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THE LAWS OF RELATIVE FATIGUE[1]

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The temerity that ventures to speak of fatigue laws may well arouse a critical attitude. But I shall not be quite so indiscreet as the title might be misconstrued to imply. For reasons that will presently appear in detail, I have no expectation that the laws of mental fatigue will be formulated in the immediate future. Oeffner's so-called laws of fatigue are obviously only empirical generalizations and summaries. My subject is really much less pretentious. It concerns only the relativity of fatigue. The laws of relative fatigue that we shall discuss might with equal propriety have been called the laws of fatigue relativity.

My excuse for selecting so threadbare a matter as fatigue for the subject of the presidential address is largely personal. As some of you know, I have been working on various phases of mental fatigue experimentally for a number of years—too long for self complacency. More than once it has seemed to me that I was following a clear experimental path out of the maze of fact, only to find myself back again at the starting point, facing the same fundamental questions. But however personal my interest in fatigue may be it certainly is not exceptional. I venture the guess that there is not a member of this Association but has made fatigue the subject of direct, indirect, or projected investigation. Certainly few psychological subjects have so widely interested investigators in the allied sciences. Few seem to have at once such far-reaching

[1] Address of the President before the American Psychological Association, New York Meeting, December, 1916.

bearings on psychological theory and the conduct of human affairs. Few present such a bewildering literature, with such an array of apparently mutually contradictory experimental results. None is more confused with an equal pressure for practical working rules. Confusion and eagerness for practical results make a situation fraught with grave peril to science. If anything could, they justify this attempt to clarify and systematize the fundamental concept of mental fatigue.

It would be an impracticable as well as an uncongenial task for me to attempt a review of the literature of fatigue, even if this were a fitting occasion. Our time limits, our precedents, and my personal interests persuade me rather to attempt what I hope may prove to be a more generally useful undertaking, namely, a substantive analysis of the problem. The first part of that task, as I apprehend it, is to clear the problem of some misleading assumptions by which faulty analogy and practical interests have confused the real issues. Thus simplified we shall try to redefine the psycho-physical problem on a scientific rather than on a practical basis.

Mental fatigue is one of those scientific problems that has suffered from too much practical importance. In the enormous number of investigations that have appeared since the publication of Mosso's "epoch-making" book, just a quarter of a century ago, educational, medical, and more recently social and economic interests have given the dominant motifs. It was indeed an alarming arraignment of the schools that they ruined the health and impaired the eyes of pupils by their excessive demands. An investigation of such charges was a direct obligation on experimental pedagogy. Scarcely less important than the school problems is a just determination of the proper duration of an industrial day, with a fair consideration for the welfare of the laborer and for the exigencies of competition. None of us, moreover, is entirely free from more personal practical difficulties in our desire to exploit most effectively the time and energy at our disposal. Now I would not for a moment be misunderstood to depreciate the importance of these practical studies. My only contention

with respect to them is that they all suffer more or less from an inadequate scientific basis. But in spite of all the confusion of alleged fact, all the premature and unverifiable pronouncements, most of us still believe that an adequate answer to such practical questions is both desirable and possible.

Less insistent and obvious, but none the less real and important, are the scientific problems of mental fatigue. For the present at least it may be of some advantage to keep separate the two lines of investigation, the practical and the scientific. The former is, in the main, quite independent of the latter. However the questions as to the nature and laws of mental fatigue may finally be answered, careful generalization from experience as to the expediency of certain work and relaxation sequences will deserve and will receive careful consideration in planning the day's work. So too the most advisable length and distribution of recesses may be settled purely empirically, entirely without reference to any of the underlying bio-chemical processes. For all such practical purposes the concept of fatigue is an accident. Its function is not to recall the implications that it has in bio-chemical science; but merely to serve as a vehicle for practical maxims, a class name for all sorts of unanalyzed hindrances to effective work. The hindrances would be just as real and the practical maxims just as valuable even if it were proven that fatigue had nothing to do with them. The scientific problems as to the real nature and conditions of a supposititious mental fatigue are quite independent of all such questions of practical expediency. Scientifically we must know the differential characteristics by which mental fatigue can be distinguished from the other limitations of the work curve; as well as its elementary forms and their interrelations. We must follow its implications as an indicator of the relationship between mind and body; and correlate it with other bio-chemical facts. It is these latter problems that appeal to me personally with especial emphasis. Mental fatigue if it exists in the physiological sense, must be connected in some direct way with the energy transformations in nervous tissue,

and the fundamental problems of inner psycho-physics. The great problem whether our mental life conforms like the rest of the organism to the underlying postulate of thermodynamics, the conservation of energy, must be answered if at all by the psycho-physics of work and fatigue. While I sincerely hope that adequately equipped attempts to explore these fundamental questions are not too far distant, there are related problems that can be examined by simpler techniques. Again mental fatigue if it exists ought to furnish us with an instrument of dynamic analysis of the mental complexes, reaching the inner mechanisms of our mental life. To estimate its possible usefulness, one has only to think of the analogous use of fatigue or adaptation in sense analysis; when we adapt out one sense quality and note the effect of its loss on the other qualities, or on perception in general. So, for example, the relative composition of two purples might be shown even if we had no other method, by adapting out spectral blue, and comparing their resulting appearance. It would seem that a similar process ought to be applicable to mental experiments when we have no other means of experimentally eliminating the various factors. One might even outline the working postulates of such an analysis as follows: I. Whenever in mental processes fatigue of one is regularly accompanied by fatigue of another there must be some dynamic factor common to both. II. Conversely, whenever the fatigue of one mental process does not show as fatigue of another, the two must depend on different dynamic conditions. III. Whenever fatigue in one process is accompanied by the improvement of another process then the two are probably related in the sense that the fatigued factor in the former was inhibitory to the second. That such postulates have borne little fruit hitherto, is not due to any inherent logical unsoundness, but rather to our misapprehension of the character of mental fatigue. At present their application to the problems of analysis would be handicapped by the very richness of the alleged correlations. It has proven embarrassing to more than one of us to teach our students on one occasion the very slight correlation between mental processes

that seem very much alike; and on another occasion to teach them how mental fatigue in general may be measured by the pulse, the ergograph, addition, reaction time, the dermal threshold, and other apparently disconnected events through a long list of accredited extrinsic tests. To be sure the reliability of these and other so-called tests is not universally admitted. But the gross discrepancies between genetic and dynamic correlations might well be taken a little more seriously. Before any of these scientific tasks can be undertaken with promise of success, we must know what mental fatigue really is, if there is any such thing, and how it is conditioned.

The Concept of Mental Fatigue

The concept of mental fatigue is so familiar that a precise analysis of its differentia has seldom seemed necessary. Statements of its meaning, when they occur, regularly emphasize a diminution of some product of mental activity per unit of time, incident to continued activity, and as Thorndike insists, recoverable through rest. Actual recovery or the capacity to recover through rest seems to me to be irrelevant. On the one hand it excludes extreme fatigue; on the other hand it fails to exclude all sorts of intercurrent disturbances. But the diminution of production consequent to continuous work with or without recoverability, is I believe an untenable criterion of fatigue.

If the word fatigue has any scientific propriety in connection with our mental life, it seems to me that it should refer to the metabolic conditions of mental action, not to any predetermined characteristic of its consequences. This is very much the same point that I made recently concerning mental work. While that was not received with the unanimity that I had hoped, in the case of fatigue at least, failure to realize the dynamic implications must lead to gross confusion. Obviously psychology or pedagogy is entirely competent to ignore the physiological concept of fatigue and to develop its own empirical concept as decreased returns of mental work. But if it ignores the metabolic implications at the beginning, it may not assume at the end that physio-

logical and pathological fatigue processes parallel the decreased returns. It is such gratuitous assumptions concerning matters of fact that make psycho-physical parallelism a dangerous working hypothesis. Moreover, such an independent psychological concept would be scientifically defensible only to the degree that work decrements, consequent to work and eliminated by rest, prove to be homologous processes with regular and definable antecedents. If on the contrary work decrements show a large variety of types, or follow any considerable variety of conditions, it would seem to be good sense and sound science to enquire whether any of the varieties of mental work decrement correspond to physiological fatigue processes. These alone would then seem to have a natural right to the name mental fatigue. From this standpoint other decrements would be regarded as pseudo-fatigues.

In order to conserve our time let me be quite direct and frank. I regard it as improbable that any of the mental work decrements so commonly treated as mental fatigue, are ever simply conditioned by true fatigue processes in nervous tissue. Conversely real fatigue may not appear as a decrement at all. Some of the evidence for this position can only be indicated here. Some of it must be given in more detail.

First one must note the physiological fact that nervous tissue *in situ* has been found quite resistant to fatigue and exhaustion under normal cirumstances. The axis cylinders apparently never fatigue except under experimental conditions when their environment is freed from oxygen, or when they are narcotized so that they are unable to use the oxygen that is present. Cell bodies are likewise resistant to fatigue under normal circumstances. They can be exhausted in experimental animals only under strychnine poisoning, after the withdrawal of normal blood supply. Langfeld has shown that in humans prolonged fasting produces no correlated decrease of neural efficiency. Reflexes like the knee-jerk and the protective lid reflex show no decrement after long series of elicitation, if care be taken to prevent intercurrent general depression of the nervous system. In those cases where

fatigue decrement of the reflexes does occur, there is evidence that neither the muscles, the nerves, nor the nervous centers have lost their irritability except to the particular stimulus to which they have become adapted. On the contrary hyper-excitability is a common if not a regular phenomenon of extreme so-called mental fatigue. At any rate it would seem that the complete cessation of mental processes, like the inability to recall an opposite, to complete a sentence, to recite a series of nonsense syllables, or to multiply four-place numbers by mental arithmetic cannot possibly mean the fatigue of nervous tissue to the corresponding degree of completeness.

A second ground against the traditional differentiæ of fatigue is their failure to exclude normal psycho-physical rhythms. In more than one respect it was an unfortunate accident that the paradigm for the interpretation of the phenomena of mental fatigue was the fatigue of a nerve-muscle preparation. Undoubtedly there are many and important analogies between the action of lower spinal arcs and cerebral processes. But after all the main task of physiological psychology begins when it seeks to understand the differences between the simpler processes and cerebral action. Similarly, in connection with a supposititious mental fatigue the regularly increasing work-paralysis of nerve-muscle preparation may be and in some respects must be a misleading model. One of the great differences is that while the extirpated preparation changes only slowly under experimental conditions when unstimulated, normal mental life precludes unchanging neural conditions. In the complex interconnections of human cortical processes the one statement that can be made with completest conviction is that the experimental subject never remains constant, quite apart from the intended experimental changes. Even under the best possible experimental conditions, the experimental change is only one of the changes that we know to be occurring. The constitution of these non-experimental changes in any given case we know only in part. We believe that consciousness itself is a process which involves more or less continuous inherent

change. We know that there are also various intercurrent physiological rhythms, cardiac, vascular, respiratory, intestinal, glandular, and muscular. Cortical action may also initiate non-rhythmic changes in the glandular, circulatory, and respiratory systems with far-reaching reactions of those changes on the cortical action that originated them.

From the standpoint of the importance of the accompanying mental changes, perhaps the most significant of the rhythms is sleep. The fatigue-hunting enthusiasm that finds in sleep the daily climax of fatigue is without physiological justification. On the contrary, we have learned from experimental investigations that for some persons evening may be the time of most effective mental work. Moreover, it is neurological commonplace that in serious extreme fatigue, sleep may be impossible. Physiologists would welcome any insight that we could give them into the causes of sleep. The fatigue climax assumption simply is not tenable. Whatever they may be, we know that the conditions of sleep are not simple. Habit, the absence of stimuli, probably widespread inhibitions, and possibly gland products and vaso-motor changes coöperate in its production. Sleep may come from restriction of activity quicker than from over-exertion. Lecturers never go to sleep. The audience may. In view of such complication of the conditions of continuous work decrement the assumption that all diminished returns consequent to work and eliminated by rest are fatigue seems to me utterly untenable.

A third ground of suspicion against the true fatigue character of most so-called mental fatigue is found in the means that are commonly used to induce it. In nerve-muscle fatigue experiments one isolates a specific tissue and stimulates it successively in the same manner. In mental fatigue experiments, on the contrary, repetition of the same stimulus is systematically avoided. The more carefully one analyses the assumptions of this anomalous technique, the more incongruous it appears. Let us take a concrete instance from what Thorndike has taught us to regard as one of the purest forms of mental work, mental arithmetic. If we strictly

followed the analogy of physiological fatigue experiments some association in mental arithmetic, say the multiplication of two times two, should be repeated until work decrement or paralysis indicated fatigue of the association process. As far as I know that is never done. It seems absurd. The experimental device of constantly changing the stimulus in fatigue experiments is defensible only on the assumption that all multiplication processes affect the same general group of tissues, and that continuous multiplication of different digits increases the sum of the fatigue of the whole. But neurologically the assumption is certainly a strange one that the nervous tissue which was involved in one association fatigued more when a variety of different associations were made than it did when all the burden fell on the same associative elements, operating continuously or in rapid succession. Moreover, there are no facts available to show that restriction to a single field like multiplication will produce greater work decrement than rapid change from one field to another. On the contrary there is evidence that the greater the complexity of the mental task the more pronounced is the decrement. Such decrement, however, is more probably due to a confusion between different paths of discharge than to fatigue of any particular path. That confusion is real and a common experience every introspective account is evidence. Theoretically it should be expected from the operation of the known laws of association. Suppose, for example, that after adding various digits to seven we come to the task of adding four. The right associate is by hypothesis well known and thoroughly practiced. But if other numbers have recently appeared in the series they also tend to be reproduced on the basis of recency. It is at least conceivable that the true associate in such a case might be difficult to recall, not at all from fatigue of the corresponding tissue but from effectual inhibition because a more recent associate appears in its stead. The necessity for inhibiting irrelevant and false associates is certainly a common experience in the elementary mathematics of some of us. But the tendency of recent ideas to recur is not in any sense a fatigue or exhaustion

process, but is probably a matter of residual excitation and summation. Such work decrement then is not fatigue but mere association rivalry.

A fourth ground against identifying work decrement and fatigue may be found in the operation of incidental inhibitions. Theoretically, every mental operation arouses more or less widespread associated reverberations which manifest themselves in the sequences of actual associative recall, and may on occasion, as we have just seen, operate to confuse the regular sequence of work by a kind of associative rivalry. Theoretically also, every actual association process involves more or less widespread inhibitions of undesirable associations. Now it is conceivable that these useful inhibitions of the irrelevant might operate to produce a pseudo-fatigue work decrement in any extrinsic test. For example, I have published experimental evidence that the most intense mental work of an examination period commonly follows the first reading of the examination questions. It is the period of adjustment to the examination as a whole, when widespread association systems are being organized. Such activities are not possible in fullest degree without corresponding inhibitions. Ordinarily distracting stimuli pass unnoticed. Even physical discomfort and pain may for a time be ignored. Now it is conceivable that if at such a time the fatigue tester should request the examinee to add digits for two minutes as rapidly as possible, the response might show a degree of work decrement that bordered on total incapacity. Or again, suppose we would measure the fatigue of a Wall St. broker, hour by hour, with the æsthesiometer test. And supposing as the hour struck we should interrupt a selling campaign that was taxing his skill as a broker by the request that he submit to our compass point test. The chances are in favor of some rather vigorous verbal defensive reactions with no discrimination at all. But if we were able to hold him to a promise and actually start the test, is there any guarantee that gross decrements in the measured function, all due to previous work and remediable by rest, might not be due to his inability to give his attention to our petty tests while his fortune was at

stake on the floor? Of course the whole situation is absurd. The most enthusiastic believer in space threshold tests would hesitate to use such results as an indication of the broker's general mental fatigue at that time.

We freely admit that these are extreme cases, and that they break the most elementary rules for experimentation. But have we any guarantee that similar discriminations against some seemingly unimportant task might not occur just after recess, or just before school lets out, when the afternoon's escapades are in the making, or any time at the interruption of seemingly important processes? Conversely is there any guarantee that the interruption of annoying or even fatiguing work by a few moments of trivial testing might not be a joyous relief, giving results that might entirely hide a supposititious real mental fatigue of the interrupted work? I am not arguing that such inhibitions would not be very much worth knowing; but merely that it confuses their real bearings to call them all fatigue.

In addition to these specific inhibitory processes which are commonly classed in psychology as phenomena of attention, we are acquainted with secondary inhibitions through a diminution of the supporting organic processes, glandular or circulatory. Of the glandular changes I have no direct knowledge. The initial increased pulse frequency, whenever complete relaxation is interrupted by any mental activity, is commonly followed by a gradually decreasing heart rate in any prolonged experimental task. We may regard this as a kind of adaptive process, an habituation to the task at hand. It is difficult to conceive of it without reference to the gradual elimination of extrinsic excitations, in which an initial general excitation is followed by an inhibition which restricts the excitement to selected processes. I have been able to demonstrate that something of this sort occurs in every normal reading pause. That continuous fixation of a trivial object is inhibitory is shown by its familiar hypnagogic tendencies. It is one of the methods of producing hypnosis. With some probability we can predict a diminution in the organic conditions for metabolism in all relatively unused neural centers

during monotonous mental work. In extreme cases continuous disuse leads to atrophy, muscular, neural, and glandular. To regard work decrement which is due to more or less complete atrophy of unused paths as fatigue would be a manifestly absurd confusion of concepts. But work decrement from secondary trophic deficiency, as in unused parts, is just as surely not fatigue. Just as in periods of excitement and important readjustment, there are undoubtedly vascular and glandular changes which increase the activity of the whole neural mechanism, reflexly reinforcing the processes that initiated them; so it is probable that general depressions of glandular or vascular origin accompany monotonous mental work, in which even the centers that are most active finally participate. But this again is not fatigue in any physiological sense.

In as far as these various processes represent work decrements or decreased returns that might be mistaken for neural fatigue they may properly be called pseudo-fatigues. We have described pseudo-fatigues of intercurrent rhythms, of residual excitation and rivalry, and of specific and trophic inhibition. The pathological evidence that work decrement is no true indicator of nervous fatigue is not new. Even to summarize it would extend our paper too far. But I think that without it, we have established the thesis that decreased returns resulting from work and recoverable by rest if you will, cannot be employed as simply and directly in the higher neural systematizations as it can in simpler tissues. Arbitrarily to define mental fatigue as work decrement is effectual self-banishment from physiological tradition as well as from clearly defined fields of investigation of the utmost importance.

Having divested the mental fatigue concept of its irrelevant content as vehicle of the various work decrements, it is now in order to inquire whether there is in our mental life a real fatigue phenomenon. I believe that there is, but its manifestations differ from the paradigm of nerve-muscle fatigue in two important particulars. These are: first, the inconstancy of the stimuli in mental work; and second, the

interaction of competing paths. These two differences combine to emphasize the relativity of all mental fatigue.

The Relativity of Fatigue

In the nerve-muscle fatigue experiment, the stimulus is always simple, and usually constant in intensity, given at regular time intervals. For a variety of reasons the stimulus that is most used is the faradic current. It is capable of fine adjustment, may be held at constant intensity over long periods, and is exceedingly effective in quantities that do not damage the tissue. No physiologist would start a fatigue experiment with stimuli of unknown and variable intensity. Unfortunately, that seems to be the only practicable method at present in so-called mental fatigue experiments. Nobody knows the relative stimulus value of two different mathematical sums. But what is vastly more embarrassing, nobody knows how to follow or to evaluate the ever-changing inner factors in the total mental stimulus, such as the force of the instructions, the personal interest of the subject in the scientific aspect of his task, in its bearing on the particular exigencies of his academic career, and so forth. It was one of the great services of Kraepelin in his analysis of the work curve to show how these inner stimuli may change during an experimental period. The meaning of that analysis as I apprehend it is not given in the precise variables or spurts that he found, nor in the assumption that they are always present, but rather in the demonstration that variables in the inner stimuli may occur and must be reckoned with. It would not take us long to add to his objectively defined list many others taken from our experimental experience, such as competition and personal pride, repetition of the instructions, encouragement and persuasion, the presence of the instructor, rewards and penalties of various sorts, and the unanalyzed mass of obligations.

I am not unaware that this matter of the inner stimuli to mental work is packed with problems that we have no adequate techniques to investigate. But that is no excuse for ignoring them. It is our business as scientists to try to

see things as they are, even if they are complex. There is at least some ground for the suspicion that most if not all our real mental fatigue of the work decrement type is really a fatigue of the inner stimuli rather than of the capacity to react. This at least would account for the extraordinary correlations in the fatigue of the most diverse functions. In many so-called mental fatigue experiments the only common factor discernible to introspective analysis is the intent to keep working as fast as possible to the neglect of competing interests.

Now in the physiological experiment fatigue may be shown in two ways, either by a rising threshold or by decreased response to a constant supra-threshold stimulus. Only in the latter case is there an obvious work decrement. The former case implies a constant work output with a gradually increasing stimulus intensity. In mental work we are often distinctly aware of similar changes in the intensity of the inner stimuli that keep us at a disagreeable or monotonous task. Mere interest in the task may lose its force comparatively early. Then the task is continued from stubbornness, the dislike to fail, sense of obligation, honor, fear of ridicule, or hope of reward, etc. All of these may operate in succession. In the end all of them may lose their force and we say, "I do not care what happens, I cannot go on with this thing any longer to-night." There may have been no important work decrement until the break, as Yoakum calls it. But the process is none the less a real fatigue if the continuation of work depends on a change of the stimuli.

All of this emphasis on the importance of the neglected factor of changing stimuli in the fatigue concept is probably sufficient to justify the formal statement of a necessary correction in the traditional definition of mental fatigue. We may call it the first law of relative fatigue, neglected rather than new. Without pretending to give it final formulation we may express it as follows: Within physiological limits, all fatigue decrement in the results of work is relative to the intensity of the stimulus.

Education and society have a very practical interest in

this phase of the fatigue problem. They make use of a large number of incentives in which as Thorndike wisely points out the changes in satisfyingness may be a real cause of work decrement. The adequate adjustment of stimuli to the development of the individual and the needs of the case would seem to be a very real problem in the training of backward and gifted as well as normal children. It seems strange that we have so little experimental knowledge of the relative value of available reinforcements. Autogenic reinforcement is, I believe, at least one factor in the underlying psycho-physics of James's 'reservoirs of power' which may be quite as significant for psychology as the action of adrenin to which Cannon has introduced us. That continuous activity under the reinforcement of emotion or even in the educational use of play may be a source of serious fatigue we have been warned by Kraepelin. Some other reinforcements are conspicuous for their insistence. Such a one is worry. It would seem to be no accident that this is so closely connected with exhaustion psychoses.

I believe that the relative value of the various inner stimuli would repay the closest study. Just now it seems to be interesting the abnormal rather than the normal psychologist. Practical experience is full of rough approximations. Their refinement by experimental techniques would not seem to be an impossible task.

It is possible that we can study relative fatigue not merely by the changes that occur during long series of repetitions but more expeditiously in the relative refractory phase which the genius of Verworn proved to be identical with the fatigue process. Since the relative refractory phase is common to all nervous tissue, I have asked the question whether we can find in mental processes a similar phenomenon. This is undoubtedly the case. In fact every mental process shows something analogous. Repetitions of all sorts seem to be avoided whenever practicable. The repetition of questions, courses, lectures, phrases, and even words is possible enough, but except for special reinforcing circumstances, it is postponed until the effect of the initial case is somewhat worn off.

The routine is regularly less alluring than the unusual. Mankind in general prefers new scenes, new plays, new walks, new jokes, new styles, new investigations. Possibly the decreased effectiveness of over-memorization is a case in point. Possibly even the loss of attention to frequently repeated processes, which is commonly regarded teleologically as a freeing of consciousness for new adjustments, may be caused by the longer refractory phase of the more complex systematizations of attention, so that the rapidly repeated task is dynamically excluded from conscious emphasis.

Works of art on the contrary are characteristically resistant to the refractory phase. Possibly this results in some way from their origin. Certainly one of the marks of good art is the constancy of its appeals. The popular song, the clever phrase, the good joke, soon finds us refractory to the point of desperation, though it is notable that we become refractory to their reception much quicker than to their execution. We like to tell old jokes better than to hear them. But the great classics in music and literature may be heard over and over with increasing satisfaction. It is not impossible that Aristotle's catharsis by dramatic representation of suffering and evil really operates by developing a refractory phase, and a kind of relative fatigue. How far this principle operates in habituation to environment, indifference to shocking conditions of poverty and morals, to suffering, and to the horrors of war, as well as to luxuries "when the novelty has worn off," I am not prepared to estimate.

It would seem that some of these or analogous phenomena ought to yield data for a scientific study of the intensity of the inner stimuli in connection with fatigue if we only knew how to use them. But the very difficulties of technique emphasize how far we are from a real knowledge of relative mental fatigue.

The simplicity of the nerve-muscle paradigm of mental fatigue is further misleading in that it gives no indication of certain important complications which are characteristic of higher systematizations, and which Sherrington called their competition. In a nerve-muscle preparation the impulse has only one possible path. In the higher nervous system on the

contrary any afferent impulse may theoretically activate every efferent path. Just which motor process it finally initiates, is determined by a kind of competition. Competition appears in the spinal reflexes though less conspicuously than in cortically conditioned action, where it is the rule. Unfortunately, however, just where it is most significant it can seldom be followed objectively by our present means of investigation. But there are clear evidences of its operation in associative thinking, in attention, and in perception as well as in conduct.

The relatively fixed tendencies of competition in the cord are probably determined very simply by neural growth and development. In higher systematizations the outcome of competition tends to follow habitual patterns which have originated in the varying life-history of the several competitors. At any given moment in a developed system of this sort, the outcome may be modified by a variety of reinforcing and inhibiting accidents. Among the latter we must count fatigue. In closely balanced competition the absolute degree of fatigue need not be high to make it a deciding factor. Indeed it is conceivable that if the balance of the other factors is close enough, an infinitesimal fatigue, or the slightest trace of a refractory phase may totally change the character of the response, just as intrinsically trivial reinforcements or accidental inhibitions may be the arbiters.

This relation of fatigue to balanced competition gives us a second type of fatigue relativity. Fatigue is relative, not only in the relation of apparent work decrement to stimulus, as expressed in our first law; it is also relative in the sense of a proportionate fatigue of the various factors in a competing group. We may tentatively express this second type of fatigue relativity in the form of a law which for want of a better name we may call the Second Law of Relative Fatigue, because it implies a higher systematization than the first law. In any complex of competing tendencies the relatively greater fatigue of one tendency will tend to eliminate it from the competition in favor of the less fatigued tendencies.

Unfortunately the mechanism of competition cannot be

studied at all in simple preparations and only imperfectly in the reflexes. The most characteristic systems are the least accessible. In the search for accessible human systems of greater complexity than the reflexes, it occurred to me, something over ten years ago, that the motor apparatus of the eyes offered some unique advantages. There we may study twelve intimately related and delicately adjusted final paths which are directly connected with reactions of considerable biological importance. Furthermore, every variation of their interaction is capable of being recorded on a single plane, without complicated mechanical devices, and without the distortions incident to the moving of heavy masses, like the limbs. Since that time the eye-movements have proved to be unusually valuable indicators of neural conditions in some forms of insanity and under the action of alcohol. In experiments that are now in progress they give promise of being the most consistent indicators of general neural conditions. In the early hopes of using them for an analysis of fatigue phenomena, I took a considerable number of binocular records of rapid successions of eye-movements after the model of the ergograph. Though reported on informally from time to time these records have never been published before because of my inability to account for some of their most conspicuous peculiarities. As these difficulties have gradually been experimentally cleared, the records have been seen to illustrate in a remarkable way some of the characteristic phenomena of mental fatigue, and pseudo-fatigue. In particular they admirably schematize the second law of relative fatigue and the "breaks" that it conditions.

Let me assume your familiarity with the technique of photographically recording the eye-movements from the corneal reflection. For the present records the eyes moved horizontally through an arc of sixty degrees, fixating successively two knitting needles which were situated thirty degrees on either side of the primary position of the line of regard. Each dot or dash on the records represents one phase of the alternating current, and a time interval of about eight thousandths of a second.

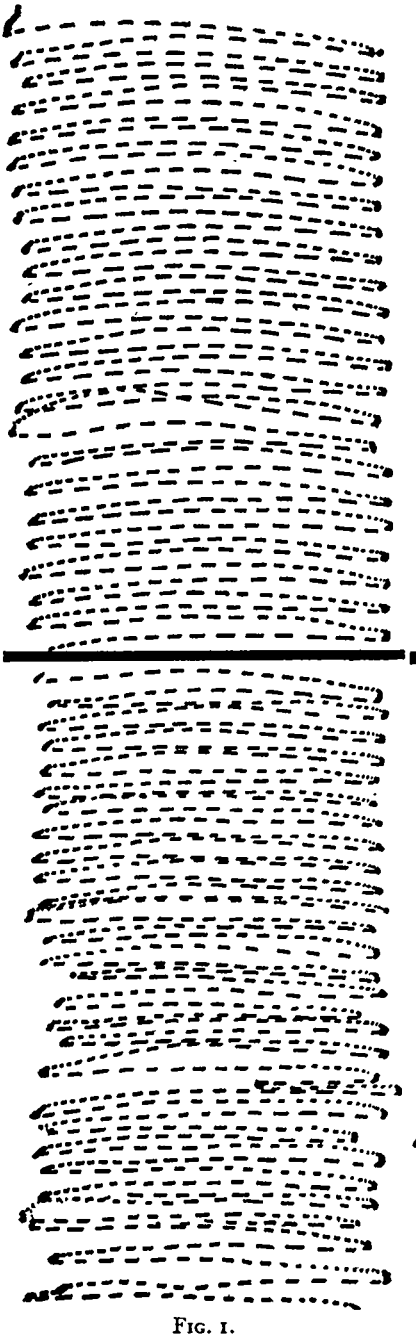

FIG. 1.

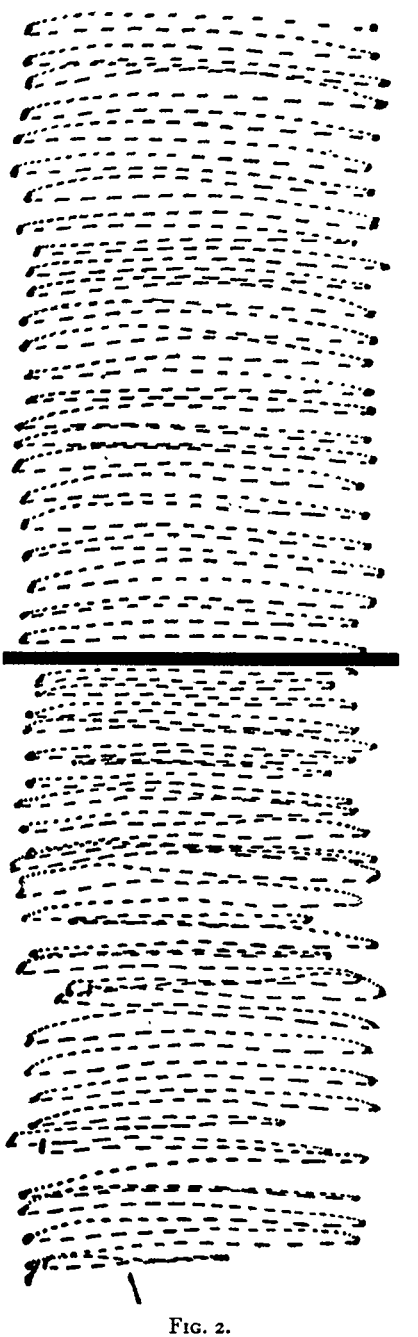

FIG. 2.

The succession of eye-movements in the records that are here reproduced was as rapid as practicable with subjectively adequate successive fixation of the two fixation marks. Some of the more characteristic fatigue phenomena which they show are: (1) The speed of movement becomes less towards the end of the series; (2) the fixations become less accurate; (3) and finally the line of movement itself becomes more irregular. Fig. 2 shows the climax of these processes in a break. The gradual decrease in angle velocity corresponds to the work decrement of extirpated muscle. But in this case, in view of Sherrington's demonstration of the reciprocal inhibition of antagonistic eye-muscles, it doubtless involves something more. The greatest angle velocity of eye-movement could only occur when the relaxation of the antagonistic was perfectly coördinated with the contraction of the agonistic muscle. The pseudo-work-decrement in this case then is not purely muscular but is in part a matter of defective coördination. The increasing errors of coördination have a similar origin. That is, the total elaboration of the contraction impulse and the corresponding relaxation of the antagonistic becomes less exact in successive repetitions of the act of fixation. But the coördination is not limited to the internal and external recti as one might expect them to be in horizontal movements of the eyes. All the records of 60″ eye-movements, which I have ever seen, show a vertical factor. In all my records this vertical factor results in an elevation of the line of regard. But it varies from movement to movement. That these vertical components are not accidents of purely muscular origin is shown by binocular records. Since the disturbances are homologous for both eyes, their origin must lie in the central nervous system. While occasional gross disturbances occur early in the series of movements, they become more and more conspicuous as the series progresses. The vertical components represent the intercurrent action of related and competing, but this is a case of non-inhibiting systems. When they become extreme they tend to interrupt for a moment the main rhythm of horizontal movements. In some cases these various disturb-

ances produce a moment of confusion and a break in the process, which in ordinary mental fatigue experiments would be interpreted as complete fatigue or exhaustion.

Our eye-movement schema for the relative fatigue of competing systems is particularly free from complications through extrinsic facilitations and inhibitions. Retinal fatigue or adaptation is reduced to a minimum by the eye-movement itself, and the consequent shift of the area of stimulation. The homologous fixation marks, under constant illumination present the same stimulus for each reaction in the same direction. Cortical conditions of the successive reactions, such as interest, attention and motives to continue at work, cannot of course be guaranteed to remain constant. But the experiment itself introduces no obvious distractions like the physical discomfort of the ergograph. Moreover, all our relative fatigue phenomena appear during short experimental periods.

In order to protect our conclusions from the dangers inherent in a single line of experimental evidence, I sought other similarly complicated coördination systems. While thus far no other has been found with all the advantages of the horizontal eye-movements, those movements of the index finger which Bergström recommended for ergographic work show a similar complication. Undoubtedly the strongest and best practiced oscillatory movements of the index finger are the flexion-extension movements. Considerably less easy for most of us are movements of the finger sideways in the plane of the hand. In any event, rapid oscillation of the finger in this direction is always disturbed by intercurrent action of the flexors and extensors. Their action prevents rectilinear movement, decreases the angle velocity, and finally may so confuse the process as to produce a break in the sequence of oscillations, quite like the disturbances of the eye-movements.

It was the phenomena of these relatively accessible complex systematizations that forced me to a re-analysis of the mental fatigue concept. I believe that our eye-movement paradigm gives us the clue not only for a more intelligent experimental investigation of mental fatigue, but also for the

interpretation of previous investigations. The very irregularity of the traditional results may be an expression of the laws of relativity. But I hope that the time has passed when an experimenter will be content to give us only the work decrement as datum for the measure of fatigue. Certainly the break can no longer be regarded as a temporary exhaustion of a function. Perhaps the least expected change that the new paradigm will make in our tradition is the place of the interfering sensations of weariness. These may, after all, turn out to be subjective indicators of real fatigue. Their effect in apparent work decrement, however, will be determined by their relative importance in the group of competing tendencies. Under normal conditions at least I doubt if we should call weariness a pseudo-fatigue.

It will be noted that the eye-movement paradigm is still much too simple to apply directly to our mental processes. In place of its anatomically restricted competition to the nuclei of the third, fourth, and sixth cranial nerves, we have reason to believe that cortical competitions are as indefinitely complicated as the various active association tendencies. That a variety of tendencies to associative reproduction are normally aroused as the effect of a mental stimulus is indicated by the facts of the association experiment. This normal spread of excitation, coupled with the effect of psychophysiological rhythms, and the complication of simultaneous stimuli from the different receptor fields, gives the competition in mental operations an almost chaotic complexity. But in addition to all that, we must extend our notion of competition and relative fatigue to those more slowly changing inner determinants of action that we call motives, controls, and the like. Indeed it seems probable that these inner factors, in so far as they are the only continuously acting factors in mental work, are more apt to be the locus of absolute fatigue than the several discrete association tendencies which are involved only occasionally in the mental task.

But aside from the obvious differences in complexity our paradigm adequately represents the fundamental processes. However long a mental process may be continued and how-

ever insignificant the decrement in returns, there comes a moment when it stops. It may be interrupted by demands for food, for sleep, or by some competing task. It may be interrupted by the gradually increasing insistence of inhibiting sensations like thirst, eye-strain, muscle pains, or pressure pains from sitting still. In any case, the work decrement of the consequent break can never be fully understood if we regard it as a direct product of fatigue, but only in connection with the intercurrent competing tendencies. Fatigue may be a contributing factor, but the apparent decrement of the break will bear no regular relation to the degree of absolute fatigue in the tissues which performed the discontinued task.

This enables us to understand why in pathogenic nervous exhaustion, the physician in search of a therapeutic measure may seek to strengthen some competing interest. He may even try to develop some fad, philanthropy, golf, the calculation of food calories, or what not, to compete with the old system and its emotional, business, or religious reinforcements. Most normal lives seem too full of competing tendencies. In my own case I have been interested in observing how every prolonged period of monotonous work like correcting papers, for example, finds before its close some insistent demand for interruption. If I successfully suppress one demand, more insistent ones arise, until finally effective voluntary reinforcement of the main task suddenly ends. The voluntary reinforcements may have developed such sensations of strain that the surrender to a competing impulse brings great relief. I know that the interruption is not permanent. I consent to it to get the lesser matter off my mind, expecting to return presently to the main task, freed from the incubus of that particular competitor. In very much the same way, after lying awake for a time on one side we turn over, not because we could not lie on that side longer, not because we expect any great improvement from the change, certainly not because we expect to lie on the other side forever. The displacement of the body mass is scarcely the product of fatigue. But in the complex of competing tendencies a little relative fatigue becomes the occasion for an

entirely disproportionate result. Possibly social unrest follows a similar course. They seek a change in the government, or the social and labor conditions, not because the present is really unendurable, not because they expect a permanent betterment. In many cases at least, they act from relative fatigue, to shift the pressure. I suppose all the phenomena of restlessness and the corresponding attractiveness of change finally reduce to competition and the relative refractory phase. They operate in work and play, in social and economic activities, in politics and in religion. Without their interference in our lives, unwelcome as it often is, we must have continued indefinitely in the direction of our first activity, with the consequent loss of that vital equilibrium on which the organism as a unit of different parts depends for its continued existence. Without their interference the initial process must always work itself out to the final collapse of complete exhaustion.

Relative fatigue, then, is not a mere limitation of human efficiency. It is not exhaustion, but prevents it. It is a conservator of organic equilibrium, as well as a condition of organic development. The incapacity of the young child for long-continued monotonous tasks may be a symptom of an active, developing mind. Lack of competition would result in mental deformity, or absolute exhaustion, just as truly as the lack of stable reinforcing systems in the adult would mean perpetual infantilism. Thus it seems to me that the principles of relative fatigue have a direct bearing on the practical problems of education which the traditional doctrine of fatigue as apparent work decrement entirely missed. The development of the capacity to sit still, to continue long at routine work, the adequate response to all formal discipline demands more than the strengthening of the corresponding neural bonds. It demands the weakening or elimination of competing tendencies. At least one of the perils of routine education arises from this depression of spontaneity. But I have expressly disclaimed any right or intent to discuss the practical side of the problem.

I cannot quite resist the temptation, however, in closing,

to point a methodological moral. There has seemed to me to be something almost humiliating in the eagerness with which tests of mental fatigue have been sought, while there is still so much that is uncertain in our knowledge of the fundamental nature of the process that we would test. If it is not too great a strain on presidential license at a meeting like this, when the program is so largely devoted to the matter of tests, I would sound a note of warning that in my opinion any tendency to supplant psychological investigation by tests would contain a serious menace to the future of psychology. Both have their proper place. But it can only lead to confusion and work to the discredit of our science if the search for practical tests blinds us to the necessity for studying the dynamics of the processes that we hope to test. We cannot afford to develop a new phrenology.