THE PSYCHOLOGICAL REVIEW

MENTAL WORK. A STUDY IN PSYCHODYNAMICS

BY RAYMOND DODGE

Wesleyan University

I. DYNAMIC PSYCHOLOGY AND PSYCHODYNAMICS

The following paper represents an effort to develop a consistent scientific concept of mental work, and to discover a practicable experimental measure of the dynamic facts.

This is no place to defend the theory or the methods of a dynamic psychology. In a previous paper¹ I pointed out that the natural and inevitable limitations of introspection force any complete science of experience and conduct to investigate the dynamic problems of the mental life by the aid of some non-introspective technique, and to complete its scientific constructs of mental reality by data which introspection cannot reach. I believe that a direct study of the dynamic facts of mental life is both a possibility and a scientific obligation.

As opposed to structural psychology the method of dynamic psychology is physiological; not in the crude sense of a science of mind implied by or studied in the science of physiology—there isn't any; not in the more apparent sense of a community of apparatus and technique—that is an accident; not in the extreme parallelistic sense of a fusion of the two sciences, each dealing with its ontologically peculiar aspect of a dualistic reality—that is problematic.

Dynamic psychology is physiological in so far as it deals with processes and operations, not merely with structural abstractions and their combinations.

¹ 'The Theory and Limitations of Introspection,' *Am. Jr. of Psychology*, XXIII., 214-229.

Like science in general, dynamic psychology is an ideal of organized knowledge which no completed body of knowledge will ever completely realize. It uses the name psychology because it is concerned with that group of facts embraced by the concepts personality, experience, and conduct. As opposed to the classification of these facts according to their immediate introspective similarities and differences, dynamic psychology is primarily concerned with their conditions, whether these are given by introspection or not.

Doubtless this will be carelessly misinterpreted as raising again the old specter of an explanatory psychology. Such a misinterpretation will be doubly unfortunate: first, because it has proven utterly impossible to carry into real science the much-discussed theoretical distinction between description and explanation. An explanatory science is a misleading ideal based on an illusion, on the confusion between familiarity and finished knowledge. Actual science knows no other extension of knowledge except correlations. Secondly, it would be unfortunate because it would assume that description can be complete without describing the conditions of the phenomenon described. But of all scientific correlations, those dealing with the conditions of a phenomenon are the most fruitful, since every phenomenon is given with the sum-total of its conditions. Dynamic psychology is then fundamentally a descriptive science, like all other real sciences.

In this sense of the term, dynamic psychology is not new. Discussions of the conditions of the processes of experience and conduct bulk large in modern psychology. Wherever the influence of variations in the stimuli, sense organ, or past experience has been studied in sense perception, there is dynamic psychology. Current studies of memory, association, personality, and conduct are largely concerned with their conditions. Psychological theory in the sense of Titchener and Külpe seems to be essentially dynamic. To the American perhaps more than to any other nation there is an immediate and peculiar appeal in the question: "What makes the thing go?" And it is significant that so large

a part of the distinctly American contributions to psychology have been made with explicit or implicit interest in dynamic problems.

Real as the interest is, and substantial as is our knowledge of the dynamic facts of mental life, dynamic psychology has long been cast in the rôle of the ill-favored sister. Partly from mistaken metaphysical piety, and partly from scientific inertia, it has been chiefly confined to conventionally circumscribed fields. Self-conscious systematic cultivation outside these fields has been relatively rare.

This is the only excuse for emphasizing the dynamic aspects of psychology. Such emphasis is an accident. It means only that a certain group of correlations demand attention and development. In the end there is only one psychology. More properly speaking, in the end there is only one science, a body of completely correlated facts. Until it is achieved, every self-conscious effort must prescribe a proximate partial goal. The particular group of correlations which we are seeking are those that give us the right to speak of mental work and mental fatigue, if indeed we have any such right at all.

Within the wider field of a dynamic psychology, which investigates the totality of the conditions of mental phenomena, it seems legitimate to define a narrower concept of an investigation of the energy transformations which correlate with mental activity. This narrower field may be called psychodynamics. While this term is commonly used for the whole field of dynamic psychology the narrower use has the backing of analogy and of actual use by Lehmann. To be sure Lehmann seems to restrict the term to a particular method of investigating the energy transformations. Psychodynamics is defined¹ by him as 'the exact doctrine of the quantitative influence of simultaneous or successive mental states on one another.' But this restriction again appears to be merely because, as he tells us in the '*Lehrbuch der psychologischen Methodik*,' under the sub-title '*Energiemessungen*,' "we have only one means of obtaining information con-

¹ '*Elemente der Psychodynamik*,' p. 25.

cerning the relations of complex psychophysical processes." I believe that I do no serious violence to Lehmann's terminology when I put these statements together and give the wider implications of the concept explicit statement. Psychodynamics is the doctrine of energy transformations conditioning mental activity. If it seems to have a more restricted use in the 'Elemente' it is only because Lehmann naturally equates 'the only available method' and the discipline.¹

Whether his method is a valid method of psychodynamics or not, this is no place to discuss. Perhaps I may merely register my personal conviction, however, that his psychodynamics has been grossly neglected. I believe that his inhibition and facilitation experiments give us data of the highest psychophysical importance, not as direct measurements of energy transformations but surely of the psychophysical relationships between various mental processes. I hope that time will do fuller justice to his theory and to his experimental facts. The present enquiry rests on entirely different presumptions and interests.

II. THE CONCEPT OF MENTAL WORK

Notwithstanding the enormous practical importance of determining the conditions that modify mental efficiency, and notwithstanding the obvious scientific advantages in establishing units of mental activity, the term mental work is indexed in only two of the recent psychological manuals. Fatigue is more generally discussed. But it seems to me that most of the experimental work of psychologists on fatigue has been distinctly non-psychological both in problem and in implication. It has suffered from lack of any clear

¹ It might seem that I have arbitrarily overlooked the term *psycho-energetics*, which could with propriety be used to express the specific correlations of mental processes and energy transformations within the nervous system. Unfortunately the term has been preempted. *Psycho-energetics* is a speculative concept involving the coördination of a suppositious psychic energy with the commonly accepted energy forms of heat, light, electricity, magnetism, etc. This theoretical concept has nothing to do with our experimental problem. Personally I would avoid any term that confuses them.

notion of what *mental* fatigue might be like if we ever came upon it. It is significant of the general confusion that mental work and fatigue appear in the classification of the Psychological Index only under Division V. *Motor* Phenomena and Volition.

While my real concern is with mental fatigue and dynamic psychology in general, a necessary preliminary task seemed to me to be the solution of a more fundamental problem. I began to doubt if we could ever straighten out the problems of mental fatigue before we had a consistent concept and a better technique for the measurement of mental work.

There seems to be considerable hesitation to use the term *work* in connection with mental activity except in the most casual and ambiguous way. It savors of mechanics and materialism. Is it not a strange circumstance, however, that while a metaphysical conscience discourages our use of the term *work* in any scientific sense, analogous derivatives from the Latin are used without the shadow of offence? The terms mental elaboration, mental operation, etc., clearly indicate that the concept of mental work is present in psychological constructs whether we legalize it or not. Indeed mental fatigue can mean nothing unless we assume that mental processes involve the second law of thermo-dynamics. With regard to more than one of our fundamental psychological conceptions one is forcibly reminded of the myope's common preference for comfortable haziness.

The German and the French psychologists are less squeamish. *Arbeit* and *Travail Intellectuel* are familiar scientific terms; but concepts are no less confused in German and French than in English. In 1898 Binet and Henri wrote "En psychologie le terme de travail intellectuel comme celui d'intelligence a une signification tre vague." The statement still holds true.

The original sense of the word *work* seems to imply human interest and effort. Work signifies activity towards a conscious end or object, to be accomplished against resistance. In the Greek, I am told, it involves distress.

At the present time probably the only scientific conno-

tation of the term is physical or mechanical. In mechanics work may be said to be done when force moves mass against resistance. Obviously the mechanical concept of work has no immediate application to mental life. It has neither mass nor acceleration. Even if at some future time a mechanical interpretation of mental life might conceivably become a possible working hypothesis, at the present time such an hypothesis would be utterly barren, and misleading.

The familiar psychological connotation of work approximates its original human implications. It is characterized by feelings of effort and strain in the accomplishment of a predetermined end. This connotation appears in the common though relatively imperfect opposition between work and play.

While the mechanical connotation would be meaningless in psychology, the differentia of the opposition-to-play species is, dynamically speaking, trivial. In the complex of mental processes out of which it is analyzable, strain and effort appear as separable accidents. They are important enough in practical life, and demand our attention in psychodynamics. But we have no right to arbitrarily limit the dynamic concept of work by purely introspective criteria. Strain and effort characterize an early stage in the development of the operation in question, or a period of relative inefficiency. There is no immediate evidence that any more energy is used in work than in play; in those cases where work is performed with a maximum of effort than in those cases where the same processes proceed as it were by inner compulsion. Neither of these concepts have proved fruitful for psychological advance. They are blind alleys in the maze of experience. Moreover, in neither of them does that dynamic psychological construction find expression that our tradition calls mental operation and mental elaboration.

It seems to me that there are good theoretical grounds for believing that the term work can never be given scientifically consistent introspective connotation. Mental processes differ introspectively according to their kind, according to their quantity, and according to their complexity, concomitants

and sequences. Obviously mere differences of kind can be no indication of the conditioning energy transfers except by analogy. Similar mental processes may well be supposed to belong to similar systems and to have similar organic conditions. Psychophysical parallelism goes so far as to assume as axiomatic that changes in the psychical process involve corresponding changes in parallel physiological processes. As I have elsewhere tried to point out¹ such propositions are proper subjects for investigation. They are not axiomatic. In fact the end of a chain of neural happenings can be held to indicate the antecedents only in the very simplest sequences. Inhibition, in some cases at least, is an active process and a rapid succession of adequate stimuli may produce a continuous refractory phase.

The common measure of mental work is number and time. In the adding test, for instance, mental work is measured by the number per minute. In reaction tests it is measured by the latent time. Fundamentally both measures are the same. It is a matter of expediency whether one measures a series of events so many per minute, or whether one measures the duration of each individual event. This output conception of mental work is convenient; practically significant, since it may deal with the actual processes of business and school; and it presents plausible physical analogies. It is the yardstick of mental achievement. If one can make n calculations per hour, n calculations is a plausible measure of the mental work of that hour. Decrease in the value of n is commonly supposed to indicate a loss of efficiency in the mechanism. By interpolation different tasks could apparently be correlated. Five sums in arithmetic are equal to ten pages of Dickens or half a page of German prose. We do practically measure off work that way. Hours of lecture are units for instructors; hours of recitation for students. If we would be extra particular we go back to the hours of preparation. For any individual student we balance off subjects in a similar way. The Latin lesson is twice as hard as the French lesson, etc. The conception is useful. Unfortunately, how-

¹ *Psy. Rev.*, Vol. XVIII., p. 167 fol.

ever, it takes absolutely no account of the conditions of work or the energy involved. It is essentially non-dynamic. To be usable it must be constantly assumed that one is doing his best, that the output is maximum or that the effort is sustained and equal. Unfortunately one reaches his maximum output perhaps only once in a lifetime. No one can do his best possible for an hour at a time. It is notorious that the ergographic curve depends not on capacity but on incentive, interest, and impulse. Our entire experimental knowledge of mental fatigue is on the yardstick basis. Time is a good enough measure for commercial and practical purposes, but an hour of lecturing to a class of one hundred and an hour of lecturing to a class of ten are somehow different in practical experience. An hour of experimentation and an hour of reading are different from an hour of bracing up a delinquent student. All are work but there is no use trying to give them scientific value on the basis of time. In factory terms one must ask how much of the plant is going under these different circumstances. The output is no real measure of work, though under some circumstances it might be a good measure of the efficiency of work. In no case does the directly observable output give any adequate measure of the dynamic process.

It seems to me to be a serious common fault in all the familiar fatigue experiments that we have developed no adequate technique for controlling either the inner stimulus, the degree of effort and incentive, or the concurrent dynamic processes. We commonly measure only one more or less arbitrarily selected partial process on which instructions center. But instructions can guarantee sustained and equal effort only so far as there are reliable subjective evidences that effort has lapsed. If the subject is allowed to see the objective results, they introduce a totally new feature into the mental complex. They may constitute stimuli either for increased effort, or for discouragement and indifference.

Products would be an adequate measure of work only when we could assume that the factory processes remain constant. But it is the experimental intent in fatigue that the processes

shall change. What is it that actually does change, in any given case, the changing products do not indicate.

Concurrent feelings of strain, effort, and exhaustion represent the noticeable organic effects of work. In many cases these introspectible organic indicators are very delicate and useful. This is peculiarly true in active touch. But they seem to have no immediate relation either to the efficiency or to the amount of mental activity. It is notorious that introspective estimation of reaction and memory, of the speed and accuracy of the familiar mental tests is unreliable under the most favorable circumstances. With supposedly decreased mental capacity of fatigue, judgment should become even less accurate, and strains will change their meaning.

While the mechanical concept of work as applied to mental life is barren, and the antithesis-to-play concept is dynamically trivial, introspective contents though often valuable are also often inconsistent and mutually contradictory. They refer to totally different, often to irrelevant facts. They appear not as the invariable conditions of mental work but as its occasional by-products.

There remains to be considered the possibility of an organic metabolic content for the concept of mental work.

Perhaps all organic concepts of work will seem as foreign to mental life as the mechanical concept seemed. Physiologically a muscle is said to do work when it tends to contract or when it exerts a pull on its points of attachment. This may be measured directly in h. m. Mental life shows no parallel. But thanks to calorimetric determinations muscle work may also be measured in terms of metabolism and expressed in thermo-dynamic units.

Whether mental activities are capable of any kind of correlation with metabolism and consequent expression in thermodynamic units is a not unimportant question. It is quite as significant for inner psychophysics as the relation between sensation and stimulus is for outer. As far as I am aware, however, it has never been seriously raised by psychologists. But a considerable body of data is already at hand partly furnished by the physiologists, who have for

some time been interested in the effect of mental activities on metabolism, and partly furnished by psychological experiments with quite a different aim.

Theoretically all nervous activity must involve metabolism. The fact of combustion, the use of oxygen, and the katabolism of nervous substance has been incontestably proven for the central nervous system of the frog. Even in the peripheral nerves, excitability and transmission are proven to depend on a normal supply of oxygen. Whatever attitude one may take towards the relation of nervous activities and consciousness, here at least is a promising direction of direct investigation. If fatigue is to mean anything to psychology more than an empirical fact of diminished production, it must correlate with the general physiology of complex nervous centers. But such correlations were absurd except on the assumption of a correlation between the general physiology of complex nervous centers and mental processes. And problems of general physiology are fundamentally problems of metabolism.

These considerations will be as binding on the parallelist as on the interactionist. For those who, like myself, believe that we must attack the dynamic problems of mental life as problems of organic life¹ the attempt to give metabolic content to the concept of mental work is inevitable.

III. THE METABOLIC MEASURE OF MENTAL WORK

Probably the only final and authoritative data with respect to the metabolic equivalents of mental work must eventually be obtained by the same technique that is relied on for the metabolic equivalents of physical work: *i. e.*, either the calorimeter or chemical analysis of the products of organic combustion. The classical work of Benedict and Carpenter on metabolism during examination² is the unique representative of calorimetric measurements of mental work. For me to say that this work deserves more attention from

¹ For a complete statement of my psychophysical working hypothesis see *Psy. Rev.*, XVIII., 173 fol.

² 'The Influence of Muscular and Mental Work on Metabolism,' U. S. Dept. of Agriculture, 1909.

the psychologists than it has received would be mere repetition. But on many counts it seems necessary that the psychologist should carry the experiments further than the problems of the physiologist demand. Unfortunately the respiration calorimeter has certain obvious limitations that practically preclude it from general use in experimental psychology. In the first place it is an expensive and elaborate technique. Secondly, it requires relatively constant conditions for a considerable time interval. And thirdly, the conditions of experimentation are so unusual that the technique itself must be responsible for considerable variation of the normal mental processes. But the calorimeter is the supreme court of appeal in questions of energy changes in living organisms. Perhaps we ought to patiently await its disclosures. But if it is the only available technique psychodynamics will make progress very slowly, and it will never be available in any general way for the indication of dynamic facts.

Fortunately the calorimeter experiments have demonstrated that changes in pulse rate are closely correlated with changes in metabolism.¹ The fact that in the same in-

¹ Von Lützow, *Deutsche Landw. Presse*, Bd. 36, p. 245, 1909; Benedict and Carpenter, 'Metabolism and Energy Transfer of Healthy Man During Rest,' Carnegie Inst., 1910, esp. page 248; Benedict, 'The Influence of Inanition on Metabolism,' Carnegie Institution, 1907; Murlin and Greer, *Am. Jr. of Physiology*, XXVII., 1911, page xvii.

Benedict writes, p. 488: "A comparison of this pulse rate with the total heat production shows a striking uniformity in fluctuations and similar comparisons with other experiments show in nearly every instance a parallelism."

Three years later Benedict and Carpenter write, p. 248: "In the course of experiments it has been observed that with very slight activity the pulse and the metabolism are at a minimum. When the activity is increased, the pulse rate is likewise accelerated, and there is an increase in the total metabolism. It has furthermore seemed clear that the increase in the pulse rate is relatively proportional to the increase in the actual muscular activity observed." Again, p. 249: "Pulse rate increases during the waking hours of the day as compared with the night. We can obtain an approximate idea of the total metabolism from the pulse rate of a subject, although the rate per minute of itself is not necessarily a general index of the katabolism of all individuals."

Still more recently Murlin and Greer write: "Experiments on dogs were devised in which the absorption of oxygen and the output of carbon dioxide were determined by means of a small Benedict respiration apparatus attached directly to the dog's trachea. Simultaneously the blood pressure was recorded. The effects of anæsthesia were controlled."

dividual pulse rate is proportionate to metabolism, with certain restrictions as to blood pressure has been proven as I understand it not only during predominantly physical work but also during long-continued mental activity.

The existence of some intimate connection between pulse and mental states is a commonly accepted fact of great antiquity. Mosso and his followers hoped to find a measure of mental activity in the distribution of the blood to the brain and other parts. Seriously controlled attempts to correlate definite circulatory changes with definite mental processes began with the classical work of Lehmann. Enormously increased data still leave the question open whether any specific mental state can be absolutely correlated with any specific change in pulse or respiration, in the sense that the one can be inferred from the other. Indeed in our knowledge of the nervous conditions of vasomotor innervation there seems to be no good reason for definite correlation with specific cerebral processes. That the circulatory system responds with great delicacy and complexity of adjustment to waves of nervous excitation is an empirical fact. But the mechanism of those adjustments is as little known to us as the nervous conditions of thought itself. As mere expressions of mental states I would deny them any peculiar analytic function in psychology which may not equally well be assumed for a considerable number of involuntary muscles and glands.

The biological function of the circulatory system, however, gives it a unique connection with the nervous as well as with the muscular activities of the body. Since the blood currents supply the conditions of all metabolism, in any adequately

"Similar experiments on several different men in widely different nutritive condition and in varying degrees of muscular activity (lying on a bed, standing, standing and lifting weights, shivering, etc.) were also done by means of the same respiration apparatus and the Erlanger sphygmomanometer. The results show a fairly close correlation in the same individual between the heart output expressed as the product of the pulse pressure and the heart rate on the one hand, and the absorption of oxygen and the elimination of carbon dioxide on the other. The relationship between carbon dioxide elimination and heart action is on the whole a little more constant than that between the oxygen absorption and heart action. Data will also be given on the volume output of the heart determined both directly and indirectly."

organized body within the limits of physiological efficiency, there must be a general correspondence between need and supply. This theoretical presumption is borne out by the experimental evidence. Muscular activity in any part of the body increases the heart rate over the rate during relaxation. The heart rate is always closely proportional to the amount of activity.

Apparently for considerable periods of sustained work the correspondence between metabolism and heart rate is much closer than for short periods.

Grounds for the unreliability of short periods are easily discoverable. The biological correspondence between need and response cannot be a coördinate preadjustment. No automatic vasomotor or cardiac excitation could be based on prophecy of action without the need of constant readjustment. No adjustment could be based on actual need without a certain lag or latent time. So whatever the mechanism, whether one of preparation or one of reaction we would expect rhythmic variations about the line of actual need. This gives rise to a serious limitation of the use of heart rate as indicator of metabolism in mental activity. To assume that the intense disturbances of short duration that occur in emotion exactly correspond to metabolic demands would be unwarranted by any evidences of correlation with which I am familiar. It is not impossible. Since the emotions represent moments of active readjustment there is some ground for expecting that they will make their own peculiar demands on metabolism. But correlations are matters of fact not of probability. A direct study of metabolism would seem to be desideratum in the dynamic psychology of the emotions. Similarly, to assume that every change in heart rate is significant of some definite though uncleared mental state would be unwarranted. Lehmann's early supposition to this effect, he himself abandoned some time ago. The rhythmic changes in heart rate due to respiration give an illustration of the danger of attempting to isolate short intervals experimentally.

Furthermore, the pulse-rate never gives direct and ab-

solute values, only relative and comparative. The pulse of muscular work is commonly known to be both higher and faster than that of muscular relaxation. The amount of acceleration produced by any given quantity of muscular work is a purely individual matter and varies within wide limits in different individuals. It is a significant factor in the organic personal equation of the individual. The pulse of the trained athlete responds to physical work very differently from that of the untrained novice. At different times and under different conditions of health the pulse of the same individual shows changes of excitability. But other conditions being constant, within the same organic equation, two different kinds of work giving rise to the same pulse rate may be regarded as probable physiological equivalents. Similarly if the kind of work remains the same difference in the pulse rates in successive experiments will indicate subjective changes.

Finally complete differentiation between nervous and other metabolic needs would require a technique for which we now have only the barest suggestions. Bearing constantly in mind the various known sources of error, we can only hope that systematic variations of the mental processes under fully controlled conditions will give a real if relatively gross indication of the energy equivalents of mental activity; *i. e.*, of the mental work.

Let us candidly admit that any correlations which we can make without calorimeter technique will be only preliminary approximations to the scientific values we seek. We are confident that further refinement will be possible if it is worth the effort. Meantime it seems likely to be useful in experimental psychology if we have at hand a direct even if a crude objective indicator of the nervous metabolism involved in the mental response to any given experience. For example this would obviously be useful in the so-called mental tests. Successive series of tests will be regarded as similar only when the metabolic indicator shows that subjective conditions are similar.

Serious intercurrent disturbances will be made apparent.

New and sudden adjustments within the series should be detected with unfailing accuracy.

In view of these considerations I believe that it is not too optimistic to say that we have in relative pulse rate a real if a crude psychodynamic measure.

If we could experimentally eliminate all extraneous intercurrent activities then it is theoretically conceivable that we could arrange various mental as well as physical activities along a metabolic scale.

It is conceivable that the energy values of mental work can be expressed not only in comparative physiological but even in thermodynamic units.

More easily realizable will probably be the indication of relative psychophysical efficiency of the same individual under different circumstances.

In short it seems to me that the metabolism coefficients of mental work should become an instrument of analysis into the relatively obscure conditions of mental life that introspection cannot directly reach.

Relative pulse rate under proper controls should not only indicate specific problems for calorimetric experimentation. It should give us some preliminary indication of their solution.

IV. THE EXPERIMENTAL CORRELATION OF PULSE RATE AND MENTAL PROCESSES

Experimental studies of the effect of mental processes on the pulse rate are numerous. Most of the classic studies have been chiefly concerned with pulse and respiration as bodily expression of the feelings and emotions. For reasons already indicated these relatively intense disturbances of short duration do not fall within the proper scope of our inquiry. We are forced to abstract from them because our theory doesn't cover them, not directly at any rate. It may be that ultimately the overflow of nervous excitation to cardiac, vasomotor, and respiratory centers will be found to follow more general biological laws. But that is another story.

Experimental data concerning the effect of intellectual

processes on pulse rate are relatively scant and widely scattered. Most of it occurs in connection with the study of attention. These data it must be confessed are far from being unanimous. Some of them appear to be the direct opposite of what our theory would demand.

For instance Mentz¹ found that every appreciable stimulus lengthens pulse and respiration. So Zoneff and Meumann² report that in most cases attention gives slower pulse; that the best concentration is contemporaneous with the slowest pulse; and (p. 51) that whenever the concentration of attention relaxes the pulse becomes more rapid. Now attention, and especially voluntary attention, is commonly held to be mental work of the most unequivocal sort. If attention really does lower the heart rate the facts are against the theory. One or the other should capitulate. If one were partial to the theory he might subject the facts to the merciless critique so common in psychological tradition and annihilate them. Experimental facts are usually surprisingly vulnerable. But perhaps one is unfortunate enough to be betrayed by his own experiments. Then annihilation is a serious matter. That is my own case. There is no doubt about it; voluntary attention to sensory stimuli of a trivial nature reduces the pulse rate more often than it increases it. But the total facts suggest their own explanation. Attention never decreases the pulse when the antecedent rate indicates anywhere near relaxation. To be explicit: the theory doesn't demand that all mental activity should increase the pulse rate. It only demands that any given mental activity superposed on an unmodified physical activity, like any given physical activity superposed on an unmodified mental activity, should have a corresponding pulse rate. If after running up two flights of stairs one sits down and twirls his thumbs, the pulse rate will not increase but decrease. The new work is much less exacting on metabolism than the old and the pulse rate should theoretically decrease. It does.

¹ 'Ueber die Wirkung akustischer Sinnesreize auf Puls und Athmung,' *Phil. Stud.*, Bd. XI.

² 'Ueber die Begleiterscheinungen psychischer Vorgänge in Athem und Pulse,' *Phil. Stud.*, Bd. XVIII., p. 44.

Similarly in an ordinarily complex environment which is filled with scientific apparatus, including complex attachments to the body of the subject, if interest in these things is successfully inhibited, and if in response to instructions, the attention is concentrated on barely visible spots on a uniform field, or on a just audible sound, a consequent reduced pulse rate doesn't oppose the theory; it confirms it. The trivial object of attention has no stimulating effect on the thought processes. In fact to think of anything besides the trivial object would mean a loss of concentration. But loss of concentration was found to increase the pulse rate, as it theoretically should. There is other evidence that attention to a single trivial matter is chiefly inhibitory. If it is continued long enough and successfully enough, it results in unconsciousness. It is the accredited method of inducing hypnosis. And apparently the onset of normal sleep is conditioned by a similar narrowing of the field of attention.

To assume that whatever one starts to do must add to the total metabolism its own quantum, without reference to its effect on the previous demands, would be too obviously absurd in the case of physical work. Suppose for example one should measure the relative metabolism of walking and running by the pulse rate method. If one were running and started to walk the pulse rate would fall. If one were standing quietly and started to walk the pulse rate would rise. If new intercurrent processes were prevented and sufficient time elapsed to eliminate the residual effects of the running the final pulse rates should be equal within the error of the natural rhythms. The apparent contradiction in the two cases, that the walking pulse is both rising and falling, is due only to a limited view of the facts. There is contradiction only on the false assumption that the running continues in undiminished force after the walking begins,—that the walking is a process that can be superadded to the running without inhibiting it.

It is obvious enough that before walking can be said to begin the running must cease. The metabolic demands of walking are not less than nothing because they are less than

those of running. Positive and negative would be absurd terms to use in such a connection. Whatever indicator one used the two values would find adequate expression only in a continuous series in which completest practicable relaxation should be the basis of comparison.

The whole argument may be transferred, *mutatis mutandis*, to mental processes of attention, etc.

When Zoneff finds that attention to sensory stimuli diminishes the pulse rate, the statement has meaning only when one knows what were the antecedent conditions of attention. That attention to trivial sensory stimuli might reduce the total complication of the psychophysical processes is obvious. It is at least conceivable that different mental processes of a similar kind make different metabolic demands, as walking differs from running. It is conceivable that noting the complex arrangement of recording devices, and the activities of the experimenter, or even that the comprehension of and adjustment to the orders of the experimenter, might make greater demands on the psychophysical organism than counting just observable dots on a plain surface. It seems to me that this alone should be a significant contribution of psychodynamics to the psychology of attention, and a suggestive answer to the question of the rôle of inhibition in attention.

When one comes to notice it, one can scarcely escape surprise that such enormously divergent preëxperimental pulse rates in the same subject should be regarded as "indifferent." Taking Zoneff's three leading subjects, Temparoff has an "indifferent" pulse rate varying from 96 (experiment No. 78) to 61.5 (ex. 72); Petkow's indifferent pulse-rate varies from 87 (ex. 2) to 58 (ex. 127); while that of Martin varies from 82 (ex. 41) to 55.5 (ex. 88). The conditions on which these enormous differences depend are not disclosed in the report. Part of the difference can readily be accounted for by the normal rhythms, respiratory, Traube-Hering, digestive, etc. Part of it may be the remnants of the physical work involved in adjusting the apparatus. Part of it is probably expression of the normal mental activity of a wide awake

man in a complex and interesting environment. I say probably because there were no precautions taken to avoid it or to estimate what couldn't be avoided. One thing is certain. If the pulse rate is any indication of metabolism, the higher preexperimental rates are not accompaniments of psychophysical inaction. In so far as attention and other processes of mental elaboration are concerned they are not moments of "indifference."

Furthermore, if one takes the trouble to compare actual differences between the "indifferent" pulse-rate and the experimentally induced rate, instead of taking Zoneff's bare statements of pulse change during the experimental period alone, it is conspicuous that really low "indifferent" pulse tends to rise consequent to the experimental stimulation, even if the latter is trivial.

Apparently in addition to the inhibitory effects of attention to a trivial object, attention to a faint auditory stimulus tends to decrease the pulse frequency indirectly through an "adaptive" inhibition of the respiratory movements, as Billings and Shepard point out.¹ This effect of adaptive respiratory changes on the heart-rate seems to follow the simple metabolic law. The muscular action of inspiration increases the pulse-rate. The relaxation of expiration decreases it. Suspended respiration after expiration, with relaxed respiratory muscles, decreases the pulse rate to a minimum. That it actually is the muscular action that increases the rate in inspiration and not merely the presence of air in the lungs may be proven by holding the nose and mouth closed and attempting to breathe. No air actually enters the lungs. The muscular activity, however, sends up the heart rate. The finest demonstration of the respiratory rhythm that I have found are the combined curves of Billings and Shepard (*op. cit.*). They not only show the correlation, but they give data for an approximate estimation of the latency of the heart's response to muscular contraction. It averages something over one pulse beat, say roughly about one second. Obviously it is not a coördinate change. Obviously also it is not a

¹ 'Change of Heart Rate with Attention.' *Am. Jr. of Psy.*, XVII, 1910.

delayed innervation, part of a systematic complex. It has the marks of a true reaction.

Decreased respiration rate must always be under suspicion as the probable cause of a consequent decreased pulse rate. This is especially true when there is reason to suppose that the respiration change is "adaptive," or voluntary. In other cases both respiratory and cardiac depression may well be regarded as naturally correlated expressions of decreased metabolism.

V. THE TELEPHONE-GALVANOMETER-SPHYGMOGRAPH

The obvious requirement of maximum psychophysical relaxation for a preexperimental base line makes unusual demands on the technique of pulse registration. The accurate registration of the pulse frequency with minimum disturbance of mental and physical activity has never been treated as a serious experimental problem. Ordinary experimental precautions would dictate that apparatus and operator should be out of sight and hearing. Yet this is far from being a general arrangement. The most conscientious arrangements in this respect with which I am acquainted are those of Shepard. There is commonly more or less hesitation to use pneumatic recorders at a distance. Distance increases latency and decreases sharpness. It is an especial hardship not to be able to adjust the recorder with all parts accessible. A more serious objection is that the ordinary body attachments greatly restrict freedom of movement. Pulse records during a written examination for example, such as I contemplated, would seem to be quite impossible with the regulation pneumatic recorders. Much has been done and will be done with pneumatic registration. With good apparatus, careful adjustments, and conscientious experimental conditions, it is relatively accurate, convenient, and cheap. But after more or less preliminary experimenting with various forms that were accessible to me, I became more and more dissatisfied with them for comparative work.

The following specifications for an ideal recorder gradually developed: (1) legibility of each pulse curve to at least

.005"; (2) evidence in the curves of changes in blood pressure; (3) simple and inconspicuous attachment to the subject; (4) transmission to an adjoining room so that the recording devices may not become an object of the subject's attention; (5) greatest possible freedom of movement for the subject; (6) frictionless recorder; (7) comparable records in two different sittings of the same individual, as well as for different individuals.

Various combinations of these desiderata of an ideal sphygmograph I have achieved for specific purposes. I have not been able to unite them all in a single instrument. The most accurate recorder, a microscope projection sphygmograph, was entirely unreliable during physical activity. See reproduction of records Nos. I., II., III.

The requirement for distant transmission immediately suggested electrical recorders. The electrocardiogram seemed to promise an ideal record. The subject might be anywhere. The records may be read with great accuracy and are directly comparable from one time to another. Several difficulties developed in practice. Changing body currents demanded constant attention and readjustment of the compensating current. The wet electrodes limited the sphere of action, while bodily movements produced gross disturbances.

Once mastered, the string galvanometer proved an ideal recorder. It remained to find a suitable current modified to place over the artery, and a suitable artery to record from. For the sake of freedom of movement I reluctantly abandoned the radial artery, and the finger pulse. As the result of a systematic exploration of the various available superficial arteries the temporal artery proved to be by far the most satisfactory. Abstracting from the intimate connection of the temporal artery with the brain arteries, which as far as I can learn is a useless accident, its bony environment offers an unusually stable foundation for a sphygmographic attachment. The frontal, mastoid, and superior maxillary bones furnish a three-point foundation which is almost entirely free from muscular disturbance. An elastic band about the head would hold any light apparatus against this

foundation without inconvenience. In actual operation it felt like a hat band and was no more uncomfortable. My subjects report that during the experiments the whole attachment usually passed unnoticed.

My first sphygmographic attachment was a double carbon contact suitably mounted and resting on the artery. It transmitted the pulse beats to the galvanometer on the principle of a microphone, and is doubtless capable of serviceable development. The most satisfactory attachment, however, proved to be a skeleton telephone receiver. The circuit of the minute magnets was completed through the string of the string galvanometer. The minute movements of the soft steel armature which rested on an insulating surface directly against the skin over the artery was sufficient to induce measurable currents in the coil of the magnets.

The subject might be anywhere, and move at will, within the radius of the flexible wire that attached him to the binding posts, communicating with the recording room. Violent bodily activity disturbs the record. But all things considered, I believe that the telephone-galvanometer-sphygmograph is the most satisfactory pulse recording device yet produced, for psychological experiments. See records II., III., IV., V., and VI.

The photographic recorder was my old falling plate device, fitted with a long cylindrical lens of short focal length.

The advantages of photographic records are not only the entire lack of friction, momentum, etc., but the possibility of securing any desired magnification within the limits of the illumination, and any desired compactness or detail entirely without modifying the instrumental error.

The temporal features of the pulse of a distant subject are given by this device with great fidelity within the limits of the chosen degree of condensation. If it were desirable individual pulse waves could be measured in units of .001". I was commonly content with .005".

The most serious limitation of the apparatus is that the height of the pulse waves cannot be accurately compared in different records. The telephone attachment gives a dis-

torted pulse curve (records II. and III.). It accurately indicates every change in the pulse wave and the direction of that change. But the height of the curve at any point depends on the initial distance of the armature from the magnets, and on the velocity of its motion. This has some decided advantages when the only significant feature of the curves is the temporal succession of its parts, because of the exaggerated sharpness of the angles of the curves. Further analysis of the peculiarities of the pulse curves is given in the legend of simultaneous records of the right and left temporal arteries by the telephone-galvanometer and the microscope recorders, respectively (records II. and III.).

The latency of the whole device depends first on the sensitivity of the string galvanometer. For any given adjustment of the string this is an instrumental constant. Secondly it depends on the velocity of the initial phase of the process to be recorded. In measuring the interval between the different waves in a series of pulsations of a similar character the latency is a constant, and is consequently irrelevant. Control records show it to be about 3σ in the instrumental complex as I used it.

VI. SPHYGMOGRAPHIC DATA¹ AND PSYCHODYNAMIC IMPLICATIONS

(a) *Mental and Physical Relaxation*

Completest relaxation probably occurs only in profoundest sleep. Consequently the metabolism of profound sleep will probably become the ultimate base line from which the various psychodynamic processes will be measured. Even in profoundest sleep the organic processes are not interrupted. Intestinal and respiratory movements, glandular action, muscular action of the heart and vasomotor system make continual requisition of the conditions of metabolism. The accumulated residual effects of the day's work are gradually

¹ The experimental work of which this paper is the outcome was made possible only by the coöperation of various groups of student subjects. I would take this occasion to thank all of my students who have served as subjects. Especially I want to thank the members of the Class in V. Philosophy, 1911-12, for their cheerful and enthusiastic help in our common work.

equalized. Fatigue stuffs are eliminated. Tissue is rebuilt. All of these processes call for food and oxygen supplies through the blood currents. Less profound sleep increases these demands by relatively incoördinate movements of the voluntary muscles and by the semi-consciousness of dream states.

The relative metabolic demands of these various conditions are not known in detail. But the metabolism of sleep and its corresponding pulse-rate is much lower than anything that we know in the waking state.

The ability to relax physically and mentally without losing consciousness varies greatly with different individuals. But even the best-known relaxation of the waking state shows higher metabolism than sleep.

Mere consciousness itself apparently has a metabolic equivalent which falls within our concept of mental work. This relatively large metabolic equivalent of the barest and least productive consciousness indicates that consciousness itself involves a widespread neural excitation. At least this factor of our working hypothesis for inner psychophysics finds additional confirmation in the facts of psychodynamics.

Arbitrary mental relaxation is by no means an equally distributed capacity. Even for the same individual it is not equal at all times. Possibly some human beings seldom emerge from the nebulous states of consciousness bordering on the dream. It is significant for example that a reduced pulse-rate is a regular accompaniment of advanced dementia. Conversely some persons regularly seem to remain throughout the waking hours at a relatively high pitch of nervous excitement, and find it quite impossible to relax voluntarily or in response to instructions. In pathology this condition is illustrated by manic excitement. The grounds for these differences are unknown. Cannon's beautiful experiments on the effect of adrenaline give additional weight to the arguments of the neurologists that persistent abnormal excitation and depression are due to chemical reagents of physiological origin.

Even in the normal subjects who submitted to laboratory investigation the individual differences in this respect were

striking. One subject always found his mind busy at something or other when he tried to relax. He "couldn't think of nothing," his real interests persistently demanding attention. Naturally his pulse rate indicated this. Except in the evening it failed to show anything like the relaxation depression of my other subjects. At the opposite extreme was an occasional subject, who could arbitrarily stop his mental processes, and fall asleep practically on demand. Both were honor students. Between these extremes there are doubtless all degrees of capacity for arbitrary relaxation.

Even in the same individual the pulse depression of relaxation varies under different circumstances. In my records it was most pronounced and regular when the subject was reclining in a steamer chair. It was less complete sitting up and still less standing. This particular difference is doubtless a matter of muscular activity, but it shows one of the difficulties of securing a constant base line. That there are likewise mental states that interfere with relaxation, appears from the fact that I could never get as low an "indifferent" pulse when the subject was in the same room with the recording apparatus. Furthermore the preëxamination pulse in all my subjects was relatively high. Even after especially long rest intervals the pulse failed to reach its normal relaxation value. Subsidiary preëxamination records from a general class, taken by palpation, showed similar results. The psychodynamic implications of this fact I shall discuss later.

Whenever the relaxation was relatively complete, the experimental introduction of muscular activity or of new mental activity invariably increased the pulse frequency.

Within the limitations of my experimental data, I can say that, provided one starts from a preëxperimental condition of relatively complete physical and mental relaxation, every new mental or physical activity is correlated with an accelerated pulse.

We can roughly equate physical and mental work. Measured by their respective metabolism as indicated by the pulse frequency, the mental processes of multiplication,

TABLE II—*Continued*

Subject G. reclining in steamer chair in quiet room.

Relaxation 1,080 σ .

After two minutes' work 860 σ .

After $3\frac{1}{2}$ minutes' work 780 σ .

Subject R., same conditions.

Relaxation 1,080 σ .

After one minute's work 880 σ .

After $3\frac{1}{2}$ minutes' work 820 σ .

- (D) Effect of violent physical work, rapid double genuflections, *i. e.*, sitting on the heels and rising to upright position.

Subject G., quiet room.

Standing quietly 726 σ .

After 4, 536 σ .

After 2 genuflections, 550 σ .

After 6, 516 σ .

Subject G. later, same conditions.

Standing quietly, 700 σ .

After 4, 536 σ .

After 2 genuflections 540 σ .

After 6, 510 σ .

Subject R., same conditions as above.

Standing quietly, 1,090 σ .

After 6 genuflections, 600 σ .

Subject R. later in afternoon, same conditions as above.

Standing quietly, 1,006 σ .

After 4, 600 σ .

After 2 genuflections, 680 σ .

After 6, 560 σ .

TABLE III

THE EFFECT OF MORE COMPLEX MENTAL PROCESSES ON PULSE LENGTH

- (A) The effect of reading and of listening to reading.

Subject G., quiet room, reclining in steamer chair. Reading Pillsbury's *Psy. of Reasoning*.

Relaxation (1050).

Immediately fol.

Passive reading 970 σ .

Active reading 980 σ .

Silent recapitulation 900 σ .

Silent recapitulation 960 σ .

Subject R., quiet room, same book, and other conditions as for subject G.

Relaxation 1,020 σ .

Passive reading 900 σ .

End of page 830 σ .

Recapitulation 940 σ .

Immediately following same conditions as before.

Relaxation 980 σ .

Active reading 920 σ .

End of page 910 σ .

Recapitulation 960 σ .

- (B) Effect of learning a series of twelve words on pulse length.

Subject R., quiet room, steamer chair, words read by assistant.

Relaxation 1,140 σ .

Beginning 1,040 σ .

Second rep. of series 940 σ .

- (C) The effect of multiplication on the pulse length.

Subject F. In room with recording apparatus. Microscope recorder. Carotid pulse. Subject reclines in steamer chair.

Multiplies a two-place number above 50 by another two-place number above 50.

Morning.

Relaxation 663 σ .

During multiplication 614 σ .

TABLE III—*Continued*

Afternoon.	
Relaxation 729 σ .	During multiplication 690 σ .
Relaxation 740 σ .	During multiplication 650 σ .
Relaxation 776 σ .	During multiplication 616 σ .
Evening.	
Relaxation 816 σ .	During multiplication 760 σ .
Subject A. In room with recording apparatus, same as F.	
Afternoon.	
Relaxation 660 σ .	During multiplication 632 σ .
Evening.	
Relaxation 714 σ .	During multiplication 646 σ .
Subject R. Quiet room. Tel. galvanometer recorder.	
Relaxation 1,140 σ .	During multiplication 980 σ .
	Nearly at end 840 σ .
	Recapitulation with considerable bodily mvt. 740 σ .

reading, or learning a series of twelve familiar words involves less work than 6, 4 or even 2 double genuflections. They are more closely approximated by the raising of a one-pound weight once every four seconds.

The relatively small energy transformations of mental work is not a surprise. A considerable amount of experimental and non-experimental evidence points in the same direction. The electrical and heat products of nervous tissue in action are relatively small compared to analogous products of muscle action. The oxygen consumption of nerve tissue is relatively small. In nerve fibers it is too small to be measured directly by present technique. It isn't impossible that the differentiation of nervous tissue and the corresponding biological division of labor is a condition of economy of energy as well as of better work.

The sequence of the metabolic demands of the various physical and mental processes varies in a suggestive way. (a) The initial demands for establishing an adjustment may be greater than the subsequent demands for maintaining it. (b) Or the continuation of the process may make increasing demands. To the first class belongs rising from a sitting to a standing position. The adjustment to the new position makes relatively large demands. After the standing position

has been attained it required relatively little increased metabolism to maintain it than to sit still in a chair. This of course is entirely predictable. Obviously the main muscular work is done in raising the trunk a foot and a half. After it is once raised the chief muscular work will be the occasional correction of a lapsed equilibrium.

It is probably less predictable that merely opening the eyes at command would increase the pulse more than the raising of a one-pound weight one foot, and that the subsequent relaxation with the eyes open in a very familiar environment may be almost if not quite as complete as voluntary relaxation with the eyes closed.

A succession of double genuflexions, on the other hand, make constantly increasing demands on metabolism.

If the sphygmographic data for short intervals have any reliable metabolic significance at all, then in the case of opening the eyes on a familiar scene the mental work involved will be a relatively short process of adjustment to the familiar conditions. Familiar environment is not a stimulus to continued mental elaboration. The fact that unchanging stimuli quickly cease to be real stimuli is introspectively verifiable in numerous instances, as in James's experiment of staring fixedly at a word. Obviously we have come upon the trail of a fundamental instance of real mental fatigue. Finally, the doctrine that mental phenomena are largely matters of adjustment has been made familiar by Dewey and his pupils. Psychodynamics seems to furnish some experimental evidence of the truth of Dewey's contention. Subsequent evidence of the course of metabolism during examination will increase that evidence.

Aside from their incompleteness, which time alone can remedy, the most serious faults in my experimental data thus far presented is the fact that all the processes are of short duration. This is doubly unfortunate. It was precisely for processes of short duration that our technique was least well adapted to serve as an indicator of metabolism. Secondly, the problem inevitably arises whether, even if the data are accurate, they correlate with the processes of

multiplication, reading, etc., or only with the commencement of these processes in response to the command of the experimenter.

With the telephone-galvanometer pulse recorder, it would have been possible to have extended any one of the processes more or less indefinitely. These are tasks for the future if I can get suitable assistance.

The extended mental work which I actually undertook to study was that for which I developed the technique, viz., the mental work of written examinations. This process was chosen because it has the unique distinction among mental processes of having had its metabolism studied with adequate calorimetric technique.¹

In the calorimetric study of examination metabolism the sphygmographic observations were incidents of only casual experimental interest. No adequate technique had been elaborated for securing their accuracy, and in consequence they have been subjected to considerable criticism. The authors were the first to note their imperfections.

1. To the untrained subject the act of taking his own pulse is an operation of some uncertainty and difficulty. The pulse may be difficult for the untrained to find, to differentiate from muscle tremors, to count, and to correlate with any definite time interval. The sphygmographic data of Benedict and Carpenter are not entirely reliable as facts.

2. If we granted their reliability as facts, we must realize that the act of finding, counting, and correlating the pulse is for the inexperienced a real, perhaps a considerable, mental task. What effect this would have on the pulse rate of the observing subject, would depend on the extent of the disturbance and the antecedent conditions. It might tend to calm a student who was worried and excited over a difficult examination question. Or again the arbitrary interruption of his train of thought might be highly unpleasant and annoying. It might be resented. Under any circumstances it would constitute a new and unusual stimulus, attention to which might constitute measurable mental work.

¹ Benedict and Carpenter, 'Influence of Muscular and Mental Work on Metabolism,' U. S. Dept. of Agriculture, 1909.

3. Finally, granting some direct effect of the emotions on the pulse rate, the unusual conditions of entering the portentous calorimeter box may have been an influence quite outweighing the examination process as such.

Benedict and Carpenter were fully alive to the need of separating the mental processes from the various motor activities that must accompany a written examination. To determine these latter they made a series of control experiments in which their subjects merely copied from two different sources approximately the same number of words that their examination paper contained. The theory of the authors was that the metabolism of the mental processes of the examination would be obtained by subducting the metabolism of the control from the examination metabolism.

Certain imperfections of the control were apparent to the authors, but at that time there were no dynamic data on which a more adequate control could be elaborated.

Unfortunately from the standpoint of interest in the pulse rate these control experiments give us no help. The work of copying was not distributed over the entire period but was concentrated at the beginnings of two periods. The consequent fall in pulse rate which is universal in the control experiments includes in every case a period of inactivity.

But a more general difficulty of the control experiment is disclosed by psychological analysis. It was assumed by the authors that in copying, the mental work, while not eliminated, was greatly reduced. In a psychological investigation this assumption needs careful scrutiny.

(a) It may be noted first that the majority of the subjects found the copying tedious, monotonous, or uninteresting, at least in part. It had at least some of the subjective marks of mental work. One of the subjects ventured to criticize it as such.

(b) If we subject the control process to psychological analysis, it will in fact appear that copying is far from being a simple motor process. Copying is in no case a mechanical reproduction of letter and word forms, but a translation of printed words into written. Psychologically it involves

sensory attention to a constantly changing group of visual stimuli, perception, and primary identification, the arousal of the appropriate motor and auditory residua, and memory, at least long enough to control the written product. That it sometimes aroused a complex intellectual reaction appears from the reports of the subjects. Now we have already seen that the mental process of reading and learning both have a measurable metabolic equivalent. The question of relative amount is a matter for analysis and experimental investigation. It is unsafe to assume too much in such cases. Briefly we must say that whereas the matter that is written during examinations is chiefly the mere reproduction of matter that was previously learned, with a certain amount of rearrangement; the matter of the control experiment has to be assimilated anew at every step. If the Benedict Carpenter metabolism experiments have any psychological meaning, it is that copying 12,000-15,000 words of text involves only slightly less mental work than the writing of a similar number of words from memory, or from internally controlled association on the stimulus of a suggestive question. Experimental tests of various conditions indicate that the continuous repetition of a few lines of familiar verse is a simpler process than copying.

The question of a thermodynamic equivalent of the mental work of examination has not been answered. It was an important service to psychology when the physiological chemist conceived and clearly formulated the problem.

It was no part of my present inquiry to investigate the specific sources of error in the pulse-rate data of Benedict and Carpenter. I was more interested in preventing them.

My first task was to provide for the registration of the pulse rate by the most innocuous and reliable means. That was provided for by the telephone-galvanometer-sphygmograph, already described. It registered the pulse in the instrument room while the examinee sat at ease in a separate room with closed doors between. He was in a familiar environment with natural lighting (bright daylight). Since I could devise no means of producing control writing move-

ments without corresponding mental work, I hit on the expedient of dividing the records of the examination period into those which were made during actual writing and those that were made in the non-writing or thinking periods. The theory of this was that while the writing movements couldn't be eliminated from the examination katabolism, either directly or indirectly, their place in the total metabolism of examination would be determined by comparing the moments when they did occur with the moments in which they were absent. In purely relative values, such as the pulse rate gives, it seemed that this was as much as one could expect even of the most rigid controls.

The principle of the technique was suggested by one of my students, Mr. W. W. Reynolds. A delicate and simple inertia-contact-breaker attached to the pen proved entirely satisfactory. It was unobtrusive and effective. When properly adjusted it gave an indication on the photographic record for every stroke of the pen. (See records V. and VI.) The device was as follows: One end of a linen thread was attached to the pen, and the free end was carried over the back of the table and attached to the end of a fine steel watch spring, two inches long, which in turn carried a little square of thin wood with an adjustable contact. The contact was so adjusted that when the pen was quiet it was just closed, while the minute movements of the pen in making the small letters were sufficient to pull the spring away from the contact before the inertia of the wooden base was overcome. Gross movements of the hand and arm were taken up by free suspension of the contact breaker over the edge of the table. To prevent unnecessary friction the thread worked over a long glass tube of about three quarters of an inch outside diameter.

A further source of error was the more or less continuous restless body movements of the examinee. While these couldn't be eliminated without undue constraint, and more or less distraction of the student from the work of the examination; like the writing movements, they might be recorded; and records with excessive movement might be eliminated. Another simple device proved highly efficient.

The technical problem was to give the greatest freedom of movement to the subject, and to record minute as well as gross movements within the least possible compass. The solution of the problem was not easy, but once found it accurately indicated the main body movements. The device was as follows: A large hemispherical tambour was held against the back of the subject just under the right shoulder blade. Connecting tubes, chiefly iron gas pipe, carried the air waves to a small Marey tambour in the adjoining room in front of the recording apparatus. If the writing subject remained otherwise quiet this showed the shoulder movements of writing superposed on the longer respiration curves. If the subject was entirely quiet it indicated his respiration. To provide for gross movements the tambour was attached to a movable support which had just friction enough to be overcome, when the rubber diaphragm was tightly stretched. With an open vent near the recording tambour, even after the most violent movements, the system quickly regained its normal sensitiveness. The freedom of movement was remarkable. The subjects leaned back in the chair, bent over their work and even tilted the chair back on two legs without consciousness of restraint and without disarranging the recorder.

Although these two devices were satisfactory for their purposes, so many other and perhaps better similar devices are possible that I have hesitated to waste space in minute descriptions. If any one is in need of similar devices, and has nothing better in mind I shall be glad to furnish blueprints of any of the parts. Or our mechanic will duplicate them at actual expense of time and material.

The results of the combined records taken every two to three minutes throughout the examination period, with a pre- and post-examination period of relative relaxation are shown in the following distribution areas.

The following tables are constructed by entering under the appropriate non-writing or writing column the number of the photographic plate on which any given pulse length occurred, against the group within which that particular

time interval falls. For example, in the records of subject R., a pulse length of from 641-660 σ occurred during the non-writing periods of the examination six times, twice in the first plate, twice in the third, and twice in the tenth plate near the end of the examination.

The digit one is used for both the first and the eleventh plate but their positions clearly differentiate them.

TABLE IV

DISTRIBUTION AREAS OF THE DURATION OF PULSE WAVES DURING EXAMINATION

Italicized record numbers indicate last half of period

I. While Subject Was Not Writing

II. While Subject Was Writing

Subject R. Examination in Economics. Paper marked Grade II.

T. in σ	Non-writing	Writing
541-560	I	
561-580	116	9
581-600	6	889
601-620	6	22889
621-640	112	111222333444488I
641-660	113300	123334445
661-680	1223335566689	14444457779
681-700	12333568990000	334444577899I
701-720	115667889	44577799999901I
721-740	66677780	57788900001I
741-760	667	5800001I
761-780	66779000	5680I
781-800	0000	
801-820	00	
821-840	0	

Subject A. Examination in Psychology. Paper marked Grade I,

	Non-writing	Writing
561-580	2	
581-600	1112223444	
601-620	111111123344	16
621-640	1123344	22244556
641-660	111444	1111112222234455566
661-680	14555660	111122233334445555
681-700	1455999	1111133333344455556688
701-720	34589999	12333334668
721-740	356900	255888880
741-760	5889000	4555689900
761-780	368	568999990
781-800	33390000	899
801-820	00000	5889
821-840	000	68990
841-860		9
861-880		66

TABLE IV—*Continued*

Subject G. Examination in the Hist. of Philosophy. Paper marked grade II. The italicized records were taken after a period of rest in which the subject went out doors and smoked a cigarette.

	Non-writing	Writing
641-660	I	8
661-680		788
681-700	2.....	3888
701-720		3357788
721-740		3888
741-760	23	355788
761-780	134	2567778
781-800	34.....	15568
801-820	127	12233344555667
821-840	111122	222222233345667
841-860	4466	345666
861-880	134	2446667
881-900	112447.....	22456
901-920	6	3555566666
921-940	46	566
941-960	144	477
961-980	4	
981-1,000		7

The distribution areas are of course made up of all sorts of rhythms and particular correlations which space forbids us to consider in detail. Exact time records on the reports and occasional time records on the examination paper permit intensive study of the details. We shall have to content ourselves at this time with the general characteristics of the series.

The records were especially planned to show the relation of the non-writing to the writing pulse. In general the non-writing periods represent two entirely different situations; moments of reading the questions and active elaboration of the answers on the one hand, and moments of rest or relaxation on the other hand. Either may occur at any time throughout the examination period. But the former is most pronounced at the beginning of the paper and the latter naturally occurs at the end in most pronounced form. The distribution, however, shows no isolated phases. The pulse frequencies throughout the examination show certain rhythms and oscillations with conspicuous general drifts, but they clearly belong to one series with only gradual changes.

It is conspicuous that, except in the case of the unusual nicotine disturbance of subject S. (indicated in this case by italicized digits) the highest pulse rates are not found in the writing periods but in the first non-writing period, *i. e.*, the period when the question paper is first seen and reacted to—the period of most active mental adjustment. The mode of the writing pulse frequency for both halves of the examination period is higher than that of the non-writing pulse frequency for subject R.; lower for subject A.; and approximately the same for subject G., exclusive of the nicotine effect.

Since we cannot assume that mental processes stop during the writing periods, one can say that, as far as the present records reach, the muscular demands on metabolism during the examination period incident to the process of writing are conspicuously less than the demands of the distinctly mental processes.

Certain personal peculiarities of subject A., in whose case the non-writing pulse is distinctly higher than the writing pulse, make his records especially interesting. Subject A. did not read the entire paper before he began to write. He read only one question at a time. This was in accordance with habit. In his case, throughout the examination, the metabolism of writing, as far as it is represented by the pulse rate, is conspicuously less than the nervous metabolism incident to the mental adjustment to the question. In his case the mechanical routine of writing operates to depress and inhibit his mental activity.

In view of the criticisms that have been heaped on the Benedict-Carpenter pulse counts, it is a source of considerable satisfaction to record that the general gross results of my records correspond point for point with theirs. Two facts stand out clearly in their data as in mine.

1. The initial pulse of the examination period is regularly high. At no time even during the pre- and post-examination periods of relative relaxation does the pulse rate reach its normal relaxation rate.

2. In all three of my cases and in sixteen of the twenty available cases of Benedict and Carpenter the pulse rate

decreases more or less gradually from the beginning to the end of the examination period.

In my own records the distribution of pulse lengths during the first half of the period shows about the same distribution curve as their distribution during the last half, but on a higher level. The modes in the two examination periods lie from 80–120 σ apart.

The evidence is unequivocal that the last half of an examination period involves less mental work than the first half. Superficially this seems to be almost absurd. There is no direct introspective evidence that the last hour of an examination period is easier than the first. There is no objective evidence that the examinee writes slower. My own papers do not show that the last half is regularly poorer than the first. On the contrary it would seem that questions assumed to be of equal value ought to require the same mental work to answer. A universal easing up of all examination papers at the end is somewhat improbable, though I must confess that, since I have been looking for it, I have found a tendency in my own case to put more difficult questions early in the paper.

Granting that the lessened metabolism at the end of the examination period is real and that the occasional differences in the character of the questions is not a sufficient cause, we are under obligations to examine the course of the physical and mental processes for a probable explanation.

1. It might be that long sitting gradually reduces the general bodily metabolism, while the mental demands remained approximately the same. Unfortunately as we have already acknowledged, a direct differentiation of muscular and mental metabolism is a difficult or impossible matter, except as one or the other changes in some known way. In a tentative judgment of the matter we must take whatever evidence we possess. Now we have evidence that in ordinary circumstances relaxation reaches a higher grade within 15' of rest than any grade that occurs during examination. Natural relaxation is necessarily prevented during examination by the writing movements, as physical activity, and by mental elaboration of the answers, and writing controls.

The gradually decreasing pulse can be understood only in conjunction with the high initial pulse. In the Benedict-Carpenter experiments, the high preëxamination pulse might seem to be explained by the novelty of the surroundings. It was an awe-inspiring matter to enter the old calorimeter box and to be shut off so completely from the normal world. On any theory of psychodynamics one would have expected active mental adjustments, quite apart from the examination. That the later, control experiments showed regularly lower initial pulse might have been explained by habituation.

My procedure was carefully planned to eliminate these possible extraneous disturbances. The examinee worked in daylight, under natural conditions, in familiar surroundings. Instrumental attachments were reduced to a minimum, and the subjects had all been long habituated to what there were. Still the initial examination pulse was high, and prolonged preëxamination relaxation did not reduce it to a normal relaxation rate.

The conditions of this high initial pulse were not unusual environment in my experiments, whatever may have been true in the Benedict-Carpenter investigation. It was not conditioned by any violent emotion. All my subjects were college seniors, and honor men. Examinations were a part of the routine work. Still the initial pulse was high. It was high as the subjects sat quietly resting before the examination paper was given them. It was highest when the examinee read his paper. Then it gradually slowed down, though it never reached the normal relaxation rate.

Is there any reasonable theory to fit these facts? Not yet. But we have at least important beginnings of a theory.

The students prepare for examination at Wesleyan, as elsewhere, with a spurt of intensive application. I am sure such preparation is not entirely good. I am not sure it is entirely bad. At best the whole subject is reviewed and coördinated. The students study for a climax. That climax is the examination period. All the relevant residua are planned to be at a maximum of reproducibility. As a rule this is apparent. Never again will the student have that

particular group of material so well in hand. If the examination is inadvertently postponed the student feels that he has been unjustly treated. He was "ready" at the time appointed. He will not be as "ready" again without additional application.

It must be some inner organic condition, this readiness to discharge. Admitting that we do not know any of the organic details, if readiness to discharge is some specially excitable condition of the relevant nervous tissues, it must be achieved and maintained by a metabolic process that should theoretically express itself in increased pulse-rate. The whole demeanor of the student before and during examination corresponds with this supposition. He has studied late, perhaps for several nights. But he does not feel sleepy or fatigued. Far from it, he is thoroughly awake, ready, and eager. He is in an unusual condition of excitement. At best the excitement is primarily of the relevant residua. But it may be more or less diffused, and useless.

The pulse-rate increases as the questions are read. Why? In an important contribution to the dynamics of the reading process, Dearborn showed that the initial fixation of each line tended to be longer than the others.¹ He conjectured that this longer initial pause provided for the general mapping out of the whole line. Following him I demonstrated that the pre-fixational reading process, even when it involved no clear perception of the word, shortened the adequate speech reaction of fixation.² As far as I have discovered that was the first experimental evidence of a temporal difference between the predominantly stimulating and the predominantly inhibiting phases of the perceptual process. There are excellent grounds for believing that all mental processes involve both inhibition and excitation. Both are commonly assumed in attention. Both may be supposed to underlie the rise and fall of excitation in uncleared residua as they are easily or remotely accessible. The increased pulse rate on reading the paper seems to correlate with the increased ex-

¹ 'Psychology of Reading,' p. 121.

² 'An Experimental Study of Fixation,' p. 53 fol.

citation of specific systems of residua as the general plan of the examination paper or of each question arranges itself. That is evidenced introspectively by the partial clearing of fragments of answers, the recognition of difficult parts, etc. A similar increased metabolism might indicate a general useless excitation. Its course has never been studied.

The gradually decreasing pulse rate as the examination proceeds correlates with the progress of inhibitory processes, as group after group of the stimulated residua discharge in action.

The post-examination resistance to complete relaxation, and relatively insistent high pulse rate, correlates with a certain residual excitation of the discharged systems. This residual excitation shows itself in the common desire to talk the paper over with some one, and the common difficulty of getting it quickly and completely out of mind.

I would not be misunderstood as holding that these various correlations are satisfactorily established. The work of correlation is only begun. It seems to me however that we have justified our concept of mental work, as a consistent and useful scientific tool; and that we have developed a group of real problems to a point where their importance is obvious and their further refinement is a matter of time and attention.

DESCRIPTION OF RECORDS I.-VI

All records read from left to right. Rise in the lines indicates positive action.

I. Simultaneous records of the carotid pulse (by the microscope-projection recorder); the respiration (Harvard pneumograph); and the electro-cardiograms (Edelmann string galvanometer, small model). The upper boundary line between the light and dark shows the pulse record (six systoles). The fine dark line shows the respiration curve (two inspirations). The lower shadow is the typical electro-cardiogram. The ordinates are 50 per 1" and show with great exactness the time relation between pulse and electro-cardiogram. The respiration pressure changes show plainly in the pulse curve.

PLATE I.

II. Simultaneous pulse records from the left and right temporal arteries, respectively. The upper curve is from the microscope-projection-recorder with the same sensitivity as in I. It will be noticed that the intercurrent oscillations that are so prominent in the carotid pulse are lacking almost completely in the temporal. This is universal in healthy subjects. The lower fainter line shows the corresponding telephone-galvanometer records. It will be noticed that pulse movements are exaggerated. The time of these changes is given with great clearness. The form of the curve is unreliable.

III. Simultaneous records as in II. but taken on a rapidly moving plate to show the relative latency of the two recorders. The sensitivity of the telephone-galvanometer-recorder was reduced to eliminate the exaggerated amplitude of the curves. Ordinates are 100 per 1". The latency of the two instruments as used in my experiments is practically identical about 3σ .

IV. Four telephone-galvanometer records from the temporal artery during rest and after a series of double genu-flections: (a) Subject standing quietly; (b) after two genu-flections; (c) after four; (d) after six. Ordinates are 50 per 1".

V. Three examination records of the temporal pulse. Each record shows the frequency of writing movements (notches on the upper edges of the records); body movements in the heavy dark line, and pulse frequency (telephone-galvanometer recorder). All three records were transmitted 30 ft. The pneumatic respiration indicator is correspondingly delayed.

VI. Examination records similar to those of Fig. V. The upper record shows a violent body movement. The lower two records clearly indicate the respiration movements. In both V. and VI. the ordinates mark off .02".