,, ...	10,430

The average scores are given graphically in Fig. 13. On comparing these curves with those for multiplication given in Fig. 3, we still find that a maximum point falls between the ordinates marked 15 seconds and 30 seconds, though nearer the former than the latter. Also, we find here the well-marked rise as the rests become longer, although neither curve in Fig. 13 shows any tendency to become linear and parallel to the horizontal axis. Evidently the fatigue induced by cancellation is not dissipated so fast as that induced by the same amount of multiplication under the same experimental conditions. Here rests of 8 minutes are not sufficient to entirely eliminate the fatigue induced by 1 minute of work. We are not able to tell whether 16-minute rests are sufficient to do so, because, not having longer rests than 16 minutes, we cannot tell whether the curve has reached its horizontal stage or not. It will be remembered that the corresponding curve for multiplication had reached its maximum after 4-minute rests. However, it is obvious that the curves given in Fig. 13 belong to the same family as those given in Fig. 3, *and that therefore the curve of recovery when the task is cancellation will be similar to the corresponding curve for multiplication.*

In Fig. 14 and Fig. 15 are given the individual curves from which the average was obtained. All these curves have a maximum at the 15" ordinate and all show the tendency to rise as the rests become longer.

The explanation given of the characteristic shape of the curves in Fig. 3 obviously obtains here also.

It seems clear then that changing the nature of the task does not affect the shape of the curve; and, as shown by the individual curves, this shape is not dependent upon the personality of the subject with either kind of work.

We may use our data in still another way to throw light on these conclusions. If we regard, as does the Kraepelin school, the work done during the first work-period each day as indicating the general disposition for that day, and compare the total amount of work done each day with the amount done in the first period of each day, we should expect the ratio to be greatest for those days on which the most favourable rests were given. Thus we should expect the ratio for the days on which a rest of 15 or 30 seconds was given to be greater than the corresponding ratio when the rests were 1-minute or were omitted. If we work out these ratios and express them graphically, we should expect to obtain a curve similar in shape to those we already have. Further, we should expect this similarity to be most marked when the work-periods were 1-minute and least marked when the work-periods were 4 minutes, because, as there are twelve 1-minute periods in a day's work and only three 4-minute periods, with the former there should be less danger of chance errors, and the fatigue incurred within each period is less.

Of course, this is a very rough method, and by itself not of great value, but the results will act as a check on those conclusions we have stated above, and further, will give an indication whether the stage of practice at which the subjects had arrived had any influence in determining the shape of the curves. For it is obvious that by comparing the total work done on any day with the work done in the first period of that day, any effect, due to the particular stages of practice of the subjects, will be nullified.

These ratios are given in Table 15. As each rest was given on two different days, there will be two ratios corresponding to each rest. The Table gives the mean of these. In Figs. 16–19 these results are expressed graphically to the same scale.

TABLE 15.—Mean ratios of total work done each day to work done in first period.

Length of Work-period.	Length of Rest.	Multiplication.	Cancellation.	Length of Work-period.	Length of Rest.	Multiplication.	Cancellation.
1 minute	......	10·99	11·23	3 mins.	......	3·85	......
1 ,,	15 secs.	11·47	12·14	3 ,,	15 secs.	3·97	......
1 ,,	30 ,,	11·84	11·96	3 ,,	30 ,,	4·02	......
1 ,,	1 min.	11·53	11·96	3 ,,	1 min.	3·90	......
1 ,,	2 mins.	11·42	11·95	3 ,,	2 mins.	3·92	......
1 ,,	4 ,,	11·45	11·89	3 ,,	4 ,,	4·00	......
1 ,,	8 ,,	11·67	12·34	4 ,,	......	2·95	2·95
1 ,,	16 ,,	11·73	12·15	4 ,,	15 secs.	2·97	3·00
2 minutes	......	5·65	......	4 ,,	30 ,,	2·99	3·02
2 ,,	15 secs.	5·92	......	4 ,,	1 min.	3·05	3·04
2 ,,	30 ,,	5·87	......	4 ,,	2 mins.	3·00	3·00
2 ,,	1 min.	5·87	......	4 ,,	4 ,,	2·93	3·01
2 ,,	2 mins.	5·85	......	4 ,,	8 ,,	2·97	2·94
2 ,,	4 ,,	5·83	......	4 ,,	16 ,,	2·98	2·98
2 ,,	8 ,,	5·87	......	4 ,,	32 ,,	3·04	3·01

Comparing Figs. 16–18 with the corresponding curves in Figs. 3 and 13, we again find a decided similarity of shape. The curves given in Fig. 19, viz., those for 4-minute periods, do not exhibit such a marked agreement with the original curves. However, both show an inclination to rise as the rests approach the maximum. We have already remarked that we should not expect to find so great an agreement between the graphs for 4-minute periods as between those for shorter work-periods. With the 4-minute periods there is greater scope for chance errors affecting the result.

The agreement, however, between Figs. 16, 17, 18, and 19, and the corresponding curves in Figs. 3 and 13, shows plainly that the stage of

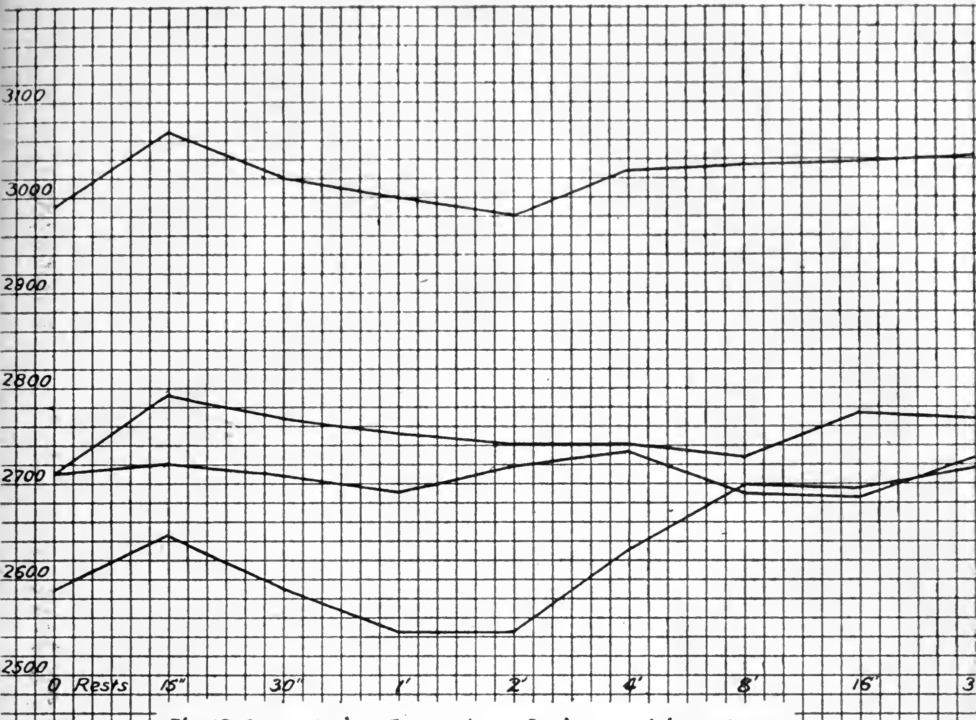


Fig. 15. Cancellation. Four-minute Periods. Individual Curves.

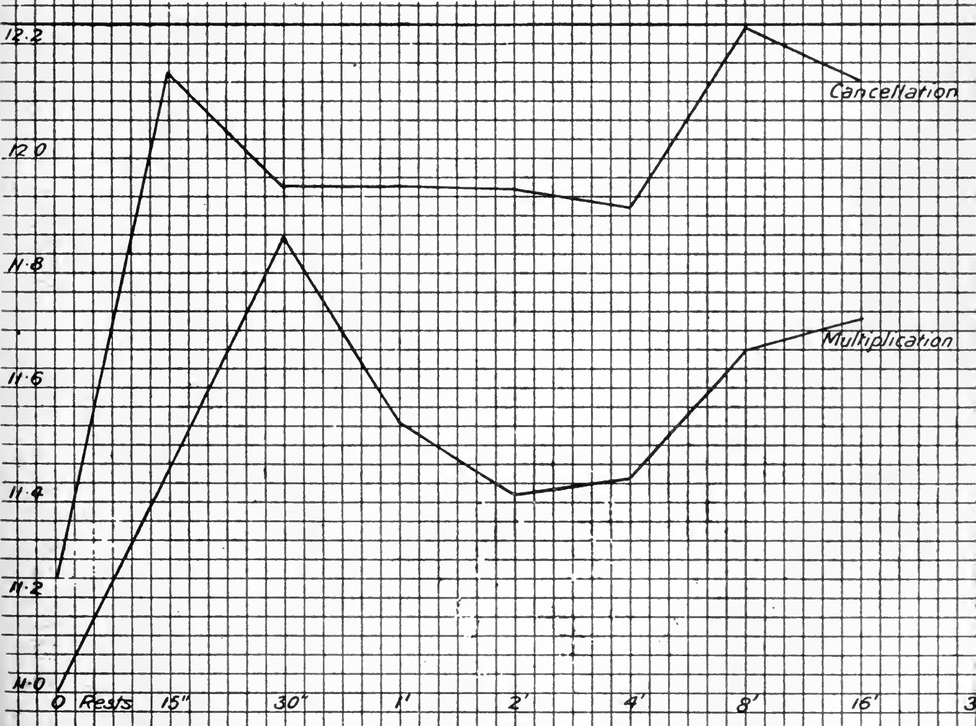


Fig. 16. Work done with one-minute Periods expressed as a Multiple of work done in the first Period.

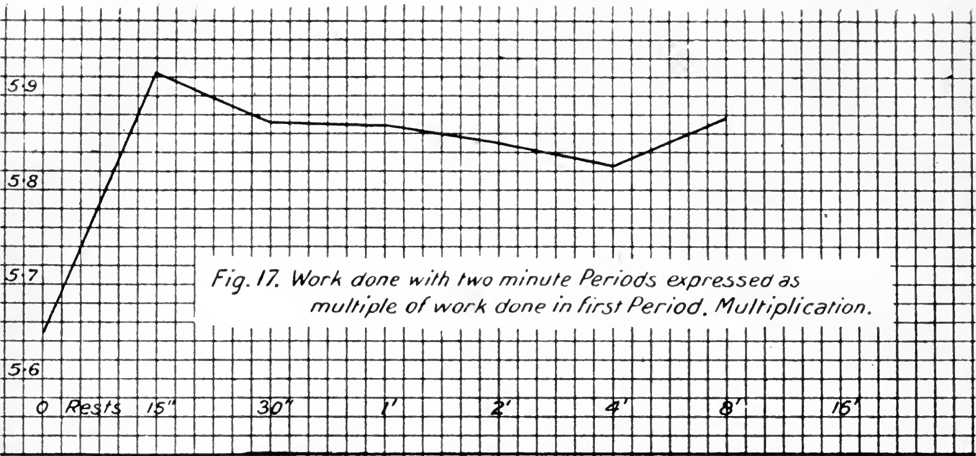


Fig. 17. Work done with two minute Periods expressed as multiple of work done in first Period. Multiplication.

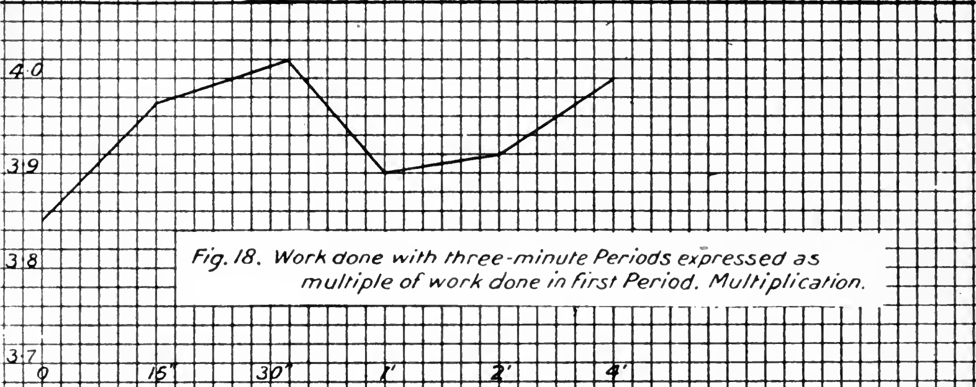


Fig. 18. Work done with three-minute Periods expressed as multiple of work done in first Period. Multiplication.

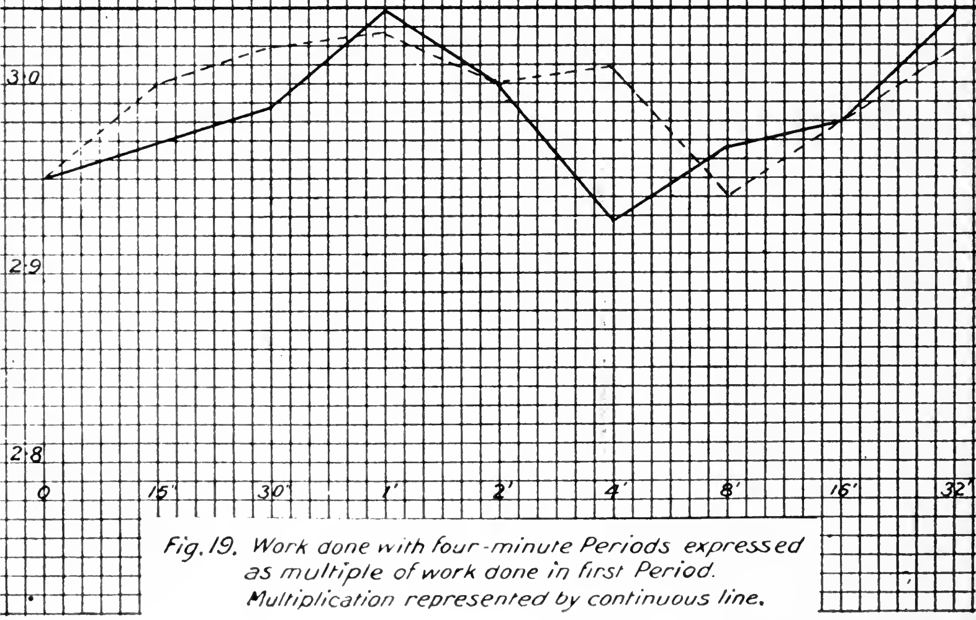


Fig. 19. Work done with four-minute Periods expressed as multiple of work done in first Period. Multiplication represented by continuous line.

practice has no influence upon the shape of these curves, and also corroborates the conclusions we have drawn above. We may here restate them with a slight addition. *The chararteristic shape of the curves given in Fig. 3 is independent of the kind of task done, and also independent of the personality and stage of practice of the subjects.* Of course the subjects used in this experiment were school-boys, but we see no reason for supposing that the same results would not hold good with older subjects.

Effect of the Inclusion of the Scores of the First Period.

At the beginning of each day's work we have assumed that the subjects were unaffected by either incitement or fatigue, and, therefore, the scores of the first work-period each day will not be comparable with those of any other period, for the latter scores will be under the influence of either a rest of a definite length, or a period of work of a definite length, or perhaps of both. Of course, theoretically, the scores for the first work-period each day ought to be constant, for there is no practice effect, and no incitement or fatigue effect, except that generated within the work-period itself. Therefore, including the scores of the first period is equivalent to adding a constant quantity to each day's results. But this added quantity, although theoretically constant, is not so practically. It is affected by the influence of random errors. It seems, therefore, desirable to omit the scores for the first period and to compare the results so obtained with the results already given. In Table 16 are given the scores for multiplication, and in Table 17 the scores for cancellation, omitting the scores of the first work-period.

TABLE 16.—Scores for multiplication, excluding the scores of the first work-period.

Length of Work-period.

Length of Rest,	1 Min.	2 Mins.	3 Mins.	4 Mins.
....	7,271	6,714	6,070	5,426
15 seconds	8,004	7,022	6,288	5,638
30 ,, ...	8,141	7,224	6,313	5,666
1 minute	7,946	7,118	6,234	5,677
2 minutes...	7,855	7,061	6,221	5,500
4 ,, ...	8,208	6,930	6,113	5,410
8 ,, ...	8,199	7,123	......	5,407
16 ,, ...	8,219	......	......	5,581
32 ,, ...	......	......	......	5,649

TABLE 17.—Scores for cancellation, excluding the score of the first work-period.

Length of Work-period.

Length of Rest.	1 Min.	4 Mins.	Length of Rest.	1 Min.	4 Mins
....	10,538	6,619	4 minutes	11,009	6,762
15 seconds	11,286	6,809	8 ,, ...	11,412	6,699
30 ,, ...	10,993	6,753	16 ,, ...	11,419	6,763
1 minute	11,169	6,698	32 ,, ...	......	6,855
2 minutes	11,117	6,653			

The graphs corresponding to these Tables are given in Fig. 20 and Fig. 21. Comparing these graphs with Figs. 3 and 13, in which results the scores of the first work-period are included, it is evident that the inclusion of these scores has not caused any discrepancies, and their exclusion does not make the graphs more regular. Both sets of curves are almost identical in shape. Throughout the rest of this work we shall make use of "inclusive" scores.

Section 3.—*Multiplication and Cancellation Alternated.*

In Sections 1 and 2 we have endeavoured to estimate the effects of fatigue incurred in doing a mental task upon the output of the same work. In this Section we will attempt to investigate the effects of fatigue induced by one kind of work upon the output of another different task. In the preceding sections we have found that the curve representing the amount of work done with varying rests, is a constant, independent alike of the task and of the individual who does the task. The question we will now attempt to answer is whether this curve will still be the same shape if we allow it to be influenced by the fatigue due to two different kinds of work. In other words, if we alternate the task, will the fatigue due to a period of multiplication be transferred to the following cancellation period, or will such a period have the same influence on the succeeding cancellation period as a rest of the same duration?

The current ideas concerning transfer of fatigue are not at all clear. Kraepelin[1] thinks that the work of Weygandt proves that fatigue is a perfectly general phenomenon, and that its effects on one kind of task are completely "transferred" to a succeeding task. It is not at all easy to determine the exact meaning of the word "transferred" in such a connection. Does Kraepelin mean that if two tasks are performed by a number of subjects in immediate juxaposition in time, and in a state of fatigue due to some preceding work, the average performance in the two tasks will be below normal to the same extent? Or does he mean that the depreciations of the subjects in the first test will correlate completely with the depreciation in the second? Only in the latter case, it seems to us, could fatigue be described as perfectly general. Evidence will be brought in Part II of this research to show that the correlations between two such tests is not only not high, but that it is approximately zero.

If Kraepelin's ideas concerning the transferability of fatigue are obscure, most other writers are still more vague. Arai,[2] for example, says "that fatigue in a special mental function, as well as in a general, is slightly transferable to other functions." She evidently means, judging from her method of experimentation, that there is a slight tendency for fatiguability in one function to be associated with fatiguability in another different function, for the subjects tested, the fatigue in each case being due to the same preceding work. But we have seen (p. 23) that the mathematical evidence on which this conclusion is based is unsound.

The most widespread assumption has been that the depreciation in one task is a measure of the fatigue induced by a different task. That is, the absolute generality of fatigue has been taken for granted. Such an error has crept into the work of even Thorndike.[3]

[1] Kraepelin, E. Die Arbeitscurve. Phil. Stud., Vol. 19, 1902., p. 479.
[2] Aria, T. Mental Fatigue. Teachers' College, Columbia Univ. Contribs. to Ed. No. 54. New York. 1912, p. 115.
[3] Thorndike, T. Mental Fatigue. Psych. Rev., Vol. 7, 1900, p. 547.

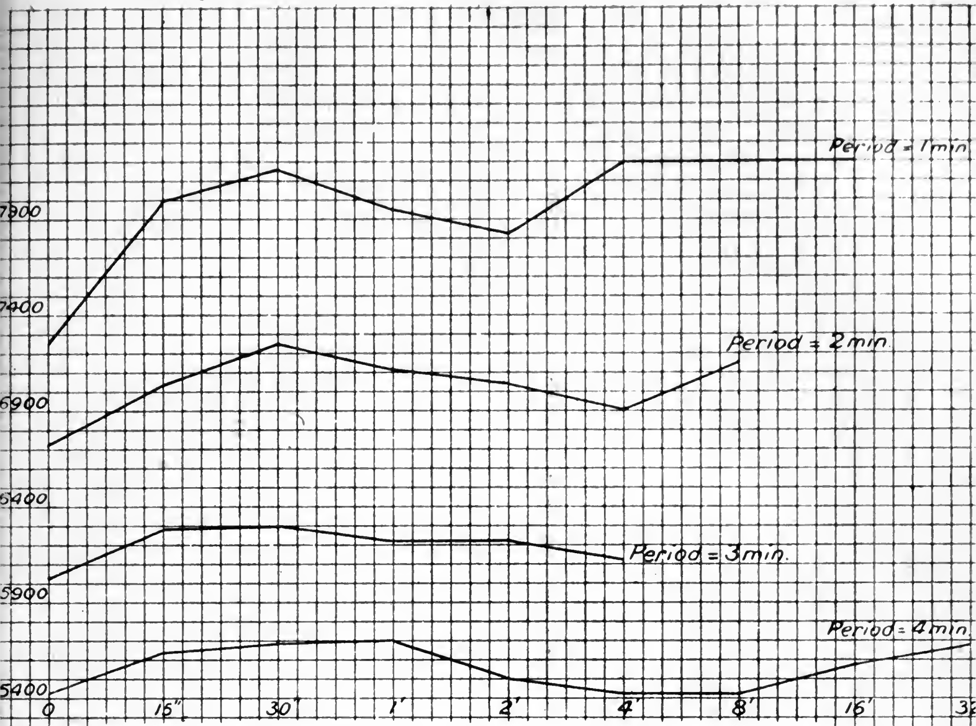


Fig. 20. Multiplication, excluding scores of first Period.

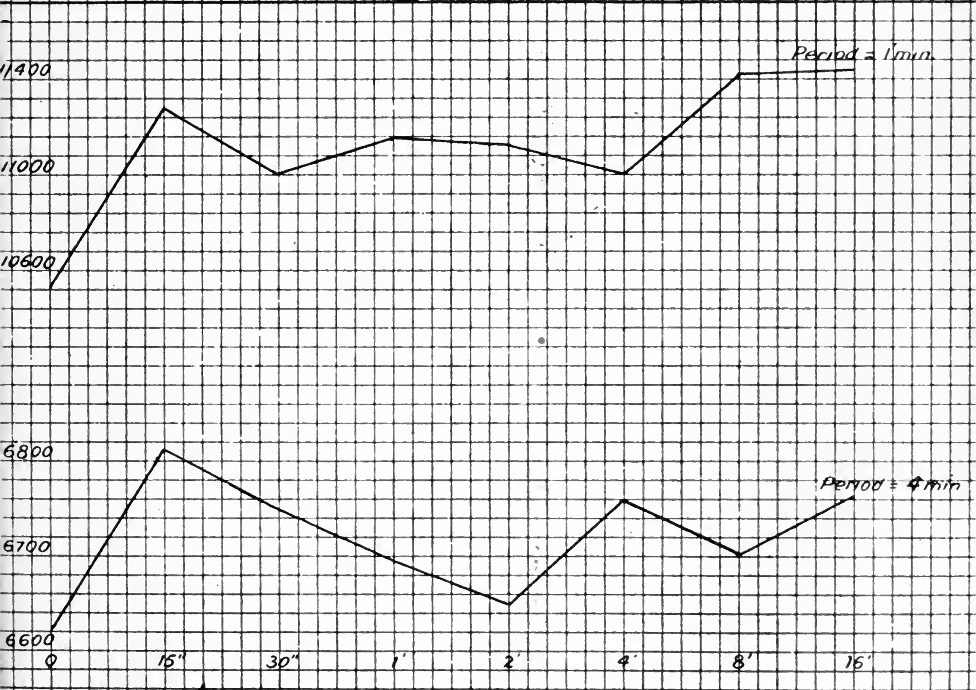


Fig. 21. Cancellation, excluding scores of first Period

Seashore[1] holds that fatigue is quite specific, and that therefore the fatigue incurred by performing one mental task has no effect upon a succeeding different task. The work of Ellis and Shipe[2] appears to support such a view. These investigators found no correlation between the depreciation in different tasks after the same fatiguing work.

Spearman and Hart[3] have reduced "intelligence" to two factors, a perfectly general factor and a factor extremely specific. These two factors are absolutely distinct and separate—one does not shade off into the other. It is quite possible that a similar state of things may exist for fatigue. Part of the fatigue incurred in performing a mental task may be general—that is to say, it may be wholly transferred from one task to any other task—and part may be quite specific, and therefore not transferable at all to a different task. Also there may be no degrees between these two factors in fatigue. The two factors may be quite independent.

Flügel[4] has found a fatigue, which is strictly specific, in his investigations upon the illusions of reversible perspective. He found that some subjects may be fatigued for one aspect of the figure attended to while apparently unfatigued for another aspect. There are also great individual differences in the amount of this fatigue. Some individuals are fatiguable for one aspect of the figure much more than for the other aspect, and in some subjects very little fatigue at all is manifested. This fatigue is not affected by the simultaneous activity of other mental processes.[5]

McDougall[6] has also demonstrated the existence of a fatigue which is extremely specific in his observations upon binocular rivalry.

There is still another view which may be held regarding fatigue. It may be both general and specific, and one may shade off into the other. If the fatigue is slight, it may affect any succeeding work of the *same* kind, but have no influence upon a *different* task to any appreciable extent. The fatigue induced by a particular task may be localised in a particular area of the cortex if its degree be slight. Further work of the same kind may extend the area of the cortex fatigued, and so affect other mental processes which are localised within the sphere of influence. This extension of the fatigued area may, with still more work, affect the whole cortex, and every mental task performed will then be adversely influenced by it. This extension of the area which is at first definitely localised may be compared to the ripples on a lake caused by a stone. If the stone is small, the ripples affect only a small part of its surface, but if the size of the stone be increased in gradations, the extent of the surface of the lake affected by the ripples is increased until, if the stone is large enough and the lake small enough, its whole surface may be thrown into movement. On this view, then, specific fatigue may extend until it becomes general.

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[1] Seashore, C. E. The Experimental Study of Mental Fatigue. Psych. Bull., 1904, p. 97.

[2] Ellis and Shipe. A Study of the Accuracy of Present Methods of Testing Fatigue. A.J.P. 1903, Vol. 14, p. 223.

[3] Spearman and Hart. The Theory of Two Factors. Psych. Rev., Vol. 21, 1914.

[4] Flügel, J. C. Some Observations on Local Fatigue in Illusions of Reversible Perspective. Brit. Journ. Psych., Vol. 6, 1913, p. 60.

[5] Op. cit., p. 75.

[6] McDougall, W. The Physiological Factors of the Attention Process. Mind. Vol. 15, 1906, p. 329.

It was for the purpose of testing these possible theories of fatigue that the plan of this part of the work was devised.

For this section we have used 1-minute and 4-minute work-periods, with the rests of the same length as in the preceding sections; but we have alternated a period of multiplication with a period of cancellation throughout the whole series. The first period each day was always multiplication. For example, when the period was 1-minute and no rests were given, the subjects would work at multiplication for 1-minute and then at a given signal change to cancellation for 1-minute and then back to multiplication, and so on. When rests were given, the procedure was similar.

When two tasks were given alternately, and the work was continuous, a certain amount of time was lost at the end of each period in changing from one sheet to the other. This would tend, if not counteracted, to make these scores lower than when rests were given. To make the loss constant for all periods, at the beginning of each period the subjects placed their pencils upon and directed their eyes towards the last letter cancelled, or figure multiplied, as the case might be, in the previous period. Then when the signal to commence was given, they, as quickly as possible, changed to the other sheet and began to work. Thus, at the commencement of every period except the first, this amount of time, which we presume to be constant, was lost.

This experiment continued for 30 days, the subjects being the same as those for Section 2. Owing to some of the boys wishing to leave London for a holiday, the series was curtailed from 34 days to 30, the two longest rests being omitted.

In calculating the results, the scores for each kind of work were kept separate.

A Plan of this work is given in Table 18, and the results are given in Tables 19 and 20.

TABLE 18.—Plan of work for Section 3.

Day.	Length of Work-period.	Length of Rests.	Day.	Length of Work-period.	Length of Rests.
1	1 minute ...	....	16	4 minutes ...	16 minutes.
2	1 ,, ...	15 seconds.	17	4 ,, ...	8 ,,
3	1 ,, ...	30 ,,	18	4 ,, ...	4 ,,
4	1 ,, ...	1 minute.	19	4 ,, ...	2 ,,
5	1 ,, ...	2 minutes.	20	4 ,, ...	1 ,,
6	1 ,, ...	4 ,,	21	4 ,, ...	30 seconds.
7	1 ,, ...	8 ,,	22	4 ,, ...	15 ,,
8	4 minutes ...	....	23	4 ,, ...	....
9	4 ,, ...	15 seconds.	24	1 minute ...	8 minutes.
10	4 ,, ...	30 ,,	25	1 ,, ...	4 ,,
11	4 ,, ...	1 minute.	26	1 ,, ...	2 ,,
12	4 ,, ...	2 minutes.	27	1 ,, ...	1 minute.
13	4 , ...	4 ,,	28	1 ,, ...	30 seconds.
14	4 ,, ...	8 ,,	29	1 ,, ...	15 ,,
15	4 ,, ...	16 ,,	30	1 ,, ...	....

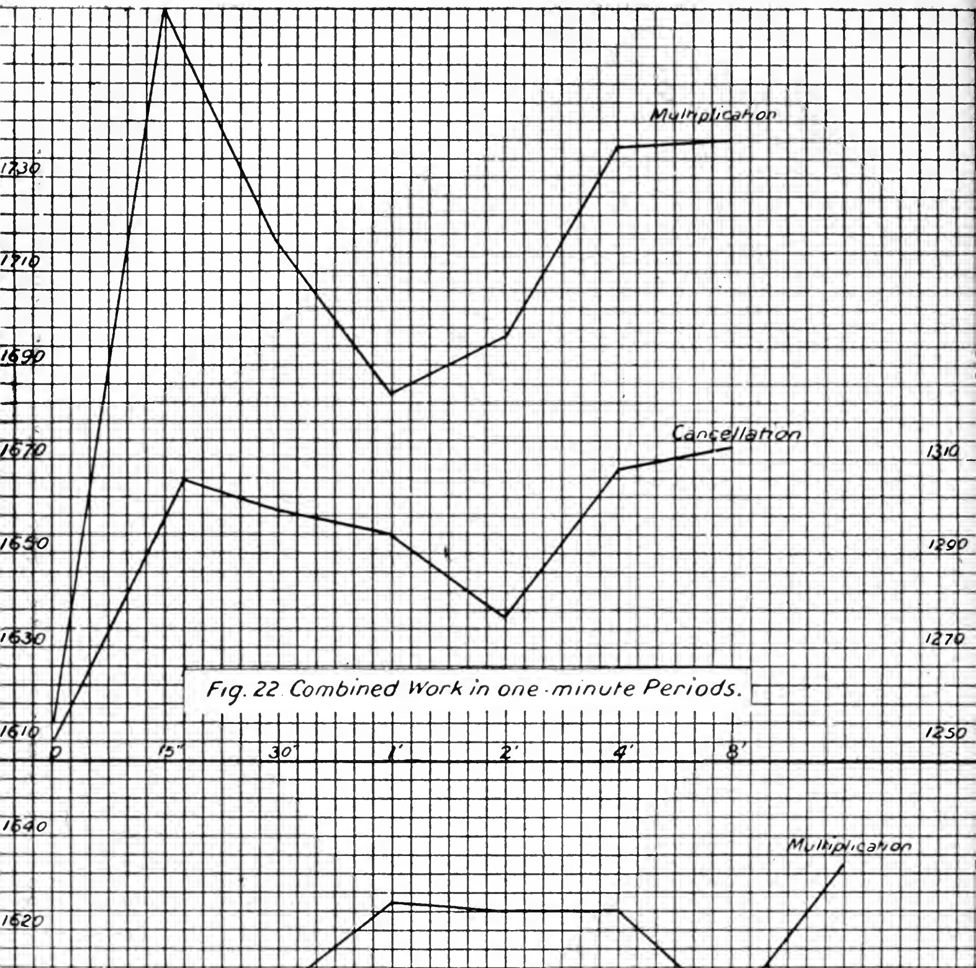

Fig. 22. Combined Work in one-minute Periods.

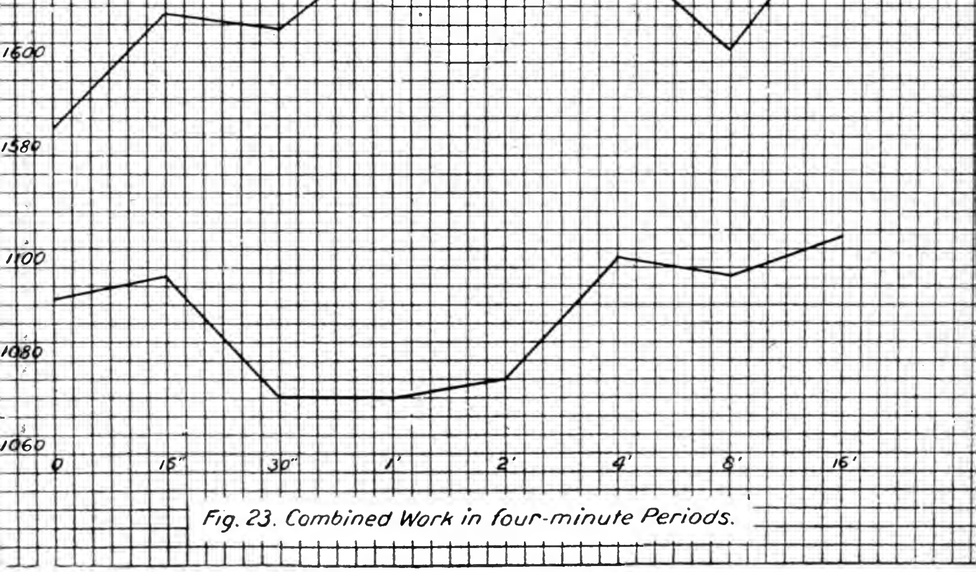

Fig. 23. Combined Work in four-minute Periods.

TABLE 19.—Multiplication scores when the two tasks were alternated.

Length of Work-period.	Length of Rest.	Multiplication.		
		1st time.	2nd time.	Total.
1 minute	....	2,152	2,851	5,003
1 ,, ...	15 seconds	2,203	3,019	5,222
1 ,, ...	30 ,, ...	2,297	2,911	5,208
1 ,, ...	1 minute	2,267	2,914	5,181
1 ,, ...	2 minutes	2,310	2,798	5,108
1 ,, ...	4 ,, ...	2,381	2,849	5,230
1 ,, ...	8 ,, ...	2,451	2,796	5,247
4 minutes	....	2,907	3,440	6,347
4 ,, ...	15 seconds	2,989	3,451	6,440
4 ,, ...	30 ,, ...	3,031	3,395	6,426
4 ,, ...	1 minute	3,070	3,439	6,509
4 ,, ...	2 minutes	3,113	3,382	6,495
4 ,, ...	4 ,, ...	3,165	3,330	6,495
4 ,, ...	8 ,, ...	3,178	3,236	6,414
4 ,, ...	16 ,, ...	3,237	3,303	6,530

TABLE 20.—Cancellation scores when the two tasks were alternated.

Length of Work-period.	Length of Rest.	Cancellation.		
		1st time.	2nd time.	Total.
1 minute	....	3,133	3,344	6,477
1 ,, ...	15 seconds	3,348	3,788	7,106
1 ,, ...	30 ,, ...	3,322	3,553	6,875
1 ,, ...	1 minute	3,164	3,574	6,738
1 ,, ...	2 minutes	3,353	3,431	6,784
1 ,, ...	4 ,, ...	3,432	3,513	6,945
1 ,, ...	8 ,, ...	3,416	3,532	6,948
4 minutes	....	2,059	2,314	4,373
4 ,, ...	15 seconds	2,114	2,275	4,389
4 ,, ...	30 ,, ...	2,098	2,189	4,287
4 ,, ...	1 minute	2,093	2,195	4,288
4 ,, ...	2 minutes	2,168	2,140	4,308
4 ,, ...	4 ,, ...	2,191	2,221	4,412
4 ,, ...	8 ,, ...	2,213	2,178	4,391
4 ,, ...	16 ,, ...	2,191	2,236	4,427

When the work-period was 1-minute there were six periods in which multiplication was done and six periods in which cancellation was done. But when the work-period was 4-minutes two periods were devoted to multiplication and only one to cancellation; and as the first period each day has no work preceding it there is only one period of each kind of work which is under the influence of fatigue. Therefore we should hardly expect the results with 4-minute periods to be so marked as those with 1-minute periods. And this is what we actually find in Figs. 22 and 23, where the scores are expressed graphically. In Fig. 24 we have given the curves of the ratios expressing the amount of work done as a fraction of the amount done in the first period.

The curves in Fig. 24 for 1-minute periods roughly corroborate the curves in Figs. 22 and 23, where the first period is included. The curve for multiplication with 4-minute periods varies so little from the straight line that no comparison of it with the curves, including the first work-period, is possible. As there was only one period of cancellation when the period was 4 minutes, we cannot give a curve for it with this method.

Discussion of the Results of Section 3.

If fatigue were a perfectly general phenomenon, as Kraepelin would have us believe, we should expect that change of work would have no influence on its results. Whatever the task, the fatigue incurred ought to affect the succeeding tasks to the same extent. Change of work, we know, affects the subjective fatigue sensations, but we are here still considering fatigue objectively. On this view there ought to be no differences between the curves representing the amount of work done with varying rests, constructed from the results given in the three sections of this part of our research, whether the whole work is of the same kind as in Sections 1 and 2, or whether the two tasks are alternated as in Section 3. Take, for example, the work of the first day, when the work is continuous, and a period of cancellation follows a period of multiplication. If the general theory is correct, the fatigue effect upon the period of cancellation, caused by the preceding period of multiplication, ought to be the same as when that preceding period was cancellation also. If, on the other hand, fatigue is perfectly specific, and its effects not transferable from one task to a different task, we should expect to find that a period of the first kind of work, in its effects upon the succeeding period of different work, to have the same influence as a rest of the same duration. For example, when multiplication for 1 minute is alternated with cancellation for 1 minute, and no rest intervenes, we should expect the amount of work done, if the general theory were correct, to be comparable with the amounts of work done in corresponding periods when the tasks were not alternated. That is, we should expect the amount of multiplication done to be approximately equal to the total amount done in the odd-numbered periods when the whole twelve periods were multiplication. Similarly, in the combined work we should expect the amount of cancellation to be approximately equal to the total amount done in the even-numbered periods when the twelve periods were wholly devoted to cancellation. In other words, if the subjects worked at maximum speed on both tasks, we should expect the curve representing the amount of work done with different rests to be independent of the task when both tasks were given alternately. But, if the specific theory were correct, and if multiplication for 1 minute is alternated with cancellation for 1 minute, with no rests, we should expect the results to be comparable with the results obtained by giving multiplication and cancellation separately with intervening rests of 1 minute. For, on this view, multiplication for 1 minute would have the same effect on the succeeding minute of cancellation as a rest of 1 minute; and, conversely, the period of cancellation would have, in its influence upon the succeeding period of multiplication, the same effect as a rest of the same length. There is still a third possibility to be considered, viz., that the fatigue effect may be both general and specific. That is to say, part of its effect may be "transferred" to any succeeding task, while part is specific to the mental function exercised, and is not transferred to a different task. If this is so, when the work is continuous and the two tasks are alternated, the period of multiplication which precedes a period of cancellation will act as a period of cancellation as far as the general factor is concerned, while it will be equivalent to a rest from the point of view of the specific factor.

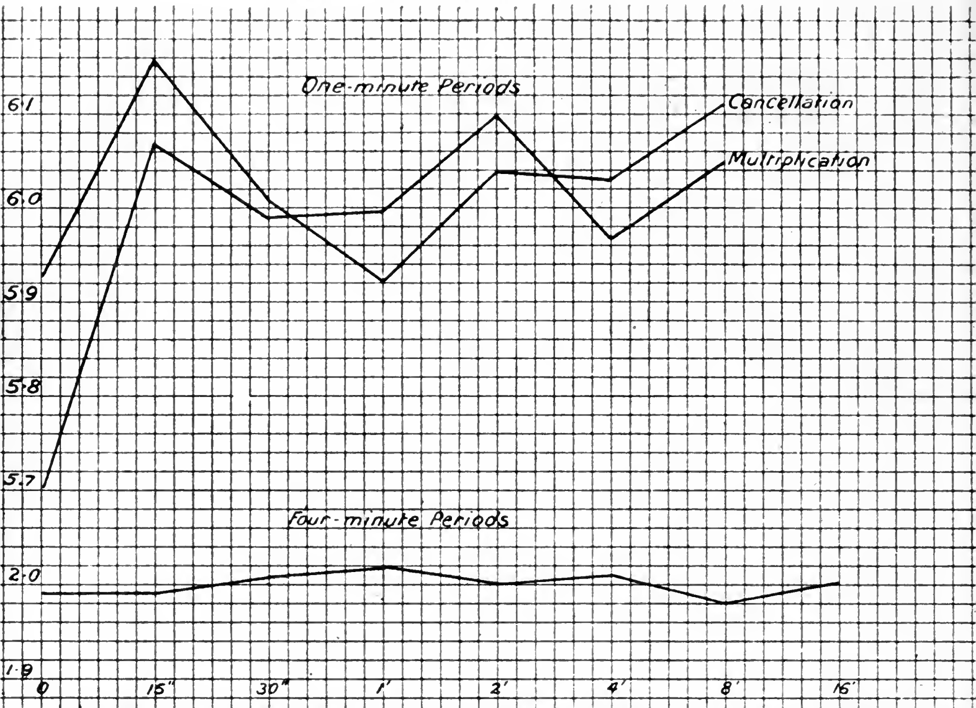

Fig. 24. Combined Work expressed as Multiple of work done in first period.

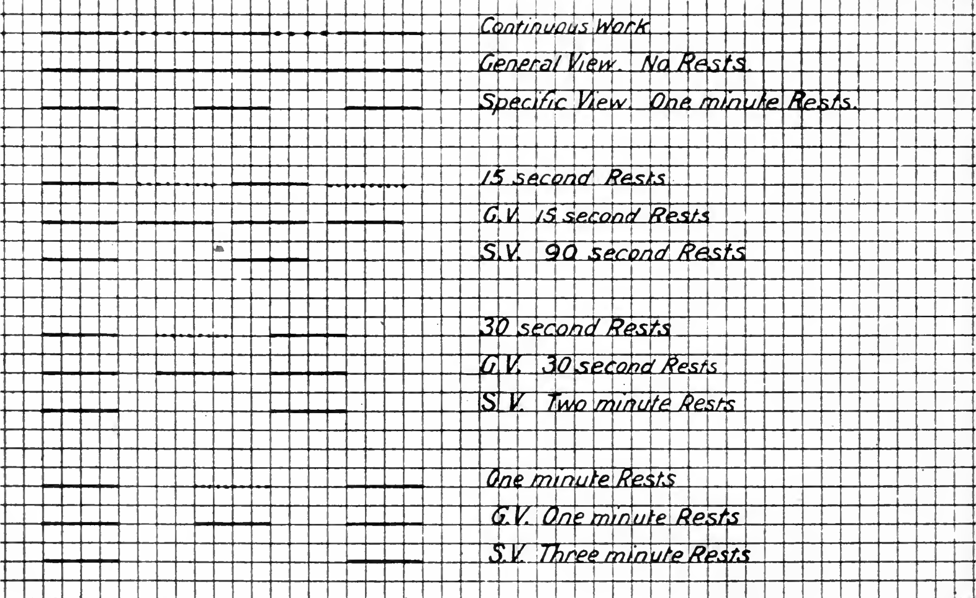

Fig. 25.

In judging the validity of each of these three theories, it seems possible to make use of the constant curves obtained in Sections 1 and 2 as a criterion.

From the results given in Tables 19 and 20 we can construct two sets of curves corresponding to the two fatigue theories, which we have called the general theory and the specific theory. If one of these sets shows a general conformation to the constant curves given in Sections 1 and 2, while the other set does not, we can conclude that the theory represented by the former is the correct one. But, if, on the other hand, neither set conforms to this constant shape yet both show certain similarities, we can take it as an indication that probably fatigue is both general and specific.

From the point of view of the general theory, 12 minutes alternated multiplication and cancellation will have the same effect as if the work were homogeneous, and therefore we can construct the curves representing this theory straight from Tables 19 and 20 without further calculation. The only difference between these curves and those of Sections 1 and 2 is that the former are the result of fewer work-periods, because now only half the 12 minutes work is of the same kind. The curves representing the general theory are given in Figs. 22 and 23. We shall have more difficulty in constructing similar curves for the specific theory. First of all it is obvious that the first point on all the curves already given will be absent from the specific theory curves. For when the tasks are alternated, there must, on this view, always be some period of rest. There can be no continuous work unless the work is homogeneous.

Let us first take the figures for 1-minute periods in Tables 19 and 20. The first total score in each table represents the work done with no actual rests, but from our particular point of view there were virtual rests of 1-minute, for each minute period of one task would have the same effect on the other task as a rest of one minute. One minute then is the smallest rest with whose effects we shall deal. The second total score is the amount of work done with 15 seconds actual rest between each work-period. On the specific theory there would here be a virtual rest of 1½ minutes between each work-period, for between each two periods of one kind of task there would come (1) a rest of 15 seconds, (2) a 1-minute work-period of the other task, (3) another rest of 15 seconds, making a virtual rest of 1½ minutes altogether. Similarly the virtual rests corresponding to all the other actual rests could be determined. Stated in general terms, if the length of the work-period be A, and the actual rest B, then the virtual rest according to the specific view would be A + 2B. Thus an actual rest of 4 minutes becomes a virtual rest of 12 minutes when the work-period is 4 minutes. In Fig. 25 is given an illustration of the actual rests with their corresponding virtual rests.

Tables 19 and 20 cannot give us the scores for virtual rests less than 1 minute in the case of 1-minute work-periods, nor the virtual rests less than 4 minutes in the case of 4-minute work-periods. For shorter rests than these a supplementary experiment was devised, whose plan is given in Table 21. In this series there were no actual rests, and periods of work took the place of the intervals of rest. When the work being tested was multiplication, the interpolated task was cancellation, and *vice versa*. If fatigue is specific, these interpolated tasks will have the same effect as rests of the same duration.

TABLE 21.—Plan of supplementary experiment for interpolated work.

Day.	Length and Nature of Work-period.	Length and Nature of Interpolated Work.	Day.	Length and Nature of Work-period.	Length and Nature of Interpolated Work.
1	1 min. cancell.	15 secs. mult.	10	4 mins. mult....	2 mins. cancell.
2	1 ,, mult. ...	15 ,, cancell.	11	4 ,, ,, ...	1 min. ,,
3	1 ,, cancell.	30 ,, mult.	12	4 ,, ,, ...	30 secs. ,,
4	1 ,, mult. ...	30 ,, cancell.	13	4 ,, ,, ...	15 ,, ,,
5	1 ,, ,, ...	1 min. ,,	14	1 min. ,, ...	1 min. ,,
6	4 ,, ,, ...	15 secs. ,,	15	1 ,, ,, ...	30 secs. ,,
7	4 ,, ,, ...	30 ,, ,,	16	1 ,, cancell.	30 ,, mult.
8	4 ,, ,, ...	1 min. ,,	17	1 ,, mult. ...	15 ,, cancell.
9	4 ,, ,, ...	2 mins. ,,	18	1 ,, cancell.	15 ,, mult.

When doing this supplementary work, the subjects were not at the same stage of practice as when doing the main experiment, so that it was necessary to give continuous alternated work with 1-minute periods to obtain data to enable us to bring the scores for this work to the same stage of practice as the scores in Tables 19 and 20. The results of the supplementary work are given in Table 22.

TABLE 22.—Scores in the supplementary experiment reduced to the same stage of practice as those in Tables 19 and 20.

Length of Work-period.	Length of Interpolated Work.	Multiplication.	Cancellation.
1 minute	15 seconds ...	4,818	6,373
1 ,, ...	30 ,, ...	4,850	6,605
4 minutes	15 ,, ...	6,308	....
4 ,, ...	30 ,, ...	6,373	....
4 ,, ...	1 minute ...	6,381	....
4 ,, ...	2 minutes ...	6,348	....

With the results given in Tables 19, 20, and 22, it is now possible to construct curves representing the amounts of work done with varying rests if fatigue is strictly specific, corresponding to the curves of Figs. 19 and 20, which represent the general theory.

Let us first take the scores for multiplication with 1-minute periods. We have already pointed out that the point on the graph, representing the amount of work done when no rests are given, will be missing. The scores for 15 seconds rests and 30 seconds rests, viz., 4,818 and 4,850, will be obtained from Table 22, and the scores for longer rests from Table 19. The score for virtual rests of 1 minute will be 5,003, which is the score when the actual rest is zero. The score for 2-minute virtual rests will be 5,208, the score when the actual rests are 30 seconds. The score for 4-minute virtual rests cannot be obtained directly from Table 19. In this table, when the actual rests are 1-minute and 2 minutes, the corresponding virtual rests will be 3 minutes and 5 minutes respectively. If we take the average of these two scores, viz., 5,181 and 5,108, we will get an approximate value for the score with virtual rests of 4 minutes, viz., 5,145. Actual rests of 4 minutes and 8 minutes correspond to virtual rests of 9 minutes and 17 minutes; and the scores for these rests, viz., 5,230 and 5,247, roughly correspond with actual rests of 8 minutes and 16 minutes. In a precisely similar manner from Tables

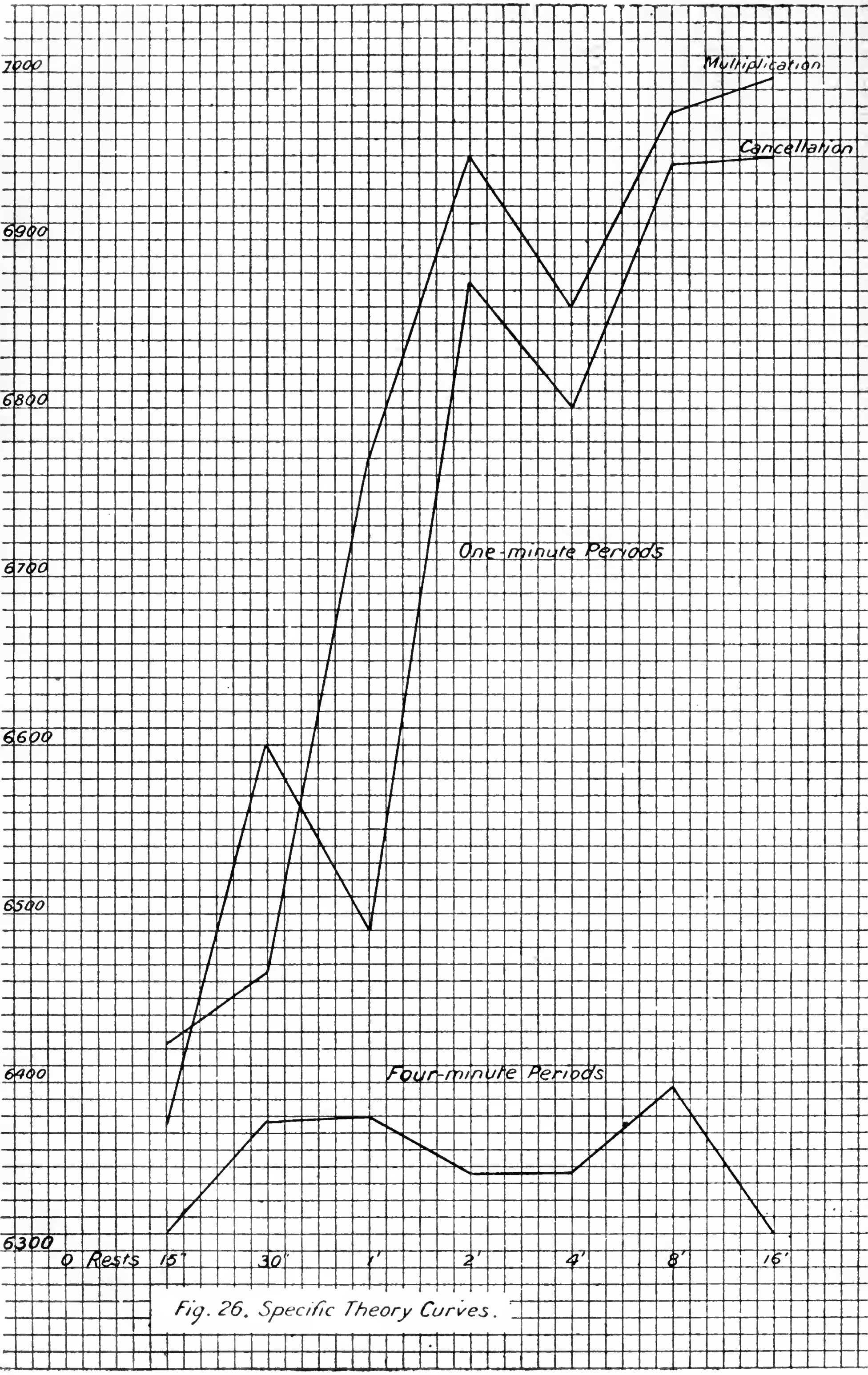

Fig. 26. Specific Theory Curves.

20 and 22 we can obtain the corresponding scores when the task is cancellation and the work-period 1 minute.

When the work-period is 4 minutes, and the task multiplication, we can obtain the scores for virtual rests less than 4 minutes from Table 22, and for rests of 4 minutes and longer from Table 19. The score for actual rests of 2 minutes, 6,495, will be the score with virtual rests of 8 minutes. The score for virtual rests of 16 minutes will be obtained by taking the mean of the scores for actual rests of 4 minutes and 8 minutes, which correspond to virtual rests of 12 minutes and 20 minutes These scores are 6,495 and 6,414, giving as the mean 6,455.

It will not be possible to give the corresponding results when the task is changed to cancellation, for, as there is only one work-period, there can be no interpolation of periods of multiplication. Therefore we can construct only three curves representing the specific theory, viz., two for multiplication and one for cancellation.

In Table 23, all the scores representing the specific theory are collected, and in Fig. 26 are given the corresponding curves, which are the curves for the amounts of work done with varying rests if the specific theory were correct.

TABLE 23.—Scores according to the specific view of fatigue.

Length of Rest.	Multiplication.		Cancellation.	
	1-min. Periods.	4-min. Periods.	1-min. Periods.	4-min. Periods.
15 seconds	4,818	6,308	6,373	....
30 ,, ...	4,850	6,373	6,605	....
1 minute	5,003	6,381	6,477	....
2 minutes	5,208	6,348	6,875	....
4 ,, ...	5,145	6,347	6,761	....
8 ,, ...	5,230	6,495	6,945	....
16 ,, ...	5,247	6,455	6,948	....

As in Sections 1 and 2, we can construct the curves when the first period, which has no work preceding it, is omitted. In this section the cancellation is always preceded by a period of multiplication, but the first period of multiplication is uninfluenced by any preceding work. Omitting the scores of the first period of multiplication we have Tables 24, 25, and 26, which correspond to Tables 19, 22, and 23 when these scores are included.

TABLE 24.—Scores in multiplication when the score of first work-period is excluded. This Table corresponds to Table 19, which gives the scores including the score in the first work-period.

Length of Rest.	Length of Work-period.	
	1 minute.	4 minutes.
....	4,125	3,156
15 seconds	4,364	3,208
30 ,, ...	4,338	3,239
1 minute	4,319	3,288
2 minutes	4,277	3,241
4 ,, ...	4,364	3,261
8 ,, ...	4,381	3,270
16 ,, ...	....	3,261

TABLE 25.—Scores in multiplication supplementary experiment, excluding work done in first work-period. This Table corresponds to Table 22, which includes the work done in the first work-period.

Length of Rest.	Length of Work-period.	
	1 minute.	4 minutes.
15 seconds	4,105	3,143
30 "	4,099	3,207
1 minute	....	3,240
2 minutes	....	3,239

TABLE 26.—Scores in multiplication on the specific view, excluding scores of the first work-period. This Table corresponds to Table 23, which includes scores of the first work-period.

Length of Rest.	Length of Work-period.	
	1 minute.	4 minutes.
15 seconds	4,105	3,143
30 "	4,099	3,207
1 minute	4,125	3,240
2 minutes	4,338	3,239
4 "	4,298	3,156
8 "	4,364	3,241
16 "	4,381	3,265

The graphs given in Fig. 27 are the curves for multiplication, excluding the first work-period, and are derived from the figures given in Table 26. It will be seen that they are similar to those given in Fig. 26. Therefore we are justified in holding that the inclusion of the scores for the first work-period does not introduce random errors which affect the curves to any marked or general extent.

We now proceed to a comparison of the three sets of curves obtained. Figs. 3 and 13 give the curves when the work was homogeneous. We will refer to these as the "normal" curves. Figs. 22 and 23 give the curves constructed in accordance with the theory that fatigue is general. We will call these the "general" curves; and Fig. 26 gives the curves constructed in accordance with the specific theory. We will call these the "specific" curves.

We will first compare Figs. 3 and 13 with Figs. 22 and 23, *i.e.*, the normal curves with the general curves. We will deal, in the first place, with the curves for multiplication, *i.e.*, Fig. 3 and Figs. 22 and 23.

In discussing in Section 1 the shape of the normal curve we found three main characteristics. There was a tendency for a maximum point to occur with 15 or 30 seconds rest. This was followed by a minimum point, whose position varied with the length of work-period. Finally the graph again rose and became horizontal. These three points are all more or less definitely present in Fig. 22 and 23. In the curves for 1-minute periods we find a maximum point occurring with rests of between 15 and 30 seconds; a minimum point with about 2 minutes rest; and finally there is a tendency for the curve to assume a horizontal position. The curve for 4-minute periods does not exhibit such an agreement with

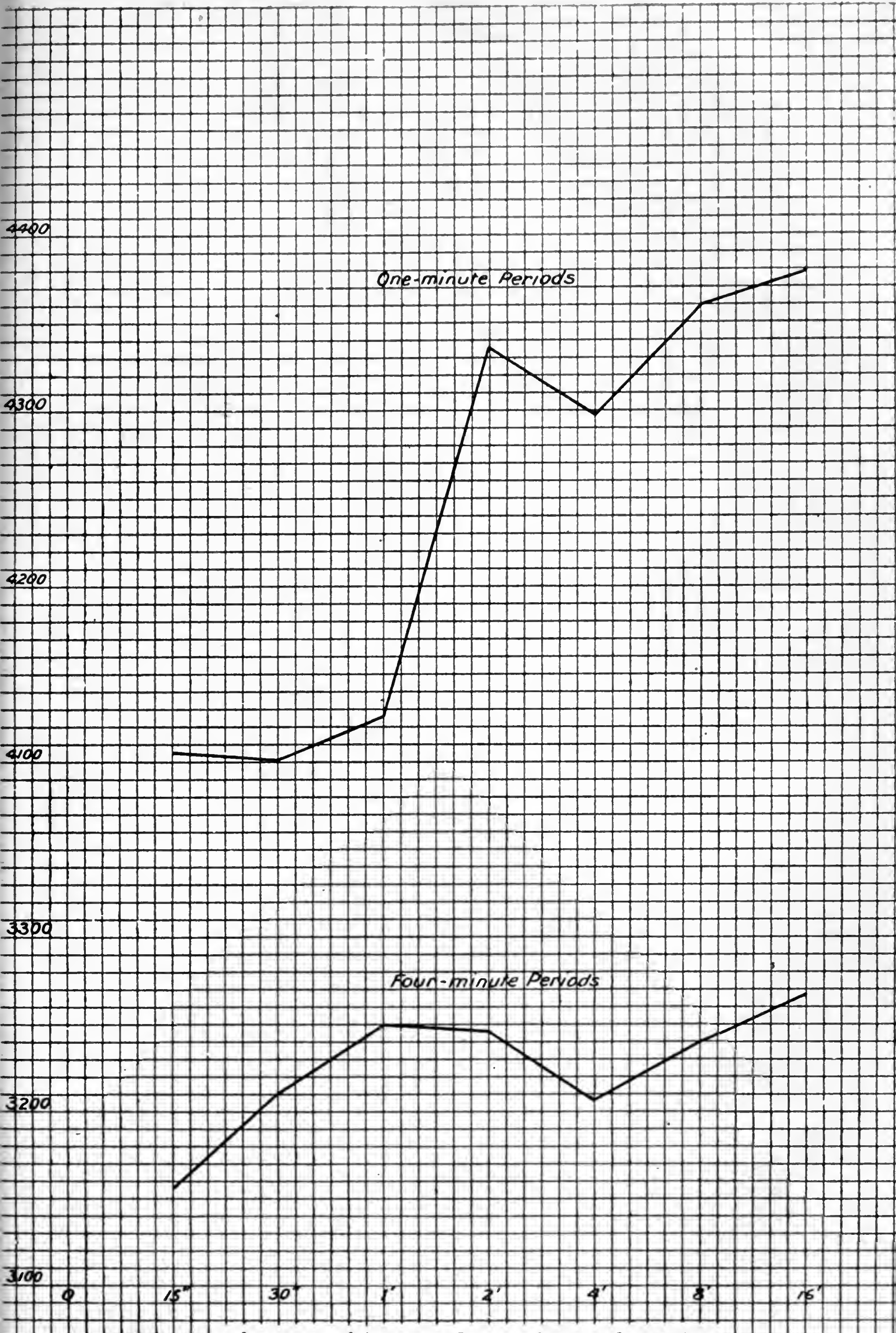

Fig. 27. Specific Theory Curves for Multiplication, excluding scores of first Period.

the normal curve. We have already given our reasons for placing more reliance upon the shorter work-periods. Here we have an additional reason, for, as only two periods are devoted to multiplication, only one is under the influence of any preceding work or rest.

Now let us take the curves for cancellation, with 1-minute work-periods, *i.e.*, Figs. 13 and 22 and 23. Here, in the general curve, there is a well marked maximum with 15 seconds rest, followed by a minimum point between 1 and 2 minutes rest, after which the curve rises rapidly, and shows signs of becoming horizontal, although this characteristic was not so evident in the normal curve for this task, Fig. 13. The 4-minute curve for cancellation is much more regular than the corresponding curve for multiplication, and agrees very well with the normal curve. We find a maximum point with 15 seconds rest, followed by a minimum point with about 1 minute rest, and then the curve again rises, though much more gradually than the corresponding curve for the 1-minute periods.

Speaking generally, and giving more weight in our judgment to the curves with shorter work-periods, we can conclude that the general curves exhibit a remarkable agreement with the normal curves. The only unsatisfactory curve is that for multiplication with 4-minute rests, and we have given reasons for believing that this curve is influenced to a greater degree by random variations than the curves with 1-minute work-periods.

Now let us take the specific curves and compare them with the normal curves, *i.e.*, Fig. 26 with Figs. 3 and 13.

In Fig. 26, the curves for 1-minute periods of both tasks show a general similarity, while that for 4-minute periods is quite dissimilar from the other two, and, owing to there being only one work-period under the influence of preceding work or rest, it deviated very little from a horizontal line. It will be best, perhaps, to confine our attention to the 1-minute curves and compare them with the corresponding normal curves.

At a first glance there seems very little agreement between the two sets of curves, and this is borne out by a more detailed comparison. In the specific curves we find no maximum point with rests of 15 seconds. In fact, the scores with rests of this length are the lowest of all; and this is what one would expect if the general theory were the true one, for on this theory virtual rests of 15 seconds, 30 seconds, and 1 minute are actually not rests at all. It is only when the virtual rests are lengthened to 2 minutes that the specific curves rise abruptly. Both curves show an almost continuous rise; and there is also no indication of the minimum with 2 minutes rest, which we found present in all the normal curves. Neither curve shows any tendency to become horizontal with rests of 4 minutes and longer. From these considerations we conclude that the curves constructed according to the specific theory show no points whatever of agreement with the normal curves. This is true of both tasks, and for all lengths of work-periods.

To sum up, we find that the general curves exhibit every characteristic which is common to the normal curves, while the specific curves show no such agreement. Therefore, we are forced to the conclusion that the general theory of fatigue is the correct one. Further, there is no indication in these results that fatigue is both general and specific. The agreement of the normal curves with the general curves, and their disagreement with the specific curves, seem to be absolute

Possible Effect of Interruptions caused by changing the Nature of the Task.

In Sections 1 and 2 the nature of the task was constant throughout the section. In Section 1 the task was always multiplication, and in Section 2 it was always cancellation. In this Section we have a change of work—an interruption of one task by another different task. We have not yet considered the possibility of the influence of this change of work. Its influence, if any, will be more marked when the work-period is 1 minute than when it is 4 minutes, because, in the former, the interruptions are more numerous; and it remains to be seen whether the sum of the effects of these interruptions is sufficiently large to affect the shape of the curves obtained. We have several possibilities :—

1. Fatigue may be specific, and then the interruption will cause a loss of fatigue effect.
2. The incitement may also be specific, and then there will be a loss of incitement effect.
3. Fatigue may be general. There will then be no loss of fatigue, and the curves obtained will not be different from the curves obtained when the two tasks were not alternated.
4. Incitement may be general, and the succeeding task will be benefited, just as when the two tasks were taken separately.
5. The interruption may also act as a distractor. The perseveration of the preceding task may then have an adverse influence upon the task being performed.

Let us take for an example the case in which there is no actual rest, *i.e.*, when a period of one kind of work immediately succeeds a period of different work. This is the simplest case, as it is not complicated by the effects of rests, although the argument in each case is the same. We know that the curves obtained when the tasks are alternated are the same shape as when the tasks are not alternated, *i.e.*, the changed work acts as work of the same kind. We are therefore faced with two alternatives—(1) Either the gain or loss of efficiency caused by the interruption acting as a distractor, the gain caused by the loss of fatigue, and the loss caused by the loss of incitement exactly balance one another. In such a case we have three influences which react upon efficiency in such a way that their net result is always either zero or a constant. (2) Or the effect of the interruption is approximately zero or a constant, and fatigue and incitement are general. In this case the effect of the interruption is either approximately zero, or else it affects the scores with all rests to the same extent, and therefore does not affect the shape of the curve.

The first alternative is certainly outside the bounds of probability, unless some reason can be advanced why the net result of these three influences, interruptions, incitement, and fatigue, should always be either zero or equal to a constant for all lengths of actual rests. We can see no reason why this should be so, especially when we consider that the length of rest between the alternated periods is not constant. Therefore we are forced to assume that the second alternative is the correct one, viz., that the effect of the interruptions is zero or a constant, and both the incitement and fatigue effects are general, and are transferred to the succeeding task.

That the interruption effect should always be constant and not zero seems to us highly improbable, for if it were appreciable at all, it ought to be affected by the varying lengths of rests. The interruption effect upon the second of two tasks which immediately followed one another ought certainly to be greater than when the two tasks were separated by an interval of time. For the effect would diminish with the length of the rest, or else one would never recover from the perseveration effect of previous work. We are led then to conclude that the influence of the interruptions is not appreciable. This result agrees very well with that obtained by Weygandt, that the effects of changes of work are almost always small.

Effects of Varying Lengths of Work-periods and Rests.

Amberg,[1] Rivers and Kraepelin,[2] Hylan and Kræpelin and Heüman,[3] among others, have investigated the effects of the varying lengths of work-periods and rests upon efficiency. It will, perhaps, be profitable and interesting to see how far the results of this present investigation bear out their conclusions.

Amberg, Rivers and Kraepelin, and Heüman are all agreed that the length of the most favourable rest depends upon the length of the preceding work-period. If we take Figs. 3 and 13, and compare the curves there for the different work-periods, it is noticeable that the curve for 1-minute periods in Fig. 3 is, if we exclude the first point whose divergence we have already accounted for (p. 31), wholly above the curves for longer periods. The ordinates here represent the net amount of work done, *i.e.*, the number of figures multiplied, or letters cancelled, correctly. Therefore for each of the rests given here the efficiency is greatest when the work-period is 1-minute, and, speaking generally, the longer the work-period, the smaller are the net scores. In Fig. 3 the curve for 3-minute work-periods seems to be an exception to this. In Fig. 13, the curves for cancellation, we get a similar result. The curve for 1-minute periods is generally above the curve for 4-minute periods, excluding the first point on the graph, although the difference here seems to be less than when multiplication was the task.

We can find mathematically the effect of each length of work-period by comparing the work done in the last period of each day with the work done in the first period.

If we take the difference between the scores of the first and last periods, when no rests at all are given, as the total effect of the fatigue induced, and subtract from this the difference between the scores of the first and last periods, when a rest is given, it will give the amount of the fatigue abolished by the rest, expressed as a gain in efficiency. Then having found the amount of fatigue abolished by the rest, we can divide by the number of separate rests, *i.e.*, 11, 5, 3, and 2, according to the length of the work-period, and so find the effect of each separate rest. We can express this effect either absolutely, or relative to fatigue effect, when no rests are given.

In Table 27 we give the figures expressing this result for multiplication. It will be seen that the effect of the rest, both absolutely and relatively,

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[1] Amberg, E. Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit. Psych. Arb., Vol. 1, 1895, p. 300.

[2] Rivers, W. H. R., and Kraepelin, E. Ueber Ermüdung und Erholung. Psych. Arb., Vol. 1, 1896, p. 627.

[3] Heüman. Ueber die Beziehungen zwischen Arbeitsdauer und Pausenwirkung. Psych. Arb., 1904, Vol. 4, p. 538.

increases with the length of the work-period. The exceptions to this statement are the figures for work-periods of 3 minutes.

TABLE 27.—Multiplication. Taking the difference between first and last periods with *no* rest as indicating the total amount of fatigue induced, the average loss of fatigue and incitement effect for each separate rest is as below.

Rest.	1-minute Periods.		2-minute Periods.		3-minute Periods.		4-minute Periods.	
	Absolute.	Relative.	Absolute.	Relative.	Absolute.	Relative.	Absolute.	Relative.
...	......	......	......	......	......	......	......	......
15 sec.	4·1	·57	13	·90	10·7	·50	53	1·87
30 ,,	6·3	·86	29·2	2·05	26·3	1·24	61	2·15
1 min.	3·5	·50	12·4	·87	6·	—·27	93·5	3·33
2 mins.	1·3	·20	—·6	—·14	—14·3	—·66	52·5	1·86
4 ,,	1·5	·25	3·2	·19	20.	·40	3·	·10
Average of all rests.	3·3	·47	11·4	·77	7·3	·24	52·6	1·86

In Table 28 are given the corresponding figures for cancellation.

TABLE 28.—Cancellation. Taking the difference between the first and last periods with *no* rest as indicating the total amount of fatigue induced, the average loss of fatigue and incitement effect for each separate rest is as below.

Rest.	1-minute Periods.		4-minute Periods.	
	Absolute.	Relative.	Absolute.	Relative.
....	......	......	......	......
15 seconds	6·2	·88	21·5	2·94
30 ,, ...	7·4	1·06	0·0	0·0
1 minute	7·1	1·00	41·0	5·12
2 miutes	7·4	1·06	—14·5	—1·81
4 ,, ...	2·5	·36	—2·5	— ·31
Average of all rests ...	6·3	·87	9·1	1·19

Here we also find that the loss of fatigue is more marked when the work-period is 4 minutes, both relatively and absolutely. The results of this research, therefore, agree with those of the writers mentioned above in showing that the favourable effect of a rest depends upon the length of the preceding period of work. But does the length of the *most* favourable rest depend upon the length of the work-period ?

In Fig. 3 we have seen in the curve for 1-minute periods that there are two rests which are more favourable than any of the others. One occurs with a rest of between 15 seconds and 30 seconds, and the other with a rest of 4 minutes or longer, the latter of which is the most favourable of all. We have also seen that the former is constant for all lengths of work-period within the limits of this investigation. It does not seem to have any dependence on the length of work-period, or upon the kind of task, for it is constant for both multiplication and cancellation. This maximum point appears to be due more to the conservation of the incitement effect than to the abolition of the fatigue effect, for we notice that with rests of 1 minute the curve again declines. We must assume that a rest of 1 minute eliminates more fatigue than a rest of 30 seconds, therefore with the latter rest there must be a much greater conservation of incitement than with the former. None of the writers referred to above used work-periods less than 5 minutes in length, and only one, Heüman, used rests less than 5 minutes in length. In this case the shortest rest was 1 minute. None of their results, therefore, would show this constant maximum point occurring with 15 to 30 seconds rest.

The point of absolute maximum efficiency, which we have determined only for the curve with minute periods of multiplication, Fig. 3, does, however, appear to depend upon the length of the work-period. From the general trend of the graphs in Figs. 3 and 13 it seems safe to conclude that the longer the work-period, the longer the interval of rest that is necessary to obtain this maximum efficiency; and this is the point to which these writers refer when they say that the most favourable rest depends upon the length of the work-period. At this point, as we have already remarked, the loss of incitement and fatigue is at a maximum, *i.e.*, with rests of this duration the subject completely recovers from the effects of the fatigue induced by the previous period of work. We would expect this point to depend upon the length of the work-period, for the fatigue due to a longer period of work would not be entirely abolished by a rest whose length was sufficient to eliminate the fatigue due to shorter period of work.

By comparing, in the curves in Fig. 3, the lengths of rests at which the relative minimum point occurs, we can get an indication of the position of the point of maximum efficiency on each. With 1-minute periods this minimum point occurs with a 2-minute rest, and with 2-minute periods with a rest of 4 minutes, while a rest of 8 minutes is necessary in the case of 4-minute periods. If the position on the graph of this minimum point is dependent upon the length of the work-period, the probability is that the succeeding maximum will show a similar dependence; and we have seen in the case of minute periods that it is an absolute maximum.

Heüman further concludes that the effect of rest is greater the longer it is. The rests used by him were 1, 5, and 15 minutes. This is not at all borne out by the results of this research, for we have seen that a very short rest may be more beneficial than a longer one.

Summary of Part I.

1. The graph expressing the amount of mental work done with varying lengths of rests interpolated between the work-periods is of constant shape. It is independent both of the task and the subject who does the task. The beneficial effect of a rest is not proportional to its length. Of all the shorter rests, one of 15 seconds is the most advantageous for work-periods up to 4 minutes in length. A rest of this duration is apparently long enough for some of the fatigue to be dissipated and short enough to conserve much of the incitement effect. The most advantageous rest of all is one that is long enough to dissipate completely both fatigue and incitement. This rest is not of constant duration for all work-periods, nor is it strictly proportional to the length of the work-period. With multiplication, using a period of 1 minute, this

rest must be at least 4 minutes, *i.e.*, a rest of 4 minutes completely removes both the fatigue and incitement incurred by multiplying for 1 minute at maximum speed. Using 4-minute work-periods, 16 minutes rest is insufficient to obtain this result. When the task is changed to cancellation, 8 minutes rest is not sufficient to obviate the effects of 1 minute of work.

2. From the curve showing the amount of work done with varying lengths of rest, we can obtain, by inverting it, the curve of recovery from fatigue. As the former curve is of constant shape, the latter will also be independent of both subject and task. This curve falls abruptly at first, then rises, and finally falls, gradually becoming less abrupt until it becomes horizontal.

3. By alternating the two tasks, and constructing the curves showing the efficiency with varying lengths of rests for each task, it is possible to use this constant curve as a standard by which to judge whether fatigue is general or specific. The curves constructed according to the "general" view exhibit a marked agreement with the standard curve, while those constructed according to the "specific" view show no such agreement. From this we conclude that fatigue is a general phenomenon, and is not specific to the task being performed.

4. The favourable effect of a rest depends upon the length of the preceding period of work; and the length of the most favourable rest also is dependent upon the length of the work-period.

Part II.—Transference of Fatigue.

1. Aim and Description of Experiment.

(*a*) *Aim of Experiment.*

The aim of this experiment was to see whether, and to what extent, fatigue is transferable from one mental function to another. Bettman,[1] Weygandt,[2] Miesemer,[3] Winch,[4] Arai,[5] and Martin[6] all conclude that fatigue is transferable, while Thorndike and Seashore deny it.

The experiment was carried out during the years 1915 and 1916 at the Psychological Laboratory of University College, London. Owing to the necessity of having the subjects for six consecutive days, the

[1] Bettman, S. Ueber die Beeinflussung einfacher psychol. Vorgange durch körporliche und geistige Arbeit. Psych. Arb. Vol. 1, 1894, p. 152.

[2] Weygandt, W. Ueber den Einfluss des Arbeitswechsels auf forlaufende geistige Arbeit. Psych. Arb. Vol. 2, 1897, p. 118.

[3] Miesemer. Ueber psychische Wirkungen körporlicher und geistiger Arbeit. Psych. Arb. Vol. 4, 1902, p. 375.

[4] Winch, W. H. Mental Fatigue in Day School Children. Brit. Journ. Psych. Vol. 4, 1911, p. 315.

[5] Arai, T. Mental Fatigue. Teachers' Coll., Columbia Univ., Contribs. to Ed. No. 54. New York, 1912, p. 9.

[6] Martin, C. W. Evidence of Mental Fatigue during School Hours. J. of Ex. Ped. Vol. 1, pp. 39 and 137. Vol. 3, p. 61.

work was done during the vacations of the schools from which the subjects were drawn. With the preliminary practice work this was not necessary, as this work could be done after school hours.

(*b*) *The Subjects.*

The subjects were all boys, drawn from the two highest classes in public elementary schools, and were 42 in number. Their ages ranged from $11\frac{1}{2}$ to $13\frac{1}{2}$ years. The schools from which they were drawn were situated in one of the poorer districts of London.

(*c*) *The Tests.*

1. *Multiplication Test.*—Simple multiplication was used for the first test. The multiplicand consisted of combinations of the figures 3 to 9 inclusive, each figure occurring 5 times in each line, and the multipliers were 6 to 9 inclusive. To make the test of equal difficulty each day, the same multipliers and multiplicand were used in the two testings on any one day, but were changed every day. The subjects were instructed to multiply as quickly as possible without "carrying," and only the units figure of the answer was written down. The test lasted 5 minutes, divided into 5 minute periods. In correcting, one mark was given for each figure correctly multiplied. As the same figures were used in the two testings on any one day, there will be a special practice effect on this day, which we assume to be of the same magnitude as on the control days.

2. *Spots Test.*—In this test, which we have called the "Spots" test, a series of groups of spots were momentarily thrown on a sheet by means of a lantern, with a photographic shutter in front of the lens. The exposure was too short to allow of eye movements, being 35-40 σ. The subjects were required to say how many spots were seen, and to do so fixated a mark placed in the centre of the sheet. The spots were not arranged in any definite order, and varied from 4 to 11, inclusive, in number. Each group was exhibited four times in each testing, thus giving 32 answers. In the second testing each day the order was reversed. In correcting, one mark was given for each group correctly answered.

3. *Memory Test.*—The method used here was that of "paired associates." Twenty-four words, arranged in pairs, were exhibited a pair at a time, by means of Muller's memory apparatus. Each pair was exposed three times, and then the first word of each pair was given, the subjects having to supply the second word; then the second word of each pair was given and the subjects were required to give the first. The order of reading out was kept constant, but different from the order of exposure, and ten seconds were allowed for writing each answer. The words used were all common monosyllabic words, and words usually associated were not given in the same list.

As these lists of words could not be chosen, so as to be of equal difficulty for each day, half the subjects did the test before any fatigue-producing work, which the other half did after that work. Then, when the experiment was repeated, the order of the tests was again reversed for each subject.

One mark was given for each correct answer.

4. *Cancellation Test.*—The matter of the test consisted of pages of French words (none of the subjects were acquainted with French).

Each line consisted of about 60 letters (± 3) and in each line there were 9 letters to be crossed out or words to be underlined.

The matter for the two tests on any one day was constant, but varied from day to day. For each group of subjects there were three tasks to do, *e.g.*, to cross out every E and T, and to underline words of three letters. The subjects were required to work as fast as was consistent with correct cancellation. One mark was given for each letter or word correctly cancelled, and one mark was deducted for each word or letter marked in error. The test lasted for five minutes, divided into 5 minute periods.

5. *The Tapping Test.*—The machine used was that manufactured by the British Mutoscope and Biograph Co., Ltd., we believe, for the purpose of measuring cinematograph films. The machine recorded the number of "taps," and in "tapping" the hand moved as fast as possible from side to side. The machines were clamped to a desk, and the hand was kept in a constant position while tapping. The position of the hand was found to be important, as, in the preliminary tests, a change in the position of the hand markedly affected the scores.

This test lasted for 30 seconds and was preceded by 10 seconds practice. The subjects were instructed to tap at maximum speed.

6. *Alphabet Test.*—The task was to pick out, from the letters of three alphabets of different colours, arranged in a definite order on the desk, all the letters in order, starting with a given letter. Each day the "starting letter" was changed. The letters were not seen until the signal to commence was given. Two minutes were allowed for this test, and one mark was given for each letter correctly picked out in that time.

7. *Descrimination of Length of Line.*—This test was found to be so unreliable that no use could be made of the results obtained from it. A description will therefore be unnecessary.

8. *McDougall's Dotting Test.*—Instead of using dots to be struck at, we used "bull's-eyes" made of two concentric circles of 1 m.m. and 3 m.m. diameter respectively. In marking, 4 marks were given if the dot was inside the inner ring, 3 if it was on the inner ring, 2 if between the two rings, and 1 if on the outside ring. The time taken for this test varied, but was approximately constant for each boy. The test differed from all the other tests in being an individual test. Therefore the last boy to do it had a somewhat longer interval than the first boy. The time for each boy to do the test was approximately 1 minute, so the last boy in the largest group, which consisted of 6 boys, had a rest of 5 minutes. The order in which the boys in each group did the test was kept constant throughout the whole experiment. In view of the length of the fatigue producing work, and in view also of the short interval between the tests (30 seconds), it did not seem possible that the subjects could recover to any great extent in 5 minutes. This seems to be borne out by the results, for no other test suffers so much, as result of continuous mental work, as this test does.

(*d*) *Incentives to work.*

The incentives to work at maximum effort were the same as those described in Part I of this research.

(*e*) *General Conditions.*

The whole experiment was carried out in the presence of the writer, and no one else was present. Artificial light was used throughout to guard against the possible effects of different light intensities. The same room was used throughout, and the subjects occupied the same seats every day. The same instructions were given each day. The days on which rests were given, instead of fatigue producing work, the subjects occupied themselves in reading stories, looking at pictures, or talking to the experimenter, and were in the latter's presence the whole time.

(*f*) *Preliminary Practice Work.*

In order to obviate errors arising from the effects of practice, it was thought advisable to have the subjects so practised that the practice curve would be approximately linear. For this purpose a series of tests, given on alternate days, and extending over three weeks, was devised. A preliminary set of experimental with ten subjects, similar in class and age to the subjects described above in this experiment, had shown that, to bring them to the desired state of practice, it was necessary to do the multiplication, cancellation, alphabet, and McDougall's dotting tests nine times each, while six repetitions sufficed for the memory, spots, and tapping tests. To obtain this result, the average score in each test, of the ten subjects, was expressed graphically, and it was seen that the graph had become approximately linear when the abovementioned amount of practice had been done. The practice curves of the 42 subjects of this experiment are given in Figs. 28-34.

(*g*) *Fatigue-producing Work.*

The fatiguing work consisted of multiplication, physical drill, cancellation, and memorising lists of three-figure numbers. The cancellation and multiplication were as described above in the lists of tests. The physical exercises were those laid down by the educational authorities of the L.C.C., and the subjects were thoroughly familiar with them. They comprised exercises for arms, legs, trunk, and neck. The exercises were done to a rhythm given by a metronome, and an interval of 2 minutes was given at the end of each 30 minutes work.

The memory work was the memorising of lists of 12 pairs of three-place numbers. The numbers had to be learnt in pairs, and 10 minutes were allowed for the learning of each list. Then the first number of each pair was given, and the subjects were required to supply the second. In marking, 6 marks were given for a completely correct answer, 2 marks were given for a correct figure given in its proper place, and 1 mark for a correct figure in its wrong place. Thus the possible number of marks for each list was 72. In writing the answers, 10 seconds were allowed before the next number was read out.

The fatiguing work lasted 90 minutes each day.

(*h*) *Plan of the Experiment.*

The experiment consisted of two parts, the preliminary practice period which has been described above, and the experiment proper, which lasted for six consecutive days. Of the six days, the first and last were control days, on which the subjects were tested twice, with an intervening rest of 90 minutes. On the other four days, which we shall call fatigue days, the subjects were tested twice with an interval of 90 minutes, during which they worked at multiplication on the first day, physical drill on the second, memorising numbers on the third, and cancellation on the fourth. We shall distinguish between the two kinds of work

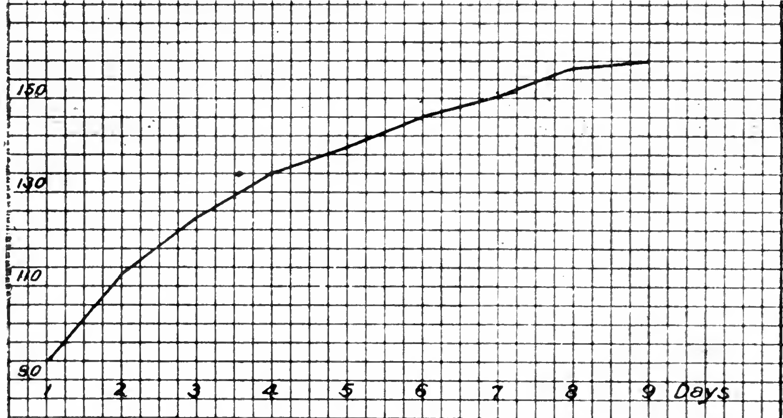

Fig. 28. Practice Curve for Multiplication.

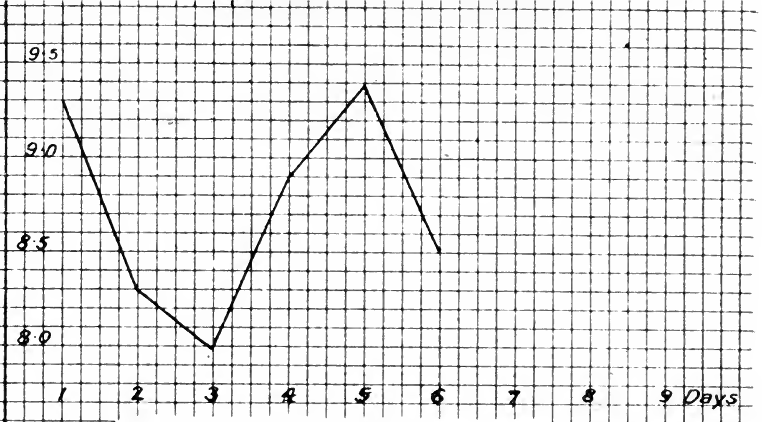

Fig. 29. Practice Curve for Memory.

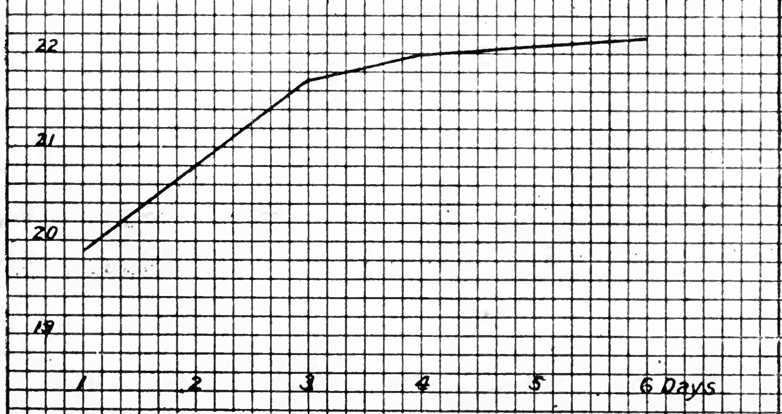

Fig. 30. Practice Curve for "Spots" Test.

Fig. 31. Practice Curve for Cancellation.

done by calling the 90 minutes continuous and homogeneous work, "continuous" work; and the shorter periods for testing the fatigue incurred, the "tests."

The following table, Table 29, shows the times and arrangement of the two kinds of work:—

TABLE 29.—Plan of Experiment.

Time.	1st Day.	2nd Day.	3rd Day.	4th Day.	5th Day.	6th Day.
9 a.m.—9·45 a.m. or 2 p.m.—2·45 p.m.	Tests. 1–8	Tests. 1–8	Tests. 1–8	Tests. 1–8	Tests. 1–8	Tests. 1–8
9·45 a.m.—11·15 a.m. or 2·45 p.m. —4·15 p.m.	Rest.	Multiplication.	Physical Drill.	Memorising Numbers.	Cancellation.	Rest.
11·15 a.m.—12 noon or 4·15 p.m.— 5 p.m.	Tests. 1–8	Tests. 1–8	Tests. 1–8	Tests. 1–8	Tests. 1–8	Tests. 1–8

The work thus lasted for 3 hours daily, during the whole of which time the subjects were in the presence of the experimenter.

The subjects were taken in groups of not more than six, and the experiment was done twice. Unfortunately, when the work was done the second time, some boys had left school, and others were unable to come to the laboratory, so that out of the 42 boys who did the experiment the first time, only 29 were able to come the second time.

Between each of the tests an interval of 30 seconds was allowed, and at the end of the fatigue-producing work each day, a rest of 1 minute was given before commencing the first test.

2. Results of Part II.

(*a*) *Practice.*

The subjects were practised every alternate day for three weeks. For the first three practice days, only the multiplication, cancellation, alphabet, and dotting tests were done, but on the remaining six days, all the tests were done once each. Of the 42 subjects who completed the experiment proper, 35 attended every practice. The other 7 completed the whole of the practice work, but could not come every alternate day, and their results have been omitted.

The average score for each test on the different practice days is given in Table 30; and in Figs. 28-34 are given the practice curves for each test corresponding to the figures given in that Table.

TABLE 30.—Average scores in Practice Tests.

Tests.	Days.								
	1	2	3	4	5	6	7	8	9
Multiplication ...	94	112	123	134	139	146	151	156	157
Spots	...	...	...	19·7	20·8	21·7	22·0	22·1	22·1
Memory	...	...	...	9·3	8·3	8·0	8·9	9·4	8·5
Cancellation ...	91	95	101	101	110	116	120	127	132
Tapping	...	...	...	149	155	155	157	158	158
Alphabet	17·5	20·6	22·5	21·9	23·2	24·2	25·2	25·6	25·9
Dotting	117	148	160	167	172	175	177	178	*

* Unfortunately the scores of this test for the last day have been lost.

The objects of this preliminary practice period were, first, to minimise the influence of variations, which we were unable to measure or control, such as those caused by the unusual surroundings of the laboratory, and the presence of the experimenter; second, to have the subjects at such a state of practice that the practice curves would be approximately linear and horizontal. If this second aim were not wholly achieved, the general course of the curve could be determined, and the effects of its non-linearity eliminated.

The curve for multiplication (Fig. 28) has not reached the horizontal stage, but the general course of the curve shows plainly that this stage is just about to be reached. As the work of the experiment proper follows the practice work, it is evident that, during the experiment proper, the curve of practice is approximately horizontal.

In Fig. 30, the "spots" test, the horizontal stage has been reached on the fifth practice day, which is also true of Fig. 32, the curve for tapping.

The curve for memory (Fig. 29) is quite irregular. The curve actually falls at first instead of rising. This is probably due to the unequal difficulty of the tests on different days. It was not found possible to eliminate this source of error, as in this connection its effects had not been foreseen, and most of the subjects had done part of the work before it was discovered that its effects were so marked. It seems very probable that the practice effect here is not great, for, unless there was a marked disparity in the difficulty of these tests, the curve should show some signs of rising with increased practice. In our description of this test we gave our method of eliminating this error in the experiment proper.

Fig. 31, cancellation, shows a departure from the usual shape of the practice curve. It is approximately linear from the beginning.

This rate of practice seems to have been maintained throughout the experiment, for on the last control day the average score for the subjects who completed the whole practice on the prescribed days was 156, or an average daily increment of 4. This practice effect, as the curve seems to have retained its linear form, would be eliminated by our method of experimentation, even though the curve is not approximately horizontal.

Figs. 33 and 34, alphabet and dotting, conform to the general shape, and have become almost horizontal at the close of the practice period. An interesting feature of the latter curve is its regularity. The curve rises rapidly at first, and gradually becomes less steep until the practice effect almost disappears.

A comparison of these curves shows a marked difference in the duration of the practice effect. In cancellation (Fig. 31), for example, the effect is still very marked on the ninth day, while in tapping (Fig. 32) there is very little effect after the first day. The other curves occupy intermediate positions.

From the general shape of these curves it will be conceded, we venture to think, that in any experiment such as this *a preliminary practice period is a necessity*. With unpractised subjects, the effects of practice are so large, and sometimes so variable, as to render precarious any deductions from their results, unless this effect can be wholly eliminated by the method of experimentation. Further, the emotional effects of strange surroundings and strange tasks are apt to affect the subjects'

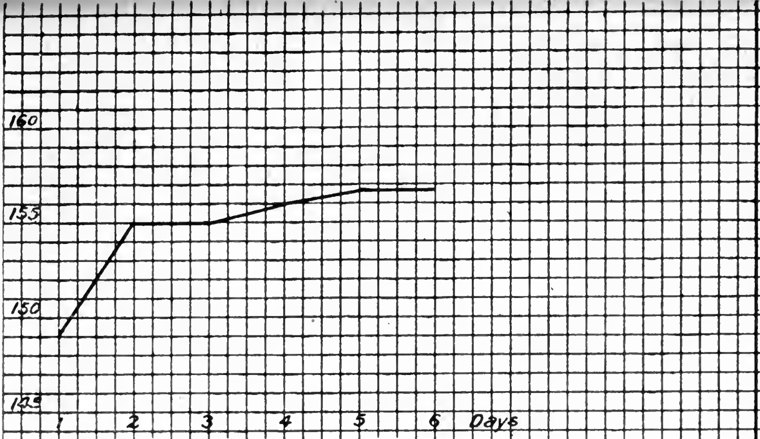

Fig. 32 Practice Curve for Tapping.

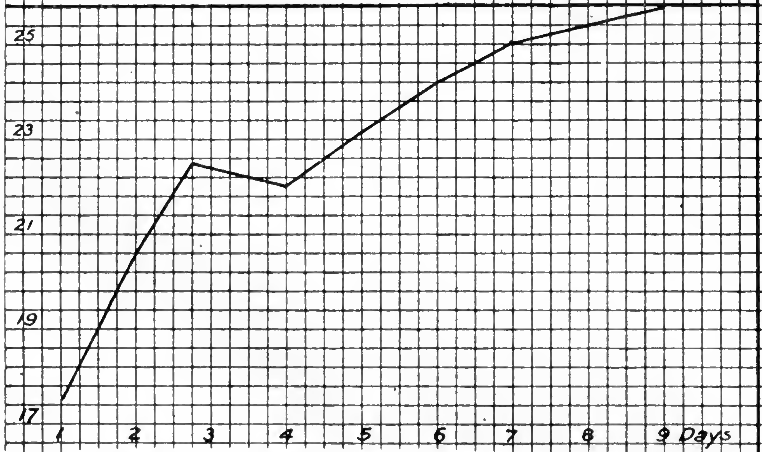

Fig. 33. Practice Curve for Alphabet Test.

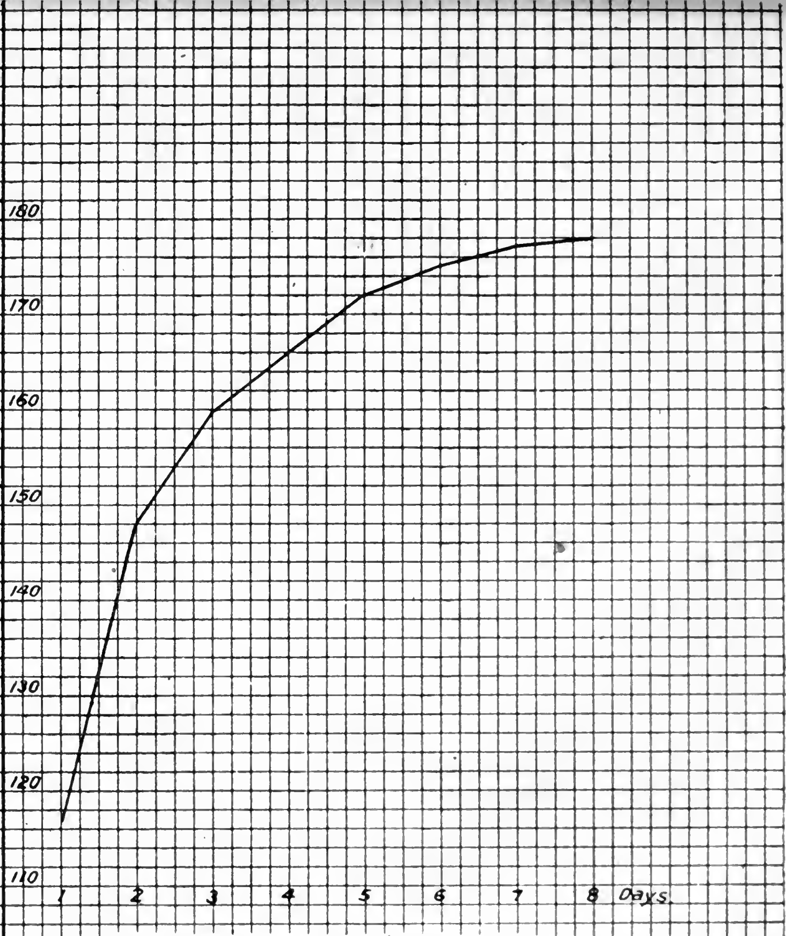

Fig. 34 Practice Curve for Dotting Test

performances in a very arbitrary way, the results of which it does not seem possible to eliminate otherwise. Although it does not seem necessary that subjects, at the time of experimentation, should be at the limit of practice, yet it does seem essential that the early stages of practice, when the curve rises so abruptly, should be passed.

(*b*) *Continuous Work and Subjective Estimates of State of Fatigue.*

In the continuous work, the coefficient of fatigue was found by comparing the amount of work done in the first 10 minutes with the amount done in the last 10 minutes, and expressing the difference between the two as a percentage of the former. In the memory fatigue work (memorising numbers) in which the subjects had not been specially practised before the experiment, the practice effect, together with the effect due to the unequal difficulty of the tasks, was so variable and so pronounced as to conceal any fatigue effect that might have been present. Consequently we have omitted these results. No calculation of the amount of muscular work done during the physical drill fatigue work was attempted, and therefore no fatigue coefficient for this work could be given.

The results for multiplication and cancellation show a decided decrease in efficiency as a result of prolonged work, the average decrease being 19·8 per cent. and 14·6 per cent., with probable errors of 1·42 and 1·04 respectively.

At the end of the continuous work, the subjects were required to record their subjective estimates of the state of fatigue. For this purpose a scale of fatigue values was drawn up. "Very tired" was the highest, then came "Tired," "Fairly tired," "Slightly tired," and lastly "Not tired at all." The terms used were carefully explained to the subjects before the experiment commenced. These values were then expressed numerically, "Very tired" being called 5, "Tired" being called 4, and so on down to "Not tired at all," which was given the value 1. The correlation between the subjective and objective estimates of the fatigue induced during the 90 minutes multiplication and cancellation work was then calculated. These coefficients were, for the multiplication, ·42, and for the cancellation ·18, with probable errors of ·086 and ·10 respectively.

These results would seem to indicate that, after multiplication work extending over 90 minutes, the fatigue sensations corresponded, to a certain extent at any rate, with the state of the subjects' efficiency. After cancellation this correspondence is not evident.

By finding the correlation between the fatigue coefficients of the continuous multiplication and cancellation, we can ascertain to what extent fatiguability with one kind of work goes with fatiguability in the other kind. This correlation coefficient was found to be ·32 with a probable error of ·09, which seems to show that there is a slight tendency for fatiguability for cancellation to be associated with fatiguability for multiplication in the same subject.

To see how far the sensations of fatigue after one kind of work, as shown by the subjects' own estimates, corresponded with those caused by other kinds of work, we have found the correlation coefficients between these subjective estimates. These are given in Table 31.

TABLE 31.—Correlation coefficients of subjects' estimates of state of fatigue. Probable errors are given in brackets.

	Multiplication.	Cancellation.	Physical Drill.	Memory.
Multiplication	....	·87 (·06)	·27 (·098)	·30 (·096)
Cancellation	·87 (·06)	....	·07 (·10)	·41 (·088)
Physical Drill	·27 (·098)	·07 (·10)	....	— ·06 (·10)
Memory	·30 (·096)	·41 (·088)	— ·06 (·10)	....

It will be noticed that the correlations between the estimates after the three kinds of mental work, multiplying, cancelling letters, and memorising numbers, are all positive and more than three times the probable error. The estimates after multiplying and cancelling are highly correlated (·87). On the other hand, the subjective estimates after the muscular work do not correlate to any extent with any of the estimates after the mental work. One cause of the high correlation between multiplication and cancellation estimates may be the fact that the subjects were apparently more distressed after this work than after the memory or muscular work. The average subjective fatigue values after each kind of work were, 3·6 for multiplication, 1·8 for muscular work, 2·7 for memorising, and 3·6 for cancellation.

(c) *Reliability of the Tests.*

As each test was given on each of six successive mornings, it will be necessary to calculate the reliability coefficient for each of five pairs of successive mornings, and then take the average. The figures are given in Table 32.

TABLE 32.—Reliability Coefficients of Tests.

Tests.	Reliability Coefficient.
Multiplication	·96
Spots	·93
Memory	·87
Tapping	1·00
Discrimination of Length	·47
Cancellation	·96
Alphabet	·98
Dotting	·94

The reliability coefficient is high for every test, except for that of discrimination of length of line, the results of which have therefore been discarded for the purposes of this experiment. The reliability of the other tests is so high as to make the correction of the correlation coefficients unnecessary. The corrected values would be barely in excess of the uncorrected values.

(d) *Average Depreciation of Tests.*

In order to see whether there was any transference of fatigue from the fatigue-producing work to the tests, the depreciation in efficiency for each boy was calculated by comparing the scores in the tests given after this work with the scores before the work. This depreciation was expressed as a percentage of the score before the continuous work. Then the influence of the time of day and the daily practice effect was found by comparing the efficiency in the second testing on the two control days with the efficiency of the first testing, and taking the average. This was also expressed as a percentage of the scores given in the first testing. Then the true effect of the fatigue-producing work was found by eliminating this depreciation or appreciation of efficiency as shown in the scores on the control days when no fatiguing work was done; *e.g.*, if the average depreciation of the two control days for a subject in any one test was —4 per cent., *i.e.*, the daily practice effect caused his efficiency to be increased 4 per cent., and if the depreciation of the same test due to 90 minutes continuous work was 10 per cent., the real depreciation would be 4 per cent., + 10 per cent. = 14 per cent. The average loss of efficiency, after thus making allowance for the results on the control days, is given in Table 33. The probable errors of these averages are given in brackets.

TABLE 33.—Average depreciation of tests due to continuous work expressed as a percentage of normal performance.

Fatigue-producing Work.

Tests.	Multiplication.	Physical Drill.	Memory.	Cancellation.
Multiplication ...	8·5 (·99)	— 1·5 (·85)	·1 (1·3)	3·6 (·62)
Spots	— ·07 (1·4)	1·4 (1·1)	·9 (1·5)	1·6 (1·3)
Memory	8·0 (4·0)	— ·4 (4·3)	9·1 (4·4)	7·9 (4·4)
Cancellation ...	1·0 (·90)	·6 (·86)	2·0 (·89)	11·1 (1·2)
Tapping	·8 (·30)	— ·28 (·32)	— ·48 (·42)	— ·22 (·29)
Alphabet ...	·17 (2·4)	1·4 (2·8)	8·0 (2·5)	1·4 (2·2)
Dotting	3·9 (·82)	1·0 (1·1)	5·2 (1·1)	6·5 (·91)
Average of all Tests. ...	3·2 (·82)	·31 (·26)	3·5 (·94)	4·6 (·96)

It will be seen from Table 33 that the average loss in efficiency in all the tests after the three kinds of mental work is appreciable. Those for multiplication and memory are almost four times the "probable error," while that for cancellation is almost five times its "probable error." The latter has been calculated in all cases by means of the formula $P.E. = \frac{\cdot 6745\,\sigma}{\sqrt{n}}$. The tests show a slight loss in efficiency after physical drill, but being hardly more than the value of the "probable error," it might, as likely as not, be due to chance. Taking the results individually, it is seen that almost all show a loss in efficiency after all three kinds of mental work. Tapping shows no loss after cancellation and memory, while the "spots" test shows no depreciation after multiplication. This general loss in efficiency is not found after physical drill. The multiplication, memory, and tapping tests all have a negative depreciation, while the loss in the remaining tests is very small.

Comparing the depreciations with their "P.E.'s" after mental work, we find six results which are less than the P.E., four which are more than once the P.E., four more than twice, one more than three times, two more than four times, and one each more than five, seven, eight, and nine times the P.E. It is obvious that these results are too large to be accounted for by mere chance. After muscular work, we find five results less than the P.E., and the other two less than twice the P.E., which effect could easily have been produced by chance.

We feel justified then, in concluding that continuous multiplication, memorising numbers, and cancellation for 90 minutes has an adverse effect upon the succeeding mental work, but muscular work for the same time does not show any such adverse effect.

One may ask with what standard are we to judge the severity of the 90 minutes muscular work. We have a fair notion of the energy involved in working at multiplication or cancellation for 90 minutes at maximum effort. The object set before the experimenter was to have the subjects working at maximum effort in the physical drill too. The exercises chosen were those which would call forth the greatest possible effort, and which are at present being used by the military authorities for training soldiers. The time was set by a metronome and no rests were given between changes of exercise. Work involving both trunk and limbs was given. The result obtained here, that muscular work does not injure succeeding mental work, is directly opposite to the results obtained by Bettman[1], who found that two hours walking, in the case of himself, had a marked effect upon succeeding work. Miesemer[2] found that one hour walking had a slightly adverse effect upon intellectual work in the case of himself, but increased his capacity for writing figures quickly. We have pointed out above the possibility of the influence of suggestion when the experimenter is the sole subject.

We can determine what proportion of the fatigue effect is transferred from the continuous work to the tests by comparing the average depreciation in the continuous work, in the case of multiplication and cancellation, with the depreciation in the tests. We are here dealing with the average proportion of fatigue transferred. In the next section, an attempt will be made by means of correlation coefficients to solve the problem whether a subject is affected similarly in all tests, after the same fatiguing work, or whether he will be more fatigued in one test than another.

In Table 34 is given the amount of fatigue as shown by the loss in efficiency in the tests, expressed as a percentage of the fatigue coefficients in the multiplication and cancellation continuous work.

TABLE 34.—Depreciation in the tests expressed as a percentage of the depreciation in the continuous work.

Tests.	After Continuous Multiplication.	After Continuous Cancellation.
	per cent.	per cent.
Multiplication	43	25
Spots	......	11
Memory	40	54
Cancellation	5	76
Tapping	4	— 2
Alphabet	1	10
Dotting	20	45
Average and P.E.	16 4·38	31 6·65

We should expect the figures for any test after continuous work of the same kind to be large, as indeed we find with cancellation. The figures

[1] Bettman, S. Ueber die Beeinflussung einfacher psychol. Vorgänge durch körporliche und geistige Arbeit. Psych. Arb. Vol. 1, 1895, p. 152.

[2] Miesemer. Ueber psychische Wirkungen körporlicher und geistiger Arbeit. Psych. Arb. Vol. 4, 1902, p. 375.

for the multiplication test after multiplication work are strangely low. The coefficient of fatigue for the multiplication continuous work is 19·8, while that for the test done 1 minute afterwards is only 8·5. We should hardly expect such a recovery in such a short time as 1 minute, although we shall find later that recovery is most rapid in the earlier part of a rest. This view is strengthened by comparing with this result the figures after the cancellation continuous work. In the cancellation test 76 per cent. of the incurred fatigue is transferred, although this test was done about 17 minutes after the completion of the continuous work, of which 17 minutes 2½ were taken up by rests, as 30 seconds' rest was given between each test. McDougall's explanation of such an effect is probably the true one. In continuous work, boredom plays a large part, and to its effect part of the loss of efficiency is due; but in a short test of, say, 5 minutes, the subject has the power of tapping fresh supplies of energy, and by an effort of the will dispels the boredom and works almost as well as ever, but only for a short time.

It can hardly be contended that the rests between the tests account for the loss of the fatigue effect in some tests, because, if this were so, we should expect the last test performed, which had the advantage of more rests than any of the others, to show the least loss in efficiency. Instead of this we find that the last test (dotting) shows the greatest average loss after all kinds of work except memory; and if we compare the depreciation with its probable error dotting suffers most of all, not excepting memory. It would seem then that the explanation must be that continuous mental work affects succeeding mental work differentially and not equally. It has a selective effect.

The average loss in efficiency of all the tests is greater after cancellation than after multiplication continuous work, the average being 16 per cent. and 31 per cent. The difference of these two averages is 15 per cent. and the P.E. of the difference 5·8, which indicates that, with so small a number of tests, the difference might possibly be due to chance, although the odds against it being so due are about twelve to one. The results of Part 1 of this research are in agreement with the results in Table 34, and testify against this difference being due to chance. It will be remembered that the persistence of the fatigue effect was much more marked after cancellation than after multiplication. A rest of 4 minutes sufficed to dispel the fatigue occasioned by multiplying for 1 minute, but a rest of 8 minutes was insufficient to dispel the fatigue due to cancelling for 1 minute. Thus the two results agree on this point: that cancellation has a greater adverse effect upon succeeding mental work than the same duration of multiplication.

(*e*) *Correlations between the depreciation in* different *tests after the* same *continuous work.*

In the preceding section we have seen that fatigue, in some cases at least, induced by one kind of work, adversely affects different succeeding work. The results given in that section, however, can give us no information upon the question whether the fatigue incurred in the continuous work affects any one subject in a corresponding degree in all the tests. That is to say, will a subject whose multiplication shows a marked loss in efficiency after one kind of continuous work also show a corresponding depreciation in other tests after the same continuous work? Does continuous work result in a general state of fatigue, so that any task done will be below normal to the same extent as any other done by the same subject? The correlation coefficients between the losses in efficiency in each pair of tests on each of the four days on which continuous work was done will throw light on this question. These correlations are given, with their probable errors, in Tables 35–38.

If a subject were affected generally by continuous work, and were fatigued to an approximately equal degree in all kinds of work, we should expect the correlation to be highly positive. But if, on the other hand, one subject may be very fatigued, say, for multiplication, and not fatigued for dotting and cancellation, and only slightly fatigued for memorising, while another, also fatigued for multiplication, may have his powers of dotting and cancelling less fatigued, and his power of memorising not fatigued at all, we should expect the average correlation to be zero. Generality of transference of fatigue demands a high positive correlation between the loss of efficiency in the tests, while complete specificity of transference demands that the correlation should be zero.

TABLE 35.—Correlation coefficients between depreciation in tests after multiplication fatigue work. The probable errors are given in brackets.

Tests.	Multiplication.	Spots.	Memory.	Cancellation.	Tapping.	Alphabet.	Dotting.
Multiplication ...	......	·058 (·104)	·21 (·099)	·29 (·095)	— ·11 (·103)	·35 (·091)	— ·11 (·103)
Spots	·058	......	·44 (·084)	·11 (·103)	·17 (·101)	·00 (·104)	— ·24 (·098)
Memory ...	·21	·44	......	— ·020 (·104)	— ·26 (·097)	·098 (·103)	— ·10 (·103)
Cancellation ...	·29	·11	— ·020	......	— ·14 (·102)	— ·087 (·103)	— ·065 (·104)
Tapping ...	— ·11	·17	— ·26	— ·14	......	— ·16 (·101)	— ·32 (·093)
Alphabet ...	·35	·00	·098	— ·087	— ·16	......	— ·018 (·104)
Dotting	— ·11	— ·24	— ·10	— ·065	— ·32	— ·018	......

Average coefficient — ·005

TABLE 36.—Correlation coefficients between depreciation in tests after cancellation fatigue work. The probable errors are given in brackets.

Tests.	Multiplication.	Spots.	Memory.	Cancellation.	Tapping.	Alphabet.	Dotting.
Multiplication ...	......	·16 (·101)	— ·22 (·099)	·14 (·102)	·003 (·104)	·20 (·099)	·014 (·104)
Spots	·16	......	·12 (·103)	·21 (·099)	— ·28 (·095)	·39 (·088)	·14 (·102)
Memory ...	— ·22	·12	......	— ·045 (·104)	— ·028 (·104)	— ·025 (·104)	·23 (·099)
Cancellation ...	·14	·21	— ·045	......	— ·10 (·103)	— ·10 (·103)	— ·15 ·102))
Tapping ...	·003	— ·28	— ·028	— ·10	......	— ·12 (·103)	·068 (·104)
Alphabet ...	.20	·39	— ·025	— ·10	— ·12	......	— ·30 (·095)
Dotting ...	·014	·14	·23	— ·15	·068	— ·30	......

Average coefficient ·015

TABLE 37.—Correlation coefficients between depreciation in tests after muscular fatigue work. The probable errors are given in brackets.

Tests.	Multiplication.	Spots.	Memory.	Cancellation.	Tapping.	Alphabet.	Dotting.
Multiplication ...	......	— ·26 (·097)	— ·39 (·088)	·14 (·102)	·00 (·104)	·13 (·102)	·08 (·103)
Spots	— ·26	......	— ·04 (·104)	— ·18 (·101)	— ·25 (·098)	·23 (·099)	— ·19 (·100)
Memory ...	— ·39	— ·04	......	— ·37 (·090)	·07 (·104)	— ·21 (·099)	— ·13 (·102)
Cancellation ...	·14	— ·18	— ·37	......	— ·41 (·087)	·25 (·098)	— ·09 (·103)
Tapping ...	·00	— ·25	·07	— ·41	......	— ·06 (·104)	·16 (·101)
Alphabet ...	·13	·23	— ·21	·25	— ·06	......	— ·13 (·102)
Dotting	·08	— ·19	— ·13	— ·09	·16	— ·13	......

Average coefficient— ·08

TABLE 38.—Correlation coefficients between depreciation in tests after memory fatigue work. The probable errors are given in brackets.

Tests.	Multiplication.	Spots.	Memory.	Cancellation.	Tapping.	Alphabet.	Dotting.
Multiplication ...	......	·03 (·104)	— ·08 (·103)	— ·09 (·103)	— ·10 (·103)	·17 (·101)	·33 (·093)
Spots	·03	......	— ·47 (·081)	— ·26 (·097)	— ·33 (·093)	·25 (·098)	·10 (·103)
Memory ...	— ·08	— ·47	......	·38 (·089)	— ·22 (·100)	— ·17 (·101)	— ·02 (·104)
Cancellation ...	— ·09	— ·26	·38	......	— ·26 (·097)	— ·02 (·104)	— ·23 (·099)
Tapping ...	— ·10	— ·33	— ·22	— ·26	......	·08 (·103)	·00 (·104)
Alphabet ...	·17	·25	— ·17	— ·02	·08	......	·28 (·095)
Dotting	·33	·10	— ·02	— ·23	·00	·28	......

Average coefficient — ·03

Examining Tables 35–38 as a whole, we find the correlations low. The average of the eighty-four coefficients is — ·024, with a probable error of ·015. The distribution is fairly even, thirty-four being positive, forty-seven negative, and three zero. Comparing the coefficients with their probable errors, we find that, of the thirty-four positives, eleven are less than their probable errors, ten are more than once but less than twice, six are twice, four three times, two four times, and one five times their probable errors. Of the forty-seven negative coefficients nineteen are less than their probable errors, eleven are more than once but less than twice, nine are twice, four three times, three four times, and one five times their probable errors. These results can be seen at a glance in Table 39. This result is only slightly in excess of what we should expect by pure chance, if the true average correlations were zero.

TABLE 39.—Ratios of positive and negative correlation coefficients to their probable errors.

	Number of Positive Coefficients.	Number of Negative Coefficients.
Less than once their P.E.'s	11	19
More than once but less than twice their P.E.'s ...	10	11
More than twice but less than three times their P.E.'s	6	9
More than three times but less than four times their P.E.'s	4	4
More than four times but less than five times their P.E.'s	2	3
More than five times but less than six times their P.E.'s	1	1

We can calculate the observed standard deviations of the correlation coefficients given in each of the Tables 35–38, and also the standard deviations of the same coefficients, which would be expected by chance alone. The latter is obtained by means of the following equation :—

$$D^2 = \bar{\sigma}^2 \left[1 - \frac{n-2}{n(n-1)} \left(2\bar{r} - \bar{r}^2 \right) - \frac{2(n-2)}{n} \; \frac{\bar{r}^2}{(1 \quad \bar{r})2} \right]$$ [1]

Where D is the required standard deviation due to chance alone;
$\bar{r}$ is the mean value of the observed coefficients;
$\bar{\sigma}^2$ the mean square "probable errors" of these coefficients x 2.2;
n the number of coefficients.

On working out, the average standard deviation expected as the result of pure chance it is found to be ·106, while the observed standard deviation is ·196. The latter is less than twice the standard deviation due to chance, and therefore is not sufficiently large to have any significance.

Further, there seems to be no reason for thinking that the average correlation would be highly negative—*i.e.*, that those subjects who are most fatigued in one test would tend to be those who are least fatigued in another test after the same continuous work. Certainly no experimental evidence has been put forward to support such a view. The question at issue among investigators of this subject has been whether fatigue is transferred generally, in which case the correlation ought to be highly positive approaching one, or specifically, in which case the correlation ought to be zero. If, then, we regard these negative coefficients to be wholly due to chance, we must regard the positive ones in the same way, for we have seen that the average correlation is negative, and forty-seven coefficients out of eighty-four are negative.

If we leave out of account the correlations after physical drill, and deal with the effects of the more mental continuous tasks, multiplication, cancellation, and memorising, we find the average correlation between the losses of efficiency in the tests to be — ·004, with a probable error of ·017, while twenty-seven coefficients are positive and thirty-four negative, showing that the inclusion of the correlation after muscular work does not materially affect the result.

[1] See Abelson. Mental Ability in Backward Children. Brit. Journ. of Psych Vol. 4, 1911, p. 313.

From these considerations it seems that zero is most probably the true value of the correlations between the depreciations of the tests. Then the true value of the probable error will be ·104, and only five positive and six negative coefficients will be more than three times, while only one positive and one negative will be more than four times, and none will be five times the probable error, which result out of eighty-four coefficients might easily happen by chance.

How shall we interpret this result? We can conceive of two possible explanations. Either fatigue is transferred specifically—*i.e.*, the fatigue coefficient for a subject in one test is no criterion of his state of fatigue in any other test; or else the average correlation has been greatly reduced by the unreliability of the measurements taken, and should be much higher if the reliability were greater. As the whole experiment was done twice we can calculate the reliability coefficient of the *depreciation* in each test. It is obvious that the lower the reliability the lower will the correlation become, because of the greater attenuation by random errors; and, conversely, if the reliability is very high, random errors must have but a feeble influence, and the correlation coefficients obtained are not to any extent lower than the true coefficients.

As some of the correlation coefficients given in Tables 35–38, and also some of the individual reliability coefficients of the depreciations in the tests are so low, the correction of each individual correlation coefficient would give, in such cases, an illusory value. The average reliability coefficient of the *depreciations* in all tests after the four kinds of continuous work is ·272,* and the average correlation coefficient of the depreciations in all the tests is — ·024.

On the whole, under similar experimental conditions, both the reliability and correlation coefficients may be expected to approximate to the arithmetic means of our present data. If we take this as a fairly typical case, we can easily calculate for it, the " true " or corrected correlations. Such a typical value is the more meaningful, seeing that the deviations of the correlations from one another are not significantly greater in amount than would be expected from their sampling errors alone. The formula for giving the corrected or " true " of the " raw " or uncorrected correlation coefficient is—

$$r^1_{ab} = r_{ab} \sqrt{\frac{(1 + r_{aa})\ (1 + r_{bb})}{2r_{aa} \quad 2r_{bb}}}$$ [1]

where r^1_{ab} is the corrected coefficient, r_{ab} the observed coefficient, and r_{aa} and r_{bb} the reliability coefficients of the tests a and b.

On working out, the " true " average correlation coefficient is found to be —·061, with a probable error of ·104—*i.e*, there are even chances that any other similar sample would give a correlation coefficient of between ·043 and — ·165.

From these considerations we are forced to the conclusion that the *average correlation between the depreciations in different tests after the same continuous work is approximately zero.*

This result corroborates those of Ellis and Shipe,[2] who found no appreciable correlation between several mental tests done after the same fatiguing work.

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*See page 78.

[1] See Spearman. Correlation Calculated from Faulty Data. Brit. Jour. of Psych. Vol. 3, 1910, p. 271.

[2] Ellis and Shipe. A Study of the Accuracy of Present Methods of Testing Fatigue. A.J.P. 1903, Vol. 14, p. 223.

In the beginning of the section, we stated, it will be remembered, that if fatigue were transferred generally the average correlation coefficient between the depreciations in the different tests should be highly positive; and, on the other hand, if fatigue were transferred specifically the same average coefficient should be zero. These results therefore show that fatigue is transferred specifically and not generally.

This being so, much of the contradiction and confusion prevalent in the results of experiments upon fatigue and its effects is easily explained. In many researches upon this subject the fatigue incurred in one performance is measured by its effects upon a succeeding different performance, which is tantamount to assuming that fatigue is transferred generally. This error is greatly in evidence in experiments performed upon school-children, by testing them at various times in the school day, and estimating the state of fatigue from the performances in these tests. The results given by these experiments may be quite inexceptionable, but the inferences drawn from them are far from unexceptionable, and, in some cases, are quite inaccurate. If a child is found to be normal in a certain test at the end of the school day, it does not follow that he is not fatigued. The only inference to be drawn from such a result is that he is not fatigued *in that particular test*; but the result gives no evidence as to his state of fatigue in any other test, nor does it give any evidence of the state of fatigue in any test of any other child who has done exactly the same work as the former child. If two subjects, after the same duration of mental work, are tested, say, in multiplication and cancellation, the first may be very fatigued in multiplication, and only slightly affected in cancellation, while the results for the second subject may be the exact reverse. He may be very fatigued for cancellation, and only slightly fatigued for multiplication.

(*f*) *Correlation between the depreciation in the continuous work and the depreciation in the succeeding tests.*

We have seen that in the continuous multiplication and cancellation work the average fatigue coefficients, found by comparing the work done in the last 10 minutes with that done in the first 10 minutes, were 19·8 and 14·6 respectively. It will be interesting to see what correlation exists between the results of the continuous work and the succeeding tests. These correlation coefficients will act as a check upon the results gained in the preceding section. If fatigue is transferred specifically we will expect the correlations to be approximately zero, except for those tests which are of the same kind as the continuous work—*e.g.*, multiplication and cancellation. On the other hand, if fatigue is transferred generally, we will expect the coefficients to be high and positive. The results are given in Table 41. It will be seen that only one result, viz., the correlation between the continuous multiplication and the multiplication test is at all highly positive. Here, if anywhere, we should expect to find a high correlation, for the task to be performed in both was exactly the same, and an interval of only 1 minute separated the one from the other. The correlation between the cancellation continuous work and the cancellation test is astonishingly low—only ·17. In this case some considerable time, approximately 17 minutes, elapsed between the completion of the continuous work and the commencement of the test. This 17 minutes was taken up by multiplication, spots, and memory tests, with the intervening rests of 30 seconds each. This lapse of time and change of task may be the cause of the low correlation, as in the case of the multiplication continuous work, and the multiplication test, where only 1 minute separated the two, the correlation is much higher, ·54. Yet if fatigue is transferred specifically this low correlation is not only not surprising, but is what might be expected. For during the 17 minutes in which they were performing the different tasks or resting, the subjects would have a chance to recover from the cancellation fatigue, and unless all recovered at the same rate the result would be a lowering of the correlation. The multiplication test correlating only to the extent of ·54 with the multiplication continuous work supports the view that the rate of recovery from fatigue varies with the individual. A rest of only 1 minute is sufficient to substantially reduce the correlation.

TABLE 41.—Correlations between the depreciation in continuous work and the depreciations in succeeding tests. The probable errors are given in brackets.

Tests.	Continuous. Multiplication.	Probable Errors.	Continuous. Cancellation.	Probable Errors.
Multiplication	·54	(·074)	·01	(·104)
Spots	·04	(·104)	·14	(·102)
Memory	— ·06	(·104)	— ·36	(·091)
Cancellation	·02	(·104)	·17	(·101)
Tapping	— ·12	(·103)	·07	(·103)
Alphabet	·19	(·100)	·12	(·103)
Dotting	·00	(·104)	·04	(·104)

Now let us take the correlations between the continuous work and the tests other than multiplication and cancellation. Of these twelve results the average correlation is ·007, with a probable error of ·027. That is to say, there are even chances that the average of any other similar sample would be between ·034 and — ·020. If we allow a deviation of four times this probable error, the odds against which happening as a result of chance are about 140 to 1, the average would lie between ·115 and — ·101, which is still a very low correlation.

The lowness of the average correlation is undoubtedly affected by the low reliability of the *depreciations* in the tests, the average of which on p. 78 we find to be only ·272, but no amount of correction for attenuation due to random errors will make such a low result significant.

From these considerations we are forced to conclude that the correlation between the continuous work and the tests which differ from them as regards the task to be performed is very low and approximately zero. This result is in complete accord with the results of the previous section, in which we found that the correlation between the depreciations in the tests themselves was approximately zero; and both results combine to show that fatigue is transferred specifically and not generally, and that the fatigue incurred in performing one task is not measureable by the depreciation of efficiency in another task done immediately afterwards.

(g) *Correlation between depreciations in the* same *test after continuous work of* different *kinds.*

We have found no appreciable correlation to exist between the depreciations in *different* tests after the *same* continuous work; and the question now arises whether we should expect to find a correlation between the depreciations in the *same* tests after *different* kinds of continuous work. To answer this question, we give in Tables 42 to 48, the correlations between the depreciations in the same test after each kind of continuous work, *e.g.*, in Table 43 are given the correlations between the depreciations in multiplication after continuous multiplication, memorising, physical drills, and cancellation respectively; and the following Tables give the corresponding results for the other six tests.

TABLE 42.—Correlations between depreciation in alphabet test after different kinds of continuous work. Average ·388. Probable errors are given in brackets.

Tests.	Multiplication.	Cancellation.	Memorising.	Physical Drill.
Multiplication	......	·41 (·086)	·63 (·062)	·56 (·072)
Cancellation	·41	......	·09 (·103)	·36 (·091)
Memorising	·63	·09	......	·28 (·095)
Physical Drill	·56	·36	·28	......

TABLE 43.—Correlations between depreciations in multiplication test after different kinds of continuous work. Average ·257. The probable errors are given in brackets.

Tests.	Multiplication.	Cancellation.	Memorising.	Physical Drill.
Multiplication	......	·04 (·104)	·44 (·067)	·01 (·104)
Cancellation	·04	......	·41 (·086)	·42 (·085)
Memorising	·44	·41	......	·22 (·099)
Physical Drill	·01	·42	·22	......

TABLE 44.—Correlations between depreciation in dotting test after different kinds of continuous work. Average ·157. The probable errors are given in brackets.

Teste.	Multiplication.	Cancellation.	Memorising.	Physical Drill.
Multiplication	......	·18 (·101)	·22 (·099)	·17 (·101)
Cancellation	·18	......	·05 (·104)	·32 (·094)
Memorising	·22	·05	......	·004(·104)
Physical Drill	.17	·32	·004	......

TABLE 45.—Correlations between depreciation in tapping test after different kinds of continuous work. Average ·203. The probable errors are given in brackets.

Tests.	Multiplication.	Cancellation.	Memorising.	Physical Drill.
Multiplication	......	·07 (·104)	·23 (·099)	·03 (·104)
Cancellation	.07	......	·38 (·089)	·18 (·101)
Memorising	·23	·38	......	·33 (·093)
Physical Drill	·03	·18	·33	......

TABLE 46.—Correlations between depreciations in spots test after different kinds of continuous work. Average ·345. The probable errors are given in brackets.

Tests.	Multiplication.	Cancellation.	Memorising.	Physical Drill.
Multiplication	......	·40 (·087)	·35 (·091)	·44 (·067)
Cancellation	·40	......	·30 (·095)	·44 (·067)
Memorising	·35	·30	......	·14 (·102)
Physical Drill	·44	·44	·14	......

TABLE 47.—Correlations between depreciation in cancellation test after different kinds of continuous work. Average ·133. The probable errors are given in brackets.

Tests.	Multiplication..	Cancellation.	Memorising.	Physical Drill.
Mulitplication	......	·094 (·103)	·18 (·101)	·23 (·099)
Cancellation	·094	......	·005 (·104)	·080 (·103)
Memorising	·18	·005	......	·30 (·095)
Physical Drill	·23	·080	·30	......

TABLE 48.—Correlations between depreciation in memory test after different kinds of continuous work. Average ·472. The probable errors are given in brackets.

Tests.	Multiplication.	Cancellation.	Memorising.	Physical Drill.
Multiplication	......	·57 (·071)	·41 (·086)	·48 (·080)
Cancellation	·57	......	·50 (·078)	·32 (·094)
Memorising	·41	·50	......	·55 (·073)
Physical Drill	·48	·32	·55	......

It will be seen that these correlation coefficients are all positive, and out of forty-two coefficients, twenty-three are equal to or greater than three times their probable errors. The average correlation for each test ranges from memory ·472, to cancellation ·133. Of course these coefficients, as is the case with all such coefficients, are affected by sampling errors; and, first, we must investigate the magnitude of these errors, in order to determine the true values of these correlations. It is evident that the average of their true (*i.e.*, corrected for sampling errors) values cannot be zero, for random errors would tend, if the true correlation were zero, to make half the individual values positive and half negative. Instead of this we find the whole forty-two values positive. Moreover, the sampling errors would have to be extremely large to account for the average correlation of ·472 for memory, if the true correlation were zero. It is obvious then that the true or corrected average correlation coefficient is positive.

We can calculate, and allow for the effects of sampling errors, by finding the reliability coefficients for the depreciations in the various rests. In Table 49 are given the average reliability coefficients and average correlation coefficients for each test.

TABLE 49.—Reliability coefficients and correlation coefficients of depreciations in *same* tests after *different* continuous work.

Tests.	Average Reliability Coefficient.	Average Correlation Coefficient.
Multiplication	·405	·257
Spots	·260	·345
Memory	·260	·472
Cancellation	·170	·133
Tapping	·245	·203
Alphabet...	·350	·388
Dotting	·210	·157
Average	·272	·279

It will be seen that the reliability coefficients are all low, *i.e.*, sampling errors have a large influence. If the sampling errors are so large as to cause the average reliability coefficients to fall from 1 to ·272, they must have similarly reduced the average correlation coefficient from a much higher value to ·279. There is a "raw," *i.e.*, uncorrected correlation of ·69 between the average reliability and the average correlation, showing that relatively high correlation is associated with relatively high reliability, and we may infer that the low average reliability is the cause of the lowness of the correlations. Owing to some of the individual reliabilities being so low, correction of the individual correlation coefficients would give, in some cases, illusory values; therefore, as in section (*e*), we may fairly take the average correlation of all the depreciations as a typical value, and correct it, instead of correcting each individual value, by means of the average reliability coefficient. The average "raw" correlation is ·279 and the average reliability ·244. Correcting for sampling errors by means of the equation given on page 175, we find that the "true" or corrected average correlation is ·74 with a probable error of ·047, which we can fairly take as a typical true correlation coefficient.

Further, it will be seen from Tables 42–48 that the coefficients in each table are, generally speaking, fairly even. There is a tendency for all the values in each table to be of the same size. This in Table 48 the values for memory are all relatively high, while in Table 47 the values for cancellation are all relatively low. As there is a tendency for relatively high correlation to be associated with relatively high reliability, [1] and as the lower the reliability, the greater is the correction to be made, there is a tendency for all the correlations to approach one central or typical value. At any rate the corrected values will deviate less from their average than the raw values do from their average.

We can easily calculate the standard deviations of the raw correlations, and compare them with the deviation we should expect to find due to chance alone. The latter can be found by means of the formula given on page 72.

[1] The correlation between the two is ·69.

TABLE 50.—Comparison of standard deviations of observed coefficients with those expected by chance alone.

Tests.	Standard Deviation of Observed Coefficients.	Standard Deviation due to chance.
Multiplication	·179	·160
Spots	·104	·157
Memory	·085	·115
Cancellation	·100	·080
Tapping	·127	·084
Alphabet...	·178	·158
Dotting	·104	·133
Average	·125	·127

The two sets of results are placed side by side in Table 50, from which it will be seen that the average observed deviation is ·125, and the average deviation due to chance alone is ·127, *i.e.*, the raw correlations do not deviate from one another as much as we would expect as due to chance. But we have shown that the true correlations would deviate from their average value even less than the raw correlations. This is, of course, no proof that the corrected values will approach a mean value, but it shows that the observed correlation coefficients do not violate the condition which must be fulfilled should they tend to approach a constant value. All that we can infer is that there is some degree of probability that they do approach a constant value, for their deviations from the average are less than we would expect from chance alone; and this typical constant value we know is very high and positive. From these results we conclude that the correlations between the depreciations in the same test after different kinds of continuous work are positive and very high, and, probably, tend to approach a constant value. To translate this result into concrete terms, each kind of continuous work affects a subject's performance in a particular test in a similar way. A subject who has his powers of performing a test, say in multiplication, greatly reduced by one kind of continuous work, will tend to have his power of performing the same test greatly reduced after any other kind of continuous work.

(*h*) *Interpretation and reconciliation of the results given in sections* (*e*) *and* (*g*).

In our investigation into the question of transference of fatigue, we have obtained two outstanding results, viz. :—

(1) After the *same* fatiguing work, the average correlation between the depreciation in *different* tests is zero or approximately zero.
(2) After *different kinds* of fatiguing work, there is a high correlation between the depreciation in the *same* test.

In our endeavour to explain and reconcile these apparently conflicting results, we will deal first with fatiguability or susceptibility for fatigue, and, secondly, with the production of fatigue.

I. Susceptibility for Fatigue.

The high correlation values in Tables 42–48 show that subjects who have a particular power, *e.g.*, multiplying, relatively fatiguable after one kind of task, will also have that same power relatively fatiguable after any other kind of task. We do not mean to say that the deterioration as exhibited by the fatigue coefficients will be the same after every kind of task, for Table 33 shows that all tests are not affected in the same degree by the same continuous work; but we do maintain that a subject's deterioration in a test is the same after all kinds of tasks *relative to the depreciations shown by the other subjects.* If the subjects were arranged in ranks according to their fatiguability in a test after a certain continuous tesk, each subject would tend to keep the same rank if the nature of the continuous work were changed but the test remained constant.

This result may be explained as being due to one of two causes: either (1) the subjects may have their general powers of performing any task fatigued, and every task performed will then be under the influence of this constant general fatigue; or (2) they may have their specific ability for performing the one particular task fatigued, and their ability for performing other tasks not affected in the same degree.

If the former were the correct view—that is, if susceptibility to fatigue were general—every task performed should show a correspondingly large fatigue effect, and the correlation between all the different tests after the same continuous work should be high and positive, but we have seen in Tables 35–38 that the average correlation is approximately zero. Therefore we are forced to accept the second explanation as being the true one, for, on this view, there would not necessarily be any correlation between the different tests. *Fatiguability is then different for each subject, and susceptibility for fatigue is specific and not general.*

II. Production of Fatigue.

Specific fatigue in the tests is most simply explained by the production of specific fatigue in the continuous work, but it is also possible that, if fatigue were general, it should affect specific powers differentially—that is to say, a subject, whose power of multiplying deteriorates owing to the general fatigue of all his powers, may not deteriorate to the same extent in, say, memorising or dotting; and, therefore, there may be no correlation between the deteriorations in these three tests.

That a general cause should have specific effects is not contrary to experience. In the administration of chloroform, for example, certain cortical areas are more readily affected than others, and cease functioning first, causing a gradual reduction of consciousness until unconsciousness ensues.[1] In the same way a general failure or loss of mental energy due to prolonged mental work might affect some specific structures in a greater degree than others; and, consequently, there may be no correlation between the effects of this loss of energy upon different tests. Thus, we maintain, a fatigue which is quite specific may be due to a cause which is quite general.

If fatigue is produced generally and not specifically, Tables 42–48 should satisfy the equation $\frac{r_{ap}}{r_{bp}} = \frac{r_{aq}}{r_{bq}}$ where a, b, p, and q are different

[1] See Johnston, H. J. The Role of Sensations and Feelings under Ether. Jour. Abnor. Psych. Vol. 4, 1909, p. 29. Jones, E. E. The Waning of Consciousness under Chloroform. Psych. Rev. Vol. 16, 1909, p. 53. Jacobson, E. Consciousness under Anæsthetics. Amer. Jour. Psych. Vol. 22, 1911, p. 333.

tests.[1] We have shown above that the correlations in these tables have a very high average value indeed when corrected for low reliability, and that they tend to approach the same value; therefore they will satisfy this equation. Of course, the number of coefficients in each of these tables is too small to place much reliance upon such a result if it were not supported by other facts; but they give a negative support to the view that fatigue is produced generally by not contradicting this necessary condition of generality.

A much stronger argument in favour of general production is given by the extremely high corrected values in these tables, which show the correlations between the depreciations in the same test after different kinds of continuous work. These values are quite incompatible with the specific theory of the production of fatigue, by which we mean that theory which holds that the fatigue incurred in doing a particular task is specific to that task, and has no influence upon succeeding different tasks. That the depreciations in each test should correlate highly after such divers kinds of continuous work as multiplication, memorising numbers, cancellation, and physical drill, clearly indicates that fatigue is produced generally, *unless the different continuous tasks contained large common factors,* in which case the correlation would be due to specific fatigue. It is quite contrary to general psychological experience to expect any definitely marked common factors in two such tasks as memorising numbers and doing physical exercises. In the work done on general ability,[2] tasks had to be very much alike, such as memorising numbers and memorising words, or cancelling two letters and cancelling four letters, before any specific factor became appreciable. Further, if there existed a high specific community between such different tasks as those we have named, we should expect to find high correlation coefficients in Tables 35–38, because, on this assumption, the common specific elements in any two tasks, however apparently different, would give a high positive correlation after the same fatiguing work, but we have seen that, in these tables, the average correlation is approximately zero. Therefore, from these considerations we conclude that fatigue is quite general in its production.

For the reasons given in this section we are forced to the rather unanticipated conclusion that *fatigue is produced generally, but transferred specifically.*

It will now be necessary to compare the results gained in this part of our investigation with those obtained in Part I.

In Part I we found that when the two tasks, multiplication and cancellation were alternated, a period of the latter, in its influence upon the succeeding period of multiplication, had not the same effect as a rest of equal duration as would be the case if fatigue were absolutely specific, but was equivalent to a period of multiplication. That is to say, a period of cancellation and a period of multiplication both have an adverse effect, not necessarily of the same magnitude, upon either multiplication or cancellation. From this we concluded that fatigue was general and not specific. A brief consideration will show that this conclusion is in complete agreement with the results obtained in this part of our research, viz., that fatigue is produced generally and transferred specifically. In

[1] See Hart and Spearman. General Ability. Brit. Jour. Psych. Vol. 5, 1912, p. 58. Abelson. Mental Ability of Backward Children. Brit. Jour. Psych. Vol. 4, 1911. p. 298.

[2] See Works of Hart and Spearman, Burt and Abelson on this subject.

Part I we were investigating the effects of two different tasks, cancellation and multiplication, upon efficiency in either cancellation or multiplication, *i.e.*, the effect of the fatigue induced by two different tasks upon the same task. It is evident that we are here dealing with the production of fatigue—the continuous work varies but the task remains constant—not with its transference from one kind of continuous work to several tests as in Part II of this research. The results of Part I could only be true if fatigue is produced generally, as both multiplication and cancellation have a similar effect upon either multiplication or cancellation; and this condition we also find necessary in Part II to explain the high average correlation between the depreciations in the same test after various kinds of continuous work.

The question now arises whether we can draw any conclusions from these results which will have a practical bearing upon education. Is a change of task as good as a rest? No doubt a change of task has an important influence upon the pupils' *willingness* to work, but does it really increase the pupils' ability to work? After, say, forty-five minutes algebra, in the course of which some fatigue must be induced, is the ability for assimilating history unfatigued?

In the light of the results gained in this research, we answer emphatically that a change of task is *not* as good as a rest, but it may be better than no change of task at all, or than an inadequate rest. If, as we have tried to prove, fatigue is produced generally, then every task will be affected by the fatigue incurred in the preceding task, but not necessarily to the same extent. In Table 33 we have seen that all the tests do not show the same loss in efficiency, unless, indeed, as may be quite possible, the fatigue coefficients in two tests are not strictly comparable; for it is a very debatable point whether in two tests which show the same fatigue coefficients, the loss in ability is the same. Objectively the comparison is easy, but it is questionable whether the same objective facts correspond to the same subjective conditions. If we interpret our results objectively, the effect of 90 minutes multiplication does not affect all the succeeding tests equally. The intervening rests cannot wholly account for the differences, for, if they did, the fatigue coefficients should decrease continuously from the top to the bottom of each column; for the last test has a much larger amount of rest preceding it than any of the earlier tests. It will be seen that the last test, dotting, suffers more than most of the others, from the adverse effects of the continuous work. No doubt, if no rest at all were allowed, the fatigue coefficients would all tend to be higher than they are. Therefore, if no rests can be given between lessons, it would seem possible theoretically to so arrange the time-table of lessons so that the average adverse effect of each lesson upon its successor would be a minimum. That is to say, if history tended to be but slightly affected by the fatigue due to mathematics, and English was greatly affected by the same subject, it would be better for history rather than English to follow mathematics. When we come to consider this question from the point of view of the individual pupil, the complexity increases, for we have seen that fatiguability is strictly specific; and if one pupil is affected in English more than in any other subject by the fatigue due to mathematics, another may find this same effect least. The only solution would seem to be the utilitarian one of taking the average effect upon all the pupils, or letting the majority decide. Whether this theoretical scheme is capable of being applied practically is for the educationalist to decide.

If rests can be given after each lesson, what is the most advantageous length of rest? The graph of the curve of recovery, Fig. 8, can throw some light on the answer to this question. We have seen on page that the results of Part 1 support the view that not only is fatigue produced generally, but also that incitement is general; and that a rest, therefore, abolishes the positive effect of incitement at the same time as it abolishes the negative effect of fatigue. From Fig. 8 we see that after one minute of hard mental work, the most advantageous *short* rest is one of about 30 seconds, because with such a rest, not only is much of the fatigue abolished, but much of the incitement is conserved. This length of rest seems also to be the most advantageous short rest when the period of work is increased to 4 minutes. Whether such a short rest would still be relatively most advantageous when the period of work is increased to 45 or 60 minutes, these results can give no answer; but the probability is, as this rest is relatively most advantageous after work-periods of 1, 2, 3, and 4 minutes, it would continue so for work-periods very much longer than these, and possibly for work-periods of 60 minutes.

Fatiguability, Improvability, and Ability.

The Kraepelin school, notably Lindley,[1] and Cron and Kraepelin,[2] have found a tendency for improvability to be associated with fatiguability; but the number of subjects in each case has been small. Wimms[3] has found a considerable inverse relationship between retentivity of practice and fatiguability in his experiments upon twelve boys, but found no correlation between improvability and fatiguability.

To see what relation, if any, exists between fatiguability, improvability, and ability, with the forty-two subjects of this research, we have calculated the correlation coefficients between each pair, the results of which are given in Table 51.

TABLE 51.—Correlations between fatiguability, improvability, and ability. In the second and third columns, the figures in brackets are for the improvability relative to the score on the last day.

Tests.	Correlation Coefficients between		
	F. and A.	F. and I.	A. and I.
Multiplication	·06	·01 (— ·07)	·34 (— ·11)
Spots	·14	— ·04 (— ·07)	·35 (·25)
Memory	·36	·19 (·29)	·19 (·16)
Cancellation ...	·09	·09 (·06)	·40 (·10)
Tapping	— ·05	·09 (·10)	·51 (·48)
Alphabet	— ·10	— ·09 (— ·32)	·41 (·27)
Dotting	·35	·16 (·18)	·36 (·47)
Average ...	·12	·06 (·02)	·37 (·23)
P.E.'s of Aver.	·04	·02 (·05)	·02 (·04)

[1] Lindley, E. H. Ueber Arbeit und Ruhe. Psych. Arb. 1900, Vol. 3, p. 534.

[2] Cron and Kraepelin. Ueber die Messung der Auffassungsfähigkeit. Psych. Arb. 1897, Vol. 2, p. 318.

[3] Wimms, J. H. The Relative Effects of Fatigue and Practice produced by Different Kinds of Mental Work. Brit. Jour. Psych. Vol. 2, 1907, p. 153.

The coefficient of fatigue is that used throughout this part of the present work. The coefficient of absolute improvability has been found by calculating the practice effect for the six days the experiment lasted, and the relative improvability coefficient has been obtained by expressing the absolute improvability coefficient as a fraction of the performance on the last day, before any fatiguing work was done. The scores for ability were those of the first day of the experiment before the continuous work was done.

The correlation which is just three times the probable error is ·28.

Between fatiguability and ability there is no definite correlation; the average coefficient is only ·12 with a probable error of ·04. Between fatiguability and improvability also there is no appreciable correlation. Between ability and absolute improvability there seems to be a definite relationship. The average correlation for all the tests is ·37, which is more than four times the probable error of the correlation coefficient, which is ·084, while the probable error of the average itself is ·02. The coefficients between relative improvability and ability are not quite so high, and one of them, multiplication, is negative. This was the only test with which the subjects were, to any degree, familiar before the experiment commenced, and, therefore, the improvability coefficients of this test might not be strictly comparable with the corresponding coefficients of the other six tests, which were relatively novel. The subjects were probably much more advanced in practice in this test than in any of the others. However, the average correlation of all the tests is ·23, which is less than three times the probable error; and, therefore, gives very little evidence of any definite relationship between relative improvability and ability, although the fact that all the coefficients except one are positive, and two are over five times their probable errors, leads us to infer that there may be some association between the two.

To sum up, we can say that, between fatiguability and ability, and between fatiguability and improvability, we find no evidence of any association. Between ability and absolute improvability there is substantial evidence of a definite relationship; but between ability and relative improvability there is only a slight probability of any association.

Summary of Part II.

1. In investigating the facts of fatigue, a preliminary practice period is a necessity, not only to enable the experimenter to estimate and control the effects of practice, but also to eliminate as far as possible the variable and arbitrary effects of unaccustomed tasks and surroundings.

2. Subjective estimates of the subjects' state of fatigue do not correspond to any great extent with the objective results as exemplified in the losses of efficiency with prolonged mental work. The subjected estimates after the three kinds of continuous *mental* work show some degree of correlation with one another, but subjective fatiguability after muscular work is not associated with subjective fatiguability after any of the mental tasks.

3. Prolonged mental work has an adverse effect upon succeeding mental work, either of the same or of a different kind; but this effect

is not apparent after prolonged muscular work. Mental work seems to have no appreciable adverse effect upon succeeding muscular work, as exemplified in tapping.

4. There is no correlation between the depreciations in different tasks caused by the same fatiguing work—that is to say, fatigue is transferred specifically and not generally. This explains the diversity of the results met with in experiments upon fatigue, where the degree of fatigue induced by a certain task is calculated by its effects upon a succeeding different task. This procedure is tantamount to assuming that fatigue is transferred generally.

Not only is there no correlation between the depreciations in different tasks after the same fatiguing work; but the depreciations in the fatiguing work itself do not correlate to any appreciable extent with the depreciations in the succeeding tasks.

5. The correlations between the depreciations in the same task after fatiguing work of different kinds are highly positive—that is to say, each subject is affected in a similar degree, relative to the other subjects, in any one task after every kind of work. Muscular work has the same effect as mental work upon the same task. From this result we infer that fatigue is produced generally, although it is transferred specifically.

6. With these subjects we find no evidence of any relationship between fatiguability and ability, or between fatiguability and improvability. There is a tendency, however, for ability to be associated with improvability.

Part III.—The Work Curve, and the Curve of Recovery from Fatigue.

1. The Work Curve.

The aim of this part of the present investigation was to determine the shape of the work curve under certain conditions. This subject has been extensively studied in Germany, particularly in Kraepelin's laboratory, and also in America, notably by Thorndike. The work-period almost invariably used has been one of 5 minutes. It seemed to us desirable that some work should be done to see the effect of shorter work-periods upon the curve of work. This question has already been touched upon by Chapman[1]. The work of this investigator differs from previous work in that he has used 2-minute work-periods, and has employed a much larger number of subjects, viz., 46 men. The task was the addition of columns of 10 one-place numbers for two 10-minute periods on each of five days. The curve was then constructed from these 460 measurements. It showed that for the first four minutes the loss of efficiency was very marked. This fall became gradually less abrupt, until, after ten minutes, the curve had become almost horizontal. The most striking characteristic of the curve is its regularity, showing that, with a large number of subjects, or with a large number of measurements from the same subject, the individual variations are obliterated, and the common characteristics of the curve become evident.

Our own Experiments.

In these experiments we have used the four subjects who had done the work described in Part 1. They were at the limit of practice in cancellation, which was the task used. The work-period was 1 minute, and the matter to be cancelled was the same as that described in Part 1 of this research. The test lasted for 20 minutes, and was done by each of the subjects on 45 days. At a given signal at the end of each minute the subjects commenced a new line. Thus in every period, except the first, some time was lost in changing from one line to the next. To place the first period on the same level as the others, ten seconds practice was given before the first period, at the beginning of which the subjects commenced a new line. Thus in each period, a certain amount of time, which we presume to be constant, was lost in changing from one line to the next. Another potential source of error was the time lost in changing to a new sheet when one sheet was completed. Each subject used about four sheets in the 20 minutes. As the subjects were at the limit of practice, if the work were always commenced at the top line of the sheet, the change from one sheet to the next would tend to always occur in the same period, and would make the scores for this period lower than would otherwise be the case. To obviate this error, the work was commenced a line further down the sheet each day. As the sheets contained 30 lines, on the 30th day of the experiment the subjects commenced work on the last line of the sheet. Thence each day the starting point was advanced two lines each day, covering the page in 15 days, and completing the 45 days of the experiment. This method of procedure would eliminate, also, any effects of very difficult or very easy lines always falling in the same period. The incentives to work at maximum pressure were the same as in the preceding

[1] Chapman, J. C. A Study of Initial Spurt in the case of Addition. Jour. Ed. Psych. Vol. 6, 1915, p. 419.

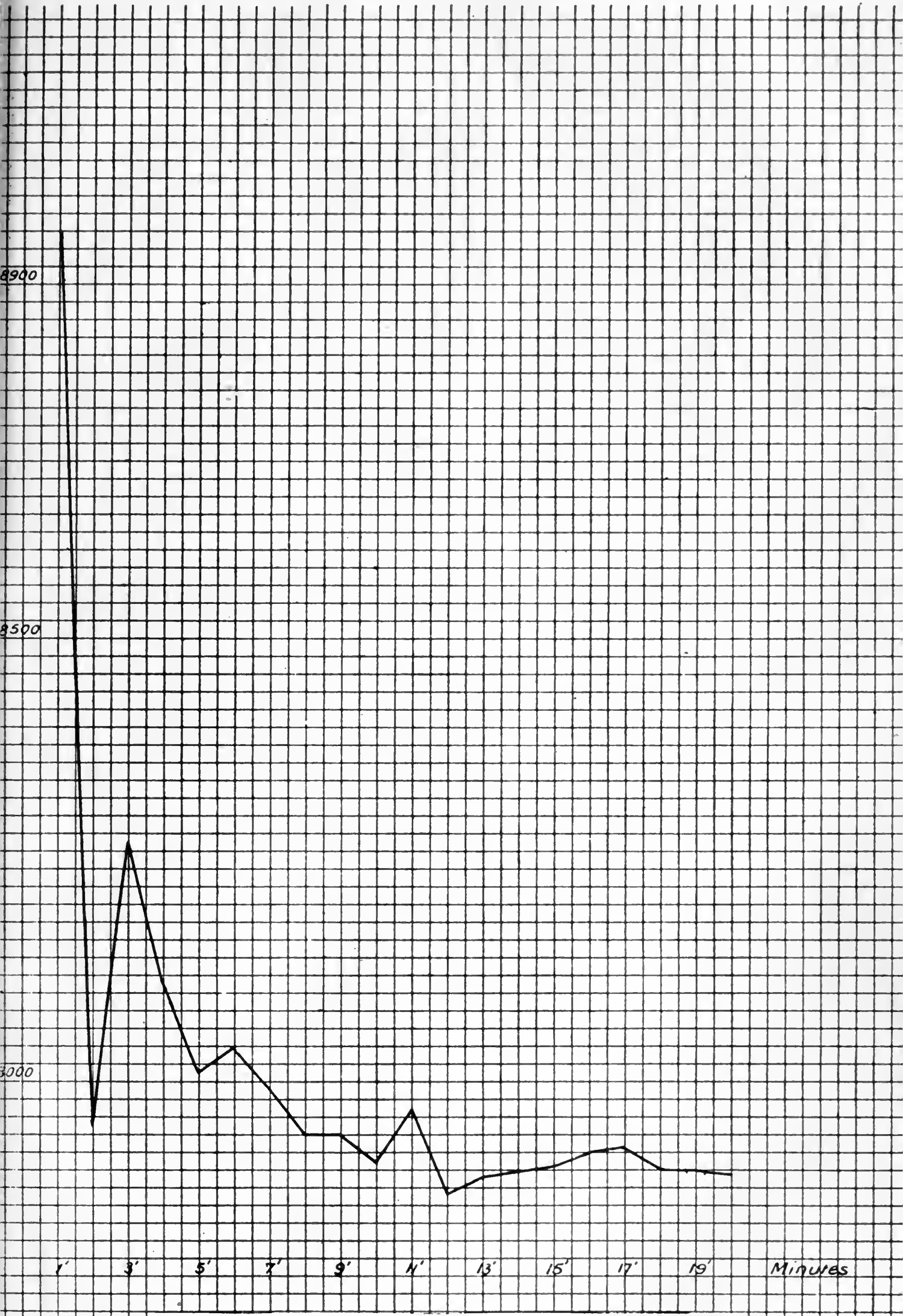

Fig. 35. Work-Curve for Cancellation in one-minute Periods
Average of 180 Measurements

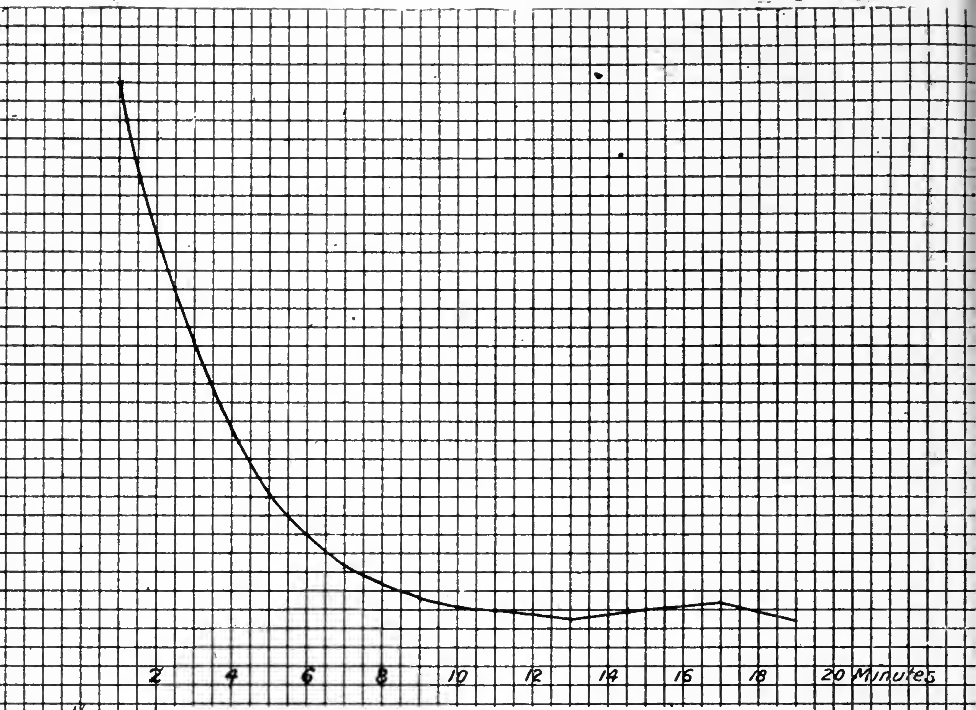

Fig. 36. Work-Curve for Cancellation in two minute Periods.

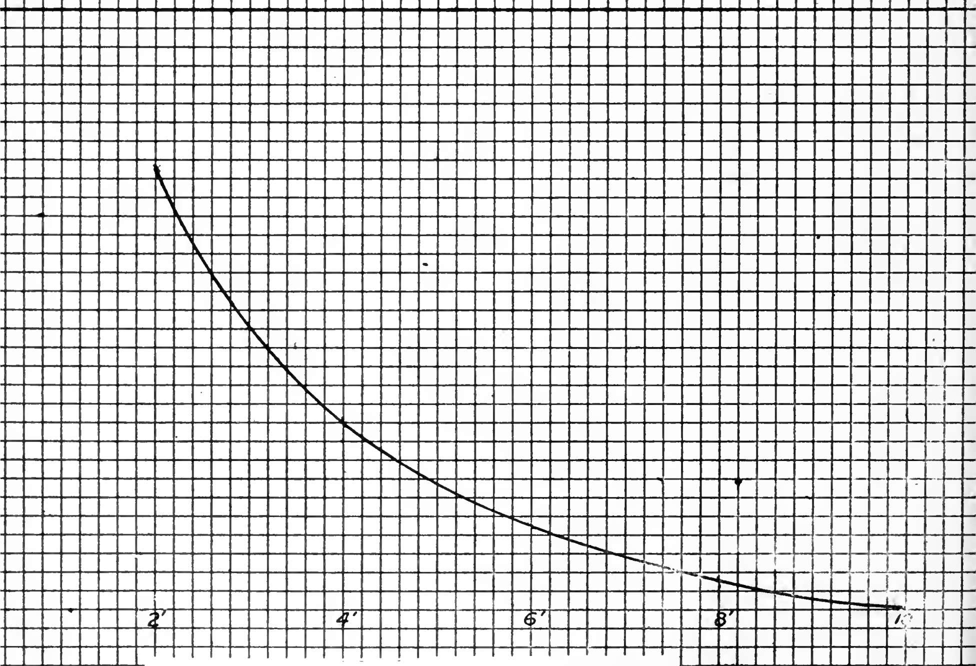

Fig. 37, Chapman's Work-Curve for Addition.

parts of this research. The scores are given in Table 52. Each number in this table is the total score for the four subjects, in one minute of work. In marking, one mark was given for each letter correctly cancelled, and one mark deducted for each letter wrongly cancelled. The curve constructed from these results is given in Fig. 35. In Fig. 36 we give the curve representing the same scores arranged in 2-minute periods, and in Fig. 37 Chapman's curve of addition for 2-minute periods is reproduced.

In Fig. 35 we find that the work curve for cancellation in 1-minute periods is fairly regular, and roughly corresponds with Chapman's curve for addition. The curve falls very rapidly at first, then more gradually, and finally, after about 12 minutes work, it becomes horizontal and continues so.

TABLE 52.—Scores in minute-periods. Cancellation.

Days.	Minutes.									
	1	2	3	4	5	6	7	8	9	10
1	426	398	409	401	410	394	388	402	390	415
2	424	409	389	409	415	398	379	385	406	386
3	431	425	427	414	436	427	411	415	406	416
4	450	426	422	444	428	418	420	440	438	428
5	413	407	417	411	404	410	420	394	421	395
6	421	405	412	449	438	415	419	415	412	431
7	443	424	428	432	430	412	415	418	424	426
8	451	418	425	440	423	419	411	421	440	430
9	437	421	415	423	415	395	414	393	391	421
10	439	439	438	426	428	440	422	432	408	428
11	442	419	409	397	413	423	407	420	408	409
12	437	426	458	426	424	420	452	443	443	438
13	430	426	425	417	407	416	411	424	434	413
14	451	448	449	429	447	451	438	445	428	435
15	438	419	370	374	400	408	416	392	376	397
16	464	435	439	414	446	448	452	417	424	419
17	427	383	397	391	365	391	412	383	335	359
18	455	419	407	422	439	427	408	407	418	450
19	431	408	361	396	360	397	376	346	371	372
20	436	387	398	400	428	410	407	412	415	395
21	402	381	382	392	362	384	362	389	400	358
22	414	416	424	437	425	443	424	428	434	422
23	417	367	391	380	356	378	390	373	363	380
24	408	366	382	387	372	404	382	392	408	385
25	406	375	396	431	361	383	388	391	391	374
26	390	372	408	388	387	379	391	395	382	405
27	392	371	411	404	376	360	363	387	370	386
28	388	378	398	360	408	377	373	361	372	354
29	394	400	389	407	382	364	376	380	383	364
30	391	394	401	413	378	356	390	399	408	420
31	397	391	387	392	355	386	374	383	381	381
32	398	392	385	418	395	388	399	382	364	392
33	438	388	435	372	384	393	396	403	410	406
34	403	366	378	361	386	409	407	367	400	398
35	412	391	397	378	417	411	398	420	402	357
36	406	424	372	398	372	374	382	377	368	404
37	439	401	390	391	388	393	369	396	393	393
38	420	347	400	384	391	419	407	410	379	394
39	426	403	390	392	427	398	392	401	415	401
40	374	347	407	384	370	440	386	395	387	359
41	443	421	377	375	425	418	396	378	414	391
42	393	348	372	364	344	362	373	350	388	381
43	438	391	437	405	412	363	427	400	372	367
44	421	389	428	381	380	378	385	397	381	390
45	433	407	446	403	403	369	373	383	392	383
Average...	422·0	399·2	406·2	403·8	400·3	400·8	399·6	398·7	398·8	398·0

TABLE 52.—Scores in minute-periods. Cancellation—*continued.*

Days.	Minutes.									
	11	12	13	14	15	16	17	18	19	20
1	385	391	398	396	387	398	393	393	404	399
2	412	392	385	381	387	409	405	394	400	399
3	403	426	415	401	409	411	393	416	425	413
4	426	446	421	427	421	413	417	428	425	430
5	415	415	397	406	415	402	415	403	418	411
6	414	415	417	411	422	411	431	417	427	437
7	427	412	397	409	398	383	414	410	404	406
8	418	430	415	450	434	439	442	415	427	436
9	386	416	409	391	408	423	396	420	400	406
10	437	427	434	420	430	412	435	417	418	436
11	403	418	395	387	402	407	387	421	407	416
12	428	447	439	427	442	435	437	431	441	448
13	409	398	407	402	402	400	401	417	412	401
14	470	440	425	444	414	421	450	438	434	426
15	394	404	372	371	362	391	376	393	387	387
16	427	413	419	414	420	427	410	408	428	388
17	363	372	412	372	384	413	398	371	370	372
18	412	438	419	416	407	420	425	419	413	430
19	376	388	359	395	398	397	395	359	371	370
20	427	390	415	398	408	407	388	413	406	421
21	402	375	398	423	417	397	388	387	397	384
22	453	418	443	423	449	448	434	430	444	431
23	356	381	388	415	388	380	362	374	373	358
24	382	377	396	371	389	373	358	389	385	375
25	391	351	385	366	388	386	385	371	420	390
26	368	364	378	382	393	356	409	418	385	384
27	387	369	395	372	398	416	411	382	386	381
28	371	365	411	358	379	390	350	379	366	394
29	350	380	396	386	400	367	364	378	384	373
30	389	385	399	397	385	372	366	379	397	422
31	383	364	398	386	361	367	378	358	380	362
32	377	391	383	398	397	369	408	416	377	412
33	408	391	394	394	364	388	372	371	400	388
34	383	379	402	392	384	375	407	408	421	386
35	407	418	382	394	367	382	392	411	366	383
36	393	353	392	366	368	378	417	388	386	394
37	406	418	378	398	350	346	401	372	379	356
38	387	420	371	403	407	403	384	399	383	382
39	396	423	362	421	434	452	414	393	373	376
40	401	362	378	352	356	373	370	382	352	350
41	412	374	360	450	444	446	397	407	379	438
42	387	382	387	360	366	370	380	358	373	373
43	405	379	371	389	369	410	397	389	400	383
44	375	403	391	393	390	371	408	411	391	397
45	360	364	403	410	409	384	369	370	386	392
Average...	399·1	397·0	397·6	397·7	397·8	398·2	398·4	398·1	397·8	397·7

The chief irregularity in the curve is the height of the ordinate representing the work done in the second minute. This is extremely low. Less work was done in the second minute than in any other of the first 7 minutes. The question arises whether this result is due to random errors, or whether it is a significant point on the work curve. Chapman's curve for 2-minute periods of addition does not show such a result, but the difference in the lengths of the work-periods may account for this. If we combine each pair of points in Fig. 35, and so make a curve with 2-minute periods of cancellation, we get the curve shown in Fig. 36,

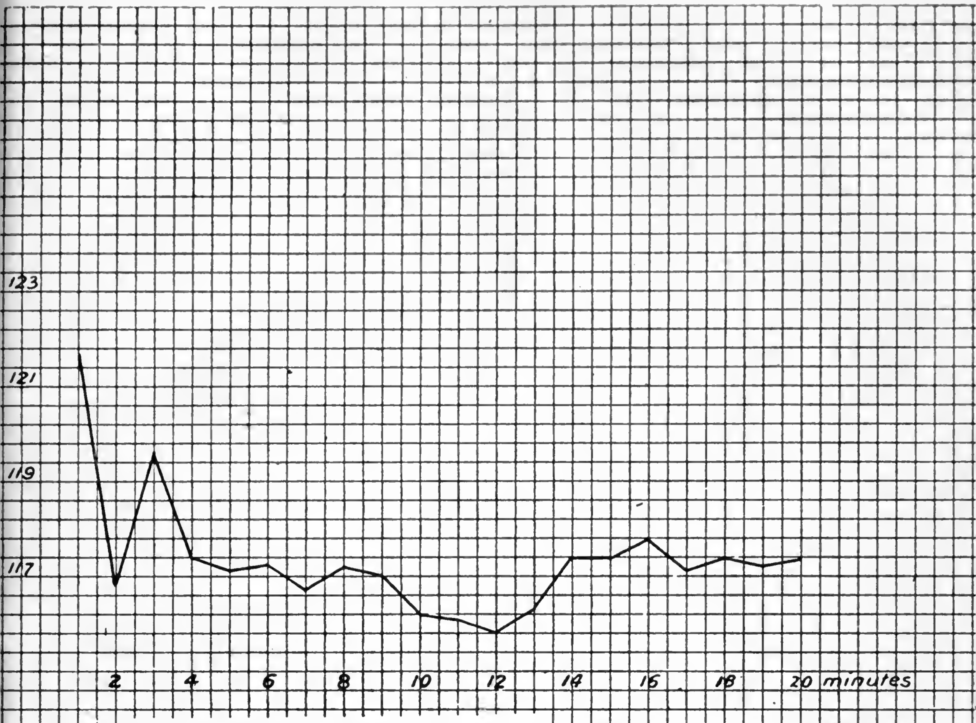

Fig. 38. Work-Curve for Subject I.

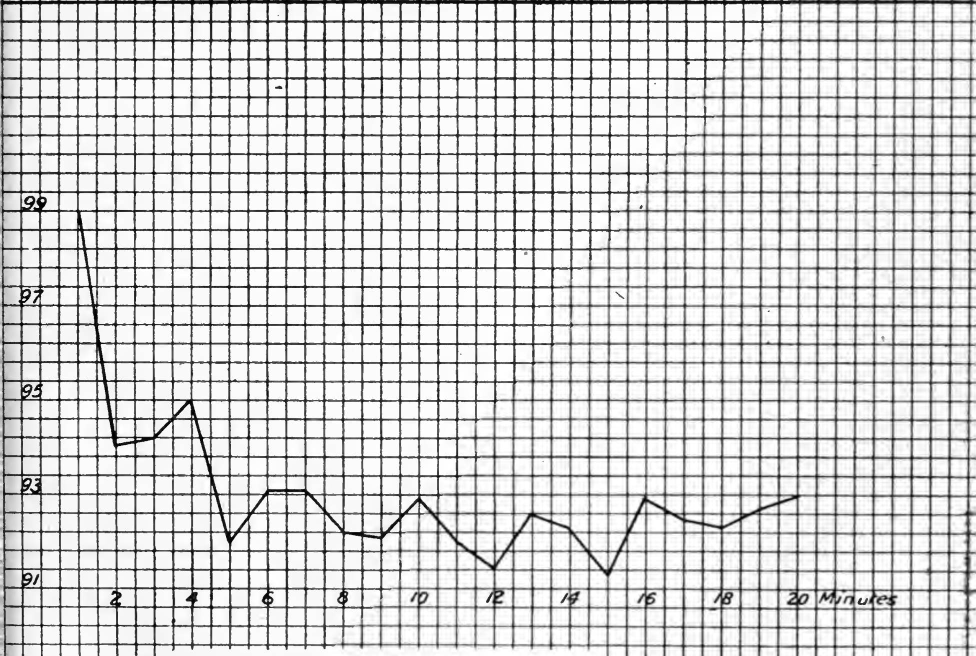

Fig. 39. Work-Curve for Subject 2

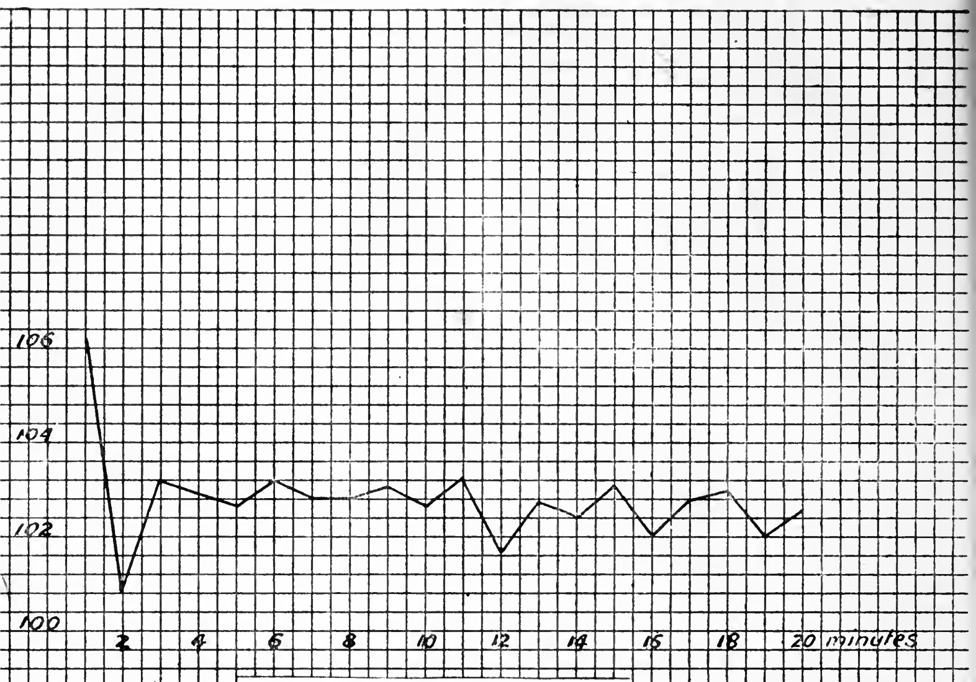

Fig. 40. Work Curve for Subject 3.

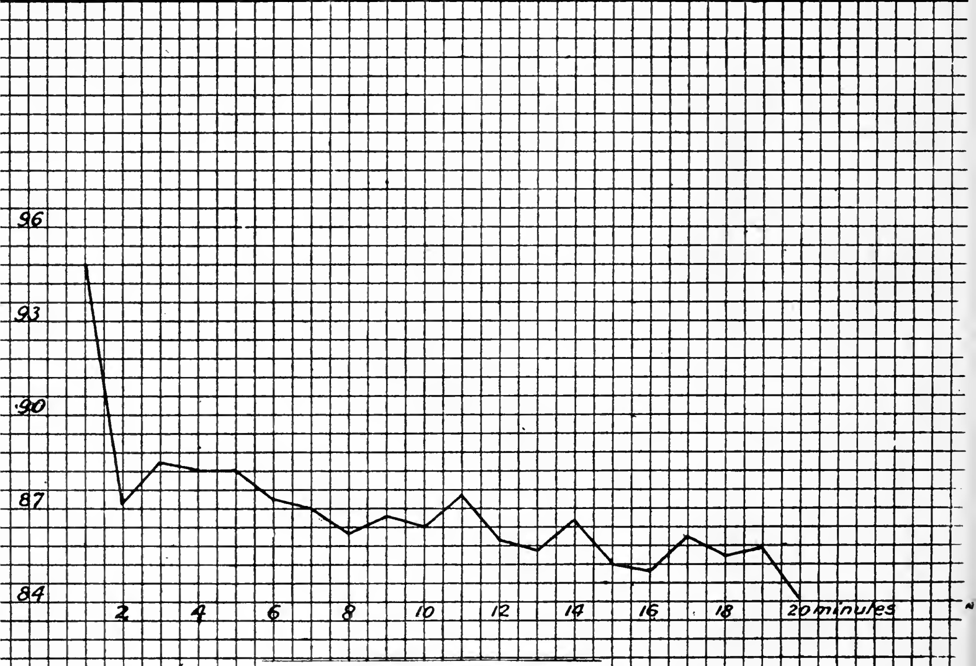

Fig. 41 Work Curve for Subject 4.

which is almost regular, and shows a remarkable resemblance to Chapman's curve given in Fig. 37. The curve in Fig. 36 becomes horizontal after about 12 minutes work, and if Chapman's curve were continued we should expect it to become horizontal in approximately the same time. It must be remembered that for these two curves absolutely different work was used. Chapman used addition while we have used cancellation. Evidently using 2-minute work-periods swamps this minimum point at two minutes, if indeed, it has significance.

In Figs. 38—41, we have given the individual work-curves of each subject, *i.e.*, each curve is the average of 45 measurements for the same subject. It will be seen that this minimum point is distinctly marked in all four, which materially strengthens the supposition that the point has significance. We can produce further evidence by calculation. The difference in the mean scores for the second and third minutes is 7·0, the means being respectively 399·2 and 406·2 (see Table 52). We can easily calculate the probable error of the difference between these two means. From Table 52 it can be seen that there is no practice effect. The subjects were at the limit of practice, and the scores for the last days of the series are no higher than those of the first days. Therefore the probable error of the difference of the means is given by the formula,

$$\text{P.E.} = \frac{\cdot 6745}{\sqrt{n}}\sqrt{\sigma_1^2 + \sigma_2^2 - 2r\,\sigma_1\,\sigma_2}.$$

where "σ_1" and "σ_2" are the mean square deviations; "n" the number of measurements in each column, and "r" the correlation between the columns.

Working out, this probable error is found to be 2·5. Comparing this with the difference of the means, 7·0, we see that the latter is hardly three times its probable error, and therefore this result gives no proof that the point in question is significant, although there is some degree of probability of its being so. There are about twelve chances to one against obtaining a difference bearing this ratio to its probable error purely as a result of random errors.

Further evidence bearing on this question can be obtained from the results given in Part I of this research. In that part subjects did five minutes multiplication and five minutes cancellation every day, sometimes in 1-minute periods and sometimes in ½-minute periods. We have constructed Tables 53 and 54 for the multiplication and cancellation scores respectively.

TABLE 53.—Total scores for 5 minutes multiplication, in minute periods. Thirty-five subjects each did the test twenty times.

	1st min.	2nd min.	3rd min.	4th min.	5th min.
Scores ...	26,375	22,886	23,100	23,077	22,930

TABLE 54.—Total scores for 5 minutes cancellation, in minute periods. Thirty-five subjects each did the test five times.

	1st min.	2nd min.	3rd min.	4th min.	5th min.
Scores ...	6,147	5,925	5,983	6,058	5,852

In Tables 55 and 56 are given the scores for 5 minutes multiplication, using shorter work-periods. In Table 55 the period used is 30 seconds, and in Table 56 the period is 15 seconds.

TABLE 55.—Total scores for 5 minutes multiplication, in 30-second periods. Thirty subjects each did the test ten times.

	1st min.	2nd min.	3rd min.	4th min.	5th min.
Scores ...	13,173	11,086	11,246	11,307	11,277

TABLE 56.—Total scores for 5 minutes multiplication, in 15-second periods. Four subjects each did the test ten times.

	1st min.	2nd min.	3rd. min.	4th min.	5th min.
Scores ...	5,658	5,180	5,242	5,088	5,012

In Tables 53–56, graphs of which are given in Figs. 42–45, we have two kinds of tasks, and three lengths of work-period, with all of which the scores for the second minute are lower than those of the first or third. There is no constant deterioration shown in efficiency, as we have seen in the curve for 2-minute periods. Moreover, all the work curves which we have been able to obtain from our data have been given. The curves given are not selected because they happen to bear out a particular theory or support a particular fact. It is highly improbable that eight independent curves, differing both as to the length of the work-period as well as the task performed, should each show the same result merely as the result of chance. Therefore, although the mathematical evidence is inconclusive, though favourable, on this point, we contend that there is a high degree of probability that a minimum point occurs on the work curve at the ordinate representing the amount of work done in the second minute. With work-periods longer than one minute, this minimum point becomes swamped.

The Individual Curves.

The individual curves for 20 minutes cancellation are given in Figs. 38–41, and it will be seen that they roughly corroborate the average results given in Fig. 35. Owing to the smaller number of measurements, the graphs are not so regular in Figs. 38–41 as in the latter. In Fig. 38 the horizontal phase seems to be reached after about ten minutes; in Fig. 39 after about eleven minutes, but in Fig. 41 there is no horizontal phase apparent. In Fig. 40 the curve becomes horizontal somewhat earlier, about seven or eight minutes. None of these graphs are regular enough for us to say definitely whether the initial point of the horizontal phase depends upon the individual or not. The random variations are too great. In Figs. 46–49 are given the same curves arranged in 2-minute periods. It will be noticed that the minimum point at two minutes has now disappeared, but the variations are still too great to admit of a detailed comparison with Fig. 36, the average curve. There are marked individual differences between the subjects regarding the loss in efficiency due to 20 minutes cancellation. The average losses for each subject, found by comparing the work done in the first minute with the average amount done in the last six minutes, when the curve has reached its horizontal phase, are 3·5 per cent., 7·2 per cent., 3·4 per cent., and 9·7 per cent. respectively.

Effect of Different Tasks on the Shape of the Work Curve.

Though the shape of the work curve obtained varies according to the length of the work-period, yet it does not seem to depend upon the nature of the task performed. Using work-periods of 2 minutes, we

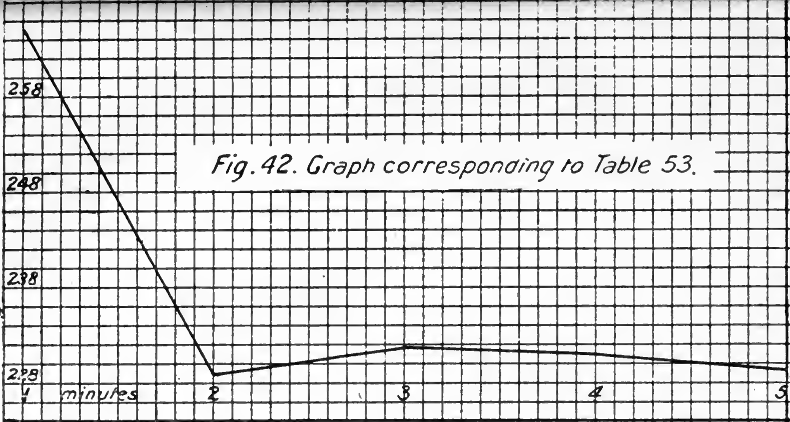

Fig. 42. Graph corresponding to Table 53.

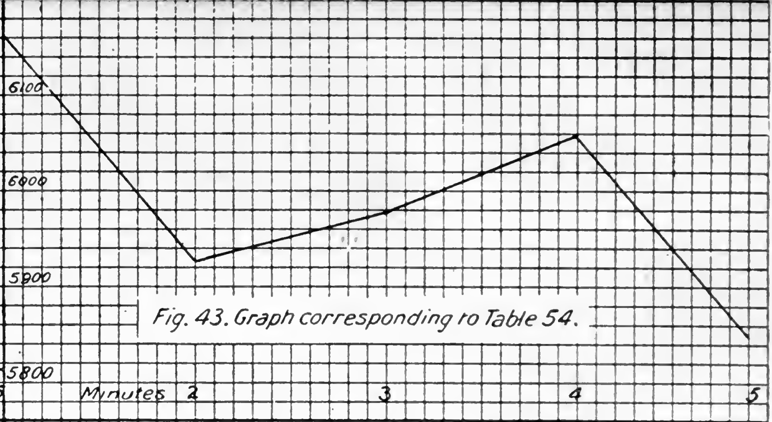

Fig. 43. Graph corresponding to Table 54.

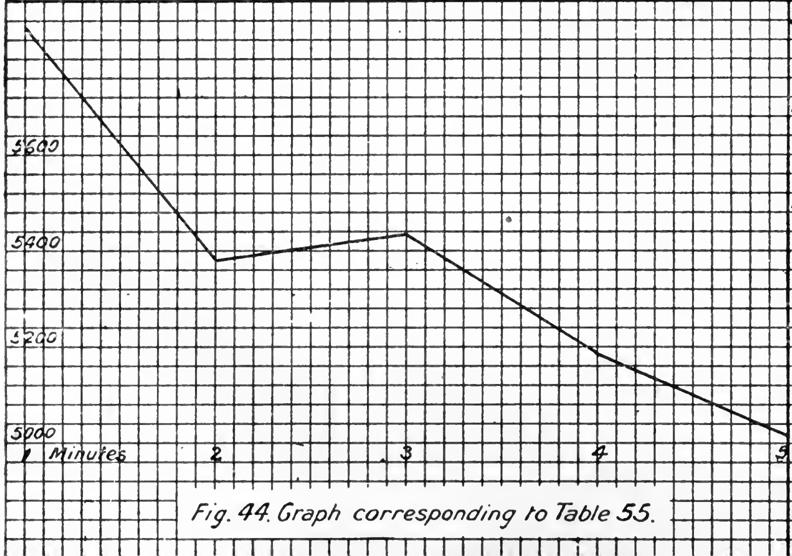

Fig. 44. Graph corresponding to Table 55.

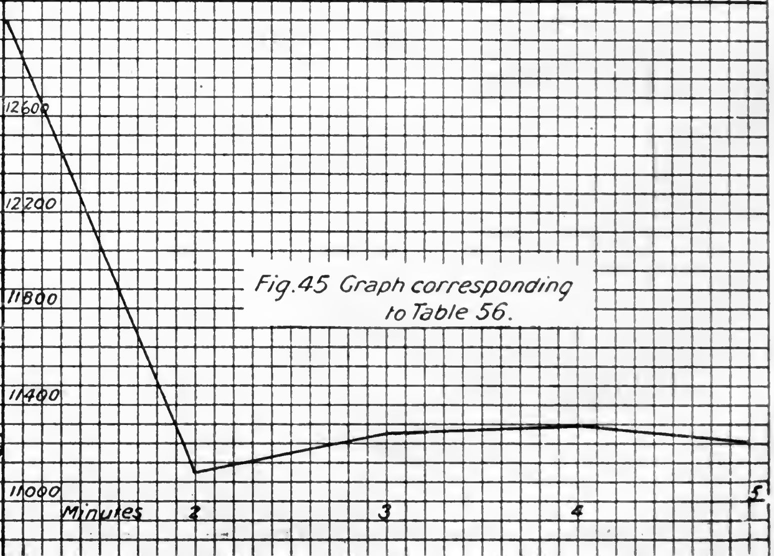

Fig. 45 Graph corresponding to Table 56.

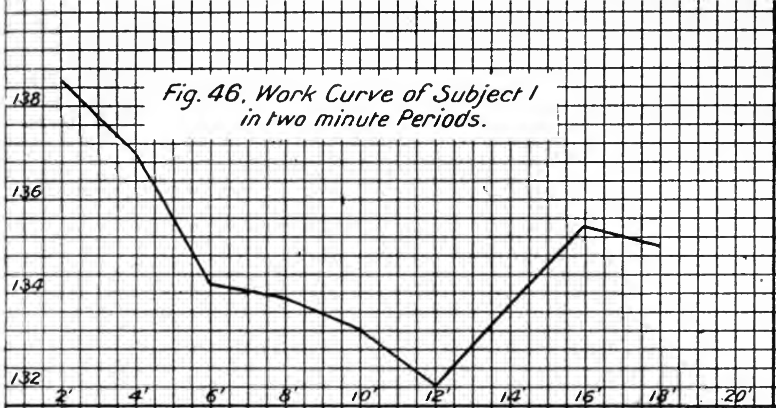

Fig. 46. Work Curve of Subject 1 in two minute Periods.

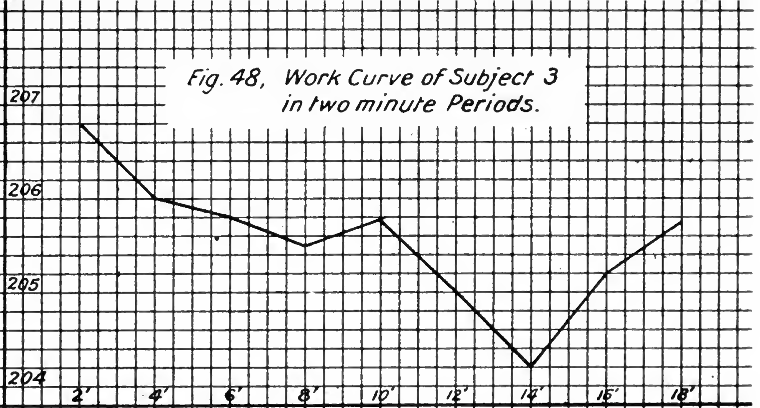

Fig. 48, Work Curve of Subject 3 in two minute Periods.

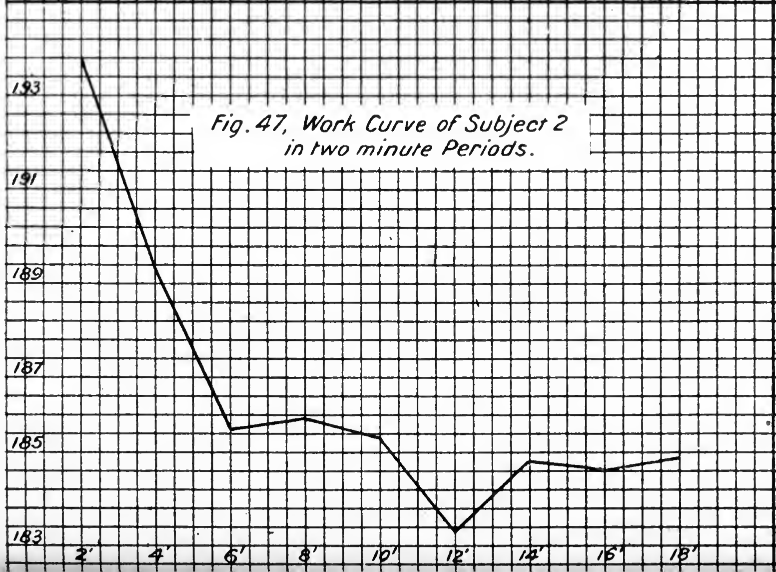

Fig. 47, Work Curve of Subject 2 in two minute Periods.

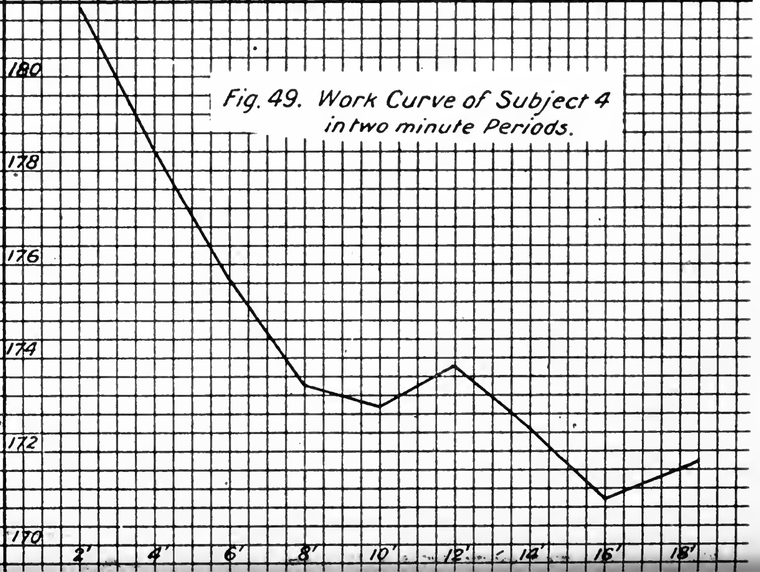

Fig. 49. Work Curve of Subject 4 in two minute Periods.

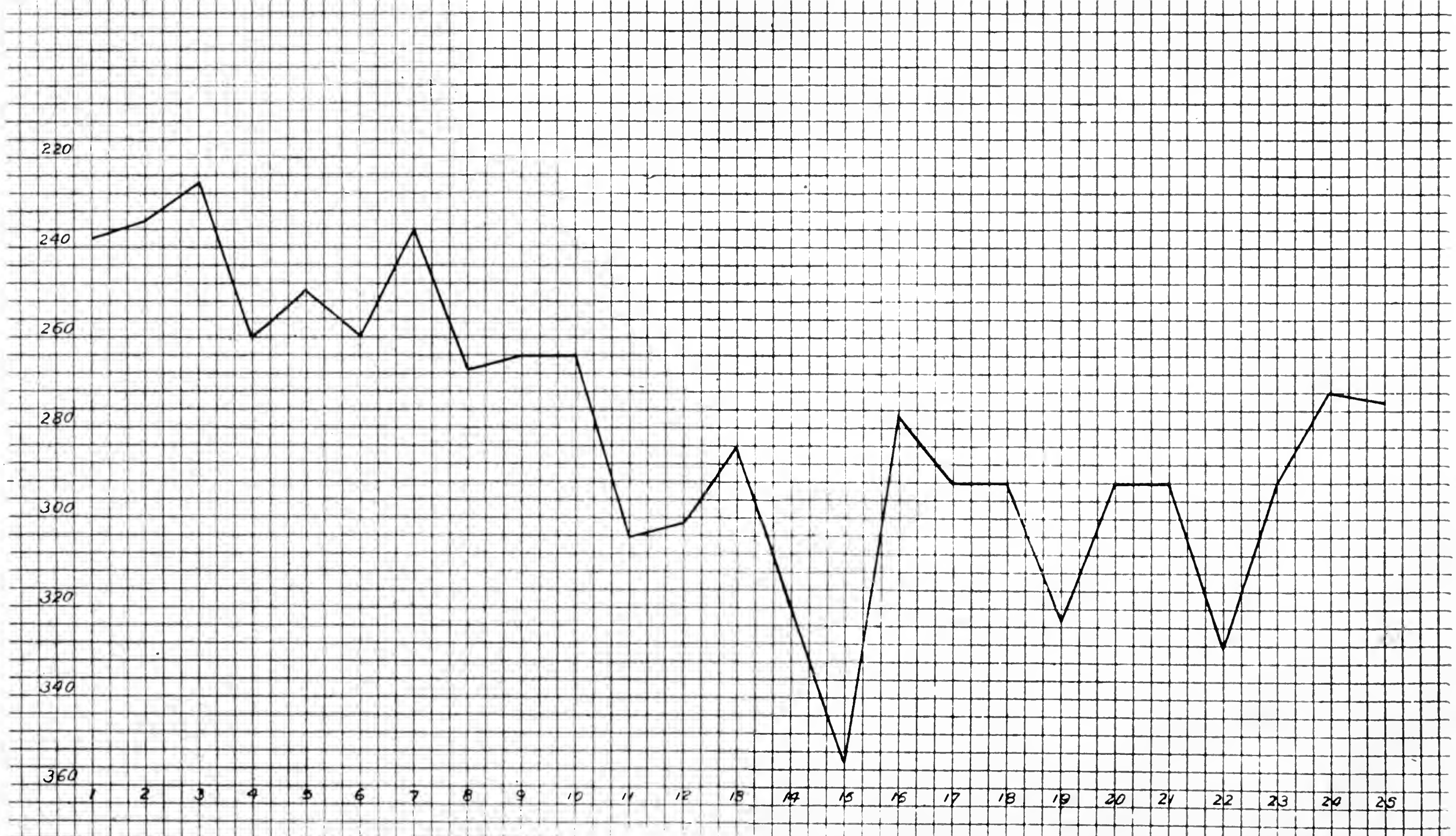

Fig 50 Twenty Minutes Dotting. Average of four performances.
Unit = 100 dots.

have seen that Chapman's curve of addition is precisely similar in shape to our curve of cancellation. In both of them the fatigue effect is confined to the first 12 minutes. Further work does not result in a further loss in efficiency. This would account, in part, for the unsatisfactory work curves obtained by the Kraepelin school, with the use of 5-minute periods. Using periods of such a length would confine the fatigue effect to the first three points on the graph.

As a means of testing still further whether this characteristic shape was independent of the task, one subject, at the limit of practice, did, on four days, 20 minutes continuous dotting. The conditions were the same as those specified for this test in Part II of this research. The machine was set at a constant speed, at which 125 dots passed the slot in one minute. This speed was fast enough to prevent the subject hitting the inner ring every time, and slow enough for him not to always miss both rings. Thus the subject had to aim at 2,500 dots in each 20-minute test. The marking was not the same as in the previous work. For convenience in adding up the scores, no marks were given for a dot within the inner ring, one mark if on the inner ring, two marks if between the two rings, three if on the outer ring, and four if outside the outer ring. The scores were then calculated for each hundred dots, and a graph made which will be comparable with the graphs of the other tasks for minute periods. Similarly, the graph constructed for the scores of each two hundred dots will be comparable with the graphs of the other tasks for 2-minute periods, as well as with Chapman's curve of addition. These two curves are given in Figs. 50 and 51. Owing to the small number of performances we do not get a smooth curve in either figure, but both roughly corroborate the curves for other tasks (See Figs. 35-37). Both fall rapidly at first, then more gradually, and, finally, become parallel to the horizontal axis. This horizontal phase in Fig. 50 begins at about the abscissa marked 1,600 dots—that is to say, after about 12 minutes work. It is surprising that in all three kinds of work—adding, cancelling, and dotting—*this horizontal phase commences after approximately the same duration of work.* It will be remembered that the subjects were always instructed to work at maximum speed, and it may be that when working at maximum pressure, upon no matter what kind of work, the point at which the work curve becomes horizontal is a constant. At any rate, it seems evident that when working at maximum speed the work curve is of constant shape, and there is a strong probability that the horizontal phase begins after a definite interval of time. It will, perhaps, be interesting to investigate still further this horizontal phase of the work curve. Is its depression below the initial point constant for all subjects, and for all tasks, or does its depression depend upon the individual, or upon the task, or upon both? The losses in efficiency for the four subjects, found by comparing the average amount of work done per minute in the last six minutes, when the average work curve is horizontal, with the amount done in the first minute, are 3·5 per cent., 7·2 per cent., 3·4 per cent., and 9·7 per cent. respectively. This result indicates that the depression of the horizontal phase of the curve, below the initial point, varies with the individual. The subject who is most fatiguable in this test shows a loss in efficiency of almost three times the amount shown by the subject who is the least fatiguable of the four. If we take the average amount of work done in the first minute as representing the subject's ability for cancellation, and compare this with the fatiguability coefficients given above, we can see that there is a distinct inverse correlation between the two. These results are placed side by side in Table 57.

TABLE 57.—Cancellation. Fatiguability compared with ability.

Subject.	Fatiguability.	Ability.	Subject.	Fatiguability.	Ability.
A	3·5	122	C	3·4	106
B	7·2	100	D	9·7	95

Subjects A and C, who show the greatest ability, also show the least fatiguability; and subjects B and D, who show the least ability, are the most fatiguable for this particular test. With only four subjects, a correlation coefficient would be quite illusory, but the tendency is clearly marked. With such a small number of subjects this result cannot be held to have a general significance; but with these particular subjects, ability for cancellation is associated with unfatiguability.

The question now arises whether the *kind of task* affects the depression of the horizontal phase below the initial point of the curve. The data available in this research are not sufficient to give any conclusive evidence on this point, but will serve merely as an indication. We should expect, on the grounds of common-sense, that the task would have a great influence on the position of the horizontal phase. A task which makes a great demand on the subject's store of energy would be expected to reach a lower point than any easy task, and the height of this horizontal phase would, conversely, be an index of the relative difficulty of the task. Lehmann[1] has settled this point experimentally in the case of muscular work. Using the ergograph with a constant load, and by varying the time of rest between each contraction, he has constructed ergograms for various rates of performing muscular work. The longer the rest between each pull, the lower would be the rate of working, and conversely. The horizontal phases of the work curve, which represent the amount of work done with the shortest intervals of rest, show the greatest depression below the initial score, while, if this interval of rest be increased up to a certain point, the work curve is a straight line parallel to the horizontal axis. In other words, the horizontal phase is on the same level as the initial point, and there is no fatigue effect. The depression of the horizontal phase is inversely proportional to the length of the interval of rest between each contraction. In our research, one subject has done, at the limit of practice, three tasks continuously for twenty minutes, viz., cancellation 45 times, multiplication 10 times, and dotting 4 times. The average has been taken of his performances at each task. Owing to the number of performances in multiplication and dotting being so small, random errors will be expected to have a large influence. Instead of finding the loss of efficiency in the average score of the last six or eight minutes, as compared with the average score in the first minute, we have found the average loss in efficiency of the 16th, 18th, and 20th minutes respectively, expressed as a percentage of the work done in the first minute. By doing this we can see if the random errors are large, by noticing the variation in these losses in efficiency for the same work. These results are given in Table 58.

[1] Lehmann. Grundzuge der Psychophysiologie. Leipsig. 1912. Erstes Buch.

Fig. 51. Twenty minutes Dotting. Average of four performances.
Unit = 200 dots.

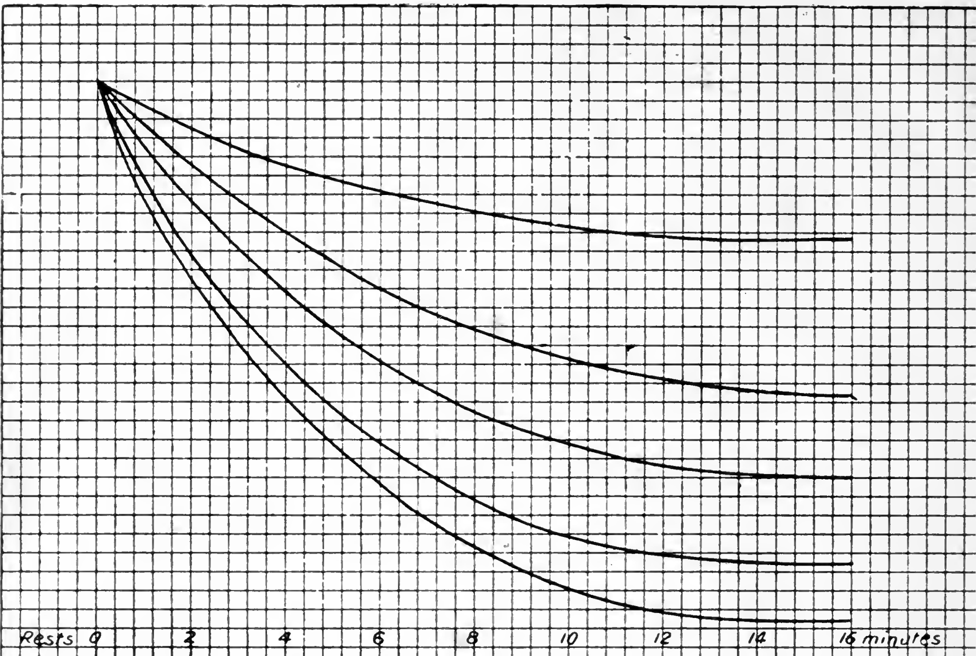

Fig. 52. Work Curves for different Tasks arranged so that their initial points coinside

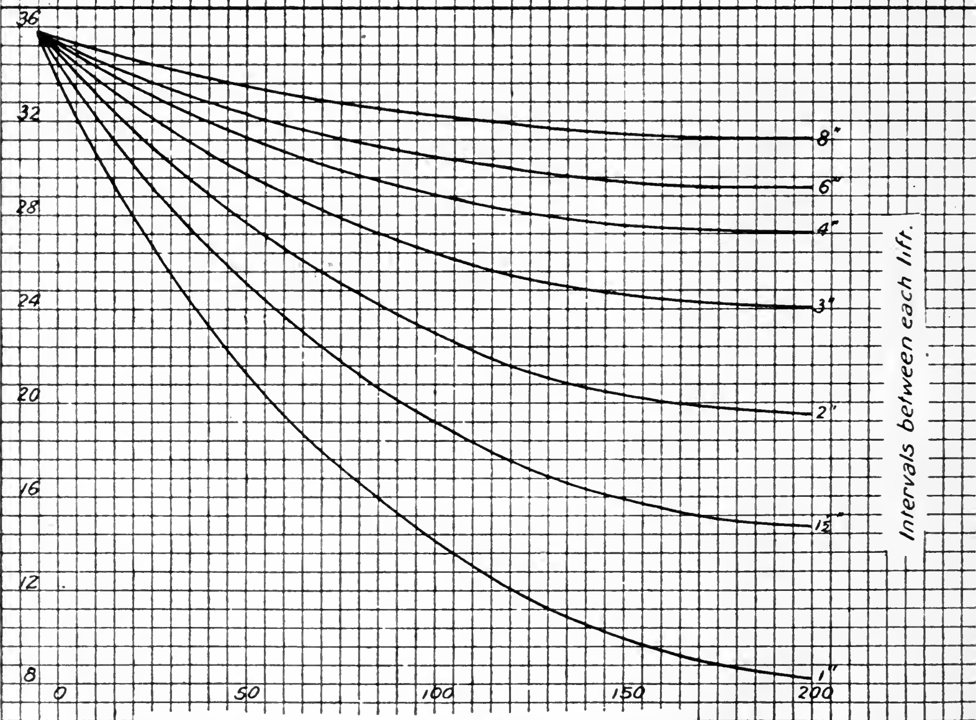

Fig. 53. Lehman's Curves for Muscular Work.

TABLE 58.—Losses in efficiency of different tasks, for one subject, at the limit of practice.

Tests.	Losses in Efficiency in the—			
	16th min.	18th min.	20th min.	Average.
	Per cent.	Per cent.	Per cent.	Per cent.
Multiplication	8·4	7·4	6·0	7·3
Cancellation	3·1	3·5	3·9	3·5
Dotting	48·0	37·0	42·5	42·5

Table 58 shows that the random errors are not nearly so large as might be expected, and are certainly not large enough to account for the differences between the three tests. The probable errors for the three tests, in the order given in the Table, are ·38, ·13, and 1·75 respectively. If we allow a deviation in these average losses in efficiency of four times the probable error, the odds against which happening by mere chance are about 140 to 1, the average loss for multiplication would lie between 5·8 and 8·8, that for cancellation between 3·02 and 4·02, and for dotting between 35·5 and 49·5.

At a first glance the conclusion to be drawn would seem to be that the task has a determining influence on the depression of this horizontal phase. With multiplication and cancellation such a conclusion is probably correct, for in these tests the subject himself controls the speed at which the figures to be multiplied or letters to be cancelled come into consciousness. If the speed at which he commences to multiply is found later to be too great, it is automatically reduced, but still he is working at maximum effort. On the other hand, in the dotting test the speed is fixed by the experimenter; and, if this speed were, at first, only slightly below the maximum speed at which the subject could strike at the dots, when his efficiency owing to fatigue falls so that this rate is above his maximum, the decrease in his scores might not be in any proportion to the amount of fatigue present. Under such conditions his score might be very greatly reduced; and, consequently, a very great apparent fatigue result be shown. It certainly did not seem to the experimenter that such a condition was true of this subject, either when watching him work, or, what is a better criterion, from the results seen when correcting his work. The speed was not at any time fast enough to prevent him aiming at every dot. But, still, such a condition as described above is possible, and the speed might have been fast enough to cause him to aim at random, and to prevent him seeing every dot before aiming. However, if we take only the figures for multiplication and cancellation, we see that the depression of the horizontal phase differs for the two tests, and this leads us to conclude that there is some degree of probability that the task has an influence on this phase of the work curve, but that these results need further corroboration.

If this is so in fact, a graphical representation of the work curves for different tasks, with their initial points coinciding, will correspond precisely with Lehmann's ergographic curves with varying lengths of rest between each contraction. Such a graphical representation is given in Fig. 52.

The Work Curve and the Curve of Recovery from Fatigue.

In Part I of this research we described the curve of recovery from fatigue, of which we gave a graph in Fig. 8. In this part, in Fig. 35, we have given the curve of work, with work-periods of 1 minute. In Fig. 8 fatigue is viewed positively—*i.e.*, a fall in the curve represents a diminution of fatigue or an increase in efficiency. In the work curve, Fig. 35, we look at fatigue from a negative point of view. A fall in the curve represents a gain of fatigue or a loss in efficiency. By turning either graph through 180 deg. round the horizontal axis, we can change our viewpoint. In Fig. 8 fatigue would then be viewed negatively, and in Fig. 35 positively. For convenience in making a comparison we have considered fatigue positively in one graph and negatively in the other. We have already stated our reasons for believing that the work curve has a minimum point at the ordinate marked " 2 minutes." If this is so in fact, we are struck with the remarkable similarity in shape between the work curve—the graph of the onset of fatigue—and the graph of recovery from fatigue by rest. Both descend very rapidly at first to a minimum point, then rising to a maximum point, which is considerably lower than the initial point of each curve. Thence the descent is less rapid, and the curves gradually assume a horizontal line parallel to the horizontal axis. In Fig. 54 we give a hypothetical curve illustrating the onset of, and the recovery from, fatigue. The curve from A to X gives the loss of efficiency through continuous work. The efficiency of the work is here considered positively and the fatigue incurred, therefore negatively. At X the subject has stopped working, and the curve from X to B represents his gain in efficiency through rest, fatigue still being viewed negatively as a loss in efficiency. The part of the curve contained between X and B will be much greater in extent than that part between A and X, for we have seen that recovery from fatigue is much slower than its onset. With the subjects described in Part 1, we saw that a rest of 4 minutes was just sufficient to eliminate the effects of 1 minute of work, and that a rest of 16 minutes was insufficient to abolish entirely the effects of 4 minutes' work; while, on the other hand, after continuous work of about 12 minutes' duration, the work curve had become horizontal. Apparently after 12 minutes' work at maximum effort a rest very much longer than 12 minutes would be necessary to obviate completely its effects. But concerning the general shape of the curves of fatigue and recovery, we can say that each is the same shape as the other, revolved round the horizontal axis 180 deg. Being similar in shape, an analysis of one will correspond precisely with an analysis of the other.

We can find strong support from chemistry and physics that the shape of the curve of recovery from fatigue should be the same shape as the work curve, but in a different position. At a first glance one might expect each curve to be the same shape as the reflection of the other in a mirror, but further consideration will show that this cannot be possible. Let us take, for example, a reversible chemical reaction, when the system is in equilibrium—*i.e.*, when the velocity of the reaction in one direction is equal to its velocity in the opposite direction. The example of a reversible chemical reaction often given is—

$$\text{Alcohol} + \text{acetic acid} \rightleftarrows \text{ethyl acetate} + \text{water.}$$

When in equilibrium some of each of these reacting components exist beside each other. Now, suppose that we increase the amount of the components on the left-hand side of the equation. Immediately the equilibrium is disturbed, and chemical action ensues from the left to the right side of the equation. We are assuming all through that the temperature is kept constant. The velocity of this chemical action will be greatest at first because the preponderance of the left-hand side products

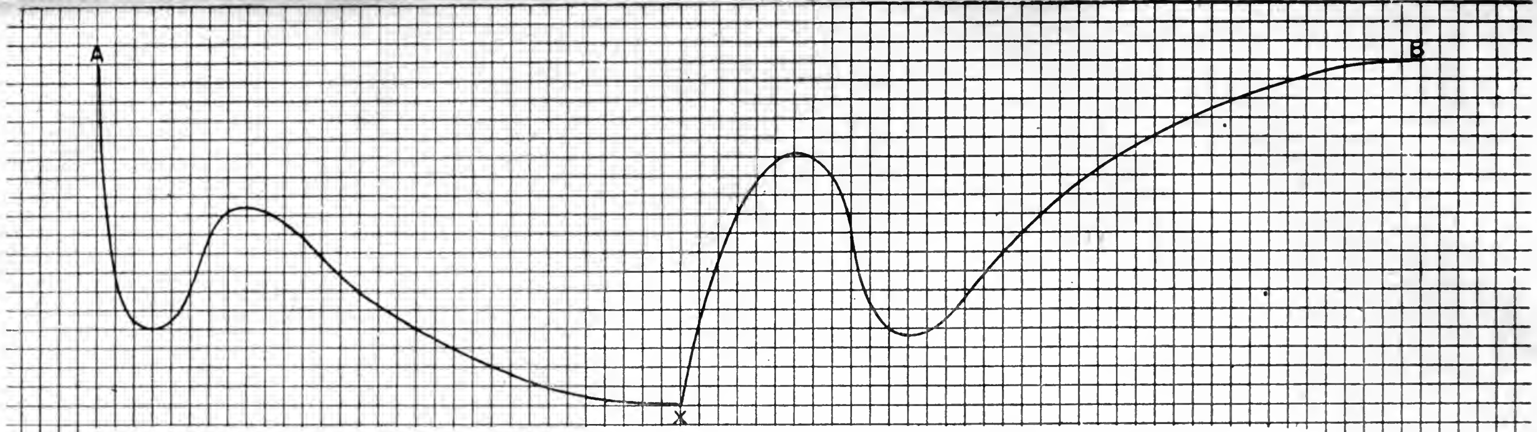

Fig. 54. Loss of efficiency through Fatigue followed by a Gain in Efficiency through Rest. Rest begins at X

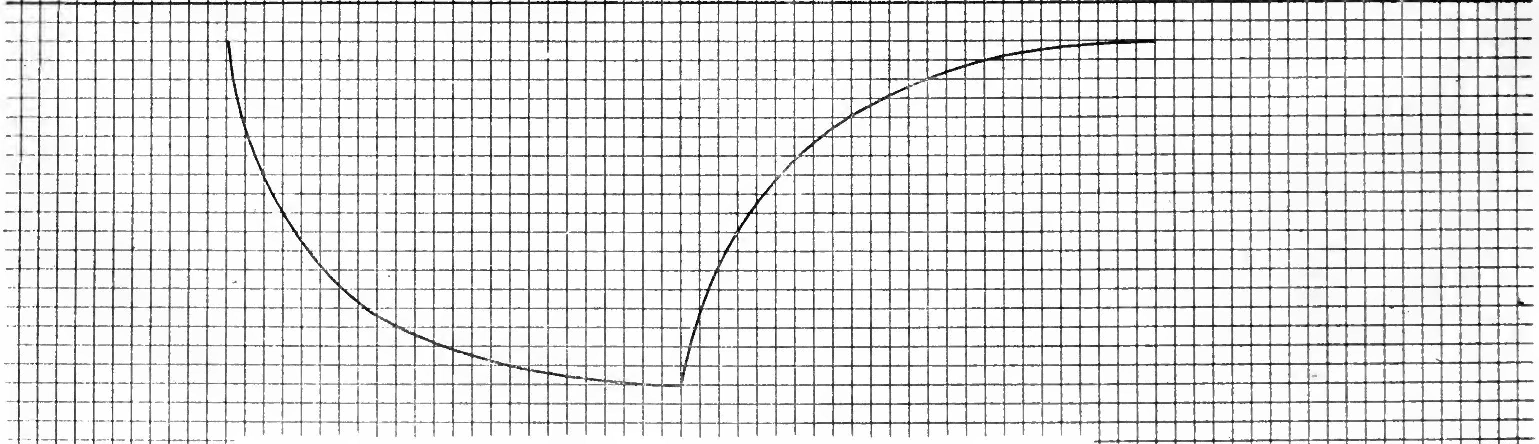

Fig 55. Graphical representation of Disturbance in a Chemical System.

will be greatest at first, and will gradually decrease as these products are changed into the right-hand side products. Therefore the velocity of the total change will be greatest at first, and will gradually decrease until a new position of equilibrium is established. If now, after this new position of equilibrium is taken up, the reaction products on the right-hand side of the equation are suddenly increased, we get the same reaction over again, *but in an opposite direction.* But the graphs representing these changes in the velocity of the total reaction are not mirror curves. They are of the same shape, but in different positions. This is evident at once from the fact that at the beginning of each disturbance of equilibrium is the velocity of the reaction greatest. A graphical representation of such a condition as we have described is given in Fig. 55. The slope of each curve must be steepest immediately after the disturbance of equilibrium.

In mental activity we have assimilation and dissimilation going on in the cortex of the brain, and these two processes correspond to the two sides of the reversible chemical reaction. If assimilation goes on at a faster rate than dissimilation we get an increase in efficiency; but, on the other hand, if the velocity of dissimilation is greater than the velocity of assimilation, we get a loss of efficiency—the fatigue effect. In a state of rest we get equilibrium between the two processes—*i.e.*, the rate of assimilation is equal to rate of dissimilation. But if we disturb this condition of equilibrium by commencing to do a severe mental task, we immediately have a change in the rates at which these two processes proceed. Dissimilation now goes on at a greater rate than assimilation, the efficiency decreases, and the work curve falls, at first steeply and then more gradually until, after about 12 minutes, a new position of equilibrium is assumed. The work curve is now horizontal. If, at this point, we suddenly cease work we get a change in the opposite direction. Assimilation has now a greater velocity than dissimilation, and the efficiency increases, rapidly at first, then more gradually, until the old position of equilibrium is regained. There is a strict correspondence between the chemical reaction and what we may call the physiological reaction. And one would not expect otherwise than that their curves should be of similar shape. Thus chemistry supplies us with strong evidence in favour of the view that the work curve and the curve of recovery from fatigue are the same shape, and that they cannot possibly be " mirror " curves.

If the work curve is the same shape as the curve of recovery, revolved round the horizontal axis 180 deg., it helps us to explain the relative minimum point on the work-curve at the ordinate marked 2 minutes, when the work-period is 1 minute. The occurrence of this minimum point here has the appearance of being due to an emotional or conative effect. The subject, although meaning to work at maximum pressure the whole time, makes a great spurt for the first minute, but then lets his effort gradually decline. Immediately this slackening-off is noticed another spurt is made, and we get the relative minimum point on the work curve. If this is the explanation of this minimum point, it is extremely strange that it should happen after the same interval of time with all the subjects. Why should it appear at 2 minutes rather than 3 minutes or 4 minutes? On the curve of recovery from fatigue we get a corresponding point, viz., between the ordinates representing 15 seconds and 30 seconds rest, and this cannot be due to spurts, as the work was not continuous, but was interrupted by the interpolated rests. If, as we have tried to prove, the curve of recovery from fatigue must necessarily be of the same shape as the work curve, similar points on the curves must be due to similar conditions. If the point on the curve of recovery corresponding to this minimum point on the work curve is not due to an emotional or conative effect, then the minimum point itself is not due to an emotional or conative effect. So far our consideration of this point has not given any positive result. It has put out of court one possible explanation of this minimum point, viz., that it is an emotional or conative effect, but has given us no positive information regarding its cause.

In attempting to give a theory of fatigue, we shall show on page 100, that the rate of fall of the work curve depends upon the excess of the rate of dissimilation over the rate of assimilation; and that, within limits, the rate of dissimilation depends upon the rate of assimilation, for these two processes correspond to the two sides of a reversible chemical reaction. If, then, these two processes are in equilibrium, and a mental task is done, immediately dissimilation becomes greater than assimilation, and the work curve drops. Shortly afterwards the work curve rises, showing that now assimilation is greater than dissimilation. The cause of this seems to be that the assimilative process has a considerable latent period. It is slow in working up to its maximum rate. If it could take up its maximum rate immediately the dissimilative process commenced, the work curve would decline the whole way, for then dissimilation would always be greater than assimilation until the horizontal phase was reached, when both would be equal. But it seems that the assimilative process reaches its maximum some time after the dissimilative process has commenced, and here temporarily the former has a greater velocity than the latter, and gives rise to the relative minimum point on the work curve at the ordinate representing 2 minutes' work.

Analysis of the Curve of Recovery from Fatigue.

In attempting an analysis of the curve of recovery from fatigue, we shall call, as before, all those influences which tend to increase efficiency, the incitement effect, and all those influences which tend to decrease efficiency, the fatigue effect. In making this analysis, we shall be guided by the following considerations:—

(1) That the combination of the component curves of incitement and fatigue must give the curve being analysed.

(2) A simple curve is to be preferred to one more complex, provided it fits the facts.

(3) There must be neither relative maximum or minimum points on either component—*i.e.*, the fatigue curve must show a continuous fall, and the incitement curve a continuous rise; for on our definition of fatigue and incitement effects given above, the former cannot cause an increase in efficiency, while the latter cannot cause a decrease in efficiency.

In Fig. 56, A is the curve of recovery from fatigue, in which the fatigue effect is viewed positively, B is the component curve of the loss of the incitement effect, and C the component curve of the loss of the fatigue effect. Curve A is the reproduction to a different scale of Fig. 8. Curves B and C are the simplest curves, which, when combined, will give the required curve of recovery. The shape of the resultant curve A, taken in conjunction with the three guides given above, provides the

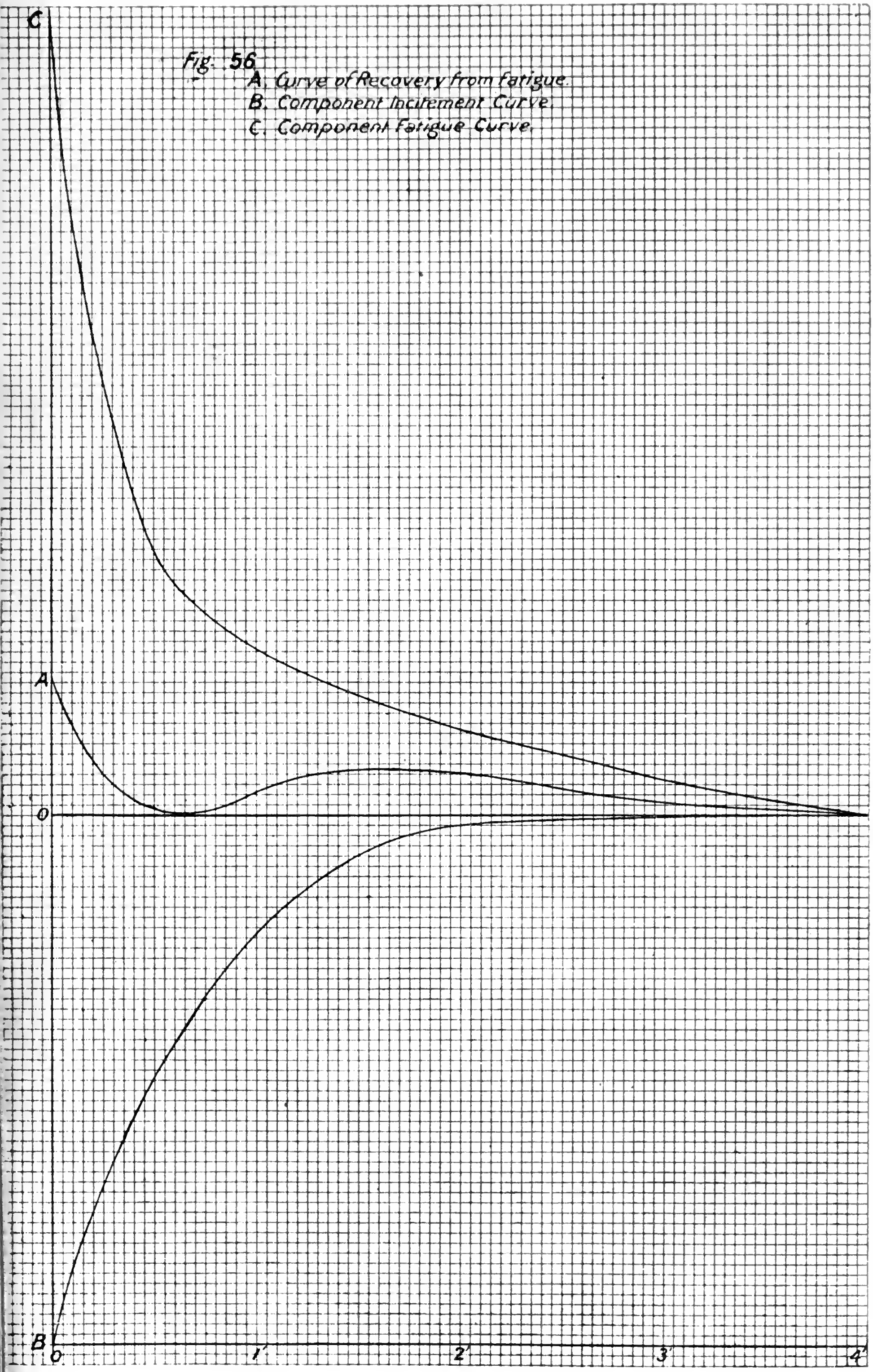
Fig. 56.
A. Curve of Recovery from fatigue.
B. Component Incitement Curve.
C. Component Fatigue Curve.
C
A
0
B
0
1'
2'
3'
4'

foundation on which to base our analysis. The fact that, at first, the curve A falls shows that here the loss of fatigue is greater than the loss of incitement; or, in other words, the loss of fatigue through a short rest causes a rise in efficiency which is greater than the decrease in efficiency caused by the loss of incitement due to the same rest. The whole resultant curve is the effect of these two opposite influences, the loss of fatigue causing an increase in efficiency, and the loss of incitement causing a decrease in efficiency. The *shape* of the resultant curve is the result of the difference in the *rates* at which these two losses proceed. After slightly more than 30 seconds this state of things is reversed. Now the loss of incitement proceeds at a greater rate than the loss of fatigue, and therefore the resultant curve A rises, and reaches its maximum with a rest of approximately $1\frac{1}{2}$ minutes. Thenceforward we find that the curve of recovery very gradually descends, until with rests of 4 minutes and longer it becomes horizontal. In this third stage, then, the loss of fatigue again proceeds at a greater rate than the loss of incitement, and continues to do so until both have entirely disappeared. At this stage both component curves either meet on the resultant curve or else run parallel with it at equal distances from it. These two component curves of fatigue and incitement fulfil all the conditions specified above. When combined, they give the resultant curve of recovery from fatigue. They are both curves of the first order, and there are no turning points on either.

A similar argument applies to the analysis of the work curve—the graph of the onset of fatigue caused by continuous work. The component curves given in Fig. 56 will, therefore, serve for the analysis of either the curve of the loss of efficiency through continuous work, or the loss of fatigue and incitement due to rest.

Summary of Part III.

1. The work curve using work-periods of 1 or 2 minutes has a constant shape, which is independent alike of the task and the subject who does the task. The constant shape only becomes apparent with a large number of independent measurements owing to the influence of random errors, Using work-periods of 5 minutes or more has the effect of obliterating important points which give the curve this characteristic shape.

The work curve falls abruptly at first, then more gradually, and finally becomes horizontal, and remains so up to the twentieth minute of work at least. There is also evidence to show that the work curve, using 1-minute peroids, has a minimum point at the second minute of work. This is apparent with both multiplication and cancellation. With the three tasks used, the horizontal phase begins after approximately 12 minutes of work at maximum effort. The data of this research point to the fact that the depression of the horizontal stage of the curve below the initial point is dependent upon the nature of the task.

Lehmann has shown that, for muscular work, the rate of working determines the depression of this horizontal phase.

2. The curve of recovery from fatigue is similar in shape to the work curve, using 1-minute periods. That these two curves should be similar in shape receives strong support from corresponding curves of the velocity of a chemical reaction. Either curve can be resolved into its component curves of incitement and fatigue.

Part IV.—Theoretical.

A Theory of Fatigue.

In endeavouring to give a theory of fatigue and incitement, we must have in mind three or four main principles. Our theory, if possible, must depend upon established facts or accepted theories or be deduced from them by analogy. Secondly, it must explain all the facts. Thirdly, it must not conflict with facts or with existing known laws. It must also be as simple as is consistent with the explanation of the facts. Any theory is better than none at all, and of two theories which explain the experimental results, the simpler is to be preferred to the more complex.

Chemical.

In the chemical laws of mass action we find a basis for a psychological theory of fatigue. Let us take, for example, a reversible chemical equation such as the following[1] :—

$$C_2H_5OH + CH_3CO_2H \rightleftarrows CH_3CO_2C_2H_5 + H_2O.$$

Alcohol, Acetic acid, Ethyl acetate, Water.

That is, 1 mol. alcohol and 1 mol. acetic acid combine to form 1 mol. ethyl acetate and 1 mol. water, or 1 mol. ethyl acetate and 1 mol. water combine to form 1 mol. alcohol and 1 mol. acetic acid. The action proceeds from right to left or from left to right, according as there is an excess of the right or left hand reaction products. After a time a state of equilibrium is reached, such that some of all four reaction products can exist beside each other. This state of equilibrium is not static but dynamic. It does not mean that not further chemical action ensues, but that the change from right to left balances the change from left to right, and continues to do so.

The velocity of the reaction from right to left is given by the equation $V = KC_1 C_2$[2] where K is a temperature constant and C^1 and C_2 are the concentrations of the reacting compounds. Similarly the velocity of the reaction from left to right is given by the equation $V^1 = K^1 C_1^1 C_2^1$, where K^1, C_1^1, and C_2^1 have similar meanings. We cannot observe either V or V^1 alone; any observations upon the velocity of the reaction deal with the difference of the two velocities, *i.e.*, $V \sim V^1$. When the state of equilibrium is reached, V and V^1 are not zero but $V = V^1$, the velocity of the change from right to left equals the velocity of the change from left to right.

Physiological.

All bodily activity involves chemical action. Alimentation provides the raw materials for building up the chemical structures destroyed by activity, while excretion removes the waste products of the chemical action. Assimilation is the building up of chemical compounds as storehouses of energy, while dissimilation is the breaking down of these compounds, thereby setting free the stored energy. Assimilation and dissimilation are at work whenever there is bodily activity, and can be considered as the two sides of a reversible chemical reaction. If $\frac{D}{A} > 1$,

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[1] Nernst. Theoretical Chem. trans. by Tizard, p. 441.

[2] Nernst. Theoretical Chem. trans. by Tizard. p. 443.

i.e., if dissimilation goes on at a faster rate than assimilation we get the state known as fatigue. If $\frac{D}{A} = 1$ we get a state of equilibrium. Dissimilation and assimilation are not then zero, but the rate of dissimilation equals the rate of assimilation, thus causing the state of equilibrium.

In muscle activity the chief dissimilation product is lactic acid. If lactic acid accumulates, the muscle is fatigued and its output of work decreases. Injecting lactic acid into an unfatigued muscle at once causes fatigue, which can be removed by washing out the acid.

Lehman[1] has applied the laws of chemical mass action to the output of work of a muscle or rather group of muscles. He investigated the ergograms obtained with rests of different lengths between each contraction. In Fig. 7, page 75, which is reproduced here as Fig. 53, he gives the curves of work obtained with the different rests.

It will be seen that each curve falls gradually until a point is reached at which the curve ceases to fall further and becomes a horizontal line. The shorter the interval of rest between two successive lifts, the lower will the curve descend before becoming horizontal, and the sooner in point of time will it assume this horizontal position.

Lehman's explanation of these facts is based on the laws of chemical mass action. When the curve of work falls, $\frac{D}{A} > 1$, the rate of dissimilation exceeds that of assimilation, but ultimately such a state is reached as will make both dissimilation and assimilation equal, and the output of work becomes constant. This state of the muscle he calls its adaptation to stimulus. If V is the velocity of the chemcial change involved in dissimilation and V^1 that in assimilation, when $V - V_1 > O$ the curve of work falls. Finally, as in every chemical reaction $V - V^1 = O$, and the output of work becomes constant and remains so. The greater the value of $V - V^1$, the greater the fatigue effect, and therefore the sooner will the period of adaptation arrive. The value of $(V - V^1)$ depends upon the rate at which the muscle works, which determines the rate of the chemical reaction.

Psychological.

The striking similarity of shape between Lehman's curves for the output of work obtained by means of the ergograph, and the curves for mental work obtained by Chapman, and by ourselves in this research, suggests an extention of Lehman's theory so as to embrace mental as well as muscular work.

The analogy between the production of muscular fatigue and mental fatigue must not, uncritically, be pushed to extremes, for so far physiology has not discovered in the brain any fatigue toxin corresponding to lactic acid in the muscles. In the present state of our knowledge we are inclined to look for the ultimate cause of mental fatigue in the failure to produce, by assimilation, a sufficient amount of mental energy to balance its dissipation by dissimilation. This ability for assimilation does not seem to be constant for the individual, but to vary according to the nature of the task, for we see on page 93 that the depression of the horizontal stage of the work curve is not the same for all tasks. Although we find in Table 33 that muscular work has a much smaller effect upon succeeding mental tests than mental work, yet we see in Tables 42–48 that the depreciations in the tests after muscular work

[1] Lehman. Grundzuge der Psychophysiologie. Leipzig. 1912. Erstes Buch.

correlate highly with the depreciations in the same tests after mental work. Evidently the whole of the fatigue induced by the muscular work is not confined to the muscles themselves, but some of it, at any rate, is cortical in origin, and so affects succeeding mental work.

Nor, on the other hand, does a chemical reaction adequately represent the production of fatigue. In the former, the reaction takes place when and wherever the reaction products of either side of the equation representing the reaction are in excess, and the rate of the reaction depends upon this excess. This cannot be true of our mental life, or else we should always be in a chronic state of fatigue, for there must be some provision for laying by a store of energy in case of emergency. Dissimilation depends in the first place upon the will. We can consciously control its rate within certain limits. But, when working with maximum effort, which is the effect of a continuous act of will, and when the initial store of energy is dissipated, the rate of dissimilation will depend upon the rate of assimilation.

We must premise a state of continuous chemical action in the cortex of the brain, in all conscious states at least, and this will be also true, in all probability, of unconscious states as well, although in a less degree. The rate of this chemical action may be the determining factor of consciousness. That is to say, if this rate falls below a certain point, unconsciousness ensues. The greater the rate of dissimilation the greater will be the amount of energy liberated, and consequently, the greater the output of work. Our ordinary rate of working may then be dependent upon the rate of dissimilation which can be continuously balanced by assimilation without calling upon our reserve of energy.

We shall assume that our hypothetical subject is at the limit of practice, and works at maximum speed the whole time. This will rule out the effects of practice and of spurts, which make the question more complex.

In an unfatigued state, the chemical constituents of the assimilative and dissimilative processes are in equilibrium, that is, $V = V^1$, where V is the rate of dissimilation and V^1 the rate of assimilation. Now let us suppose that the subject commences to do a continuous mental task at maximum effort. More mental energy is needed, and immediately, therefore, V increases, $V - V^1 > O$, and we get a fall in the work curve—the fatigue effect. The increase in the rate of dissimilation causes excess of some of its resultant chemical reaction products, and therefore to increased chemical activity in the opposite direction, *i.e.*, assimilation. There seems to be experimental evidence to show that the assimilative process has a considerable latent period. In the work curve given in Fig, 35 we find that it rises after the second minute of work, instead of falling continuously, as we would expect if the rate of dissimilation were always greater than the rate of assimilation for the first 12 or 14 minutes of continuous work. Evidently in the third minute of work the rate of assimilation is greater than that of dissimilation, consequently dissimilation increases, more energy is liberated, and the curve rises. We have above given reasons for thinking that this is not a conative or emotional effect[1]. The assimilative process does not reach its maximum immediately, but after about 3 minutes, after which its rate cannot be further increased. If then assimilation is now at a maximum, why does the work curve still fall? If the rate of dissimilation depends upon the rate of assimilation, and the latter is now constant, why is the curve not

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[1] Page 95.

horizontal? The reason for this seems to be that the initial store of energy is not completely dissipated. Dissimilation can draw upon the reserve of energy, accumulated in an unfatigued state, as well as upon the energy provided by assimilation. At the same rate as these reserves decrease, when assimilation is at a maximum, V decreases, and the work curve falls to a point at which the whole of the accumulated energy has disappeared. From this point V depends solely on V^1, and as the latter is still constant, the former is also constant and the work curve is horizontal. From the work curves given in Figs. 35, 37, and 50, the point at which the initial reserve of energy is exhausted would seem to be after about 12 minutes of continuous work, whatever the task may be, so long as the subject is working at maximum effort.

The amount of work done in unit time will depend upon V, for the rate of working will be proportional to the rate at which mental energy is liberated, which depends upon V^1 and also upon the initial store of energy. The decrease in V is caused by the fact that V^1 is not sufficient to balance V. If V^1 could be so increased as to always equal V, there would be no fatigue effect manifested, and the work curve would be a straight horizontal line. When $V = V^1$, we have a state of chemical equilibrium which has its parallel in the horizontal phase of the work curve. In Lehman's words, the organ has adapted itself to the stimulus. This state of equilibrium will continue indefinitely, unless further chemical action ensues, which would seem not to be before 20 minutes continuous work, when the process described above will be repeated, and a new position of equilibrium be assumed.

One would suppose that the difficulty of the task would determine the time that elapses before the work curve becomes horizontal, for V would depend upon the difficulty of the task. The experimental evidence seems to incline to the view that this horizontal phase is reached after a definite interval of time with all tasks. Evidently, *when working at maximum effort,* the rate of dissimilation pursues a constant course, and the amount of energy involved in doing any task at maximum speed is always the same.

We have seen that when the work curve becomes horizontal, $V = V^1$ and V^1 is here at its maximum. The rate of working, being dependent upon the rate at which mental energy is liberated, is a function of V, and as $V = V^1$ for this part of the curve, the depression of the horizontal phase will be a function of V^1, the maximum rate of assimilation, which we have assumed to be constant for the individual.

When speaking of fatigue in this account we have included incitement as well. In chemical action we only observe $V \sim V^1$, but never V and V^1 separately. So the physiologist measures $D \sim A$. In psychology, the work curve is the representation of the difference between the fatigue and incitement effects. We cannot observe either separately, but by analysis of the resultant curve we can give the approximate course of each component.

In Fig, 56, the analysis of the curve of recovery from fatigue by rest, which is similar to the analysis of the work curve, the graph of the onset of fatigue through work, the component curves of fatigue and incitement will give some indication of the curves of the rates of dissimilation and assimilation. Curve A will represent the curve of dissimilation, which is the resultant of B, the curve of the rate of assimilation, and C, the curve of the dissimilation of the initial store of energy.

This theory is not affected by the question of general and specific fatigue. If we accept the theory of cerebral localisation of function, then specific fatigue can be explained as the psychological effect of chemical activity in some specific cortical area, while general fatigue will be due to chemical action involving the cortex as a whole. Both forms can be explained in precisely the same way. If this theory truly explains the facts given by experiment it will be possible to construct a theoretical work curve, and to compare it with the experimental work curve.

For the sake of simplicity we shall assume that neither the process of assimilation nor dissimilation has any appreciable latent period. We have seen above that the rate of dissimilation determines the shape of the work-curve, for the rate at which mental energy is liberated determines the output of work at any period. The rate of dissimilation we have also shown to be a function of the rate of assimilation, and also of the initial reserve store of energy. If the assimilation process has no appreciable latent period, it will be constant for the individual, and, therefore, the rate of dissimilation due to assimilation alone, neglecting the initial store of energy, will also be constant, and its graph will be a horizontal line. Therefore the *shape* of the work curve will depend upon the rate at which the reserves of energy are dissipated; for as the rate of dissimilation due to assimilation alone is a constant, it will not affect the *shape* of the work curve but only its *position*. The rate at which the initial store of energy is dissipated will correspond to the rate at which the theoretical work curve falls. If, as above, we call the rate of dissimilation V, then $V = KC_1C_2 - - - - - - - C_n$, where K is constant and $C_1C_2 - - - - C_n$ are the concentrations of the chemical compounds containing the reserve of energy. As $C_1 - - - - - C_n$ will be greatest at the commencement of the dissimilative process, the rate of this process will be greatest at first, and will gradually decline as the energy containing compounds are dissimilated, until finally its rate will be zero, *i.e.*, the curve of dissimilation will be horizontal. Thus the theoretical work curve will continuously decline, and the rate of this decline will be greatest at first and finally will be zero. Its shape will be precisely similar to the experimental work curve given in Fig. 36, the work curve with periods of 2 minutes. The theoretical work curve, if neither the dissimilative nor assimilative process has any latent period, will then be similar in shape to the work curve constructed from the experimental results; and we have shown above that the peculiar "hump" in the work curve with 1 minute periods, (Fig. 35) can be explained by assuming that the assimilative process has a considerable latent period. The question now arises whether the dissimilative process has a latent period or not. If it had a latent period, as we have assumed in the case of the assimilative process, the work curve ought to rise at first, which we have not found to be characteristic of the experimental work curves, for the amount of work done in the first minute is always greater than in any other minute. Either, then, the dissimilative process has no latent period, *i.e.*, it reaches its maximum immediately, or else, as seems more probable, the latent period is so short as to be inappreciable with work periods of 1 minute in length. Work curves with shorter work-periods than ours might possibly show such a latent period, although in the work curves given in Tables 55 and 56, where the work-periods were 30 seconds and 15 seconds, there is no indication that the work curve rises at first. The scores for the first three half-minutes in Table 55 are

7,298, 5,875, and 5,633, the score for the first half-minute being considerably higher than the corresponding score for any other half-minute. Similarly, the scores for the first three 15-second periods in Table 56 are 2,980, 2,678, and 2,586, the score of the initial 15 seconds being much higher than any other. These results seem to indicate that, if the dissimilation process has a latent period, it must be of extremely brief duration.

Sydney: William Applegate Gullick, Government Printer.—1920.