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# Psychological Review

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# THE PSYCHOLOGICAL REVIEW

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## PSYCHOLOGY AS A SCIENCE OF CRITICAL EVALUATION

BY J. R. KANTOR

*University of Chicago*

The present confusion in the domain of psychology with respect to its subject-matter and method need not be interpreted as a failure to justify its claims to be a science. It is indeed an indication of progress that psychologists are attempting to specify precisely what is the content and method of their domain. The difficulty of this specification is of course in great part owing to the extreme complexity of mental phenomena, but in spite of this difficulty, there are indications that psychology is beginning to interpret its phenomena in accordance with the logic of science.

The fundamental purpose of science is to isolate certain phenomena, and to describe them so adequately, that their significance is understood, or that control over them is established. Science is essentially a process of critically evaluating facts and conditions, which are brought to its notice because of various human problems. Each particular science evaluates, that is, endows with meaning some specific type of phenomena. The importance of employing an adequate method of interpreting phenomena is therefore obvious, for otherwise the descriptions cannot but mislead the scientist and fail to satisfy the needs of his problems. In all sciences there is one outstanding condition of correct evaluation, and that is to keep as close as possible to observed data, and not to neglect complexity of fact, in an effort to gain simplicity of description. A comparative study of scientific procedure

indicates strikingly that psychology has been the least critical of all sciences in its methods, although it should have been most cautious, because of the presence of inherent difficulties in its facts. Experience has shown that psychology must especially avoid descriptions of mental functions given exclusively in terms of external reactions, or states of consciousness. To explain a mental function we must not confine ourselves exclusively to either the mental, physiological, or physical aspects of the process studied; in an adequate treatment each phase must be included. Professor Angell has indicated how utterly unfruitful it would be to attempt to state such a process as memory in merely objective terms.<sup>1</sup> When therefore the evaluation function is well understood and critically employed in the field of psychology, that discipline will find its place in the domain of science amply secured and safe from invasion. This paper attempts to define the function of critical evaluation as it applies in psychology, and to point out the consequences of its correct use in that discipline.

The critical evaluation function, which constitutes scientific activity, is an amplification of experienced events, and makes for a consistent control of the further progress of experience. We might say the evaluation function of science is itself a type of experience, because in the final analysis it is one of the several ways in which individuals are in contact with known objects. It is clear that upon this basis no science may start with some type of *a priori* classification. For example, the question whether phenomena are mechanical or teleological may be asked only after the study of the concrete facts is concluded, and it must be remembered that these terms have meaning only as functions in the interpretation of facts.

The facts of science are evaluations of phenomena; they develop through a series of stages, as the phenomena are brought under greater control of the individual. The simplest forms in which the world of objects, forces, and conditions have any meaning for us, are attitudes which the organ-

<sup>1</sup> PSYCHOL REV., 1913, 20, p. 255.

ism assumes toward its surroundings. These attitudes in their simplest form are, in common with all acts of a conscious being, psychophysical, and must of course be described as such. These attitudes which are simple psychophysical adjustments, constitute the meaning of objects or other aspects of the experience eliciting the attitude. One of the simplest meanings that the candle flame can have for us is the act of withdrawing our hand. The meaning cannot be detached from the act, and this leads to the statement that the meaning of the candle flame is the act. This evaluation of the experience is of course an abstraction, since it is evident that we are describing or evaluating only a partial happening. It would require a series of responses to give the entire meaning of the candle flame. The object might be described as the 'hand attraction reaction,' as 'body trembling reaction,' and in other ways. When attitudes become significant for the individual through repeated contacts or some other means, the object is evaluated in terms of the attitude. A ball is an object to throw; a dog is something to pet, or to run from. This entire process is a clarification of the meaning of the object, or more broadly of the experience-situation. When the meaning of the experience-situation becomes clearly appreciated by the individual, it becomes detached from the situation, and is handled as a fact, by some sort of symbol.

A certain definite experience is symbolized as 'partaking of food and digesting it'; this is a simplified abstraction from a complicated system of actions. In this example we observe a typical first step in detaching a meaning from an act-situation. This step involves the reference of the adjustment-behavior to some external object as its occasion. The development of knowledge concerning objects is an elaboration of their meanings, and goes on as a process of acquiring control over objects by way of controlling the possible responses to be made to them. The greatest control over objects, which is scientific control, consists in arriving at such a determination of an object's possibilities as to know for definite, limited purposes every effect that the object can bring about in the individual. Any specific purpose is the

solution of a definite problem which initiates the scientific investigation. The dentist knows most of the possible effects of nitrous oxid when he discovers its anæsthetic properties, and the harmlessness of after-effect. It is apparent that a scientific evaluation in its symbolization of phenomena gets farther and farther from actual effects; that is, it represents them.

As scientific knowledge advances, the control over objects is paralleled by the growing remoteness between happenings and their symbolization. The concept of force as a scientific instrument is far removed from any actual motion, though it must in some capacity or other serve in the control of all movements. This point is further illustrated by the case of ether, which is a scientific idea or symbol, representing so many phases of experience that it does not point to any actual one. The symbolic character of a psychological description emerges in the consideration of a perceptual experience. The abstractive nature of the description seems clearly indicated by the fact that it is impossible to mention all the factors involved, and the more adequate and accurate the description the more it reproduces the experience. The ideally perfect description would involve forcing the individual who receives the description to have the original experience. The best way to describe my perception of an object is to point to the object; so some person can also perceive it.

The world of science then consists of knowledge, 'constructs,' which mean or represent conditions or objects, which bring about definite changes in the individual in contact with them. The main point to be emphasized here is the continuity which runs through from the actual occurrence to the scientific description, if that description be genuinely valid. It is clear then why the type of evaluation which obtains in physics and chemistry is not serviceable in the field of psychology. There are entirely too many differences in the two types of facts. A significant difference is that conscious behavior is much closer to the individual than physical phenomena. In the case of conscious phenomena the scientist



studies his own behavior or behavior like his, while in the realm of physical phenomena the scientist studies action of an extremely different object. The meanings or symbols in the two cases differ very markedly, and cannot be thought of as equally valid in representing identical experiences. The description of conscious behavior in terms of physical symbols or ideas implies a misrepresentation of the thing or event described. When such faulty description is employed the evaluatory concepts are not derived from concrete facts, and whenever an evaluation concept does not reach back to actual happenings in the experiences of individuals, it must be rejected as a scientific tool.

The traditional failure of psychology to study critically its phenomena, and to describe them in adequate, scientific terms may be attributed to the persistence of a mistaken ideal of science. Psychologists have believed that the ideals of science were best, if not entirely, realized in the domain of the physical disciplines. In the early history of psychology this led to faulty conceptions as to what constituted scientific description, a fact which was intimately related with current conceptions concerning the relation of psychical and physical phenomena. It must be remembered that scientific psychology developed under the auspices of the Weber-Fechnerian psychophysics. It was a peculiar turn in scientific history which made such a vast and still growing science originate from such an extraordinary and fanciful doctrine, which in the beginning gave impetus to the development of experimental psychology. This peculiar origin was not without its bad effect, since the attitude became prevalent that the material of psychology, while in some sense identical with physical things, could still be considered as separate and independent of them. This viewpoint has of course taken different shapes, while maintaining its original sense. This led psychologists to declare that the domain of psychology differs from the domain of the natural sciences only in point of view. Psychology is assumed to get into more direct contact with its objects, and its knowledge is thought to be more concrete and immediate. This assumption continues

the long entertained prejudice that we have a more immediate acquaintance with conscious phenomena, than with other facts of science.

Coincident with the adoption of a mistaken viewpoint concerning scientific description, the early, scientific psychologists, who have shaped the course of the science, fostered an unfruitful attitude concerning the methods of psychology. They assumed that psychology should accept as valid the type of analysis which is used in chemistry. Such procedure is an immediate consequence of assuming that psychology, like chemistry, deals with a homogeneous material.<sup>1</sup> A description of its phenomena would then be exhausted by three problems to be solved in succession. "The *first* is the *analysis* of composite processes; the *second* is the *demonstration of the combinations* into which the elements discovered by analysis enter; the *third* is the *investigation of the laws* that are operative in the formation of such combinations."<sup>2</sup> Psychology is thus made to deal with some sort of stuff which must be reduced to its simplest elements, and this reduction leads to the hypostatization of certain functions. The viewpoint that psychology must work as the other sciences do, and must find similar stuff to work upon, brought with it the consequence that the experiences with which psychology concerns itself are wrongly described and hopelessly misinterpreted. The extreme, introspective psychologists reduce the mental functions to a series of psychic states, which entirely fail to represent the phenomena they are attempting to describe. The phenomena are made into abstractions that can never be found outside the descriptions.

The failure of the behavioristic psychology to achieve any advantage in the description of psychological phenomena, may be attributed to the fact that it only ostensibly gives up the wrong attitudes of the structural psychologist. The behaviorist makes a splendid attack upon the mechanics of mental states, but wants to substitute just as vicious a formalism in terms of stimulus and response. The resulting

<sup>1</sup> Cf. Titchener, 'The Psychology of Feeling and Attention,' p. 291.

<sup>2</sup> Wundt, 'Outlines of Psychology,' 1907, p. 28.

descriptions of the behaviorists are entirely lacking in the essential factors which constitute a conscious behavior,<sup>1</sup> and consequently do not represent actual psychological phenomena. It seems unbelievable that any person investigating the actions of conscious beings, whether animals in the upper scale of development or the human individual, should attempt to reduce this behavior to reflexes and motor habits. One cannot avoid the question as to what sort of science it can be which reduces conscious behavior called emotion, to 'muscle twitching' and 'glandular secreting,' or language, to mere motor habits?<sup>2</sup>

The behaviorist apparently fails to consider sufficiently the function and purpose of a scientific method. This failure comes out in the fact that because he concluded that the introspective method was not the only direct method of ascertaining psychological facts, then it was possible to do away with all factors of behavior which implied the use of introspection. This entire procedure indicates in an excellent way the meaning and importance of an adequate method in psychology. If the inadequacy of an introspective method which was developed to investigate mental states is established, then that method must be improved in order that the actual phenomena may be studied and described. To jump to the extreme that conscious behavior can only be studied by an objective method, results in the assumption that conscious behavior is purely physiological. This procedure demonstrates the necessity of a scientist to evaluate the phenomena he deals with; so that he can solve his problems on the basis of the data involved in those problems. The critical interpretation of conscious behavior such as language,

<sup>1</sup> Note that I do not say consciousness. Every fact of consciousness is a conscious behavior, a complex action involving always besides the mental factors, also organic, muscular, and glandular processes.

<sup>2</sup> Watson, *Journal of Philosophy*, 1916, 13, p. 591. It seems clear that the behaviorist means in many cases to include more factors as constituting human behavior than he overtly asserts are present. He certainly must mean to include under the term habit a good deal besides mere muscular and glandular function. It is significant in this connection that Watson, for example, is constantly stressing the matter of terminology. This implies an acceptance of the facts of conscious behavior, if not the name.

memory, thought, and emotions for example, cannot tolerate the description of these behaviors as 'states of consciousness,' or as 'muscle twitching.' These processes must be described as organic events, which are in relation to circumstances necessary for their production, and which have other events following as consequences.<sup>1</sup> The emphasis will be placed upon the actual, concrete happening. The method will be such as to give an adequate place in the description to all the specific factors involved, whether they are muscular, glandular, neural, affective, conative, or cognitive. In consonance with the accumulation of facts pointing to the specific adjustmental nature of conscious behavior, the description of human action will involve in addition to the factors mentioned, the particular conditions under which the acts take place. Thus, in order to describe a conscious behavior as implying a sentiment or not, the immediate conditions under which it occurs must be known. "To tell a child who is quite innocent of any feeling or sentiment, who is merely grabbing for something to put into his mouth, that he is selfish or greedy, is to requalify a mode of response in this way."<sup>2</sup> Whether an act is the emotion of anger or not depends not only upon the muscular changes, but also upon the meanings and the affective elements involved.

We might inquire into the motives which led the behaviorist to adopt a method which totally misinterprets the phenomena with which he deals. It was the endeavor to introduce such necessity and certainty into the study of psychological phenomena; so that "the findings of psychology become the functional correlates of structure and lend themselves to explanation in physicochemical terms."<sup>3</sup> The motive seems to be to get rigid absoluteness into psychology even at the expense of losing everything else. It is an elequent commentary upon the behavioristic movement to have a psychiatrist speak of it as a psychology, "whose conception of vital human functioning is suggestive of nothing so much as of a fire crackling through a carpet of dry leaves.

<sup>1</sup> Cf. Dewey, *Journal of Philosophy*, 1918, 15, p. 32.

<sup>2</sup> Dewey, *loc. cit.*, p. 34.

<sup>3</sup> Watson, 'Behavior,' p. 28.

It reaches to no depths, it involves no profound smoldering sources of conflagration, it leaves no real scars beneath the surface."<sup>1</sup> If the behavioristic attitude seems to promise so little for the solution of concrete problems of human maladjustment, there must be something radically wrong with it.

The inefficiency of behaviorism to meet the needs of a genuine understanding and control of phenomena is in part accounted for by its mistaken idea concerning the purpose of psychological science. Professor Watson, for example, asserts that the purpose of psychology is to predict the behavior of a person under definite circumstances.<sup>2</sup> These circumstances are of course described by the behaviorist in terms of stimuli, since all behavior is a matter of response to stimuli. The purpose of psychology cannot be more unsatisfactorily stated, since the context makes it clear that Professor Watson entertains an ideal of prediction employed in physical science, and that means to reduce behavior to empty abstractions. The logical consequence of making prediction the end of psychology is to formalize and distort the meaning of responses in the way that a physicochemical statement would do. Such concepts of responses are obviously not derived from concrete facts of experience. The only prediction possible in human behavior is the very simple anticipation of a possible uniformity in action in response to phases of experience consciously abstracted from a total situation. Nothing further than this is possible, since human actions are indefinitely more than mere muscular and glandular function. The other factors of human behavior make such actions entirely unpredictable in the sense that physical phenomena are predictable. A critical reading of Professor Watson's writings on this point indicates that he really has little confidence in such prediction, and that his concepts of stimulus and response are not nearly so empty of content as he implies. In spite of this fact, however, he permits the ideal of prediction to distort his descriptions of human behavior.

Progress in psychology depends upon the correction of two

<sup>1</sup> Jelliffe, *Journal of Philosophy*, 1913, 10, p. 269.

<sup>2</sup> *PSYCHOL. REV.*, 1917, 24, p. 337.

conditions. In the first place, psychology must give up the attempt to describe its facts in terms of abstruse, logical abstractions. Its descriptions should be made in terms of what actually does occur, and not in terms which fail to render any exact detail of the identity and significance of conscious behavior. This is the fact with respect to 'mental states,' and 'stimulus-response' descriptions. The correction of this condition will automatically emend the second condition, namely, that so much pressure is put upon psychology, that it must necessarily trespass too far upon the premises of related sciences.<sup>1</sup> Why should it be necessary for psychology bodily to borrow physiological facts in order to have any positive materials? That this has been a 'protective device' for psychology is exemplified by the large place the sensations occupy in psychological text-books and treatises. The so-called higher functions are just beginning to receive the attention they really require. The backward state of the psychology of thought is easily traceable to the prejudice which gave thought a purely sensorial setting, or an exclusive, sensorially derivative origin. To illustrate some specific improvements in the description of conscious behavior, we might point out some of the factors which are almost entirely excluded from the average descriptions of memory processes.

We may begin with the matter of personal identity. In every memory situation there is a specificity of intimate details, which indicate a personally continuous experience. The possibility of memory implies a continuity of empirical facts, which are all centered about an individual. One might put this in another way, and say that the group of unified human experiences constitute the individual, so far as the memorial processes go. When I remember to pay a bill I owe, I merely complete a relation, which I began at some previous time. There is a continuous series of conditions of which any specific factor is an interrelated part. These parts of the experience are all concrete, human happenings connected by more or less traceable bonds. The contraction of the debt is part of the experience of wearing

<sup>1</sup> Cf. Angell, *PSYCHOL. REV.*, 1913, 20, p. 268.

my new coat, which experience is very closely related to paying the debt. The emphasis of personal identity in the memorial situation would give us a new attitude toward that mental function, and eliminate some of the artificial difficulties. We could not consider memory as a mysterious revival of the dead past; we would not ask, "where is the idea when it is not in consciousness?" The facts of memorial retention concern the objects and events of an individual life, and since the particulars of one's experience vary in character and importance, there is a resulting competition for the occupation of the center of the experiential stage. Forgetting is the natural relegation of given particulars to the wings of this stage, while forgotten but recoverable facts are particulars of experience pushed off the stage, but still in the theater. Totally forgotten objects and events are disconnected particulars split off from the total organization of a person's experience. When any item is in any way an influence upon behavior, it is still a part of a simple experience. This influence goes on in spite of the usually successful evasion of introspective detection. These slightly functional particulars, when inadequately observed, are described as vestiges and traces of memory. As against this view we must look upon these factors of memory as ordinary, concrete events, which occur in everyday experience. In order to describe the concrete workings of an actual memory function the concepts used must be invariably derived from the actual phenomena observed. The logic of science dictates a humanistic description of human behavior; to use purely objective terms results in a falsification of the facts.<sup>1</sup>

A genuine, functional attitude would cast some light upon the problem of belief in memories. James makes memory consist of an object of any faculty, such as perception or reasoning, to which adheres the emotion of belief.<sup>2</sup> The discussion amounts to this, that any memory object bears a peculiar, active relation to our present sensations and emo-

<sup>1</sup> "One suspects that the thrills of young love when so portrayed will present a somewhat clinical not to say mortuary appearance." (Angell, *PSYCHOL. REV.*, 1913, 20, p. 267.)

<sup>2</sup> 'Principles,' ch. xvi., p. 652.

tions. To go only this far is to observe the close connection between the several experiences which are included in a total memory act. The determination of what actually goes on is now cast aside, and there is undertaken the dogmatic substitution of abstractions, such as ideas, which explain nothing. Instead of carrying out the empirical continuity of different phases of a span of human experience, psychologists usually attempt to show a connection between ideas. James quotes Mill as describing the complexity of memory, which includes the idea of himself at the moment of remembering, and that of himself in the past moment of conceiving, and also containing the whole series of *states of consciousness* between those two moments. To the writer this relic of mental chemistry carries no significance, since he cannot conceive of anyone being an idea. A critical examination of the memory process should give us an entirely different description. My confidence in my memory is not explained by the fact that it is an idea *containing* other ideas. It is explained by the fact that what I remember is a part of my experience, and is brought to mind by some related object or condition of my total conscious behavior. The reality of memory is undoubted, because it is a part of an organized experience, and makes its own appeal. In this connection it must not be forgotten that there are degrees in which one experiences events. We may be absent from places, though we are present, and this fact conditions succeeding experiences.

A critical determination of the memory processes will banish those chemical relics, the ideas, from the description of memory; or there will be made a new evaluation of what is meant by an idea. It seems entirely impossible to explain memory as a meaningless mechanics, consisting of series of ideas, logically or illogically chasing themselves through the mind. To refer to James again as a leader in the direction of a functional psychology, we find him insisting that association refers to things thought of and not ideas.<sup>1</sup> There seems to be an appreciation that human experiences have in them more content than ideas suggest. With James this

<sup>1</sup> 'Psychology,' I, p. 554.



happened to be one of those flashes of genius peculiar to him which is not further developed. He correlates these things with nerve paths, and if he does at all avoid the empty dance of idea-atoms, he substitutes what is just as bad, a neural mechanics. When we consider the function of ideas in a memory process we must immediately freight those ideas heavily with human content. It is highly improbable that there should be a human, memory function unless the ideas concerned were carrying actual, human conditions. The fact of a man remembering to post his wife's letters is not explained by the contiguity of ideas, but involves a series of concrete happenings, which form an organic whole. Unfortunately, too frequently there is a dissociation of these happenings, which may be explained by the fact that the individual events group themselves in other combinations.

The peculiar mystery of the recall situation is resolved when we give up the abstract, logical entities. When a memory process is operating, some person is making use of a larger part of his experience than is contained in a given moment. This is not an abstract dragging in from nowhere of an idea by another idea, by virtue of the fact that both were in the mind before. When one performs today what he promised in a past time, it is perhaps because the individual, who wishes to have others think well of him, agrees to do something, and finds it naturally expedient to do it at the appointed time. The element of social pressure is extremely important. The recall function is a matter of actual arrangement of an individual's experiences under influence of definite, human conditions. To describe this experience as a connection between idea 'a,' and idea 'b' is to fail entirely to comprehend the event. To say that ideas connect themselves because of previous connections, is to create a mystery since the need arises for some explanation to account for any particular present association. The only meaningful and useful account of the facts of revival which bring about, or fail to bring about coördinated and coherent human actions must be based upon the actual contact of an individual with things and their relations. Excluding the explanations, we

find in the Freudian literature many suggestions of the specific interplay of experience elements in situations of remembering and forgetting.

What is true of memory is true of all other conscious behavior. In the case of thought processes, an adequate evaluation function must take into account, besides obvious association of ideas, the intimate carriers of these ideas, whether sensational, physiological, or even factors of a more recondite sort. There must be indicated the important place of the so-called subconscious phases of the thought processes. A scientific description of the thought functions should bring out with details some of the specific conditions concerning the problem which calls out the thought behavior. The main point here, is that unless one enumerates all the necessary facts concerning a conscious behavior, the conditions of description are not satisfied. It is especially important to avoid a description of human behavior in purely mental terms. A thought process for example, is not only mental, but also physical, social, and human. It is no more conditioned by the fact that it involves images, bodily attitudes and physical facts, than by the fact that it always has as a setting some specific, problematic situation.

In general, an adequate use of the evaluation function will lead us to determine conscious behavior as it actually is, for the purposes which really guide the investigation, and not for the convenience and comfort which accrue from logical coherence. If it is necessary for the understanding of psychological phenomena to give them their neurological and physiological setting, they must be connected with those facts, but the one set of facts should never be substituted for the other. Such procedure indicates in every instance the clumsy juggling with products of false analysis. The behaviorist, who correctly points out the insufficiency of the doctrine that there is present in human behavior some kind of stuff, which is seriously characterized as unextended, existing for itself, or intangible, and which brings about appropriate responses to stimuli, is just as much at fault in reducing the entire situation to logically simple behavior processes. Any

critical observation indicates that not even the simple adjustments are reducible to the physiological functions, which the extreme behaviorists attempt to make out as the sole constituents of those acts. When the complex behaviors are considered, the insufficiency of the analysis is glaring. To deny the conscious factors found in higher types of behavior, or to reduce them to the crude functioning of a nervous system, is to display unmistakable symptoms of tyroship in the manipulation of the evaluation function.

Psychology, if it is to be a science, must take its function to be that of describing actual facts. These facts are not existing objects or conditions, which await reception; neither are they assumptions imposed upon the experience studied. A fact, for the psychologist, as well as for every scientist, must be the most critical determination of existential conditions, and an evaluation of some phase of genuine experience. It is always a knowledge construct, and is more or less valuable for purposes of control and understanding, as it keeps in contact with actually existing circumstances. One of the first essentials of a scientific attitude is non-sectarianism. This makes for a determination of phenomena on their own merits. They are not strained to conform to a pre-conceived notion of what ought to exist in the domain under discussion. The evaluation function of the scientific level of experience is experience conscious of itself. It should therefore give an accurate account of its development.

# CHROMATIC THRESHOLDS OF SENSATION FROM CENTER TO PERIPHERY OF THE RETINA AND THEIR BEARING ON COLOR THEORY

## PART I.

BY C. E. FERREE AND GERTRUDE RAND

*Bryn Mawr College*

### INTRODUCTION

In the work reported in this paper a determination of the chromatic thresholds of red, green, blue and yellow in energy terms has been made at near-lying points from the center to the periphery of the retina. The incentive for making this study has been twofold: (1) We have wanted to make an investigation of the chromatic sensitivities of the central and peripheral retina that would be more nearly quantitative than those that have previously been attempted. (2) A detailed investigation of the sensitivity gradient for the four colors red, green, blue, and yellow from center to periphery of the retina has an important bearing on certain points of color theory. Two of these points will be considered in the second part of this paper.

As a part of a general investigation of retinal sensitivities, we had planned several years ago (1) to make determinations both of the achromatic and chromatic sensitivities to wave-length that would be quantitative up to the standard accepted for the physical recording instruments. One of the requirements for such a determination is, as was pointed out at that time, that the stimuli shall be rated in units that can be compared. This requirement could not be met until measuring instruments were obtained which were sufficiently sensitive for work in the visible spectrum and which were non-selective in their response to wave-length. It was further stated that if a rating is to be made which may fairly be considered as quantitative, it must also be possible from the

data at hand to compare numerically the amounts of response as well as the amounts of stimuli used to arouse the response. That is, while a radiometric rating of the stimuli is necessary for this purpose, an equally important point to be considered is what amounts of response can be employed with sureness of principle in meeting the quantitative requirement. In this regard it was pointed out that of the amounts of response that have at different times been used or suggested for the determination of sensitivity—namely, equal amounts, equal sense differences, the liminal threshold, the just noticeable difference, and the average error—perhaps only the first two can by common agreement be regarded as numerically comparable; and that the validity of the use of the others for the more strictly quantitative work should be tested by checking the results against those obtained when the rating is based, for example, on equal amounts taken as standard. This interchecking of results for achromatic sensitivity is now in progress in our laboratory, in which case all of the determinations mentioned above may be made. It can not be done, however, in case of chromatic sensitivity until it is first determined whether the judgment of equal saturations can be made with an acceptable degree of precision. Furthermore, a determination of comparative sensitivities in the peripheral retina, based on equal amounts of response, while not impossible, is neither very convenient nor very feasible. For the purpose of the present paper, therefore, we have been content to deal with the determination of the chromatic threshold for the wave-lengths in question and of their variation from the center to the periphery of the retina, which work has an interest of its own independent of its bearing on a determination of comparative sensitivities, and to reserve a report on the more strictly quantitative features of the general problem for a later paper. In making these determinations it was our intention to work at nearly lying points from the center to the periphery of the retina in several meridians. The work was interrupted, however, by the pressure of other investigations when the determinations had been made in only two meridians, the temporal and the

nasal. Results can be given at this time, therefore, for only these two meridians.

### CONDITIONS UNDER WHICH THE WORK WAS DONE

The determinations were made under the following conditions. (1) The colored lights used were taken from the spectrum. There are two reasons for this in an investigation of the kind here undertaken. (a) The stimuli should be as homogeneous with regard to the visible wave-lengths as possible,<sup>1</sup> and (b) they should be free from the infra-red and ultra-violet radiations which would affect the thermopile used to measure the intensity of light, but not the eye. The stimuli employed were a narrow band of red in the region of  $670\ \mu\mu$ ; of yellow in the region of  $581\ \mu\mu$ ; of green in the region of  $522\ \mu\mu$ ; and of blue in the region of  $468\ \mu\mu$ . The breadth of analyzing slit used in isolating these bands was maintained constant at 0.5 mm. The range of wave-lengths obtained was approximately  $660\text{--}680\ \mu\mu$ ;  $575\text{--}587\ \mu\mu$ ;  $518\text{--}526\ \mu\mu$ ; and  $468\text{--}474\ \mu\mu$ . The spectrum was gotten and the different wave-lengths were presented to the eye by means of the apparatus described in the *Journal of Experimental Psychology*, 1916, 1, pp. 247-284: 'A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral.' In every case the light was examined for impurities at the analyzing slit by means of a small Hilger direct vision spectroscope provided with an illuminated scale. When found, impurities were absorbed out by thin gelatines selected so as to cut out as little of the useful light as pos-

<sup>1</sup> The presence of the alien visible wave-lengths affects the results of a determination of chromatic sensitivity in two ways: (a) through physiological inhibitions and interactions it decreases the amount of the color response, and (b) it increases the energy measurement. In their work on the determination of the visibility of radiation in the red end of the visible spectrum, Hyde and Forsythe (*Astrophysical Journal*, 1915, 44, p. 289) found impurities in the prismatic spectrum to the value of about 20 per cent., at  $0.76\ \mu$ . In our own work both on achromatic and chromatic sensitivity determinations made with and without provisions for absorbing the scattered light, show differences in result which are great enough to be considered of significance. This is true in particular for determinations of chromatic sensitivity, in which case the chromatic response may be reduced quite appreciably as a result of the physiological interactions produced by the alien wave-lengths. In some cases, for example, even the complementary wave-lengths may be present.

sible. These gelatines were placed over the analyzing slit and were held in position by short clips fastened to the front surface of the jaws the edges of which formed the slit.

(2) The determinations of the threshold were made in energy terms. Measurements were made at two places: at the analyzing slit and at the eye. In making the threshold determinations of the stimulus light it was found to be convenient first to make the colors all equal in energy value. The reductions needed for the equalization were made by appropriate adjustments of the collimator slit. Since the blue represents the smallest amount of energy of any of the colors employed, they were all made equal in energy to the blue of the spectrum used, namely, the prismatic spectrum of a Nernst filament operated by 0.6 ampere of current. From this intensity they were reduced to the threshold by means of the especially constructed sectored discs described in an earlier paper,<sup>1</sup> and the energy values computed from the simple law of the disc. These discs, it will be remembered, were cut from hard sheet aluminum, No. 20 B. and S. gauge, 0.9 mm. thick, and of two sizes for just noticeable difference determinations, 19.5 and 17 cm. in radius. The total variation of range of open aperture is from 0° to 348.75°. A strong objection to the use of sectored discs when fine changes are needed such as are required, for example, in threshold and just noticeable difference work, is the difficulty of obtaining and measuring accurately sufficiently small amounts of change. Such discs are ordinarily constructed with two or more open sectors and a change in one is multiplied as many times as there are open sectors. Moreover, an error made in the measurement of one sector is multiplied by the number of open sectors. This latter difficulty becomes especially significant in working with intensities at or near the threshold, where a small error may represent a high percentage of the total open sector. We have sought to overcome these difficulties in three ways. (1) Our discs for a low total aperture are so constructed that one sector may be varied at a time. (2) The sector is moved by

<sup>1</sup> *Journal of Experimental Psychology*, 1916, 1, pp. 271-274.

means of a micrometer screw. This device for minute changes in the value of the open sector is so constructed as to be readily attached and removed from the disc. And (3) a special protractor has been designed fitted with a movable arm carrying a knife edge and Vernier scale graduated to read to minutes. Obviously some such precise means of making and measuring small changes in the disc are of prime importance in the work of determining the threshold and just noticeable difference. If such means are not at hand the average error of setting and measurement is apt to exceed that of the sense judgment.

The method of making the energy measurements by means of a thermopile has already been described in a previous paper. However, because the procedure is as yet somewhat unfamiliar it may not be out of place to give again brief description of how the measurements are made. A description at one of the places at which they were made, namely the analyzing slit, will be sufficient to show in a general way the method we have employed. The thermopile to be used was placed in position immediately behind the slit and a blackened aluminum shutter was interposed in the path of the beam of light between the slit and the end of the objective tube of the spectroscope. Preliminary to the exposure of the thermopile to the light to be measured, the current sensitivity of the galvanometer was tested by means of a special device<sup>1</sup> provided for this purpose in the construction of the galvanometer. With regard to this procedure it may be pointed out that the current sensitivity of the galvanometer varies with the period or time of the single swing of its needle system. Since it is not possible to control the field so as to get this period always the same, it is necessary, if results are to be compared, to take some sensitivity as standard and to convert all readings into deflections for the standard sensitivity by means of a correction factor determined at each sitting. For a detailed description of the method of de-

<sup>1</sup> This device consists of a special galvanometer coil, dry battery circuit, and switch board with finely graduated resistance. For a description of this device, see 'Radiometric Apparatus for Use in Psychological and Physiological Optics,' *PSYCHOL. REV. MONOG.*, 1917, 24, No. 2, pp. 63-65.



termining this factor, see PSYCHOL. REV. MONOG., 1917, 24, No. 2, pp. 60-65.

The thermopile was next connected with the galvanometer and the light allowed to fall on its receiving surface until a temperature equilibrium was reached (ca. 3 sec. for our thermopile). The deflections were read by means of the telescope and scale and the readings are corrected to standard sensitivity by means of the factor previously determined. The final step in the process of measuring was the calibration of the apparatus, *i. e.*, the value of 1 mm. of deflection in radiometric units was determined for the area of thermopile exposed. To do this a radiation standard, the value of the radiations from which is already known, had to be employed. The standard used by us was a carbon lamp specially seasoned and prepared for the purpose by W. W. Coblentz (4) of the radiometric division of the Bureau of Standards. This lamp was placed on a photometer bar 2 meters from the thermopile and operated at one of the intensities for which the calibration was made, in our case 0.40 ampere. The thermopile was exposed to its radiations with the same area of receiving surface as was used in case of the lights measured, and the galvanometer deflection was recorded. From the deflections obtained the value of 1 mm. of deflection, or the radiation sensitivity of the apparatus under the conditions given, was computed from the known amount falling on the surface of the thermopile. Having the factor expressing the radiation sensitivity of the apparatus, the deflections produced by the wave-lengths of light measured were readily converted into energy units. The radiation sensitivity of the linear thermopile used by us was computed in a given case, for example, from the following data. The energy value of the radiations per sq. mm. at a distance of 2 m. from the standard lamp operated by 0.40 ampere was  $90.70 \times 10^{-8}$  watt. The deflections of the galvanometer produced by this intensity of radiation falling on the same area of receiving surface as was used in measuring the lights employed as stimuli, when corrected (*a*) to a sensitivity of  $i = 1 \times 10^{-10}$  ampere, and (*b*) for the absorption of the glass cover of the thermo-

pile, was 346.870 mm. The area of the surface exposed was 4.400 sq. mm., and the time of exposure was 3 sec. The sensitivity of the instrument per sq. mm. of receiving surface was, therefore,  $115 \times 10^{-10}$  watt. By means of this factor the galvanometer readings produced by the different wavelengths of light may readily be converted into the energy value of light falling on the receiving surface of the thermopile.

(3) The field surrounding the stimulus, and the preexposure were always maintained as nearly as possible at the same brightness as the stimulus at the threshold value of sensation. For want of better pigment materials these surfaces were made from the Hering standard gray papers. It was found to be necessary to change the brightness of the surrounding field and preexposure frequently for each stimulus because the brightness value of the color at the chromatic threshold changed quite rapidly from the center to the periphery of the retina. There were two causes for this change. (a) The intensity of the light had to be increased quite a great deal from center to periphery to give the chromatic threshold from point to point; and (b) the achromatic value of the colors does not remain the same from the center to the periphery of the retina. The gray that matched the stimulus in achromatic value at each point was determined by the equality of brightness method.<sup>1</sup> With reference to these determinations, it may be said that with the stimulus reduced to the chromatic threshold the color difference between the stimulus and the gray was so small that the equality of brightness judgment was not difficult to make. The match was made in every case for the part of the retina under investigation. It had to be attained by a series of approximations. That is, the threshold was first obtained at the given point with no especial control of brightness of preexposure and surrounding

<sup>1</sup> The Hering papers did not present a sufficiently wide range of reflection coefficients to match the brightness range of the threshold value of the stimulus light from the center to the periphery of the retina. For example, at the threshold of sensation at the center of the retina, the darkest of the Hering papers illuminated by the rather strong light of the room was brighter than the stimulus light; and in the far periphery of the retina through a zone varying in breadth from 2 to 13 degrees for the different colors, the lightest of this series of papers was darker than the stimulus color.

field. The brightness of preëxposure and surrounding field was then made to match this value of the stimulus and the threshold was redetermined. This procedure was repeated until a threshold was obtained that required no further change in these surfaces to match it in brightness. In order to make the specification of the brightness of the preëxposure and the surrounding field independent of the illumination of the room and of the variability of the reflection coefficients of different issues of the Hering papers, the brightness was in each case determined in candlepower per sq. in. This determination was made by means of a Sharp-Millar portable photometer with the test plate removed. The instrument was calibrated against a magnesium oxide surface obtained by depositing the oxide from the burning metal. By this method the reflecting surfaces were used as detached test plates. The readings were converted into candlepower per sq. in. by the following formula: Brightness =  $\frac{\text{foot candles}}{\pi \times 144}$ .<sup>1</sup>

For the sake of reproducibility of conditions from time to time in a given laboratory or in different laboratories, it is obvious that a photometric specification should be given in all cases not only of the general illumination of the room but of the brightness of all surfaces for which precision of control is of importance to the results of the work.

(4) The illumination of the room was kept at a constant value. Two features are necessary for this control. (a) A means must be had of detecting small changes of illumination. This may be accomplished by a portable photometer of the Sharp-Millar or Macbeth type, for example, furnished with a daylight screen, or of the simpler type described by the writers in a previous article (5). And (b) a means must be had also of producing small variations in the illumination of the room, else the changes due to fluctuations in the external light can not be compensated for with the precision and minuteness of control that is needed. This is accomplished in our optics room by two systems of thin white curtains running

<sup>1</sup> By multiplying these values in turn by 486.8 they may be converted into millilamberts, a term frequently used by engineers to specify small brightness quantities.

on spring rollers beneath the skylight. One of the systems of white curtains and the light-proof curtain run lengthwise of the room; the other system of white curtains runs across the room. By means of the white curtains either small local or small general changes can be produced in the illumination of the room; and by means of the light-proof curtain larger changes may be produced ranging from full illumination to the darkness of a moderately good dark room. The light-proof curtain is of a breadth equal to that of the room and runs in a deep light-tight boxing. The white curtains are narrower and are made to overlap at the edges. These curtains run on wire guides so distributed as to prevent any sagging or wrinkling. Above these curtains are pivoted two large diffusion sashes of glass ground on one side completely filling the skylight opening. These sashes diffuse the light in the room giving an even distribution of illumination and rendering, because of that fact, an even and precise control easier to accomplish. In a careful specification of the conditions under which the work is done a very important item is to give a photometric specification of the illumination of the room. This may be done in foot or meter-candles as desired. If the illumination is uneven it should be done systematically throughout the room. If, on the other hand, it is pretty uniform, it is usually sufficient to give its value in three or more directions at the point of work. In case of the present work, for example, the value of the horizontal component was 30.49 foot candles; the vertical component, 121.95 foot candles; and the 45 degree component, 82.97 foot candles.

(5) The amount of light entering the eye was made independent of variations in the size of the pupil. Independence of change in size of pupil was especially needed in this work because of the large variations in the intensity of light used. Such control is very easy to accomplish with the means of presenting the light to the eye that is used in our apparatus. All that is needed is to keep the image that falls on the pupil of a constant size and smaller than the pupil throughout its entire range of variations in the given series of experiments. Not only can this variation be determined in preliminary

experiments as a guide to the size of the image that is needed, but the image itself can be compared with the pupil at each observation. For details of the method of exercising this control see 'A Substitute for an Artificial Pupil,' *PSYCHOL. REV.*, 1916, 23, 380-383.<sup>1</sup>

## RESULTS

A statement of the results of the investigation is given in Tables I.-VIII. The nearness of the points investigated to each other was determined by the rapidity with which the sensitivity decreased in the meridian in question. In those regions in which the decrease was gradual the determinations were made at points separated by as much as 5 degrees. In regions, however, where the rate of decrease was rapid or unusual features were present, the determinations were made at points separated only by 1 degree. A graphic representation of the results of these tables is given in Charts I.-IV. In these charts degree of eccentricity is plotted along the abscissa and the value of the threshold in watts ( $10^7$  ergs per sec.) is plotted along the ordinate. In Charts I. and II. the values of the threshold from the center of the retina to the limits of sensitivity are plotted. In case of the red, yellow and blue, it will be remembered from statements made in former papers that the limits of sensitivity for lights of high intensity coincide with the limits of the field of white light vision. This, however, was not the case for the green stimulus. By no increase of intensity were we able to make the limits of green sensitivity coincide with the limits of white light vision. In Charts III. and IV. the above values from the center of the retina through the region of gradual decrease of sensitivity are plotted on a larger scale. This is done because when plotted on the scale used in Charts I. and

<sup>1</sup> Since the article referred to above was published we have devised and constructed a very convenient attachment for our analyzing slit by means of which the length of aperture of the slit may be varied by half millimeter steps. Space will not be taken here for a detailed description of this device. In brief, it consists of two knife edged jaws moving in the vertical, operated by means of a ratchet and spring. Still finer control could be secured, of course, by means of a micrometer screw. Constructed in this latter form the device would be very serviceable as a means of producing finely graded changes of intensity.

II. the curves fall so closely together that the relative sensitivities to the four colors are not clearly represented. That is, the range of the values for the threshold from the center to the extreme periphery of the retina is so great that in Charts I. and II., in which the entire range is represented, a scale value had to be chosen which is so large as almost to obscure the smaller differences in relative sensitivity to the different colors in the region of gradual decrease in sensitivity.

TABLE I

## CHROMATIC THRESHOLDS FOR RED, NASAL MERIDIAN

In this table are given the values of the threshold for red ( $670 \mu\mu$ ) at 22 points in the nasal meridian. The intensity of light at the analyzing slit was  $94.27 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure. (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye ( $\text{Watt} \times 10^{-12}$ )	Density of Light at Campimeter Opening ( $\text{Watt} \times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye ( $\text{Watt} \times 10^{-12}$ per Sq. Mm.)
0	0.000764	0.375	30.80	0.174	9.34
5	0.000764	0.375	30.80	0.174	9.34
10	0.000764	0.438	36.00	0.203	10.90
14-17		Blind Spot			
20	0.000764	0.75	61.70	0.349	18.68
25	0.002125	1.00	82.20	0.465	24.90
30	0.003744	2.50	205.50	1.163	62.25
35	0.005088	3.75	308.30	1.744	93.38
40	0.005088	4.00	328.80	1.860	99.60
45	0.005210	4.50	369.90	2.093	112.05
50	0.005210	5.00	411.00	2.325	124.50
55	0.005902	6.00	493.20	2.790	149.40
60	0.006268	8.00	657.60	3.720	199.20
65	0.006838	12.25	1007.00	5.696	305.00
70	0.007408	20.00	1644.00	9.300	498.00
75	0.01140	23.00	1890.60	10.695	572.70
80	0.01262	26.00	2137.20	12.090	647.40
82	0.01587	34.00	2794.80	15.810	846.60
85	0.02116	50.00	4110.00	23.250	1245.00
87	0.02686	114.00	9370.80	53.010	2838.60
88	0.03093	180.00	14796.00	83.700	4482.00
90	0.05088	270.00	22194.00	125.550	6723.00
92	0.05088	338.00	27783.60	157.170	8416.20

In this and the following tables the light was taken from the spectrum of a Nernst filament operated by 0.6 ampere of current. In all cases the pre-exposure and surrounding field were made as nearly as possible the same brightness as the stimulus.

With reference to the results given in Tables I.-VIII., the following points may be noted. (1) The characteristics of response of the eyes for which results are given have been

very widely investigated for both central and peripheral vision. They have been chosen for this and other work especially because of their normality and practised precision of behavior. Their spectrum luminosity curve, for example, agrees (6) very closely with the average curve obtained by

TABLE II

## CHROMATIC THRESHOLDS FOR RED, TEMPORAL MERIDIAN

In this table are given the values of the threshold for red ( $670\text{ }\mu\mu$ ) at 25 points in the temporal meridian. The intensity of light at the analyzing slit was  $94.27 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure. (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye (Watt $\times 10^{-12}$ )	Density of Light at Campimeter Opening (Watt $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)
0	0.000764	0.375	30.80	0.174	9.34
5	0.000764	0.375	30.80	0.174	9.34
10	0.000764	0.438	36.00	0.203	10.90
15	0.000764	0.75	61.70	0.349	18.68
20	0.002125	1.00	82.20	0.465	24.90
25	0.003867	2.75	226.10	1.279	68.48
30	0.005902	8.00	657.60	3.720	199.20
33	0.006431	14.00	1150.80	6.510	348.60
35	0.007408	17.00	1397.40	7.905	423.30
40	0.01404	29.00	2383.80	13.485	722.10
44	0.01791	38.00	3123.60	17.670	946.20
45	0.02686	104.00	8548.80	48.360	2589.60
46	0.03093	160.00	13152.00	74.400	3984.00
47	0.03663	216.00	17755.20	100.440	5378.40
48	0.03663	216.00	17755.20	100.440	5378.40
49	0.03663	216.00	17755.20	100.440	5378.40
50	0.03663	216.00	17755.20	100.440	5378.40
51	0.03663	216.00	17755.20	100.440	5378.40
52	0.03663	216.00	17755.20	100.440	5378.40
53	0.03663	216.00	17755.20	100.440	5378.40
54	0.03663	216.00	17755.20	100.440	5378.40
55	0.03663	245.00	20139.00	113.925	6100.50
58	0.05088	270.00	22194.00	125.550	6723.00
60	0.05088	300.00	24660.00	139.500	7470.00
61	0.05088	315.00	25893.00	146.475	7843.50

Nutting (7) for 18 observers. Also their normality of chromatic response has, at various times, been checked up by a number of observers. Data on this point may be found in nearly all of the work that has been published from this laboratory. In case of the present work the systematic point by point determination has been made only upon the one

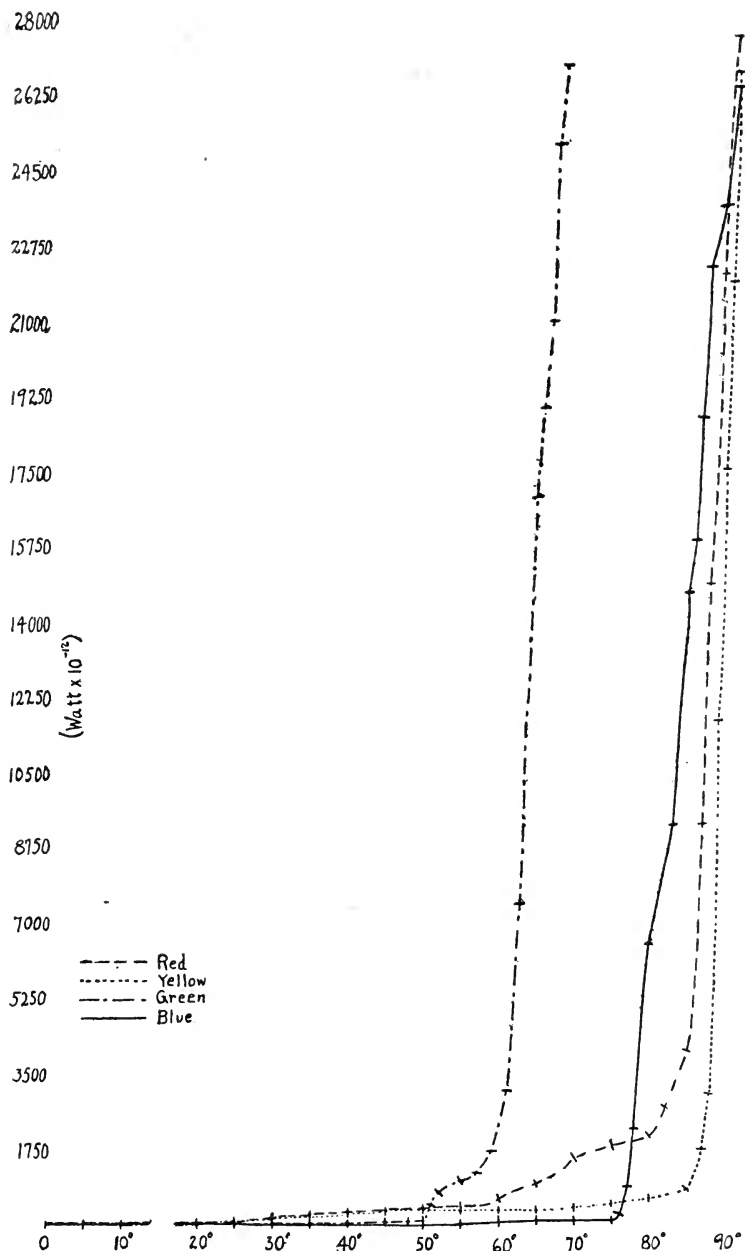


CHART I. Chromatic Thresholds for the Four Colors, Nasal Meridian. In this chart and Chart II., degree of excentricity in the field of vision is plotted along the abscissa and the value of the threshold along the ordinate. Of the three values of the threshold given in the tables, the total amount of light at the campimeter opening and at the eye is represented.



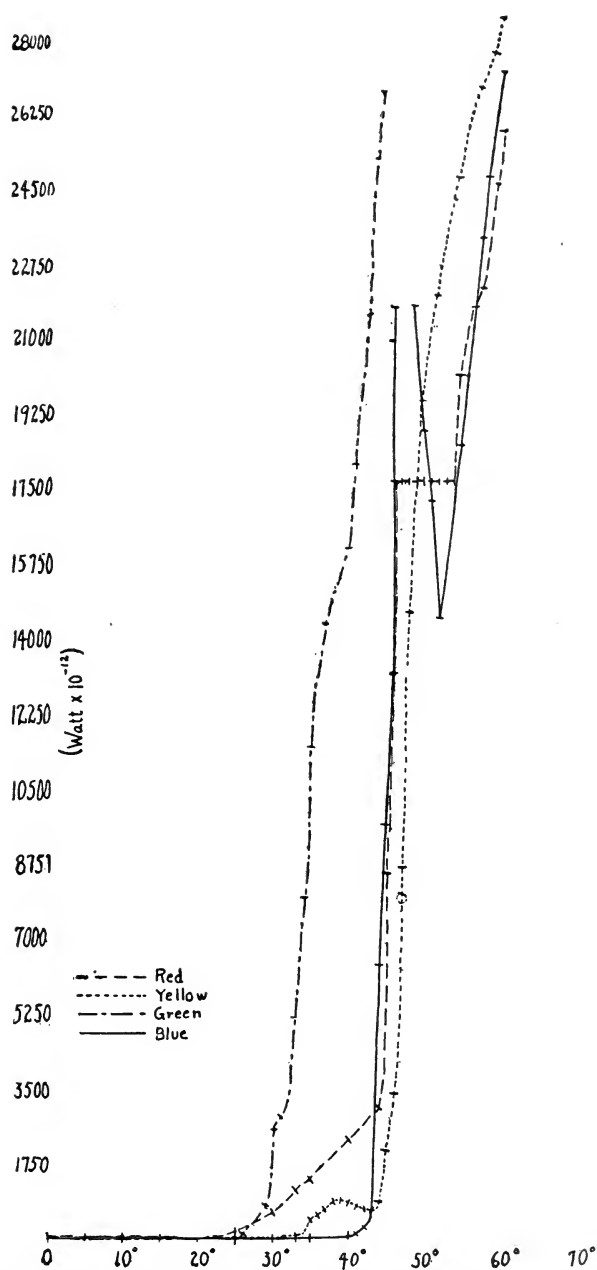


CHART II. Chromatic Thresholds for the Four Colors, Temporal Meridian.

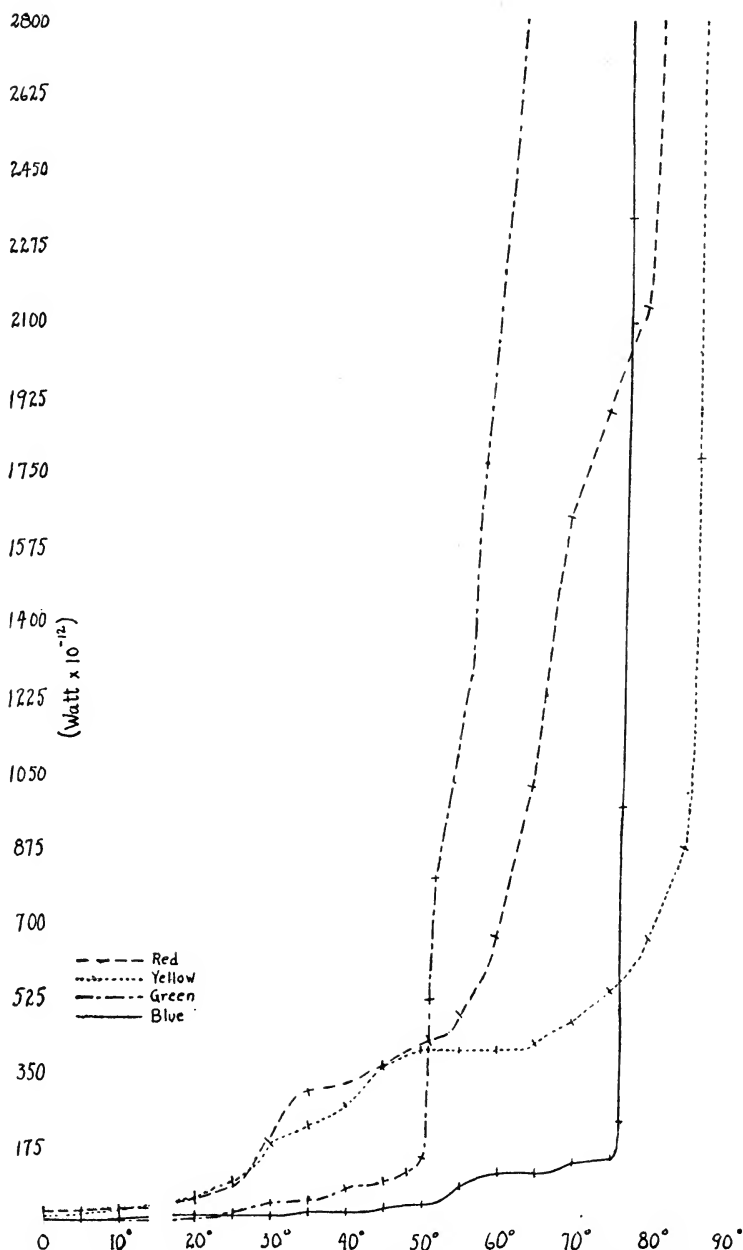


CHART III. Chromatic Thresholds for the Four Colors, Nasal Meridian. In this chart and Chart IV., the values represented in Charts I. and II. respectively, from the center of the retina through the region of gradual decrease of sensitivity are plotted on a larger scale. This is done because when plotted on the scale used in Charts I. and II., the curves fall so closely together that the relative sensitivities are not clearly represented.

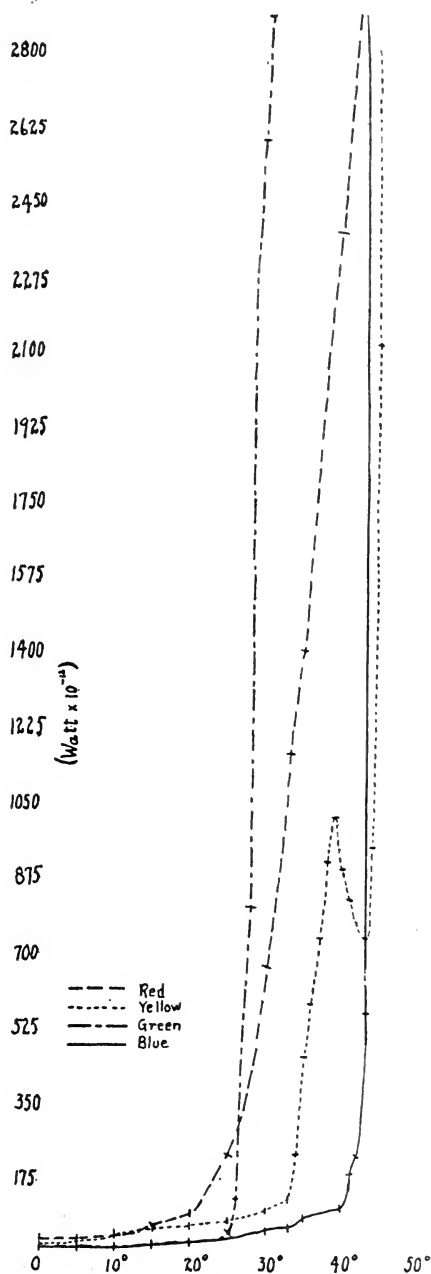


CHART IV. Chromatic Thresholds (enlarged scale) for the Four Colors, Temporal Meridian.

observer. However, one or more check observers have been used on points of the work of especial importance to theory, such as the irregular distribution of sensitivity to the pairs of colors, the criss-crossing or interlacing of limits, the areas analogous in type to the Schumann case of color-blindness, the deficiency of sensitivity of the far periphery of the retina to green, and the inability to get a red or green that is stable in color tone for all parts of the retina.

TABLE III

## CHROMATIC THRESHOLD FOR YELLOW, NASAL MERIDIAN

In this table are given the values of the threshold for yellow ( $581 \mu\mu$ ) at 23 points in the nasal meridian. The intensity of light at the analyzing slit was  $93.10 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye (Watt $\times 10^{-12}$ )	Density of Light at Campimeter Opening (Watts $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)
0	0.002125	0.20	16.30	0.092	4.94
5	0.002125	0.25	20.40	0.115	6.175
10	0.002125	0.375	30.56	0.172	9.263
14-17		Blind Spot			
20	0.003378	0.75	61.13	0.346	18.525
25	0.003867	1.25	101.88	0.576	30.875
30	0.006431	2.375	193.56	1.094	58.663
35	0.006919	2.875	234.31	1.325	71.013
40	0.006956	3.50	285.25	1.614	86.450
45	0.007326	4.50	366.75	2.074	111.150
50	0.007408	4.875	397.31	2.247	120.413
55	0.007408	4.875	397.31	2.247	120.413
60	0.007977	5.00	407.50	2.305	123.500
65	0.008547	5.125	417.69	2.363	126.588
70	0.009768	5.75	468.63	2.651	142.025
75	0.01140	6.75	550.13	3.112	166.725
80	0.01384	8.00	652.00	3.688	197.600
85	0.01404	10.75	876.13	4.956	265.525
87	0.01587	22.00	1793.00	10.142	543.400
88	0.02523	38.00	3097.00	17.518	938.600
89	0.03093	144.00	11736.00	66.384	3556.800
90	0.05088	216.00	17604.00	99.576	5335.200
91	0.05088	270.00	22005.00	124.47	6669.000
92	0.05088	330.00	26895.00	152.130	8151.000

(2) Up to 15-20 degrees the accuracy of the threshold values is doubtless lessened by the difficulty of making and reading the adjustments for such small open sectors, even with our micrometer device for making the adjustment and the

Vernier scale for reading it. On this account it may well be that the threshold at the center is somewhat high for all of the colors, inasmuch as open sectors of less than one-tenth of a degree are rather infeasible to use. For the exact deter-

TABLE IV

## CHROMATIC THRESHOLDS FOR YELLOW, TEMPORAL MERIDIAN

In this table are given the values of the threshold for yellow ( $581 \mu\mu$ ) at 29 points in the temporal meridian. The intensity of light at the analyzing slit was  $93.10 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure. (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye (Watt $\times 10^{-12}$ )	Density of Light at Campimeter Opening (Watt $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)
0	0.002125	0.20	16.30	0.092	4.94
5	0.002125	0.25	20.38	0.115	6.175
10	0.003175	0.50	40.75	0.231	12.350
15	0.003378	0.625	50.94	0.288	15.438
20	0.003456	0.75	61.13	0.346	18.525
25	0.003867	0.875	71.31	0.403	21.613
30	0.005210	1.125	91.69	0.519	27.788
33	0.005779	1.50	122.25	0.692	37.050
34	0.007326	2.75	224.13	1.268	67.925
35	0.009768	4.50	366.75	2.075	111.150
36	0.01140	7.00	570.50	3.227	172.900
37	0.01262	9.00	733.50	4.149	222.300
38	0.01262	11.25	916.88	5.186	277.875
39	0.01262	12.50	1018.75	5.763	308.750
40	0.01262	11.00	896.50	5.071	271.700
41	0.01262	10.00	815.00	4.610	247.000
42	0.01262	9.50	774.25	4.380	234.650
43	0.01262	9.00	733.50	4.149	222.300
44	0.01262	11.50	937.25	5.302	284.050
45	0.01404	26.00	2119.00	11.986	642.200
46	0.01791	42.00	3423.00	19.362	1037.400
47	0.02686	106.00	8639.00	48.866	2618.200
48	0.05088	180.00	14670.00	82.980	4446.000
50	0.05088	240.00	19560.00	110.640	5928.000
52	0.05088	270.00	22005.00	124.470	6669.000
55	0.05088	304.00	24776.00	140.144	7508.800
58	0.05088	330.00	26895.00	152.130	8151.000
60	0.05088	340.00	27710.00	156.740	8398.000
61	0.05088	350.00	28525.00	161.350	8645.000

mination of the absolute values of the threshold in this part of the retina, the discs can be used with more accuracy when a substantial reduction is first made by means of some other device. In considering the absolute values given above, the state of general adaptation of the eye must also be kept in

mind. The horizontal component of illumination at the point of work was, it will be remembered, 30.49 foot-candles; the vertical component, 121.95 foot-candles; and the 45 degree component, 82.97 foot-candles.

TABLE V

## CHROMATIC THRESHOLDS FOR GREEN, NASAL MERIDIAN

In this table are given the values of the threshold for green ( $522 \mu$ ) at 23 points in the nasal meridian. The intensity of light at the analyzing slit was  $91.50 \times 10^{-8}$  watt.

Degree of Excentricity.	Surrounding Field and Pre-exposure (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye (Watt $\times 10^{-12}$ )	Density of Light at Campimeter Opening (Watt $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)
0	0.002125	0.10	8.00	0.045	2.425
5	0.002125	0.10	8.00	0.045	2.425
10	0.002125	0.125	10.00	0.057	3.031
14-17		Blind Spot			
20	0.003175	0.167	13.33	0.076	4.042
25	0.003378	0.375	30.00	0.170	9.094
30	0.003744	0.625	50.00	0.283	15.156
35	0.003867	0.75	60.00	0.340	18.188
40	0.005210	1.00	80.00	0.453	24.250
45	0.005779	1.25	100.00	0.566	30.313
48	0.005983	1.50	120.00	0.680	36.375
50	0.006268	1.875	150.00	0.849	45.468
51	0.006919	6.50	520.00	2.945	157.625
52	0.007408	10.00	800.00	4.530	242.500
55	0.007977	13.50	1080.00	6.116	327.375
57	0.008954	16.00	1280.00	7.248	388.000
59	0.01018	22.50	1770.00	10.193	545.625
61	0.01425	40.00	3200.00	18.120	970.000
63	0.02116	94.00	7520.00	42.582	2279.500
65	0.02686	212.00	16960.00	96.036	5141.000
66	0.03093	238.00	19040.00	107.814	5771.500
67	0.03663	263.00	21040.00	119.139	6377.750
68	0.05088	315.00	25200.00	142.695	7638.750
69	0.05088	338.00	27040.00	153.114	8196.500

(3) The numerical values in Column 1 of these tables represent, of course, points in the field of vision. What the corresponding points on the retina are, could not be determined without knowing the net displacement, if there be such displacement, of the image towards the principal axis of the refracting system as the beam of light enters the eye more and more obliquely. Just what the factors are that might contribute to this displacement in a refracting system so

complex as that of the eye, is somewhat difficult to determine. The following are perhaps worthy of consideration: the difference in the optical density of the media traversed by the incident and the emergent rays, and the greater combined refracting power of the cornea and the anterior surface of the lens than of the posterior surface of the lens. Another

TABLE VI

## CHROMATIC THRESHOLDS FOR GREEN, TEMPORAL MERIDIAN

In this table are given the values of the threshold for green ( $522 \mu\mu$ ) at 19 points in the temporal meridian. The intensity of light at the analyzing slit was  $91.50 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure. (Candle Power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye (Watt $\times 10^{-12}$ )	Density of Light at Campimeter Opening (Watt $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)
0	0.002125	0.10	8.00	0.045	2.425
5	0.002125	0.125	10.00	0.057	3.031
10	0.002125	0.125	10.00	0.057	3.031
15	0.003175	0.167	13.33	0.076	4.042
20	0.003378	0.25	20.00	0.113	6.063
25	0.003456	0.375	30.00	0.170	9.094
26	0.005779	1.50	120.00	0.680	36.375
28	0.007408	10.00	800.00	4.530	242.500
30	0.01018	32.00	2560.00	14.496	776.000
31	0.01140	36.00	2880.00	16.308	873.000
33	0.01384	65.00	5200.00	29.445	1576.250
34	0.02116	100.00	8000.00	45.300	2425.000
35	0.02523	144.00	11520.00	65.232	3497.000
37	0.02686	180.00	14400.00	81.540	4365.000
40	0.02686	201.50	16120.00	91.280	4886.375
41	0.03093	226.00	18080.00	102.378	5480.500
43	0.03663	270.00	21600.00	122.310	6547.500
44	0.05088	305.00	24400.00	138.165	7396.250
45	0.05088	335.00	26800.00	151.755	8123.750

thing which should be taken into consideration in the use of the campimeter, is the fact that the front of the cornea, and not the optic center of the refracting system of the eye (roughly speaking), is the point from which the graduations on the instrument are laid out. From considerations such as these, it can be understood, perhaps, why an object of an excentricity of  $92^\circ$  as read on the campimeter scale, or approximately  $2^\circ$  back of the plane tangent to the anterior surface of the cornea at or near the point of entrance of the line of

regard, is still visible. In this connection the difference in the curvature of the cornea of different eyes should also be taken into account.

TABLE VII

## CHROMATIC THRESHOLDS FOR BLUE, NASAL MERIDIAN

In this table are given the values of the threshold for blue ( $468 \mu$ ) at 26 points in the nasal meridian. The intensity of light at the analyzing slit was  $91.97 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure. (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Campimeter Opening (Watt $\times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye (Watt $\times 10^{-12}$ per Sq. Mm.)
0	0.000764	0.10	8.06	0.046	2.440
5	0.000764	0.10	8.06	0.046	2.440
10	0.002125	0.167	13.40	0.076	4.067
14-17		Blind Spot			
20	0.002125	0.25	20.15	0.114	6.100
25	0.002125	0.25	20.15	0.114	6.100
30	0.003175	0.25	20.15	0.114	6.100
35	0.003378	0.375	30.23	0.171	9.150
40	0.003456	0.375	30.23	0.171	9.150
45	0.003744	0.50	40.30	0.228	12.200
50	0.003744	0.625	50.38	0.285	15.250
55	0.003867	1.125	90.68	0.513	27.450
60	0.003867	1.50	120.90	0.684	36.600
65	0.003867	1.50	120.90	0.684	36.600
70	0.003867	1.75	141.05	0.798	42.700
75	0.003867	1.875	151.10	0.855	45.750
76	0.003989	3.00	241.80	1.368	73.200
77	0.005210	12.00	967.20	5.472	292.800
78	0.005779	29.00	2337.40	13.224	707.600
80	0.006956	82.00	6609.20	37.392	2000.800
83	0.01262	116.00	9349.60	52.896	2830.300
85	0.03093	183.00	14749.80	83.448	4465.200
86	0.03093	198.00	15958.80	90.288	4831.200
87	0.03663	234.00	18860.40	106.704	5709.600
88	0.03663	277.00	22326.20	126.312	6758.800
90	0.05088	295.00	23777.00	134.520	7198.000
92	0.05088	328.00	26436.80	149.568	8003.200

That there is this wide extension of the field of vision in the temporal meridian seems to be well established by other investigators. Baas, for example, quoted by de Schweinitz (8), finds the average limit for ten observers to be  $99^\circ$ . Traquair (9) and others also find the limits to extend beyond  $90^\circ$ . In our own work we have found it to be carried out as far as  $92^\circ$  in a number of cases. For the explanation of this



TABLE VIII

## CHROMATIC THRESHOLDS FOR BLUE, TEMPORAL MERIDIAN

In this table are given the values of the threshold for blue ( $468 \mu\mu$ ) at 26 points in the temporal meridian. The intensity of light at the analyzing slit was  $91.97 \times 10^{-8}$  watt.

Degree of Excentricity	Surrounding Field and Pre-exposure. (Candle-power per Sq. In.)	Value of Threshold			
		Degrees Open Sector	Total Amount of Light at Campimeter Opening and at Eye ( $\text{Watt} \times 10^{-12}$ )	Density of Light at Campimeter Opening ( $\text{Watt} \times 10^{-12}$ per Sq. Mm.)	Density of Light at Eye ( $\text{Watt} \times 10^{-12}$ per Sq. Mm.)
0	0.000764	0.10	8.06	0.046	2.440
5	0.000764	0.10	8.06	0.046	2.440
10	0.000764	0.10	8.06	0.046	2.440
15	0.002125	0.167	13.40	0.076	4.067
20	0.002125	0.25	20.15	0.114	6.100
25	0.003175	0.375	30.23	0.171	9.150
30	0.003378	0.625	50.38	0.285	15.250
33	0.003456	0.75	60.45	0.342	18.300
35	0.003456	1.00	80.60	0.456	24.400
40	0.003744	1.25	107.50	0.570	30.500
41	0.003867	2.25	181.35	1.026	54.900
42	0.005088	4.00	322.40	1.824	97.600
43	0.005779	7.00	564.20	3.192	170.800
44	0.01262	80.00	6448.00	36.480	1952.000
45	0.03093	120.00	9672.00	54.720	2928.000
46	0.03093	164.00	13218.40	74.784	4001.600
46.5	0.03663	270.00	21762.00	123.120	6588.000
47-48			Blind to Blue		
49	0.03663	270.00	21762.00	123.120	6588.000
50	0.03663	234.00	18860.40	106.704	5709.600
52	0.03663	180.00	14508.00	82.080	4392.000
55	0.03663	230.00	18538.00	104.880	5612.000
56	0.03663	250.00	20150.00	114.000	6100.000
57	0.05088	270.00	21762.00	123.120	6588.000
58	0.05088	290.00	23374.00	132.240	7076.000
59	0.05088	306.00	24663.60	139.536	7466.400
61	0.05088	338.00	27242.80	154.128	8247.200

fact there is of course the alternative possibility of a loss of exact fixation, which possibility we have not ignored in our own thinking. However, if there is a loss of fixation, we have not been able to detect it by any means we have as yet been able to devise.

## DISCUSSION OF RESULTS

From data such as are presented in these tables and charts the following comparisons may be made in so far as the threshold may be regarded as a measure of sensitivity: (a) the sensitivity to a given range of wave-lengths from point to point

in the same meridian; (b) the sensitivity at corresponding points in different meridians; and (c) the sensitivity to the different wave-lengths in any given meridian. From the latter comparison, if properly made, an estimate of the selectiveness of the chromatic response at the threshold of sensation may be had. In making these comparisons several points of interest may be noted.

(1) *The Irregularities in the Curve of Sensitivity for the Different Colors in a Given Meridian.*—The rate of decrease in sensitivity after a certain degree of excentricity has been reached shows a great irregularity for the individual colors. This irregularity is greater in the temporal than in the nasal meridian. The following are some of the more notable examples. (a) The plateau in the curve for red in the temporal meridian. That is, the curve representing the amounts of energy required to arouse the just noticeable red sensation in this meridian rises almost imperceptibly from the center of the retina to about 20 degrees. From there it rises sharply to about 44 degrees. From 44 to 47 degrees the curve is almost vertical. Between these two points the energy required to arouse just noticeable sensation changes from  $31.236 \times 10^{-10}$ – $177.552 \times 10^{-10}$  watt. From 47 to 54 degrees occurs the plateau referred to. Between these two points there is practically no change in sensitivity. (b) The hump in the curve for yellow in the temporal meridian occurring between 33 and 43 degrees. From 36 to 39 degrees there is a sharp drop in sensitivity and from there to 43 degrees almost as sharp a rise. Between 36 and 39 degrees the energy required to arouse just noticeable sensation changed from  $5.705 \times 10^{-10}$  to  $10.188 \times 10^{-10}$  watt. This is an area of deficiency or partial blindness to yellow. There is in this area no corresponding loss in sensitivity to the blue stimulus nor to the other colors. Moreover, there is no detectable change in the cancelling or after-image reactions to yellow. That is, this area shows the characteristics of the Schumann case of color blindness. (c) The quick rise in the curve for all of the colors near the limits of sensitivity. This feature is very marked both in the nasal and temporal meridians, but more marked in the nasal than

in the temporal meridian. In the temporal meridian the rise begins around 45 degrees for red, yellow and blue and at 26 degrees for green. In the nasal meridian it begins around 75 to 85 degrees for red, yellow and blue; and at 51 degrees for green. In this region were found the limits of sensitivity in our previous work with the Hering pigment papers with the degree of illumination, etc., employed. And (d) the area of total blindness to blue from 47 to 49 degrees in the temporal meridian. On all sides of this area is a border of lowered sensitivity, the sensitivity falling off quite sharply as the area is approached. This area also shows the characteristics of the Schumann case of color blindness. That is, there is no corresponding decrease in sensitivity to yellow, red or green and no corresponding change in the cancelling or after-image reactions.

(2) *The Great Difference in Sensitivity at Corresponding Points in the Temporal and Nasal Meridians, more Especially in the more Remote Portions of the Retina.*—This difference is already so well known in a general way as to need no especial discussion here. Our data, however, may be considered as contributive in two regards: (a) the large number of points investigated and (b) the rating of the stimuli in units that can be compared numerically. In comparing the distribution of sensitivity at corresponding points in the two meridians it may be of interest among other things to note the difference in the order in which the sensitivity to the different colors falls off in case of red, yellow and blue as the limits of sensitivity are approached. So far as the relative sensitivity to red and green is concerned, this point bears upon the changes in the color tone of red in the two meridians in passing from the center to the periphery of the retina. As will be noted later in a more detailed statement of color tone changes, the red stimulus employed in the investigation was sensed as red in the nasal meridian from the center to 60 degrees, from 60 to 86 degrees as yellowish red or orange, and 86 to 92 degrees as red. In the temporal meridian it was sensed as red from the center to 30 degrees, from 30 to 47 degrees as yellowish red or orange and from 47 to 61 degrees as red. That

is, the zone in the far periphery of the retina in which the weakly aroused yellow component of the excitation is below the threshold in red, is relatively broader in the temporal than in the nasal meridian, as the relation of the curves of sensitivity to red and yellow would indicate should be the case.

(3) *The Striking Absence of Uniformity of Ratio of Sensitivity to the Pairs of Colors, Red and Green, and Blue and Yellow, from Center to Periphery of the Retina.*—From the center to about 27 degrees in the temporal meridian and about 51 degrees in the nasal meridian, the sensitivity to green is greater than to red. At these points a radical reversal of sensitivity takes place and the sensitivity to red is much greater than to green. A reversal of sensitivity occurs also for blue and yellow; but at a point farther removed from the center of the retina, namely, at about 44 degrees in the temporal meridian and at about 77 degrees in the nasal meridian. Even up to the points of reversal with their radical deviations in relative sensitivity, an inspection of the charts, especially III. and IV., will show how very little ground there is for any claim that constancy of ratio exists between the colors red and green, and blue and yellow from center to periphery of the retina. (A further discussion of this point will be given in the second part of this paper: 'Bearing of Results on Color Theory.')

And (4) *the Correspondence of the Distribution of Sensitivity to Red, Green and Yellow with what might be expected from the Changes in the Color Tone of Red and Green in Passing from the center to the periphery of the retina.*—For example, in passing from the center to the periphery of the retina the red stimulus used in making the threshold determinations given in the foregoing curves was sensed as red from the center to about 60 degrees; from 60 degrees to about 86 degrees it was sensed as a yellowish red or orange; and from 86 degrees to the limits of sensitivity it was sensed again as red. Corresponding to this, it will be noted that there is in this meridian a fairly close agreement in sensitivity to red and yellow from the center to about 60 degrees, at which point there is a rela-

tively sharp decrease in sensitivity to red. That is, from about 60 to 86 degrees there is much less sensitivity to red than to yellow. At about 86 degrees there is a sharp decrease in sensitivity to yellow, and from this point on to the limits of sensitivity a fairly close agreement again in sensitivity to the two colors. In the temporal meridian the red stimulus was sensed as red from the center of the retina to about 30 degrees; from there to about 47 degrees as yellowish red or orange; and from 47 degrees to the limits of sensitivity as red. Similarly in this meridian there is a fairly close agreement in sensitivity to red and yellow from the center to about 30 degrees; from 30 degrees to about 47 degrees there is considerably greater sensitivity to yellow than to red; and from this point on to the limits greater sensitivity to red than to yellow. In case of green, in the nasal meridian the greater loss in sensitivity to green as compared with yellow begins at about 51 degrees; and in the temporal meridian at about 26 degrees. Correspondingly at these points the green stimulus began to be sensed as yellowish green and continued to be sensed in this tone until the limits of sensitivity to green were reached, from which point on for a short distance it was sensed as yellow.

## BIBLIOGRAPHY

1. FERREE, C. E., AND RAND, G. A Note on the Determination of the Retina's Sensitivity to Colored Light in Terms of Radiometric Units. *Amer. Jour. of Psychol.*, 1912, 23, pp. 328-332.
2. FERREE, C. E., AND RAND, G. A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral. *Jour. of Exper. Psychol.*, 1916, 1, pp. 271-274.
3. FERREE, C. E., AND RAND, G. The Selectiveness of the Achromatic Response of the Eye to Wave-length and its Change with Change of Intensity of Light. *Studies in Psychology*. Titchener Commemorative Volume, 1917, Worcester, Mass., pp. 285-287.
4. COBLENTZ, W. W. Measurements on Standards of Radiation in Absolute Value. *Bulletin Bureau of Standards*, 1914, 11, pp. 87-100.
5. FERREE, C. E., AND RAND, G. A Simple Daylight Photometer. *Amer. Jour. of Psychol.*, 1916, 27, pp. 335-340.
6. FERREE, C. E., AND RAND, G. *Op. cit.* Titchener Commemorative Volume, pp. 304-306.
7. NUTTING, P. G. The Visibility of Radiation. *Philos. Mag.*, 1915, 29 (6), pp. 301-309.
8. DE SCHWEINITZ, G. E. Diseases of the Eye. 8th ed., W. B. Saunders Co., 1916, p. 81.
9. TRAQUAIR, H. M. *British Journal of Ophthalmology*, 1917, 1, p. 216.

# THE LEARNING CURVES OF THE ANALOGIES AND THE MIRROR READING TESTS

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The results of an experiment concerned with the learning curves of two mental tests are presented in this paper.<sup>1</sup> As regards the questions motivating the investigation and the general technique employed, the experiment is comparable with studies that have been reported by Wells (8) (9), Burt (1), Hollingworth (2) (3) (4), Thorndike (7), Myers (5), and others. It is possibly somewhat unique in one respect only, in that the content of the test material given to the subjects was new. The curves consequently represent the ability of the subjects to react to new content as the result of previous experience with similar content, and not their ability to improve as the result of the repetition of old material.

The essential issue discussed in the series of studies referred to is the relationship between the results of mental testing and the learning process. More specifically, four considerations are involved: (a) the relationship between initial tests and subsequent, average, maximum or final achievement; (b) the closely allied, but not identical, question of the extent to which individuals maintain throughout a practice series their relative positions established in the beginning trials; (c) the question of variability in performance as it is correlated with general excellence, complexity of the test, and duration of practice; and (d) the various aspects of the approach to the physiological limit.

These four considerations demand investigation in con-

<sup>1</sup> The tests were conducted in the Psychological Laboratory of the University of Pittsburgh during the academic year 1916-17. The subjects were selected from the undergraduate students in the department. The writer was most ably assisted in the experimental work by Miss Margaretta Weber, Miss Margaret Cochrane, Mr. A. W. Kornhauser and Mr. J. Russel Willison.

nection with the learning curve of any one test. An additional differentiation includes the evaluation and classification of each test investigated in this manner as regards its correlation with intelligence and with other tests. Moreover, the distinction should be noted between practice curves based upon a repetition of the same content and those constructed as the result of reactions to new material.

It is perhaps scarcely necessary to announce that the results and conclusions set forth in the present paper are limited in their scope and that their absolute verity is by no means insisted upon. The most serious difficulty encountered by the experimenter was in the attempt to compromise between an extensive and an intensive program of procedure. Repeated warnings have been issued to the student of mental tests as to the necessity of his employing a respectably large number of subjects before submitting his findings to statistical treatment; but it is interesting to note that similar admonitions have not been imposed upon the investigator of the learning process. Such an investigator is now beginning to realize, however, that the correlation coefficient and its attendant measurements, as well as the graphic presentation of results, are indispensable for the adequate interpretation of his results. They are employed in this article. The writer can offer as an excuse for his limited number of subjects only two facts: that the tests were individual and time-consuming, and that the war called a large number of his subjects before they had completed their practice series.<sup>1</sup>

### THE ANALOGIES TEST

A series of 250 different analogues was prepared for this experiment. They were typewritten in groups of 5, with 5 groups of 5 analogies on each of 10 pages, each page being numbered. Some of the analogues were obtained by combining various pairs of associates, given in the Woodworth-Wells monograph (10). Others were original, some of them being local in character.

The subjects reported at the laboratory once a week, at

<sup>1</sup> 21 subjects completed the Analogies series, and 22 the Mirror Reading tests.

a fixed day and hour. At the beginning of each period each subject was given one page of analogies—25, or one tenth of the entire series. The remainder of the period was devoted to the additional experiments described in an ensuing section of this paper. The learning series for the analogies thus extended over 10 weeks. The experimenter gave the series orally, pronouncing the three stimulus words at intervals of one second, and recording the time with a seconds stop-watch. The watch was started as the first word was pronounced; it was stopped after the response word was spoken. The suggestion given by Woodworth and Wells, that time should be recorded at the instant that the subject begins to articulate his response, was not followed. In many instances the response was not accepted, and on these occasions the subject was informed of the fact and was forced to continue his search for the word while time was being recorded against him. In addition, it was thought that even when the first response was the proper one the procedure here followed provided a more constant method of recording time than that suggested by these writers.

The subject was given a maximum time of 60 seconds for his response. In instances of failure to respond properly he was credited with a response of like duration. Obviously, a mere disregard of failures would have affected the averages unfairly; but at the same time, a penalty greater than that imposed would have resulted in numerous unwieldy fluctuations in the curves. The procedure followed inflicted a sufficiently severe penalty for purposes of differentiating between good subjects and poor, even though it favored the latter with a degree of charity almost absurd.

The subjects were not given preliminary examples of the type of thing desired of them. It was thought that such a method would be entirely illegitimate in a learning experiment in which the initial scores were objects of investigation. A uniform statement was made to each subject, in which the exact nature of the task was explained as specifically as the omission of an illustration permitted.

It was evident, before the experiments were inaugurated,



that the individual analogies were not of equal difficulty. It was further surmised that the relative degrees of difficulty encountered would vary with the individual subjects, rather than with the analogies themselves; but it was obviously imperative to demonstrate that this second assumption was not entirely gratuitous. As a safeguard against the possibility of a chance series of increasing or decreasing difficulty, the pages were presented in different orders. Ten of the subjects were given the pages in their numbered order, from 1 to 10, while the remaining subjects were given them in the reverse order, from 10 to 1.

*General Characteristics of the Results.*—All of the subjects improved with practice. A graphic representation of the

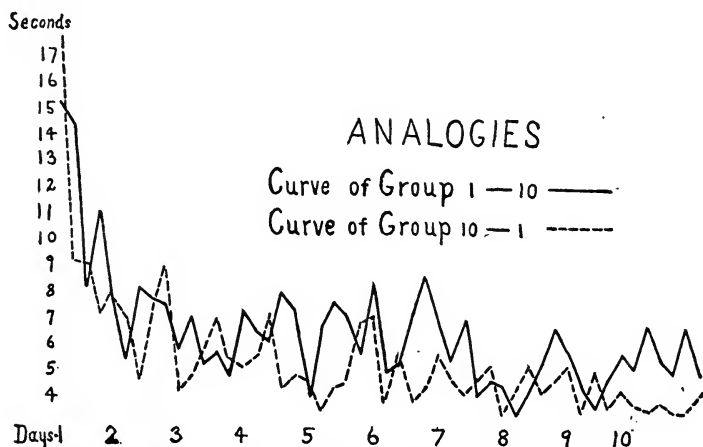


FIG. 1. Group Curves of the Analogies Practice Series.

fact is furnished by the curves in Fig. 1. Both of them are combination or group curves, based upon the records of the two respective groups, as indicated in the figure. As each point in either curve represents the average of 5 analogies, there are 50 points in the curves, each 5 of them in turn representing a day's practice.

While a comparison of these two curves discloses a slight superiority on the part of one group, the degree of correspondence between them is sufficiently obvious to warrant the conclusion that the curves, in the main, show actual practice

effects. An essential feature of both is the initial improvement, generally found to characterize practice.<sup>1</sup> This improvement is marked during the first 10 scores, or during the first two days. The ensuing parts of the curves are noticeable chiefly for their numerous fluctuations from a line showing a weak tendency to continue the slope towards the base line. Evidently, pronounced improvement was made only in the first stage of the learning.

*Improvement.*—The significant results brought out by the data are concerned with (1) the range of improvement, (2) the correlation between the amount of improvement and the general excellence of the individual subjects, and (3) the approach to the physiological limit.

The fact has been stated that in each instance there was manifest improvement. Its amount was calculated by taking the first day's record for each subject as 100% and computing the decrease in percentage terms found in the last day's record. The difference between the initial 100% and the percentage of the final record was taken to represent the percentage of improvement. Thus, C's average for the first day was 15.5 seconds, and his record for the last day was 4.3 seconds, representing a decrease of 73% over his first trial—the greatest improvement found. In contrast with this, the record of U begins with 4.1 seconds and ends with 3.8 seconds, showing the minimum improvement of 6%. The average percentage of improvement was 51.<sup>2</sup>

It should be noted that the record of the final day was not necessarily the best. In fact, the average scores made on the ninth day were superior to those made on the tenth and last. But it was thought that the record following the greatest amount of practice was the most representative measure of the effect of that practice; moreover, the selection of the best individual scores would have meant records taken from

<sup>1</sup> Peterson's distinction between the time curve and the curve of attainment is scarcely applicable in the present instance, since it represents a series of unit learning tasks performed with decreasing amounts of difficulty or endeavor, or with increasing amounts of attainment, as measured by time records. See Peterson, Joseph, *op. cit.* (6).

<sup>2</sup> The significant numerical results are given in the table on page 61.

days 6, 7, 8, 9 and 10—a range rather too extensive for purposes of comparison.

While introspections were not called for, the remarks volunteered by the members of the practicing group indicate the general subjective nature of the task. All of them reported that while the first tests were being conducted they were elaborating ideas of the kinds of relationships desired. Most of them say clearly that the relationships could be classified into five or six types. This rather elementary explanation, however, does not suffice for the improvement beyond the initial stage. The alert subjects testified that, after they had fallen in with the general scheme of things, the correct reactions came without any intermediary idea of the class of reaction called for. The slowest performers likewise dispensed with any connecting idea or concept. They stated frankly that their reactions were guesses, more or less wild, and it was evident that they relied upon the click of the stopwatch to inform them when their attempts were successful. An intensive conscious attack seemed to characterize neither the mentally alert nor the mentally slow; it was reserved for the intermediate class. The correct reactions for the excellent subjects came quickly and mechanically; for those who made the poorest records, they came after a trial-and-error process.

The relationship between general excellence and the amount of improvement is pronounced and unambiguous. The best subjects made the smallest amount of improvement; and while the poorest ones showed the greatest amount, at their best they were still inferior to the good ones at their worst. This fact is established by the correlation obtained,<sup>1</sup> and it is represented graphically by Fig. 2. The correlation between general averages, ranging from 14.7 seconds to 3.8 seconds, with the percentage of improvement, was found to be + .52, P. E., .1059. In Fig. 2 the curves of the five

<sup>1</sup> Both the Rank-Difference and the Pearson formulae were employed. The Pearson coefficients served as a check upon the results found by the Rank-Difference formula; and the differences found between them are negligible, since the writer can claim at the most only the presumption of relationship. All coefficients reported in this paper were those obtained by the Rank-Difference method.

poorest and the five best subjects are plotted. The contrast between these two groups as regards improvement is obvious.

The meaning of the physiological limit of ability to react in any learning process should be defined rather explicitly in order to avoid possible ambiguity. At least two variations

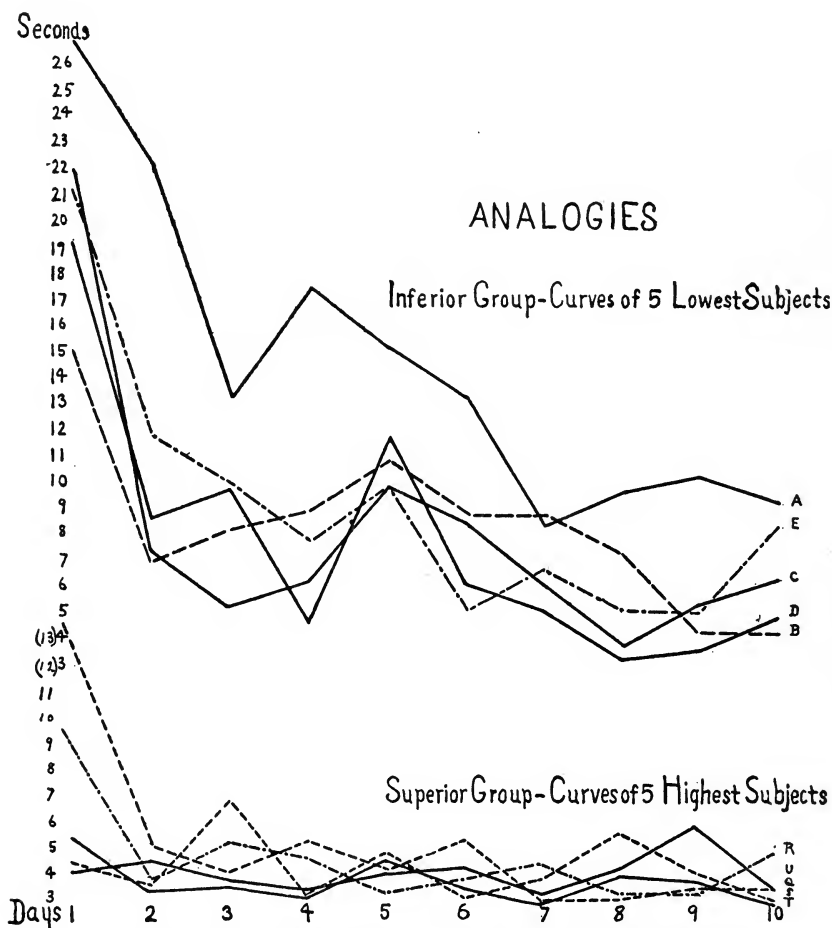


FIG. 2. Individual Improvement Curves of the 5 Best and the 5 Worst Subjects.

in the meaning of the phrase can be proposed. In the analogies test, considered strictly as the limit above which a purely verbal reaction is impossible, it may be represented and measured by the minimum amount of time necessary for the sub-

ject to articulate a word after having heard its cue spoken. In the practical attempt to measure this limit as just defined, the subject would, of course, be given beforehand the response word, as well as its verbal stimulus, and would be instructed to react with that word. Or, the limit may be conceived of as the minimum amount of time necessary to react under the conditions of the experiment, with the response word not known beforehand. This time interval would be determined empirically after an infinite amount of practice; it could not conceivably exceed the limit first defined. In this case, the physiological limit would tend to be uniform with all subjects, as our results indicate; whereas, in the second case, it would be more of an individual measurement. It should be noted, however, that an identity exists between the two in respect to the best subjects.

The physiological limit, as measured by the sheer ability to respond with a stimulus word to a word previously memorized, was actually attained in the present experiment by some of the subjects. The record of 3.2 seconds, made at times by subjects S, T, and U during the last three trials was not excelled when these subjects were instructed to react with a given word. Moreover, additional tests indicated that this mechanical limit was practically the same for all of the subjects, irrespective of their scores in the normal tests. According to this criterion, all approached and some reached a relatively fixed *physiological* limit.

Not only technical considerations of major interest, but practical and applied aspects of decided importance, find their focal point in the inter-relationships existing among the three phases of the learning process under consideration; namely, general excellence, improvement, and the approach to the limit of progress. If it should become established that the inferior subject is capable of making the greater gain in any learning process, it would follow that practice and education result in homogeneity and uniformity.<sup>1</sup> If it should appear

<sup>1</sup> The terms 'superior' and 'inferior' as employed in the text are distinctions based solely upon the average scores of the subjects. For the present it may be noted that this usage of the terms is conventional and arbitrary; in subsequent sections of this paper, reasons are urged in justification of the assumption that a good average score is an index of actual superiority.

that the reverse of this is true, the tendency of learning would then be towards diversity in ability. The educational corollaries dependent upon the demonstration of one or the other of these principles are numerous and indeed significant.

Investigators have presented results and conclusions somewhat at variance. The actual results are not necessarily incompatible, since they may depend upon variables, such as the nature of the learning task upon which they are based and the range of the equipment of the learners for the task. But the conclusions advanced, to the extent that they are general, are likely to be irreconcilable. Wells mentions the tendency of the inferior subject to catch up with the superior, and explains this in terms of the physiological limit (8). Myers, however, reports that the superior subjects made the maximum gains; and, reinforced by the support of Colvin and others, defends the definition of intelligence as adjustment (5).

While the results of the present investigation are in harmony with those given by Wells, it will be noticed that the correlation found between minimum improvement and general excellence is due chiefly to the extreme good and poor subjects. The fact is more patent in the curves of Fig. 2 than it is in the coefficient obtained. It may be that this relationship characterizes only the measurements found in the extremes of a distribution of scores, or it may apply only to learning activities far on the way towards perfection.

Two lines of explanation are open. The first has just been referred to: a pronounced amount of improvement may be impossible for the superior individual because of the mere fact that he inaugurates his reactions at a point relatively near his limit. Having no great distance to travel, his journey is brief. But other factors seemed to function in the analogies tests and, as will devolve later, in the reading tests. As contrasted with the inferior, he may initiate his attempts at a point more remote from the 'just barely not any' stage, or the zero. And this probably carries with it the implication that certain preliminary states of the learning are for him omitted altogether. The intelligent learner is not, then,

the one who eliminates most rapidly; he is rather the one who dispenses with the eliminating process.

These considerations lead to a revision of the current definition of intelligence that would explain it as ability to show improvement or adjustment. The demonstration of capacity for improvement may be taken as an indication of inferior, not superior, ability. Intelligence thus becomes defined in terms of immediate, consistent and uniform adjustment, not in adjustment considered as capacity to improve by leaps and bounds.

To the extent that this concept proves to be applicable to learning processes involving intelligence factors, the isolated learning curve will carry its own mentality label. If a series of descending curves were constructed for the purpose of representing the typical grades of intelligence, they would describe the following forms: (1) A low grade of intelligence would find expression either in a horizontal curve, far above the base line, showing that the series of reactions were equally difficult or impossible; or in a descending curve, marked by violent fluctuations, but never closely approaching the base line. (2) A more evident display of ability would show itself in a descending curve more successful in attaining a satisfactory level of achievement. (3) Superior mental endowment would find its graphic representation again in a horizontal curve, but in this instance it would lie in close proximity to the base line.

The question of the relative differences in the value of increments in ability at different positions on the curve is pertinent at this point. The fact that when the limit is being approached such increments call for an increased amount of some factor, has been frequently stated, and since the publication of the historical studies of Bryan and Harter this factor has usually been defined in terms of increased effort. But no evidence was at hand to demonstrate that the better subjects attained the score of 3.2 seconds because of any such heightened intensity. The relative ease with which maximum efficiency was attained was attested to by the better subjects, and it was strikingly obvious to the experimenter.

*Initial Scores and Subsequent Achievement.*—The facts of-

ferred in the preceding section of this paper have to do with the difference in the rate and extent of improvement found between the good and the poor subjects. This difference may exist, but at the same time it may develop that the initial scores have a prophetic value. The coefficients obtained in the attempt to determine the relationship between beginning and ensuing records are as follows:

Between

The averages of all analogies and the averages of the first 10, + .89, P.E., .0317.

The averages of all analogies and the averages of the first day, + .87, P.E., .0374.

The averages of the first 10 analogies, and the averages of all subsequent analogies, + .76, P.E., .0642.

The averages of the first day and the averages of all subsequent analogies, + .80, P.E., .055.

The averages of the first day and the averages of the last day, + .59, P.E., .0988.

The relationship of the beginning score to the average score is not of primary significance, since the beginning record, when it is relatively large, will unduly influence the average. But as the results show, the beginning record, when it is relatively poor, not only affects the average, but discloses a lack of aptitude for the reactions that is not compensated for by subsequent improvement. A creditable beginning more truly serves as an index of future attainment, since it establishes an upper as well as a lower limit of ability.

Myers (5) found that the later scores could be anticipated to a large extent from the beginning records. Hollingworth's conclusion is more conservative. According to this contributor, the relationship between the early and later stages in the repetition of tests is in part a function of the test itself, and in part a function of the location of the measurement taken in the series—the correlation in general increasing as successive tests from the initial one are compared with final measurements.<sup>1</sup> The coefficients found in the present in-

<sup>1</sup> Hollingworth, H. L., *op. cit.* (4), p. 258.



vestigation would seem to indicate a fairly definite tendency towards the correlation in question.

*Variability.*—The factor of variability was calculated in percentage terms by ascertaining the mean deviation of the scores of each subject and by finding the ratio between this and the mean of the scores. This series of measurements was correlated with the series of average scores, with the result of + .68, P.E., .0776.

Again a presumption of relationship seems to be warranted. The inferior subjects appear to be the more variable in their responses. Supplementary facts concerning the behavior of the subjects recorded in the experimenter's notes may explain, in part, this result: the inferior subjects, as compared with the better ones, were seemingly more uncertain in their responses; they were more inclined to resort to trial-and-error; they were evidently working under shifting degrees of embarrassment; and they were obviously more affected by absolute differences in the difficulty of the various analogies. This last statement is of chief importance. In the series of units constituting the analogies test, the more difficult ones were easily apprehended by the better subjects, but since the limit of the capacity of these subjects was practically attained, no pronounced difference in reaction time between analogies of different degrees of difficulty was in evidence. The physiological limit was a barrier imposed against a relative shortening of the time for the easy analogies. The absence of this barrier from the path of the slower subjects permitted their speeding up or their slowing down in more appreciable proportion to the varying difficulties in their way.

### THE MIRROR READING TEST

At the completion of the analogies test for each period, the subject was presented with three pages of prose, reading from the top to the bottom, but from right to left. In constructing this material, the typewriter was used, with the carbon sheet reversed. This resulted in a carbon copy of the selection being typed on the reverse side of the sheet, with

the individual letters, as well as the words, extending from right to left.

The copy consisted of six units of four lines each to the page, typed in capitals exclusively, with all punctuation marks omitted. The material was of a quasi-nonsense variety. It was taken from a translation of Plato's Republic; but the selections, somewhat abstract in their original version, were rendered even more innocuous as to meaning by a wholesale process of stringing together isolated or broken phrases or clauses. A unit chosen at random from the experimenter's key reads as follows:

THEN THIS ADVANTAGE THE RECEIPT OF PAY DOES NOT COME TO EACH  
FROM HIS OWN ART BUT STRICTLY CONSIDERED THE ART OF HEALING  
PRODUCES HEALTH AND BENEFITS THAT WHICH IS ITS APPOINTED OBJECT  
WE SEE CLEARLY THAT A GOVERNMENT NEVER PROVIDES THAT WHICH IS

The cumulative decimal stop-watch was used in this experiment. One unit of reading was exposed at a time, after which the subject was given a rest of a few seconds. Time was recorded for each half page. Seven weekly periods were given to the entire reading series, resulting in a total of 21 pages of mirror prose being read by the subjects. Following the procedure in the analogies test, half of the subjects were given the pages in one sequence, and the remaining group in the reverse order. The subjects were required to correct all errors as they read.

*General Results.*—In Fig. 3 the two combination curves for the two groups are given. The initial slope is in evidence, extending between the first two periods, and to a less pronounced degree, between the second and third periods. A rather small and uniform decline characterizes the remaining part of the curve.

Numerous and spontaneous comments made by the subjects at the completion of the periods, together with the observations recorded by the experimenter, reveal certain aspects of the learning process. In general, the subjects enjoyed it far more than they did the analogies series; they regarded it, possibly with a certain degree of psychological

insight, as more of a game and less of a measuring device for intellectual ability.

The reading, or more properly the translating, of the passages was rather spasmodic, a process comparable with the struggle of an inadequately prepared student in a language class. Certain words were unfailing sources of difficulty: these were either the longer words, which taxed with unusual severity the newly formed habits, or the monosyllables that aroused the insufficiently repressed normal reading habits. Such words as 'architect,' 'ignominious' and 'precluded' are examples of the first type of difficulty; while the tendency

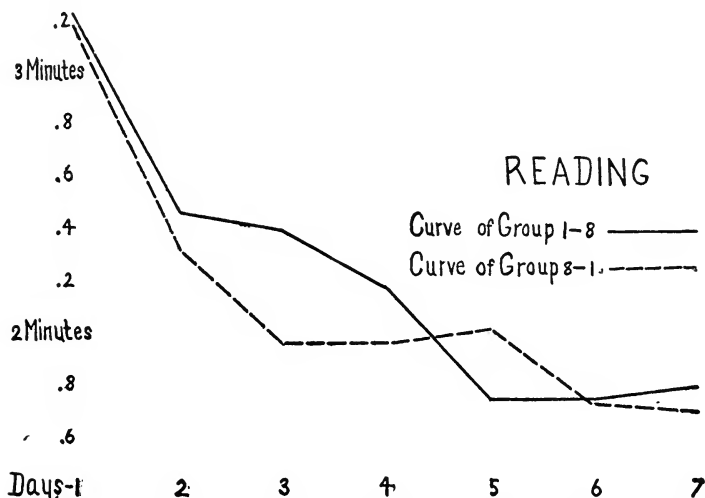


FIG. 3. Group Curves of the Reading Practice Series.

to translate *erom* as 'from' and *saw* as 'saw' (instead of 'more' and 'was,' respectively) illustrates the second type of error. If we may accept the unanimous testimony of the subjects, the process was essentially one of relearning to read; and of the various schemes employed for that purpose, the word and phonetic methods were utilized—the latter as a temporary device, the former as the standard method. A new word was at first spelled phonetically, but on its reappearance it was usually recognized visually.

The initial acceleration of the learning again seems explicable in terms of the early acquisition of new and the subor-

dination of old general habits. The act of directing the eye from the right to the left was a habit rather quickly established, and the device of spelling out the beginning letters of a word was discovered early. The importance of the aid derived from the meaning element was minimized by the subjects, but it was probably underestimated by them; at least the extent to which it could be relied upon was ascertained during the first period. The matter of relinquishing the normal reading habits was attended to with a considerable degree of difficulty; but the necessity for so doing was impressed upon the subjects in their beginning attempts. It should be emphasized that the rapid initial progress was made during the first part of the first trial—a fact not expressed in the graphs.

*Improvement.*—The inferior subjects in the reading, as in the analogies test, showed the greater amount of improvement. This factor, expressed in percentage terms, was correlated with the general average, and yielded a coefficient of  $+ .44$ , with a P.E. of  $.12$ .

The explanation advanced for the similar result in the preceding test was partly in terms of the physiological limit in its relationship to the starting point. However, the application of this concept as an explanatory principle in the present case presents certain difficulties, since the control experiment designed for the purpose of establishing the limits of the subjects was productive of rather anomalous results.

This control experiment, conducted at the completion of the mirror reading tests, consisted merely in determining the most rapid rate of normal oral reading for each of the subjects. Time was recorded for four separate readings of one page each. The averages of these four readings were correlated with (1) the averages of the first day in the test readings and (2) the corresponding averages for the last day. The coefficients respectively were  $+ .42$ , P.E.  $.1270$ , and  $+ .31$ , P.E.,  $.0874$ . No special significance in the difference between them is warranted; but the coefficients together seem to point to a relationship between the two types of reading, a relationship that one might expect, attributable to positive transfer effects.

Of more significance is the coefficient found by correlating (1) the differences between the most rapid test and the most rapid control readings with (2) the most rapid test readings. These differences were expressed in the number of words read per second. The coefficient in question was  $+ .39$ , P.E., .1344. If this is to be accepted as disclosing a tendency, it means that the subjects who made the best test records made proportionately better records in the control readings, while those who were slow in the mirror reading tests were proportionately less adept than their superiors in normal reading.

In another respect the estimated physiological limits of the analogies and mirror reading tests differ. In the analogies test the subjects seemingly approached a uniform limit: in their ability to respond to a spoken word they were alike. But in the control tests for the reading experiment the deviation or variability was greater than it was in the mirror reading itself. The average number of words read per second in the control reading was 4.38, A.D., .51, while in the test reading the average was 1.69, A.D., .31.

The contrast, then, between the results of the analogies and mirror reading tests may be summarized as follows: in the reading test the superior subjects, at their points of maximum achievement, were farther away from their estimated physiological limits than were the inferior subjects; and these limits were more variable. In the analogies test, the superior subjects at their best were closer to their limits than were the inferior; and the limits in this case were not only less variable, but they were more nearly identical with the estimated limits for the inferior individuals.

In spite of these discrepancies, the view that the most rapid rate of normal reading may represent the physiological limit of mirror reading is retained by the writer. These two reading processes elicit the same motor responses. They differ only in the nature of the visual symbols eliciting these responses. The body of available psychological knowledge concerning positive and negative transfer and retroaction would seem to support this view, and the coefficient of  $+ .50$

given above lends additional corroboration. Assuming that a positive relationship exists between the two types of reading, the issue is this: would prolonged practice in normal rapid reading tend to reduce or to increase the amount of variation in the control records actually found? One is certainly justified in believing that if prolonged practice would reduce the variation, and would result in the records being brought more closely together, the revision would be upwards—that is, all subjects would approach the 6.6 words<sup>1</sup> (or better) standard.

A more specific formulation of the suggestions just advanced leads to two opposed hypotheses: (1) It may be assumed that with sufficient practice in normal reading all subjects would approach a fixed and uniform limit, which, for illustrative purposes, may be stated as 6.6 words per second; and that this limit is also the physiological limit in mirror reading. The attainment of this record in mirror reading would involve, for both our inferior and superior subjects, an improvement far in advance of that actually brought out in the experiment; but it would mean relatively more improvement for the inferiors. The results in this case would duplicate those found in the analogies tests. (2) Or, it may be assumed that extended practice in normal reading would establish a more scattered series of limits in that accomplishment than the series represented by the four trials recorded. It would then follow that the superior subjects would necessarily show greater improvement than the inferior ones in order for both of them to attain their respective points of maximum achievement.

The first of the two hypotheses seems to be the more reasonable. The tendency of practice in mirror reading was to reduce the range and the variability of the series of scores. If normal reading is, as reasons were given for believing, comparable with mirror reading, the same result would be expected.

*Initial Scores and Subsequent Achievement.*—The beginning scores in mirror reading are related to ensuing scores.

<sup>1</sup> The best record in normal rapid reading made by any of our subjects. (See Table.)



The reader who begins well will continue to do well, while the reader who begins poorly will remain consistently below the standards set by the better subject. This tendency is even more apparent than it is in the analogies records, the reason being, undoubtedly, that the reading habits involved are the definitely established and highly individual normal reading habits adjusted to the new conditions.

The coefficients obtained are as follows:

Between

The average of all trials and the average for the first three trials, + .92, P.E., .0225.

The average of all trials and the averages for the first day, + .94, P.E., .0169.

The averages for the first three trials and the averages for all subsequent scores, + .88, P.E., .0338.

The averages for the first day and the averages for all subsequent trials, + .91, P.E., .0254.

The averages for the first day and the averages for the last day, + .85, P.E., .0416.

*Variability.*—The tendency again is for the inferior subjects to show slightly more variability. The coefficient obtaining between the percentages of variability and average scores is + .57, P.E., .0988. The reason given for this fact in the association test seems to apply likewise to mirror reading.

### CORRELATIONS BETWEEN THE TESTS

No correspondence exists between the rankings of the subjects in the analogies and the mirror reading tests. The coefficient obtained by correlating the averages of both tests for the first day was  $-.02$ , and for the second day  $+.08$ . While these results indicate absence of correlation, it may be noted that the tendency is in the direction of positive relationship. This is in agreement with the conclusion stated by Hollingworth to the effect that practice increases the positive coefficients between tests.

In one respect, the demonstrated lack of relationship is of significance. It furnishes justification for the conclusion

that the similarity between the two tests as regards the initial slope, the greater improvement of the inferior subjects, the greater variability of the inferior subjects, and the reliability of the initial scores as indexes of future accomplishment, is due to the nature of the tests themselves, rather than to the personnel of the practicing group.

#### SUMMARY

In both the analogies and the mirror reading practice series, the superior subjects, as contrasted with the inferior, disclosed less capacity for improvement and less tendency toward variability.

Two explanations account for the relatively small amount of improvement made by the better subjects: they began their tests at a point nearer the physiological limit, thus precluding the possibility of pronounced gain; and they curtailed the preliminary stage of the learning.

This relationship between improvement and relative merit suggests the principle that intelligence should be defined in terms of immediate adjustment, and not in terms of capacity for improvement. It functions, not by making pronounced gains in a learning process, but by omitting the preliminary excess and random reactions and establishing a level of attainment near the limit of ability, thus making decided improvement an impossibility.

As the result of two special tests given at the completion of the practice series, estimates were made of the limits of capacity of each subject for the analogies and for the mirror reading tests. In the analogies tests, the better subjects attained this limit, which was practically uniform for all subjects. In the mirror reading tests, the better subjects were farther away from their limits than were the inferiors; and these limits were more variable than the test results themselves. These facts find explanation in one of two assumptions: either that the estimates of the limits of ability in reading were inadequate, inasmuch as they were based upon the initial records of a test probably subject to improvement; or that the explanations accounting for improvement



TABLE  
ANALOGIES

Subject	Average	Average First 10	Average First Day	Average Omitting First 10	Average Omitting First Day	Percentage of Improvement	Percentage of Variability	Average Last Day
A	14.7	30.5	26.0	14.1	13.5	63	25	9.7
B	9.4	25.8	21.5	8.7	8.0	50	32	5.4
C	8.6	23.4	15.5	8.0	7.8	73	25	4.3
D	8.0	27.4	19.3	7.2	6.7	68	36	6.3
E	7.8	23.9	16.6	7.1	6.8	71	31	6.6
F	7.5	20.9	15.1	6.9	6.6	30	32	10.7
G	6.6	26.2	16.5	5.8	5.5	76	31	4.0
H	6.5	12.5	11.8	6.3	6.0	60	23	4.8
I	6.0	17.6	10.7	5.5	5.5	62	26	4.1
J	5.7	15.2	8.7	5.3	5.4	58	26	3.9
K	5.5	10.7	7.4	5.2	5.3	34	16	4.9
L	5.4	5.2	9.2	5.4	5.0	59	25	3.8
M	4.8	10.4	6.5	4.6	4.7	45	18	3.6
N	4.7	7.6	6.1	4.7	4.5	38	16	3.8
O	4.5	6.1	5.1	4.5	4.5	40	21	8.1
P	4.4	5.9	4.5	4.4	4.4	27	19	3.3
Q	4.3	13.7	9.8	3.8	3.6	65	25	3.5
R	4.2	10.3	6.6	4.1	4.0	48	22	3.4
S	4.0	12.2	9.4	3.7	3.4	61	26	3.6
T	3.9	5.7	5.7	3.8	3.7	46	14	3.1
U	3.8	5.0	4.1	3.5	3.8	06	11	3.8

MIRROR READING

Subject	Average	Average First 3	Average First Day	Average Omitting First 3	Average Omitting First Day	Percentage of Improvement	Percentage of Variability	Average Last Day	Control Reading Words per Second	Mirror Reading Words per Second
L	3.08	6.65	5.81	2.80	2.62	66	28	1.99	5.00	1.99
T	3.05	5.34	4.91	2.87	2.83	52	16	2.37	4.66	1.09
E	2.99	5.23	4.95	2.80	2.66	54	24	2.28	3.58	1.30
B	2.71	4.65	4.29	2.56	2.44	59	19	1.82	3.54	1.34
J	2.44	3.15	2.92	2.42	2.38	34	12	1.73	3.88	1.30
O	2.42	4.81	4.32	2.24	2.11	62	27	1.66	3.99	1.57
U	1.41	3.36	3.02	2.33	2.30	38	19	1.89	4.44	1.44
S	2.40	3.84	3.65	2.29	2.19	46	20	2.00	5.09	1.39
D	2.33	4.00	3.62	2.20	2.12	48	16	1.91	2	
H	2.22	3.39	3.24	2.13	2.04	50	22	1.64	4.11	2.12
C	2.14	3.00	2.93	2.08	2.00	40	14	1.78	4.17	1.46
K	1.97	3.34	2.91	1.86	1.81	47	11	1.55	4.66	1.57
R	1.96	2.79	2.64	1.91	1.86	41	11	1.54	4.24	1.57
(X) <sup>1</sup>	1.90	3.02	2.83	1.81	1.77	49	15	1.47	2	
F	1.86	2.35	2.32	1.82	1.79	28	13	1.67	5.00	1.75
G	1.85	2.43	2.50	1.81	1.74	39	12	1.53	3.63	1.63
I	1.84	2.99	2.80	1.75	1.68	51	15	1.38	3.58	1.79
M	1.80	2.85	2.68	1.72	1.66	46	14	1.46	2	
A	1.79	2.58	2.47	1.73	1.67	42	20	1.44	4.35	2.32
N	1.76	2.47	2.43	1.84	1.65	46	12	1.32	6.60	2.02
P	1.54	2.10	2.11	1.47	1.43	36	13	1.36	5.00	2.15
Q	1.40	2.17	1.98	1.34	1.30	45	12	1.09	4.91	2.32

Numerical Results of Practice Series. All Results except those in the 'Percentage' Columns are Stated in Seconds and Decimal Parts of Seconds.

<sup>1</sup> Subject (X) was given Reading Tests only.

<sup>2</sup> No records obtained.

in the analogies test are not applicable to the results of the mirror reading test. The first mentioned assumption is accepted by the writer, on the grounds that the control test places a premium upon the same type of ability required in mirror reading. This being the case, practice in the control test—i. e., normal, oral reading—would result in relatively more improvement on the part of the inferior readers, and therefore in a more uniform series of measurements representing the limits of ability in mirror reading.

Initial scores in the analogies and reading tests were found to be fairly reliable indexes of subsequent attainment. The difference in the rate of progress between the superior and the inferior subjects was not sufficient to change the relative positions of the subjects at the end of the series.

Absence of correlation was found between the rank positions of the subjects in the two experiments. This seems to indicate that the explanation for the results found must be referred to the learning itself, and not to the individuals comprising the practicing group.

#### REFERENCES

1. BURT, CYRIL. Experimental Tests of General Intelligence. *Brit. J. of Psychol.*, 1909, 3, pp. 94-177.
2. HOLLINGWORTH, H. L. Correlation of Abilities as Affected by Practice, *J. of Educ. Psychol.*, 1913, 4, pp. 405-414.
3. HOLLINGWORTH, H. L. Individual Difference Before, During and After Practice. *PSYCHOL. REV.*, 1914, 21, pp. 1-8.
4. HOLLINGWORTH, H. L. Vocational Psychology. 1916, Chap. XI.
5. MYERS, GARRY C. Some Variabilities and Correlations in Learning. *Am. J. of Psychol.*, 1918, 29, pp. 316-326.
6. PETERSON, JOSEPH. Experiments in Ball Tossing: The Significance of Learning Curves. *J. of Exp. Psychology*, 1917, 2, pp. 178-224.
7. THORNDIKE, E. L. Educational Psychology. Vol. II.
8. WELLS, F. L. The Relation of Practice to Individual Differences. *Am. J. of Psychol.*, 1911, 22, pp. 1-13.
9. WELLS, F. L. Practice Effects in Free Association. *Am. J. of Psychol.*, 1911, 22, pp. 1-13.
10. WOODWORTH, R. S., AND WELLS, F. L. Association Tests. *PSYCHOL. MONOG.* 1911, 13, No. 57.

# AN EXPERIMENTAL INVESTIGATION OF CERTAIN ALLEGED RELATIONS BETWEEN CHAR- ACTER AND HANDWRITING

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At least since 1662 when Camillo Baldo published in Italy his treatise on the method of determining the character of a person by his handwriting (2, p. 2), there has been in existence a more or less systematic and growing body of belief on this subject. During this period graphology has been regarded with favor and respect by many individuals of note, among whom may be mentioned Leibniz, Goethe and Sir Walter Scott (2, p. 7 ff). An extensive body of literature including periodical publications has come into existence on the subject, particularly in France and Germany. It has often been associated with physiognomy, chiromancy and magic (4, p. 126). The claims of the graphologists are frequently very extravagant. Two of the more recent expounders of graphology deserve special mention because of the positions they occupy in the field of the recognized sciences. One is William Preyer (10), professor of physiology at Jena and author of one of the classical works on infant psychology. The other is George Schneidemühl (11), professor of comparative pathology at the University of Kiel. While stoutly asserting the necessity of reducing graphology to the status of a true science, these writers show little less credulity than the pamphlet written on the same subject by William L. French and put out by a pen manufacturing company for advertising purposes (6). The more pretentious works on graphology are predominantly *a priori* deductions from very general principles which are often true enough in themselves. These deductions are copiously illustrated by reproductions of handwriting often of illustrious or notorious personages. Though extensive collections of handwriting seem to have

been made by some of these writers, none of them have gone beyond a more or less casual observation. Crépieux-Jamin, it is true, reports what purports to be an experiment but it is without scientific value (2, pp. 106-137).

As for so much else, we are indebted to Alfred Binet for the only serious attempt to bring the rather elusive subject matter of graphology under objective scientific conditions. Binet's report of his experiments makes a volume of 257 pages (1). He was especially fortunate in having as collaborators a number of professional graphologists one of whom, Crépieux-Jamin, is probably its most famous modern exponent (4, p. 123). In his first experiment Binet presented each of his subjects with 180 addressed envelopes which had been written in about equal numbers by men and women. The subjects were to tell the sex of the writer in each case. Crépieux-Jamin made the highest score, 79% of right determinations. Another graphologist made a score of 75%. A number of individuals with no special training made scores ranging from 73% to 66% of right cases (1, pp. 8-11). It will be noted that chance alone would yield 50%. This experiment has been repeated with untrained subjects by Downey (5) and Starch who each obtain substantially similar results.

In another experiment Binet obtained samples of writing from 37 individuals of recognized intellectual eminence such as Renan and Bergson. With each of these he paired the writing of a person of similar education and general social level but of very mediocre intelligence. The subjects were asked to state which of each pair of writers was the more intelligent. Crépieux-Jamin came off best again with the astonishing score of only three errors out of 36 determinations, or nearly 92% correct. Six other graphologists scored respectively 86%, 83%, 80%, 68%, 66% and 61%—all distinctly superior to chance (1, pp. 73, 101 ff.). Binet concludes: "On pourrait dire, introduisant ici une distinction nécessaire, que les signes graphiques d'intelligence ont une réalité incontestable, mais ne concordent pas nécessairement et constamment avec une grande intelligence, quoique cela arrive le plus souvent" (1, p. 100).

As a final test Binet investigated the ability of the graphologists to tell the character of the writers from their handwriting. He secured specimens of the handwriting of eleven notorious assassins. With each of these he paired the writing of an honest citizen in a rather humble walk of life. The subjects were directed to tell which of each pair of writers was superior in general morality. This was a more severe test apparently, though the number of right judgments continues to exceed chance. Once more Crépieux-Jamin comes off best with three errors out of eleven judgments, or 73% right. Two other graphologists made a score of five errors each, which is little better than chance (1, pp. 245-246). Summing up all three experiments Binet says: ". . . cette conclusion, c'est que dans la graphologie, comme dans la céphalométrie, probablement aussi dans la chiromancie—il y a quelque chose de vrai" (1, p. 251). And again: "Qu'il s'agisse du sexe, de l'âge, de l'intelligence ou même (ce dernier point avec plus de réserve) qu'il s'agisse du caractère, nous arrivons toujours à la même conclusion" (1, p. 252).

Such results and conclusions from a psychologist of Binet's repute suggests that we may not without a previous scientific examination safely ignore the claims of graphologists, however extravagant and apparently unfounded they may seem. The somewhat surprising revelations of character contained in physiognomy even as seen in ordinary photographs, should make us still more cautious (8, p. 48 ff.). Indeed facts are not wanting which render even probable at least a weak relation between handwriting and certain traits of character. Dearborn found, for example, a distinct tendency of the hands and neck to flex under unpleasant and to extend under pleasant stimuli (3). From this it is not a very far cry to the possibility that a confident or ambitious state of mind should produce a tendency to extension in the arm during writing which would make the lines slope upward from left to right. Upward sloping lines are in fact the generally accepted graphological sign of ambition. Neither would it be very surprising to find a slight tendency for energetic or forceful people to write with heavier pressure and so to make

heavier lines than less forceful persons. Various types of heavy lines are in fact common graphological signs of "force." It was with such possibilities in mind that the present experiment was undertaken.

Binet sought to test the skill of individual graphologists. In contrast, the present experiment seeks to test the truth of certain graphological theories, *i. e.*, certain correlations alleged to exist between specific traits of handwriting and traits of the writer's character. Obviously the reasons given by graphologists for their findings may be either more or less accurate than the findings themselves. The traits of character investigated were chosen as being at once associated with traits of handwriting which were susceptible of objective measurement and being among the less improbable of the relations alleged. They are shown in the following table together with the trait of writing most commonly supposed to indicate each:

TABLE I

Ambition.....	lines of writing sloping upward.
Pride.....	lines of writing sloping upward.
Bashfulness.....	writing traced with fine lines.
Force.....	(a) heavy lines, (b) heavy bars on the t's.
Perseverance.....	long bars on the t's.
Reserve.....	closed a's and o's.

The subjects were 17 students of the University of Wisconsin, all members of the same medical fraternity. Each man was first asked to write, in his ordinary manner, a paragraph from a popular magazine. The writing was done in each subject's own room, at his regular desk and with his own pen. The paper was uniform throughout, unruled, of good quality, size 8.25 by 10.75 inches. The material written is as follows, the parentheses indicating particular m's, n's and t's which were measured:

It see(m)ed to be a big (t)own; he wo(n)dered just why, in all his days, he had never made more (t)han four or five (t)rips to Brooklyn, but he did not wo(n)der for lo(n)g. His heavy (t)houghts dragged back to his own plight a(n)d the call just ahead of him. Cus(t)om a(n)d frie(n)dship co(m)bined would deprive him even of the fee he was about to earn. Not so much as the wre(t)ched car-fare he was spe(n)d-i(n)g would be his reward for a (t)t(e)n(di)n(g) the sufferi(n)g me(m)ber of the Ca(tt)erson fa(m)ily. In the ma(tt)er of plain mo(n)ey profit, Brooklyn so far measured up to the Hallas s(t)a(n)dard in every respect.

TABLE II

		Ambition			Pride	Bashfulness					Force					Perseverance				Reserve		
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
Subject	Rank in Ambition	Average Upward Slope of Line in mm.	Rank in Upward Slope of Line	Rank in Pride	Rank in Bashfulness	Av. Width of Upward Stroke of t in .0007 Inches	Rank in Narrowness of Upstroke of t	Index of Av. Width of m's and n's	Rank in Narrowness of m's and n's	Rank in Force	Av. Width of Bars of t's in .0007 Inches	Rank in Width of Bars of t's	Index of Width of Bars on t's	Rank in Index of Width of Bars on t's	Rank in Perseverance	Av. Length of Bars on t's in mm.	Rank in Length of Bars on t's	Index of Length of Bars on t's	Rank in Index of Length of Bars in t's	Rank in Reserve	Index of the Openness of a's and o's	Rank in Tendency to Close a's and o's
A	15.5	+ 5.90	1	13	1	22.9	15	1.49	4	14	26.4	4	1.15	7	16	12.30	3	2.34	3	6.5	25	15
B	4.5	+ 2.80	7	4	2	24.1	16	1.85	8	12	29.6	3	1.23	4	4	8.85	11	1.34	14	3	13	8
C	1	+ 1.25	14	1	10	17.3	8	1.00	1	3	18.1	12	1.05	13	1	10.55	5.5	2.05	5.5	2	7	3
D	17	- 2.90	16	14	15	20.7	13	2.10	10	15	18.7	11	.90	15	17	9.80	7	1.78	9	16	23	14
E	4.5	- 2.75	15	3	4	20.6	12	1.33	2	7	43.2	6	2.10	1	9	6.05	16	.88	17	10	40	17
F	14	+ 1.30	10	7	6	15.3	4	1.59	5.5	11	23.2	1	1.52	2	14	7.50	13	1.36	10	8.5	8	4
G	7	- 15.00	17	9.5	11	16.1	6	1.59	5.5	9.5	17.1	14	1.06	11	8	7.45	14	1.04	13	17	9	5.5
H	9	+ 3.90	3	12	3	21.4	14	2.37	12	16	24.2	15	.84	17	5	9.70	8	1.80	8	15	22	13
I	10	+ .33	12	15	5	15.8	5	1.72	7	5	13.3	10	1.39	3	11	10.55	5.5	2.48	2	1	20	11
J	2	+ 1.70	9	5	8	14.2	9	2.39	16	6	19.8	15	1.10	17	6	7.25	15	1.26	16	5	16	9
K	11.5	+ 2.30	8	17	7	18.2	9	2.02	9	9.5	20.1	8.5	1.12	9	10	12.90	2	1.33	15	13	3	1
L	13	+ .87	11	2	12	27.8	17	2.29	11	2	31.2	2	1.06	12	13	9.50	10	2.07	4	4	30	10
M	6	+ 4.30	2	2	9	19.5	11	2.29	11	4	20.6	7	.91	10	12	5.85	17	1.80	7	6.5	35	16
N	8	+ 3.10	13	8	6	18.9	10	3.67	17	1	6.8	13	1.19	14	2	22.30	1	1.402	11	14	12	7
O	3	+ 3.50	6	6	17	5.7	1	3.67	17	8	20.1	17	1.20	5	7	8.25	12	1.54	11	11	6	2
P	15.5	+ 3.90	5	11	14	16.7	7	1.36	3	1	10.8	16	.88	16	15	12.20	4	2.05	5.5	12	22	12
Q	11.5	-	4	16	16	12.2	2	3.26	15	17												

When the writing was finished the subject was given a set of sixteen small cards, each card containing the name of one of the other subjects. The card bearing the subject's own name was left out. He was directed to arrange the cards in the order of the amount of ambition possessed by the person indicated on each. Thus the name of the most ambitious person in his opinion would be placed first, the name of the next most ambitious person second, and so on. The rank thus given each subject was recorded on a special blank by the experimenter, after which he shuffled the cards with care. This process was repeated with each of the traits of character listed in Table I. and with each of the subjects. Assurance was given them that the records would be kept in such a way as to secure the strictly confidential nature of the results. The same experimenter conducted this part of the experiment throughout. He was on a basis of easy, friendly acquaintance with all the subjects. Because of the intimate personal relations obtaining in small semi-social fraternities, these subjects were peculiarly able to judge the character of each other.

From the 17 rankings thus obtained, the average position of each subject in each trait was found. The subjects were then re-ranked in each trait according to the size of these averages, the smallest (*i. e.*, highest) average rank in a given trait being 1, the next smallest being 2, and so on. These final rankings for the various traits of character appear in appropriate columns in Table II.

It was next necessary to secure corresponding rankings with respect to the various traits of handwriting supposed to be correlated with the respective traits of character. This was done by measuring the various traits of handwriting involved and then ranking the subjects on the basis of these measures. The nature and the extent of the relations existing between various traits of character and handwriting were then computed from the appropriate series thus obtained by means of Spearman's correlation formula:

$$R = 1 - \frac{6\sum D^2}{n(n^2 - 1)}$$



As indicated in Table I., ambition is very generally held by graphologists to be revealed by a pronounced upward slope of the lines of writing. Similarly, the lack of ambition is indicated by a corresponding downward slope, while intermediate degrees of ambition have intermediate slopes.<sup>1</sup> Since all of the lines of the various specimens of writing were of approximately the same length, the relative upward or downward slope of a given specimen would be indicated accurately by the average distance that the right end of the lines stood above or below the point where the lines would have fallen had they run across the page strictly at right angles to the margin. Each line of each specimen was thus measured to the nearest half-millimeter. The final averages with appropriate signs appear in Table II., column 3. The corresponding ranks of the various subjects appear in column 4. Their ranks in ambition as judged by their fellows, appear in column 2. A mere glance at these two series of ranks shows at once that there is no particular connection between upward sloping writing and ambition. It is true, subjects D, I and L agree fairly well as to their positions in the two series. On the other hand subject A who has the most upward sloping writing of the group is among the weakest in ambition, ranking 15.5. Again subject C who is the most ambitious member of the group has a distinctly downward sloping writing, ranking 14! When all the tendencies to agreement and disagreement are taken together in the Spearman formula, the correlation is found to be  $-.20$ . Clearly there is no tendency whatever for the ambitious person to write in an upward sloping direction.

Some graphologists have held that ascending lines indicate pride.<sup>2</sup> The rank of the various subjects in pride as judged by their fellows is accordingly given in column 5. The correlation between pride and upward sloping lines is found to be  $-.07$ . There is no evidence of the alleged relation.

<sup>1</sup> Crépieux-Jamin (2), p. 78; Deschamps (4), p. 61; French (6), p. 5; Frith (7), p. 33; Preyer (10), pp. 27, 181; Schneidemühl (11), p. 171, 299.

<sup>2</sup> Crépieux-Jamin (2), p. 172; Schneidemühl (11), p. 299. See also Frith (7), p. 100.

Regarding bashfulness, graphologists are in less agreement. Schneidemühl says that it is indicated by writing traced in fine lines (11, pp. 196, 299). Accordingly, the width of each subject's writing was measured just at the bend of the upward stroke on the ten t's indicated by parentheses in the passage (p. 66). The measurement was made by means of a microscope containing a scale in the eye-piece. With the lens system used, the units on the scale were equal to .0007 of an inch. The average width of the ten strokes measured are shown for each subject in column 7. The corresponding ranks are shown in column 8. The ranks of the various subjects in bashfulness as judged by their fellows are shown in column 6. The correlation of narrowness of lines and bashfulness as represented by these two series of ranks is  $-.45$ . There is clearly no tendency for bashful people to write with fine lines.

Correlation was also computed between bashfulness and the tendency to lateral narrowness of the m's and n's. Both the height and width of the m's and n's marked by parentheses in the passage (p. 66) were measured to the 64th of an inch, by superposing an Osborn transparent glass scale (9, p. 93) and reading with a hand lens. Interference due to natural differences in the size of writing were eliminated by taking as the index of the proportionate width of the m's and n's, the quotient of the average width divided by the average height. These indices are shown in column 9 and the corresponding ranks in column 10. The correlation of bashfulness with narrow m's and n's is found to be  $+.38$ . This is the largest positive correlation obtained in the present study. It is rather ironical that it also should be the only relation investigated which, to the knowledge of the writer, has never been put forth by any graphologist.<sup>1</sup> The  $-.45$  obtained between bashfulness and thin lines is the largest correlation of either sign obtained in the study and rather surprising. But, unfortunately for the theory of Herr Schneidemühl, it is negative. A real negative correlation is, to be sure, as interesting and important as a positive one. It will

<sup>1</sup> See however, Crépiaux-Jamin (2), pp. 91 and 96; Frith (7), p. 38.

be proven later, however, that neither of these correlations are greater than might result from chance.

'Force' is held by graphologists to be indicated variously by heavy bars on the t's<sup>1</sup> or by heavy writing in general.<sup>2</sup> We have already at hand exact measures of the heaviness of the upstrokes of the t's in connection with bashfulness (column 7). The ranks shown in column 8 must be reversed in this connection, 17 becoming 1, 16 becoming 2, etc., since here the rank is according to *width* instead of *narrowness*. The rank of the various subjects with respect to force as judged by their fellows is shown in column 11. The correlation of force with heavy writing as indicated by these rankings is — .17. There is here no evidence in favor of the alleged relation.

The width of the bars on the same ten t's was measured at their widest points by the microscope as described for the up-stroke. These averages are shown in column 12, and the corresponding ranks in column 13. This trait of writing gives a correlation coefficient with 'force' of — .06. There is no evidence that forceful people cross their t's heavier than anyone else.

It might be urged that 'force' is indicated by the *relative* width of the bars on the t's as compared with the heaviness of the rest of the writing. A convenient index of this relative width is afforded by the quotient obtained by dividing the average width of the bars on the t's (column 12) with the average width of the up-stroke of the same t's (column 7). These quotients are given in column 14, and the corresponding ranks in column 15. They yield a coefficient of correlation with 'force' of + .27. This is no greater than chance affords.

Perseverance is held by certain graphologists to be indicated by prolonged bars on the t's.<sup>3</sup> The length of the bars on the same ten t's already mentioned, were measured to the nearest half millimeter. The average results are shown in

<sup>1</sup> Crépieux-Jamin (2), p. 191; Deschamps (4), p. 84; French (6), p. 5; Frith (7), pp. 55, 120. See also Schneidemühl (11), p. 295.

<sup>2</sup> French (6), p. 17; Preyer (10), p. 168.

<sup>3</sup> Frith (7), p. 97; cf. Crépieux-Jamin (2), p. 95.

column 17, and the corresponding ranks in column 18. The rank of the various subjects in perseverance as judged by their fellows appears in column 16. Perseverance and the tendency to make long bars on the t's as thus indicated, yield a correlation of exactly zero.

Recognizing the fact that a long bar on large writing might not be of the same significance as one of the same length on small writing, this disturbing factor was compensated for as follows: The height of the ten t's was first measured and averaged. The quotient obtained by dividing the average length of the bars on the t's by the average height affords a reliable index of the proportionate length of bar. These quotients appear in column 19, and the corresponding ranks in column 20. These yield a correlation with perseverance of + .16. There is clearly no connection here between perseverance and the tendency to make long bars on the t's.

Reserve is held almost universally by graphologists to be indicated by a tendency to close tightly the small a's and o's, whereas frankness is indicated by the opposite tendency to leave them open at the top.<sup>1</sup> It was found that no writer completely closed all his a's and o's, or left them all open. Accordingly a table was made of all the a's and o's showing for each subject the number closed, the number just open and the number wide open. The number of open letters plus twice the number wide open was finally taken as the index of openness. These indices are shown in column 22. The corresponding ranks in tendency to close the a's and o's appear in column 23. The ranks of the subjects in reserve as judged by their fellows appear in column 21. Reserve and the tendency to close a's and o's as thus indicated, yield a correlation coefficient of - .02. Once again we find no support for the claims of the graphologists.

A brief summary of the correlations obtained in the foregoing experiments is presented in Table III.

<sup>1</sup> Crépieux-Jamin (2), p. 87; French (6), p. 11; 13, 23; Frith (7), p. 102; Preyer (10), p. 155; Schneidemühl (11), p. 297.

TABLE III

## CORRELATIONS OF CHARACTER WITH HANDWRITING

Ambition with upward sloping lines.....	— .20
Pride with upward sloping lines.....	— .07
Bashfulness with fineness of line.....	— .45
Bashfulness with lateral narrowness of m's and n's.....	+ .38
Force with heavy handwriting.....	— .17
Force with heavy bars on t's.....	— .06
Force with heavy bars on t's, varying size of writing compensated for.....	+ .27
Perseverance with length of bars on t's.....	.00
Perseverance with length of bars on t's, varying size of writing compensated for.....	+ .16
Reserve with closed a's and o's.....	— .02

There appear in this table fully as many negative correlations as positive. The average of the entire ten is — .016, or practically zero. And since the above are a fair sampling of graphologists' claims as to the relation between handwriting and character, this figure may be taken with some assurance as typical of the whole.

As an interesting check upon the technique employed, an equal number of correlations were computed from pure empirical chance. Symmetrical lotto blocks numbered from 1 to 17 were shaken thoroughly in an improvised dice box, thrown upon a table and arranged in a row by mechanical methods. The numbers of the blocks were recorded in the order thus appearing just as if representing the ranks of the subjects in a trait of character. The blocks were then shaken for about a minute, arranged in a row as before and the number recorded, this time as if ranks of subjects in a trait of handwriting. Then the correlation between the two orders was computed by Spearman's formula exactly as in the regular experiment. The following successive correlations were obtained by this means:

+ .22, — .23, + .07, + .10, — .17,  
— .07, + .19, — .26, + .47, — .15.

The large positive correlation of + .47 which it is possible to obtain by this means should be compared with the correlations of + .38 and — .45 in Table III. The average of the group as a whole is + .017, which may be compared with a similar average derived from Table III.

The strictly negative results of the present experiment are not necessarily in conflict with the results of Binet. The two experiments do not test exactly the same thing, as has already been suggested. It is probably significant, however, that Binet's graphologists made very bad records on the only test of character he employed, even where the differences between the pairs of writers were extremely gross. The astonishing ability of Binet's subjects to tell intelligence from handwriting should receive special investigation. It is possible that the individuals whose handwriting was used in this experiment were too well known to furnish satisfactory material for subjects who make a special point of studying the handwriting of eminent men.

## BIBLIOGRAPHY

1. BINET, ALFRED. *Les Révélations de l'Ecriture*. Paris, 1906. Pp. 257.
2. CRÉPIEUX-JAMIN, J. *L'Ecriture et le Caractère*. Paris, 1895. Pp. 441.
3. DEARBORN, GEO. V., and SPINDLER, FRANK. Involuntary Motor Reaction to Pleasant and Unpleasant Stimuli. *PSYCHOL. REV.*, 1897, 4, pp. 453-462.
4. DESCHAMPS, LOUIS. *La Philosophie de l'Ecriture*. Paris, 1892. Pp. 160.
5. DOWNEY, JUNE. Judgments on the Sex of Handwriting. *PSYCHOL. REV.*, 1910, 17, pp. 205-216.
6. FRENCH, VILLIAM LESLIE. *What your Handwriting Reveals*. New York, 1914. Pp. 19.
7. FRITH, HENRY. *How to Read Character in Handwriting*. New York. Pp. 138.
8. HOLLINGWORTH, H. L. *Vocational Psychology*. New York, 1916. Pp. 302.
9. OSBORN, ALBERT S. *Questioned Documents*. Rochester, 1910. Pp. 501.
10. PREYER, WM. *Psychologie des Schreibens*. Hamburg, 1895. Pp. 230.
11. SCHNEIDEMÜHL, G. *Handschrift und Charakter*. Leipzig, 1911. Pp. 319.

## THE CORRELATION BETWEEN VISUALIZATION AND BRIGHTNESS DISCRIMINATION<sup>1</sup>

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Although the notion of imagery 'types' is generally accepted, in some form or other, very little if any experimental work has been done towards determining the underlying reason for the existence of different types. While practice, and also heredity, are generally conceded to be factors, we as yet do not know their relative importance. However, before we can understand the influence of either we must know more concerning the neurological factors upon which or through which they may exert their influence.

In this connection the explanation frequently offered is one which assumes individual differences in the relative degrees of development or of the sensitivity of different tracts as a basis of the differences in imagery. Segal, for example, has advanced a 'Perseveration-tendency' theory. "*Wir haben den Typus durch die stärkere Perseverationstendenz, welche die den Typus bedingende psychophysische Disposition auszeichnet, zu erklären gesucht.*"<sup>2</sup> (1) As evidence he points to his results, which seem to show that material presented to the sense corresponding to the 'type' of the subject is more readily memorized and recalled, and this is believed by him to be due to the greater 'Empfänglichkeit' of this sense department. The evidence cited is, however, contradicted by the work of others, even though some have gone so far as to assume that the 'type' of the subject may be determined through the small differences in results obtained by different modes of presentation.

Others turn more directly to differences in sensory abilities. Miss Washburn, for example, combines this with experience

<sup>1</sup> From the Psychological Laboratory of the University of Michigan.

<sup>2</sup> Segal, 'Ueber den Reproduktionstypus und das Reproduzieren von Vorstellungen,' *Archiv f.d. Ges. Psychol.*, 1908, 12, p. 216.

and habit in an attention theory. She says: "'Imagery types' may, I think, be most readily explained as attention types, due to innate differences in sense discrimination and to habits formed by the nature of one's environment and work."<sup>1</sup> Others deny or doubt any such relation between imagery 'types' and sensory ability, but so far there has been lack of experimental work bearing directly upon this problem, which is one fundamental to the whole notion of imagery.

The present investigation was undertaken to discover if there is any correlation between sensitiveness of the visual tracts, in so far as that is measured by accuracy in brightness discrimination and effectiveness in visualization. Assuming that brightness discrimination is determined in some degree by the sensitiveness of visual tracts the results would test Segal's hypothesis. Discrimination was measured by the accuracy with which the observers could set a Lummer-Brodhun photometer, visualization by success in making simple geometrical constructions mentally. Seventy-five students in psychology were used in each test.

One point of distinction generally overlooked must be emphasized in all discussions of 'imagery types' because this expression is regarded by some as having quantitative, by others a qualitative significance. That is, a person whose imagery is nearly exclusively visual may yet be a very poor visualizer where the clearness or distinctness of imagery is considered, and from the latter viewpoint rank far below another who has a great deal more of the other kinds of imagery with relatively less of the visual. For example, suppose the intra-individual distribution of imagery for A is visual 85 per cent., auditory 15 per cent.; and for B, visual 45 per cent., auditory 55 per cent. Now B's visual imagery may be clearer and more distinct than A's and B may readily succeed in visualizing complex figures where A fails. One of the most exclusively visual of our subjects was in our test unable to visualize any but the simplest problems, failing in eight of the twelve. To say that A has a greater percent of visual than of auditory imagery, or to say that he has rela-

<sup>1</sup> Washburn, 'Movement and Mental Imagery,' p. 44.



tively more visual imagery than B is one thing, while to say that A's visual imagery is more or less clear than B's is another.<sup>1</sup> If now the sensitivity of the different sensory tracts be brought into the discussion, the explanation, if accepted, in the first case would be that A's visual tract is relatively more sensitive than his own auditory; in the second case it would be that A's visual tract is less sensitive than B's visual tract. Our purpose in the work here reported has been to test the latter proposition. In other words, can individual differences in the ability to visualize be explained as due to differences in the sensitivity of the visual tract in so far as the latter is measured by the limen of brightness discrimination?

The first of the writers, in connection with other and more extensive work suggested by Professor Pillsbury and dealing with imagery, was using a test for visualization and it seemed an opportune time to begin an investigation of the relation between effectiveness of visual imagery and the sensitivity of the visual tract. Effectiveness of visual imagery was measured by success in twelve problems, the solution of which involves visualization. The last forty-six subjects tested were given four extra problems in addition to the main list of twelve. The following are examples of the problems used:

7a. Think of a square. Draw two horizontal lines dividing the square into three equal rectangles. Now draw the two diagonals of the square. Into how many parts is the square divided?

7b. How many of these parts are triangles?

9a. Think of a square. From the middle of the square draw lines to each of the two lower corners. From the middle of the bottom line draw lines to each of the upper corners. Into how many parts is the square divided?

9b. How many of the parts are triangles?

Each problem was typewritten upon a long narrow strip of paper, the length of which prevented it from being used as a sensory basis for the solution of the problems. Each part of each of the above examples was considered as a separate

<sup>1</sup> Segal, *ibid.*

problem. As the slips were marked face downward on the table, with the numbers placed on the back, the subject was able to know when he came to a series of connected problems. The subject sat facing a bare wall. The instructions, generally read first by the subject while the experimenter busied himself about the room, were as follows:

"After the experimenter says 'ready' you will turn over the first card, read carefully, and give the answer called for as soon as you can. If the answer given is wrong you will be so informed, and you will continue to work until you give the correct answer, or until you have worked five minutes. If you have not succeeded in the five minutes the experimenter will stop you, and at the signal 'next' will turn over the next card, etc. The work is all to be done 'mentally' and you are not to use the card or any object in the room in any way as an aid, nor will you trace the figures on the table with your fingers. Otherwise you may move your hands about as you wish. A record will be kept not only of the time required to give the correct answer, but of the number of wrong answers given as well. They are equally important."

From previous experience in giving tests these instructions seemed to be required. The subjects were at liberty to reread them at any time. One practice problem was allowed at the beginning of the test and no record was kept of it.

A fuller report of this test will be made in a later publication. It assumes that the person with the clearer visual imagery will succeed more quickly than one whose imagery is less clear. In formulating and selecting the problems to be used the effort has been made to eliminate all which would permit the subject to attain the result by calculation rather than by visualization. Without exception the subjects, who were beginning students without preconceived notions as to 'types' and who were tested before the question had been discussed in the class, attributed success or failure to ability or lack of ability to visualize.

Previous experience with the test seemed to show that unless the subject is compelled to continue until the correct answer has been given, the results have very little value.

Both the objective and the introspective results show that the scores, either of time or successes, based on the first answer are often too dependent on factors other than visualization. The method of scoring as finally adopted was based on the time required for a correct solution. This brings up the question of the proper treatment of the failures. We counted each failure as 400 seconds. The correlation between the scores as thus obtained and the scores that would result from scoring failures after 240 seconds of work, counting failures as 300 seconds, is .98. This is to be explained by the fact that the subject succeeds, if at all, within the first three or four minutes. While this procedure combines the result in a single score, it is not above question. Results are given below for comparison which eliminate entirely those subjects who failed in any problem. We also compare the limina of those who succeeded in all, and of those who failed in one or more of the problems.

The brightness discrimination at Professor Pillsbury's suggestion was determined by using the Lümmer-Brodhun photometer. The method followed was that of least noticeable differences. The photometer furnishes a ready, simple, and accurate means of obtaining results, and has the advantage of reducing the space error to a minimum. Half the readings were obtained with the center area in the photometer darker at the start, the other half with the relation reversed. The apparatus was set up in a dark room, and large black screens kept the light from the 25-watt Mazda lamps from the eyes of the subject, and from lighting the room generally. The readings were obtained with the moveable lamp at an average distance from the photometer of 130 cm. The stationary lamp was placed in a large box with a 4.5 x 4.5 cm. opening which was covered with ground glass. Ten minutes were allowed for darkness adaptation. Ten values were obtained from each subject.

The second of the writers gave the test for brightness discrimination, the first the test for visualization. Seventy-five subjects were used for both tests. In testing the last forty-six subjects, the tests were given in immediate succes-

sion, the visualization test first. The time allowed for darkness adaptation served as a period of rest. This was done to equalize for the two tests the effects of general fatigue before beginning, of off days, colds, etc. In both tests the subjects were tested individually. The difference limen for different subjects ranged from 1 to 5 cm.

Coefficients of correlation were computed not only on a basis of the whole number tested (75) but also on a basis of the last forty-six tested. Then in each case the coefficients are given which result when those are eliminated whose readings in the test for brightness discrimination showed an average deviation of more than one-third of their average. Then because of possible question as to the validity of the treatment of failures, the coefficient which results from the elimination of those who failed in any problem is also given, where this does not reduce the number of subjects below twenty. The coefficients given were obtained by converting R-values (foot-rule method) according to the table given in Whipple's Manual.<sup>1</sup> Results are also given for the last forty-six, which include sixteen problems, instead of twelve.

Whole number (75) tested, 12 problems

1. Seventy-five Subjects . . . . .	.23
2. Fifty-seven Subjects, eliminating most variable . . . . .	.17
3. Thirty-one Subjects, eliminating also those who failed . . . . .	.26

First group tested (29)

4. Twenty-nine Subjects . . . . .	.42
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Second Group Tested, forty-six Subjects, 12 problems

5. Forty-six Subjects . . . . .	.13
6. Thirty-six Subjects, eliminating most variable . . . . .	.13

Second Group Tested, 16 problems

7. Forty-six Subjects . . . . .	.06
8. Thirty-six Subjects, eliminating most variable . . . . .	.03

While these results may seem to indicate some slight dependence of visualization, as tested, upon visual sensitivity, yet the evidence is far from conclusive. It will be noted that the correlation is higher for the first than for the second group. The former were laboratory subjects and on the whole a better

<sup>1</sup> Whipple, G. M., 'Manual of Mental and Physical Tests,' Part I, p. 44.

class of subjects than the second group. Being also the first tested, they were less likely to be familiar with any of the problems of the visualization test beforehand. Therefore it might seem that their results were more valid. On the other hand, they took the two tests on different days and the tests for brightness discrimination were not quite so thorough as with the second group. On the whole the evidence seems to be that the difference in the case of the two groups is due simply to chance involved in sampling, for in the second group in particular there were several who ranked among the best in one and the poorest in the other test. As at least half of these were low in the visualization test and high in the other, the lack of correlation can hardly be charged to the possibility that some of the second group were informed beforehand of the tests. Of course each subject was pledged to secrecy in this regard.<sup>1</sup>

If failures alone are considered, we find that the average brightness limen for those who did not fail in any problem is 2.28 cm., while for those who did fail it is 2.38 cm. Occasionally a subject was found who, though failing in two or three problems, would nevertheless finish the remaining problems in considerably less than the average time required for those problems. This seems to indicate that while the limits of a subject's ability to visualize may be low, yet within those limits he may work more rapidly than another who, though slower, will not fail in any. It might be that, with a series of problems including some of greater difficulty, and graded to form a scale, and in which only the ability to visualize is considered and the time disregarded, different results might be obtained. Further work will proceed along this line.

It might be thought that the correlation was due to the general intelligence of the subjects or to some other factor that would appear in all tests. That this was not the case is apparent from the results in other tests. For the same sub-

<sup>1</sup> These results are not corrected for attenuation as none of the usual methods are applicable. The fact that each partial group shows a higher correlation than the total would indicate that the correlations would be considerably higher were this correction to be made.

jects scores were available for memory, as tested by exposing for six seconds lists of eight letters and figures, and also for speed in written multiplication. In the first case the correlation with brightness discrimination is .13; in the second case it is — .03.

#### SUMMARY

1. Distinction must be made between the qualitative and the quantitative aspects of individual differences in imagery.
2. The correlation for visualization and brightness discrimination is slightly positive. The evidence seems to be that individual differences in visualization cannot be explained as due to differences in visual sensitivity.
3. No correlation was found between brightness discrimination and either memory for letters and digits or speed in multiplication.

# THE PSYCHOLOGICAL REVIEW

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## REPORT OF THE PSYCHOLOGY COMMITTEE OF THE NATIONAL RESEARCH COUNCIL

Respectfully submitted to Dr. George E. Hale, Chairman National Research Council  
BY MAJOR ROBERT M. YERKES,  
*Chairman of the Psychology Committee.*<sup>1</sup>

### I. ORGANIZATION OF THE PSYCHOLOGY COMMITTEE

The Psychology Committee was organized in April, 1917, at the request of the Chairman of the Research Council, because of the appearance of psychological military problems and the formulation of plans for participation in the war by the Council of the American Psychological Association.<sup>2</sup>

The constitution of the Committee, together with such changes as have occurred by reason of resignation or additional appointments, is indicated below: James R. Angell,<sup>3</sup> J. McKeen Cattell, Raymond Dodge, Shepherd I. Franz, G. Stanley Hall, Walter Dill Scott, Carl E. Seashore, Edward L. Thorndike, John B. Watson, G. M. Whipple, Robert M. Yerkes, Chairman, and the late John W. Baird, Vice-chairman.

Simultaneously with the organizing of the Psychology Committee of the National Research Council, the Council of the American Psychological Association authorized the appointment and designated the chairmen of twelve committees of the Association to deal with various aspects of the rela-

<sup>1</sup> Grateful acknowledgment is made by the Chairman of the Committee to Captain Richard M. Elliott for his assistance in the preparation of this report.

<sup>2</sup> An account of the events which preceded the organizing of this committee and of its early activities was published under the title "Psychology in Relation to the War" in the *PSYCHOLOGICAL REVIEW*, 25, 85-115, March, 1918.

<sup>3</sup> Angell and Scott were appointed in October, 1917. Cattell resigned in October, 1917. Baird was appointed Vice-chairman in March, 1918.

tions of psychology to the war. With a few exceptions, these committees were ultimately organized by their chairmen. Subsequently all which attained complete organization and became active were constituted subcommittees of the Psychology Committee of the Research Council. This was done for convenience and efficiency of administration, since the chairman of the Psychology Committee of the Research Council was also, during the year 1918, President of the American Psychological Association and in this capacity responsible for the operation of the Association's war committees.

The following list of committees which have been active for varying periods during the military emergency at once supplements the history of psychological service previously published in this journal and supplies a scheme for the presentation of the results achieved by the Psychology Committee and through its coöperation.

1. Committee on psychological literature relating to military affairs. Chairman, Madison Bentley.

2. Committee on the psychological examination of recruits. Chairman, Robert M. Yerkes.

3. Committee on psychological problems of aviation, including examination of aviation recruits. Chairmen, Harold E. Burt, George M. Stratton, and Edward L. Thorndike (serving successively).

4. Committee on the selection of men for tasks requiring special aptitude. Chairman, Edward L. Thorndike.

5. Committee on recreation in the army and navy. Chairman, George A. Coe.

6. Committee on problems of vision which have military significance. Chairman, Raymond Dodge.

7. Committee on pedagogical and psychological problems of military training and discipline. Chairmen, Charles H. Judd and William C. Bagley (serving successively).

8. Committee on psychological problems of incapacity, including those of shell-shock and reëducation. Chairman, Shepherd I. Franz.

9. Committee on problems of emotional stability, fear and self-control. Chairman, Robert S. Woodworth.



10. Committee on "Propaganda behind the German Lines." Chairman, James R. Angell.

11. Committee on acoustic problems of military importance. Chairman, Carl E. Seashore.

12. Committee on tests for deception. Chairman, John F. Shepard.

13. Committee on adaptation of psychological instruction to military educational needs. Chairman, Raymond Dodge.

In addition to the lines of service indicated by the titles of committees, special work was done by individual members of the Psychology Committee or of its subcommittees as follows:

14. For the Division of Military Intelligence, on methods of selecting and training scouts and observers, by John B. Watson and Madison Bentley.

15. For the Chemical Warfare Service, on psychological problems of the gas mask, by Raymond Dodge, John W. Baird, and Knight Dunlap.

16. For the Committee on Education and Special Training of the War Department, on adaptation of army intelligence tests for use in the Students' Army Training Corps, by Lewis M. Terman.

17. For various military and civilian agencies, studies of special problems in learning, methods of instruction, and methods of selecting for special tasks, by L. L. Thurstone, Herbert S. Langfeld, and Robert S. Woodworth.

## II. COMMITTEE SERVICE AND EXPENDITURES

The Psychology Committee depended chiefly on three instrumentalities for the conduct of its service and research: subcommittees, conferences, and appointments in the army or navy. The results obtained through these several instrumentalities will be presented under the subcommittees or special topics listed above.

Though many of the subcommittees have seemingly played unimportant rôles, it is nevertheless clear that this form of organization was indispensable for the instigation and main-

tenance of service. Appointments in the army and navy rank next in importance, for it usually was found desirable to have experienced and highly trained specialists administer for the army or navy the methods which were prepared and recommended by the Committee or its subcommittees. The conference has served the important purpose of bringing together, for a relatively short period, a group of individuals especially interested in and competent to discuss and advise concerning certain immediately important practical questions.

Although the activities of the Psychology Committee have extended over a period of nearly two years, its expenditures, omitting clerical help, office rental, and supplies which were furnished by the Research Council, amount to less than twenty-five hundred dollars. Of this sum approximately nine hundred dollars was used for the personal assistance of psychologists in the work of the Committee or of the Division of Psychology, Surgeon General's Office; about one thousand dollars for conference expenses, and the remainder for printing, preparation of charts, and incidentals.

### III. ORGANIZATION AND SERVICES OF SUBCOMMITTEES AND SPECIAL INVESTIGATIONS OF THE PSYCHOLOGY COMMITTEE

I. *Committee on Psychological Literature Relating to Military Affairs.*—This committee was originally authorized by the Council of the American Psychological Association, which at the same time designated Howard C. Warren as chairman. Because of Warren's inability to serve, Madison Bentley was made chairman of the committee. Bentley, although never assisted by an organized group, rendered important service by supplying the various committees and subcommittees with references to pertinent psychological articles and with summaries of the more important of such articles.

This work ceased when Bentley accepted appointment in the Signal Corps to assist with the work of the Aviation Examining Boards.

He had already succeeded in demonstrating to the satisfaction of psychologists who were directing the activities of

subcommittees that the psychological literature relating to military problems was not of great practical importance in connection with the emergency, because most of the tasks presented to or discovered by the committee demanded engineering of the pioneer sort.

2. *Committee on the Psychological Examination of Recruits.*—This committee of the Psychological Association was accepted as a subcommittee of the Psychology Committee of the Research Council in November, 1917. It was organized in May, 1917, with the following membership: Walter V. Bingham, Herbert H. Goddard, Thomas H. Haines, Lewis M. Terman, F. Lyman Wells, Guy M. Whipple, and Robert M. Yerkes, chairman.<sup>1</sup>

During June, July, and August of 1917, the committee prepared and directed the trial of group and individual procedures for psychological examination of recruits. Subsequently, various members of the committee<sup>2</sup> made indispensable contributions by assisting with the revision of the methods.

The final meeting of this committee was held on June 17, 1918, on which date it was called in conference by the chairman of the Psychology Committee of the Research Council to consider the scientific utilization of methods and results of mental testing in the army. At this conference, it was decided to attempt to preserve for subsequent careful analysis and statistical study at least 200,000 of the original records of psychological examinations of recruits. At the same time, the committee carefully considered and formulated for the guidance of the Division of Psychology, Medical Department of the Army, advice concerning the revision and utilization of methods of examining.

The methods prepared for the army by this committee, consisting of a procedure for the examination of large groups of subjects and also a series of tests for individual examination, were accepted by the Medical Department of the Army

<sup>1</sup> The committee was assisted in its early work on methods by N. J. Melville, E. A. Doll, and members of the Department of Psychological Research of the School for the Feeble-minded, Vineland, N. J.

<sup>2</sup> Notably, Bingham, Haines, Terman, and Whipple.

in August, 1917, for official trial. They have since been thoroughly revised and importantly supplemented in the light of extensive results.

During November and December of 1917 thorough military trial of these methods in four cantonments yielded results which led the Surgeon General to recommend to the War Department the extension of psychological examining to include "all company officers, all candidates for officers' training camps, and all drafted and enlisted men."

This extension was approved by the War Department in January, 1918, and at the same time a Division of Psychology was created in the Office of the Surgeon General to organize and direct this new variety of service.

To supply the requisite trained personnel for psychological examining, a school of Military Psychology was established at the Medical Officers' Training Camp, Fort Oglethorpe, Georgia. Approximately one hundred officers and three hundred enlisted men were given two months of intensive training in this school. The instruction included, in addition to training in procedures of army psychological examining, the courses in military drill, army paper work, and medical department administration, regularly required for medical officers of the army.

The methods were carefully revised during January and February, 1918, as a result of the official trial; the necessary equipment to supply approximately two hundred examining officers was manufactured, and the various forms of examination blank were printed in large editions in order that all examining stations should be adequately equipped.

Suitable buildings for psychological work were either assigned or constructed in the principal army training camps, and within a few months the service was satisfactorily organized.

The principal results of psychological examining may be summarized statistically and their applications briefly indicated.

The work of mental examining was organized finally in thirty-five army training camps. A grand total of 1,726,000

men had been given psychological examination prior to January 1, 1919. Of this number, about 41,000 were commissioned officers. More than 83,000 of the enlisted men included in the total had been given an individual examination in addition to the group examination for literates, for illiterates, or both.

Between April 27 and November 30, 1918, 7,749 (0.5 per cent.) were reported for discharge by psychological examiners because of mental inferiority. The number of recommendations for assignment to labor battalions because of low-grade intelligence was 9,871 (0.6+ per cent.). A total of 9,432 men (0.6+ per cent.) were recommended for assignment to development battalions in order that they might be carefully observed and given preliminary training to discover, if possible, ways of using them in the army.

During this same period of six months, there were reported 4,744 men with mental age ratings below seven years; 7,762 between seven and eight years; 14,566 between eight and nine years; 18,581 between nine and ten years. This gives a total of 45,653 (3 per cent.) men under ten years' mental age. It is extremely improbable that many of these individuals were worth what it cost the government to maintain, equip, and train them for military service.

The original purpose of psychological examining in the army was stated as follows in the preamble to the plan first submitted to the Surgeon General: "The Council of the American Psychological Association is convinced that in the present emergency American psychology can substantially serve the government, under the Medical Corps of the Army and Navy, by examining recruits with respect especially to intellectual deficiency, psychopathic tendencies, nervous instability, and inadequate self-control."

It was the expectation of psychological examiners that their principal service would be assistance in the prompt discovery and proper disposition of mental defectives. Long before the official trial of methods of examining had ended, however, it had become clear that various other applications were desired by officers of the line and that the significance of

psychological service would unquestionably be much broader than had been supposed. The official medical inspector of this work in his report listed its chief purposes as: (a) to aid in segregating the mentally incompetent, (b) to classify men according to their mental capacity, and (c) to assist in selecting competent men for responsible positions.

With the extension of psychological examining, these three lines of application rapidly became differentiated, and both line and medical officers discovered, for themselves or with the assistance of psychological examiners, new and important ways of utilizing mental ratings to increase military efficiency and to lessen the cost of training and maintenance. The principal practical uses of these ratings common to the majority of the training camps in which the service was organized are listed below:

(1) For the discovery of men whose superior intelligence warranted their consideration for promotion, special training or assignment to positions of unusual responsibility or difficulty; (2) for assistance in selecting suitable candidates for officers' training schools, non-commissioned officers' training schools and other special training organizations; (3) for the guidance of personnel adjutants in the assignment of recruits so that organizations might be built in accordance with desirable intelligence specifications or, in the absence of such specifications, so that their different constituent parts, such for example as the companies of a regiment, should possess approximately the same mental strength, thus avoiding the risk of weak links in the army chain; (4) for the prompt discovery of men whose low grade intelligence or mental peculiarities rendered them of uncertain value in the army, and the assignment of such individuals to development battalions for observation and preliminary training; (5) for the discovery and recommendation or assignment to labor battalions of men obviously so inferior mentally as to be unsuitable for regular military training, yet promising serviceableness in simple manual labor; (6) for the discovery of men whose mental inferiority unfitted them for any sort of military duty and whose rejection or discharge should therefore be recommended

to medical officers; (7) for utilization in connection with the organization of special training groups so that each group might be instructed or drilled in accordance with its mental capacity, thus avoiding the delay incident to dull or awkward individuals and enabling the especially able men to proceed rapidly and ultimately to take special forms of training in preparation for promotion or other forms of responsibility.

The judgment of the army concerning the practical value of this work has been very clearly indicated by reports of commanding officers. On completion of the official trial of methods in four cantonments, approximately seventy-five per cent. of the regimental and company commanders who were more or less familiar with the psychological ratings and their proposed applications expressed their approval of this new line of service and the opinion that it should be continued, extended, and its military usefulness increased. As the organization of the service was gradually perfected and the officers of the line and the Medical Corps became acquainted with its actual and possible values to them, the proportion of favorable opinion tended to increase.

The psychological service was so organized in each large training camp that a staff of four officers of the Sanitary Corps, six enlisted men, also trained in military psychology, and thirty to forty privates temporarily assigned for service as scorers, clerks, and orderlies, could, when necessary, examine and report on from one to two thousand recruits per day. During one month the total number of psychological examinations in the camps approached three hundred thousand.

The results of psychological examining in the army have two particularly important bearings. Their primary significance is in connection with the development and improvement of methods of mental measurement which are applicable alike to industrial, educational, military, and other practical situations. Equally important, however, with the advance in the development of methods, are the scientific results of this extensive mental survey.

The most important single achievement of the group of psychologists which developed the methods for army mental

testing is the creation of a practical, serviceable, and reasonably reliable method of group examining by which, if necessary, as many as five hundred individuals may be examined at one time.

The improvement in methods of individual examining due to the army work and experience is also extremely important, for more than eighty-three thousand individual examinations were made and the attention of a large number of expert examiners was concentrated for months on the perfecting of procedures and the increase of their practical military value. The Performance Scale for the examination of foreign and illiterate subjects, developed by army psychological examiners, has wide applicability. Like the group examination devised especially for subjects incapable of reading and writing English to any considerable extent, it marks a most significant advance in mental examining.

It is already evident that the contributions to methods of practical mental measurement made by this committee of the National Research Council, and by the psychological personnel of the army, are profoundly influencing not only psychologists, but educators, masters of industry, and the experts in diverse professions. New points of view, interest, and expectations abound. The service of psychological examining in the army has conspicuously advanced mental engineering, and has assured the immediate application of methods of mental rating to the problems of classification and assignment in our educational institutions and our industries.

If the full value of the labor on methods of mental measurement and the results of their use in the army is to be achieved, this work must be adequately reported and suitably published. The psychological staff of the Surgeon General's Office has made a tentative arrangement which promises to provide a satisfactory permanent scientific record.

It is proposed to prepare, first of all, a comprehensive official report of psychological service for the Surgeon General of the army and to recommend its publication by the Government. This report will present the history and organization



of psychological examining in the army; an account of the methods as originally recommended, as revised, and as finally applied throughout the army; the history of the organization of the School for Military Psychology, and of the appointment training and assignment of the psychological personnel of the Sanitary Corps, and, finally, a summary account of the results of psychological examining and their values to the army, together with recommendations or report concerning the relations of this service to the permanent United States Army. It is believed that this report can be limited to approximately three hundred pages.

For scientific purposes, it is planned to prepare more detailed reports on methods and results which it is hoped may be published as memoirs of the National Academy of Sciences.

The initial volume, the manuscript of which is nearly completed, will consist of three parts: (1) An introductory description of the inception and organization of psychological service. (2) A detailed description of the methods of army examining in the various forms in which they have been tried out. This part of the volume will reproduce the directions for examination and the several examination blanks, together with all other printed materials, norms, and other standards of judgment which are essential for the proper understanding of the methods and their uses. (3) As a third part, it is proposed to present a complete account of the results of official trial of the methods in four National Army cantonments. This will include a description of the principal phases of the early organization of psychological service in the army and of the general relations of results of examining to revisions of method and extension of the work to the entire army.

In a second volume it is planned to present results of the examination of approximately 1,600,000 soldiers. In addition to a general description of results, the volume will present a statistical study (based upon data secured with the Hollerith System) of approximately 200,000 records of examination chosen so that they adequately represent states, arms of the service, negroes and whites, and types of camp.

Finally, miscellaneous materials which cannot properly

be placed in the preceding volumes will be published either separately or as a third volume. It is not improbable that these studies should be placed in psychological periodicals rather than the National Academy series. So far as the time of the psychological staff permits, these miscellaneous studies will present results bearing on such generally important topics as the nature and distribution of illiteracy among negroes and whites, and the relation of illiteracy to intelligence; the intelligence of the negro as compared with that of other racial groups; the relation of intelligence to geographical distribution and its significant bearings upon educational, industrial, and other social problems; the geographical distribution of intelligence in relation to the newly arrived or partially assimilated immigrant; the relation of intelligence to occupation and the significance of army occupational classification and intelligence ratings in connection with the practical problems of securing and utilizing mental specifications for vocational guidance; the mental characteristics of conscientious objectors and their significance for military service, educational activities, and social or governmental obligations. These are only a few examples of the miscellaneous studies which should be based on the abundant data of examination and special report available in the Office of the Surgeon General.

The preparation of the several volumes and special studies enumerated above is already well advanced. It is hoped that the volume on methods and initial results may be ready for publication early in the spring of 1919 and that it may be followed promptly by the volume on the analysis of results, the official report, and the more important of the miscellaneous studies.

The above plan, it is believed, provides as adequately as is now practicable for the publication of the results of military psychological service in the interests of the army and of other governmental agencies as well as of education and industry.

3. *The Committee on Psychological Problems of Aviation, including Examination of Aviation Recruits*, was authorized by the Council of the American Psychological Association.

It was made a subcommittee of the Research Council in November, 1918. Under the chairmanship of Harold E. Burt, the committee, whose other members were W. R. Miles and L. T. Troland, undertook the selection and development of mental and physiological tests which promised *a priori* to be indicative of aptitude for flying. Various existing forms of apparatus were adapted for the tests and several new forms were devised and constructed. Although the primary intention was to proceed purely empirically to determine which tests were indicative of flying ability, it was also proposed to undertake the development of tests bearing on the mental and physiological state of the aviator during flight.

The evaluation of tests by trial on cadets at the Army Aviation Ground School, Massachusetts Institute of Technology, was begun early in June, 1917.

The following tests were given:

1. Patellar reflex with two stimuli in succession, a gradually decreasing interval between stimuli.
2. Electrical threshold.
3. Cardiograms and records of respiration while reclining and while "chinning" oneself.
4. Finger movement; first and second fingers moving together as rapidly as possible through an uncontrolled distance.
5. Swaying; standing with a helmet beneath smoked paper.
6. Visual acuity; Ives gratings.
7. Memory test (Dodge's); words exposed one letter at a time.
8. Inhibition of winking reflex.
9. Eye reactions to light; moving from fixation point to a spot of light which appears.
10. Speed of eye movements.
11. Ocular pursuit movements; following pendulum.
12. Reversed maze; tracing it visibly and then invisibly and rotated.
13. Association reaction with crucial words involving fear, falling, etc.
14. Motor learning; learning a fixed series of reactions with two alternatives by trial and error.
15. Auditory difference threshold with loud standard similar to the sound of a motor.
16. Distance and velocity estimation; moving target passes across

- opening at constant rate and then disappears; the subject tries to stop it when it has reached (invisibly) a given point.
17. Continuous choice reaction; each stimulus being produced by the preceding correct reaction.
  18. Tapping with index finger vertically between two fixed contacts.
  19. Equilibrium reaction time; subject sitting on platform which tilts suddenly; choice reaction to the direction of tilt.
  20. Simple visual reaction time.
  21. Simple auditory reaction time.
  22. Fatigue; ergograph with middle finger.
  23. Emotional stability; changes in pulse, breathing and arithmetical performance as affected by a revolver shot.

About seventy-five cadets were given the above series of tests. Arrangements had been made to correlate test performances with records from the flying schools as soon as these men learned to fly solo and also when they received the rank of "Reserve Military Aviator." Unfortunately the exigencies of the situation in Europe interfered with this program, since it became necessary to send some of the men direct from the ground school overseas. Records of flying ability were obtained for twenty-five of the men tested.

In the summer of 1917 the committee was reorganized. Burt resigned and George M. Stratton who had been working independently on tests for aviators at Rockwell Field, San Diego, was appointed chairman. Edward L. Thorndike was chosen as executive secretary, and John B. Watson, Warner Brown, Francis Maxfield, and H. C. McComas were added to the membership. Stratton had tested over fifty cadets in the following capacities: auditory reaction time, visual reaction time, emotional stability, steadiness and standing, perception of gradual tilt of the body as a whole, dexterity, and the power to continue in imagination certain segments of curves presented visually.

A combined score showed that of the six aviators who made the lowest scores, five men subsequently were relieved from flying because of failure to learn to fly. Stratton's results, together with the entire data of Burt, Miles, and Troland, and later the findings of Maxfield, were pooled for comparative analysis under the direction of Thorndike, who under-

took a statistical study of the relation between the results of the tests and the degree of success achieved in flying.

It was early foreseen that it would be necessary to select a number of tests, each properly weighted, as the practical means of predicting aptitude for flying. Provision was made by the army for a systematic and detailed tryout of promising tests by Captain Stratton and Captain Henmon at Rockwell Field and Kelly Field in coöperation with the Committee on Classification of Personnel. The personnel of the group on whom the tests were tried out included one hundred men chosen on the basis of their special skill in flying, one hundred chosen as relatively inapt at flying, and one hundred candidates of unknown ability. This work, carried on in the spring of 1918, resulted in provision by the War Department for further research by Captain Stratton, and the authorization of four special examining units to apply the tests to candidates for cadetship.

To Major John B. Watson of the subcommittee on avia-tional problem was assigned, in the summer of 1917, the task of organizing methods, other than medical, to be used by the examining boards for the selection of personnel. Watson also assisted in organizing a group of research psychologists to collaborate with physiologists and medical officers in the study of avia-tional problems at the Bureau of Mines, Washington.

Special mention should be made of the Psychology Section of the Medical Research Laboratory at Hazelhurst Field, Mineola, Long Island, which developed from the work inaugurated in Washington by Watson and his associates. At this station, Major Knight Dunlap<sup>1</sup> was primarily responsible for the development of a series of psychological tests to assist in determining the ability of candidates for the aviation service to withstand the effects of high altitudes. Oxygen insufficiency was produced by the Henderson rebreathing ap-

<sup>1</sup> For an account of the psychological work of this station see Knight Dunlap: "Psychological Research in Aviation," *Science*, N. S., 49, 94-97, Jan. 24, 1919; "Manual of Medical Research Laboratory." War Department, 1918, pp. 163-199, and "Medical Studies in Aviation," (IV. Psychologic observations and methods.) *Jour. Amer. Med. Assoc.*, 71, 1382-1400. October, 1918.

paratus, and the important resulting effects on attention and on voluntary sensory-motor coördination were made the basis of practical tests for rating aviators.

In general the method employed called for the performance by the subject of a group of continuous tasks involving co-ordinated reactions during the gradual decrease of oxygen supply. The composure of the subject and his ability to comprehend instructions were noted. Attention and motor tendencies were recorded on a fixed scale of types, as were also the moments when the effects of oxygen insufficiency attained a certain standardized importance, especially the final moment of 'complete inefficiency' which would have been followed quickly by a complete breakdown and unconsciousness if the reactor had not been given air.

The psychological section of the Mineola Station, at first under Major Dunlap and later under Major Stratton, trained and sent into the field units for administering these tests to aviation cadets. During the last half of 1918, beside the conduct of psychological tests of the ability of aviators to withstand high altitude and the consequent lack of oxygen, further research was carried on for the discovery of special aptitude for flying. To this end, experiments were instituted at Taylor Field, Montgomery, Alabama, under the immediate charge of Captain McComas, and at Souther Field, Americus, Georgia, under the immediate charge of Lieutenant Bagby with special apparatus and methods in the following regions: The judgment of differing rates of motion which intersect, pursuit movements of the hand, the power to trace and re-trace a given course, the strength and maintenance of a maximal grip, the time of complex reaction to visual signals. These experiments are now in progress and it is too soon to give their outcome even in summary.

J. F. Shepard coöperated with the subcommittee on aviation in devising tests for aërial observers. The Shepard method of modifying photographic plates is worthy of special notice.

Captain H. C. McComas of the subcommittee also conducted work along somewhat similar lines.

H. L. Eno and O. V. Fry developed apparatus for measuring the aviator's ability to point his plane quickly and accurately in a desired direction, as at an enemy plane.

Major Watson was sent to Europe to gather statistics on the qualities essential to success as a military aviator.

Dr. Parsons of the Navy received help from the committee in giving tests to every candidate for flying status in the naval air service. Parsons' study of the relation of the duration of nystagmus after rotation to flying ability yielded negative results, which are corroborative of Thorndike's findings, and supported by Dodge's analysis of nystagmus reactions.

From August 4, 1918, Thorndike served as chairman of the subcommittee on aviation. The Department of Military Aëronautics had requested that he be designated as expert to prepare and put into operation methods of psychological testing appropriate for examining the personnel of that department. To that effect he studied the system used by the aviatational examining boards for the selection of candidates in the air service and also the intelligence tests in general use in the army. Especially by modification and supplementation of the latter he developed a test of mental alertness to measure mental ability of the order requisite for success in the air service.

From the records of over two thousand flyers, Thorndike determined the relation between actual success in the work of a military aviator over the lines and age, social status, intellectual ability, business achievement, athletic ability, and many other characteristics.

A testing and rating plan, a part of the general plan for the selection and classification of officer material in the Students' Army Training Corps, was adopted by the Procurement Branch of the Personnel Section of the Air Service and was to have been put into operation in November, 1918, for the selection of over one thousand aviation cadets per month from the Students' Army Training Corps.<sup>1</sup>

#### 4. *Committee on the Selection of Men for Tasks Requiring*

<sup>1</sup> This account is supplemented by E. L. Thorndike: "Scientific Personnel Work in the Army," *Science*, N. S., 49, 53-61, Jan. 17, 1919.

*Special Aptitude.*—This committee was organized with the following membership: James C. Chapman, Truman L. Kelley, Walter Dill Scott, Edward L. Thorndike, chairman. It was constituted a committee on 'Tests of special skill' by the Executive Board of the Research Council in November, 1917.

Numerous problems of special skill dealt with by this committee ultimately led, through the activity of Thorndike and Scott, to the organization of the Committee on Classification of Personnel in the Army.

By request, the following summary account of the services of the Personnel Committee has been prepared for this report by Walter V. Bingham, Secretary of the committee.<sup>1</sup>

"The Committee on Classification of Personnel in the Army was created by Secretary Baker, August 5, 1917, as an instrument to increase the value of the army's man-power through securing the most effective placement of each man. This has demanded an exhaustive study of the entire army organization to determine where ability of various kinds is required, and the development and supervision of an army personnel system to discover the occupational, educational and military qualifications possessed by the recruits and to insure their assignment to the proper units.

"The Committee was organized with Walter Dill Scott as Director, E. L. Thorndike as Chairman, and W. V. Bingham as Executive Secretary. The other members were J. R. Angell, R. C. Clothier, Raymond Dodge, H. L. Gardner, J. F. Shepard, E. K. Strong, Jr., L. M. Terman, J. B. Watson, and R. M. Yerkes. The places of five of the original members have since been filled by J. J. Coss, W. R. DeField, W. B. Hale, P. J. Reilly and J. J. Swan.

"A large force of able and devoted civilian and military associates of the Committee, in Washington, in the camps, and overseas, has made possible the realization of its plans.

"For this program an initial appropriation of \$25,000 was

<sup>1</sup>An official account of this work is in preparation for early publication by the War Department under the general title "The Personnel System of the United States Army." There will be two volumes: I. The Evolution of the Personnel System; II. The Personnel Manual.



made, and as the scope of the Committee's responsibilities grew, additional appropriations were approved until the total amounted to \$851,650.

"While it happened that the original membership of the Committee consisted almost wholly of psychologists, many industrial and business specialists in employing, classifying, and assigning men were called upon to insure the successful prosecution of the work. This has included the following activities:

"(a) *Classification and placement of enlisted men.* Personnel offices have been established in all army divisions, depot and training camps, coast defense stations, aviation fields, special training camps, for Staff Corps and at other army posts. In these offices a special card system furnished accessible information as to the educational, occupational, and military qualifications of every man. With a minimum of clerical work this system selected 973,858 men for transfer largely into technical units in the Engineers, Aviation, Ordnance, and other Staff Corps, and even more men for transfer within the divisions or camps. Sixteen civilian supervisors, directed by the Committee, acted in an organizing and supervisory capacity in the field. Approximately 450 officers and 7,000 men were engaged in this personnel work. The number of soldiers interviewed by trained examiners and classified according to their best army usefulness was in all, approximately three and a half million.

"(b) *The allotment branch or central clearing office of the committee* in Washington received reports on the numbers of skilled tradesmen found in each contingent of the draft, received and consolidated requisitions from the Staff Corps for specialists, and prorated these requisitions among the various camps according to their supply of necessary skilled men. On November 11, requisitions for roughly 600,000 men of designated qualifications had been filled here. Information was available at any moment for the Operations Division of the General Staff concerning the occupational qualifications of all men in the several depot brigades, army vocational schools, and similar sources of supply.

“(c) *Trade specifications and index of occupations.* Definitions of the many hundred different trades needed in our military establishment were prepared after exhaustive study, and were brought together in a book, “Army Trade Specifications.” This index is an indispensable reference for Staff Corps and camp personnel officers in securing the skilled personnel needed.

“(d) *Tables of occupational needs and personnel specifications.* Tables were prepared showing in detail the needs for skilled workers in each sort of platoon, company, regiment, or other unit. These tables were studied, criticized, and approved by army units at the front in France, and later formed the basis for organizing quickly the newest divisions. Out of these occupational tables have developed the Personnel Specifications which have now been completed for the enlisted personnel of four hundred different kinds of organizations.

“(e) *Trade tests.* To increase the accuracy of selecting skilled workers among the enlisted men, a system of practical trade tests was devised, standardized, and installed in twenty camps. At the time of the armistice about 130,000 men who claimed occupational skill had been trade tested.

“(f) *Personnel work for officers.* Qualification cards for officers, furnishing a record of occupational, educational and military experience and a rating by superior officers, were developed and put into use throughout the army. These cards are filed in Washington, and duplicates filed in the custody of the Division commanders for their own use in assigning their officers.

“(g) *Rating of officers.* A uniform system of rating officers was developed. This rating system was first installed in the Officers’ Training Camps as an aid in selecting candidates for commissions. Later it was used in selecting candidates for Officers’ Training Schools. Now its use is universal both in America and in France as a means of securing every three months a rating on every officer as an aid in determining promotion, demotion, discharge, and appointments to the Officers’ Reserve Corps.

“(h) *Commissioned personnel specifications.* Definitions

of duties and qualifications of no less than 500 different kinds of officers in the various arms and branches of the service have been prepared, after ascertaining the answer to the question, "Just what does he do?" These commissioned personnel specifications are for use in recruiting officer material, in selecting men for training as officers, and making assignments. Statistical studies have been made of the relative significance of age, education, civilian earnings, intelligence and other qualifications of officers in the different corps and arms of the service.

"(i) *Coöperation with the air service.* The methods of selecting aviators were investigated and checked by reference to the actual success or failure of the pilots. An improved system of tests for aviation candidates was introduced and a new program of examination and selection installed.

"(j) *Coöperation with the Provost Marshal General's Office.* Plans for securing classificatory information regarding all registrants were submitted to the Provost Marshal General and were partially embodied in the draft questionnaire.

"(k) *Development Battalions.* The Committee coöperated closely with the General Staff and the Surgeon General's Office in preparing and introducing the plans for segregating, sorting, training, and utilizing the partially fit.

"(l) *Coöperation with the Surgeon General's Office.* Some assistance was given to the Division of Psychology of the Surgeon General's Office in devising and administering the intelligence tests for enlisted men and officers. The psychologists in turn have tested 1,726,000 soldiers and furnished personnel officers with their intelligence ratings to assist in making assignments, balancing units, and selecting men for special responsibilities.

"(m) *Coöperation with the Navy.* At the time of the armistice, representatives of this committee were assisting the Navy and the Marine Corps to prepare and install a complete personnel classification program to correspond with the army system. In addition, one member of the Committee has done valuable work in refining methods of selecting and training men for special duties in the Navy, such as gun-pointing,

hydroplane listening, the work of the fire-control squad, and the lookout.

“(n) *The War Service Exchange.* This branch of the Committee was established January 18, 1918, to receive and classify applications of persons desiring to serve the government and to refer them to the branches of the service needing them, and to coöperate with other agencies in locating and supplying men needed for special purposes by the various branches of the service. This organization relieved high officials of the War Department of the necessity of devoting valuable time to the interviewing of the many influential men who came to Washington to offer their services to the government. It also cared for a total of about one hundred and ten thousand written proffers of service. It placed approximately ten thousand men, including many of superior attainments.

“(o) *Personnel work in the American expeditionary forces.* Members of the committee studied the personnel needs of the A. E. F., and with the cordial approval of General Pershing, established there a personnel organization similar to that in America. The Officers' Qualification Cards have had their widest usefulness overseas, in supplying replacements and in locating rare specialists in emergencies.

“(p) *British experience.* Detailed study was made of the working of the British personnel organization, which in some respects is far superior to ours. Special reports and exhibits obtained in London from the British War Office covered their whole program of recruitment, classification, trade-testing, assignment and transfer; industrial furloughs; weekly consolidation and analysis of strength reports; and plans for demobilization.

“This in outline is a picture of the Personnel work, begun in the National Army cantonments with the arrival of the first contingent of the draft. Neither the civilians nor the army officers who initiated this development dreamed of the scope it would so rapidly assume or the share it would have in effecting the speedy organization of a well balanced army, trained and ready for the critical hour in France.

"The Committee on Classification of Personnel in the Army as such has disappeared. After fourteen months of service under the Adjutant General of the Army, it was transferred to the General Staff and merged with the Central Personnel Branch, newly created to supervise the procurement, placement, transfer and promotion of officers throughout all branches of the army. This means that centralized control of personnel work for both officers and soldiers is recognized and thoroughly established as an integral part of the United States Army organization."

5. *Committee on Recreation in the Army and the Navy.*—Originally authorized by the Council of the American Psychological Association, under the chairmanship of George A. Coe, this committee was organized with the following membership: William C. Bagley, Rowland Haynes, J. T. Patrick, J. H. Tufts, and the chairman.

On acceptance of his appointment the chairman formulated a plan of work which included important investigations and the establishment of profitable coöperative relations between his committee and the various civilian agencies concerned with recreational activities in military training camps.

Various unfortunate circumstances delayed the fulfillment of the original plan and it was finally decided that the Commission on Training Camp Activities and the Y. M. C. A. were satisfactorily meeting the urgent demands of the situation.

6. *Committee on Problems of Vision which Have Military Significance.*—Authorized in April, 1917, by the Council of the American Psychological Association, this committee was made a subcommittee of the National Research Council shortly after its organization. Its membership comprised R. P. Angier, H. A. Carr, L. R. Geissler, S. P. Hayes, G. M. Stratton, L. T. Troland, and Raymond Dodge, chairman.

The chairman of the committee most generously contributed his time and professional skill to the departments of war and navy. Since much of his practical work demanded secrecy and was imperfectly known even to the Psychology Committee, he has been asked by the chairman of the com-

mittee to prepare for this report a summary account of his war service. This contribution, which follows, is one of the most interesting chapters of the scientific history of the war. It is more detailed than other sections of this report because practically nothing has been published heretofore on most of the topics.

*Report of Lieutenant Commander Dodge*

"The subcommittee on vision shared with some of the other subcommittees in Psychology the disadvantages of mutual inaccessibility. Questions that were submitted commonly required an immediate estimate for prompt action. There was neither time for nor expectation of a complete or final solution. The military situations that we had to analyze were not infrequently of a highly confidential character. In some cases the suggestion that the matters be referred to a committee for investigation met with quite emphatic disapproval. Moreover, the initial visual problems that we came upon commonly turned out to be so intimately related to non-visual factors that they became the least important part of the investigation. So it came about that in spite of the enthusiastic coöperation of a splendid committee, the committee form of partitioned investigations in our case proved impracticable during war times. We had to work as individuals at top speed.

"In the early psychological war problems in whose solution it was my privilege to participate, the conspicuous features were the failure of military authorities to appreciate the reality of the mental problems, even when they were the main problems of a situation, and the failure of military traditions to meet the new problems of human engineering which modern warfare occasioned. The great service of the military psychologist was to analyze out of the situations the human and mental problem, to give these phases of the military problems precise formulation and then to bring to bear his laboratory and scientific traditions in meeting the situations as they were analyzed.

"The military danger in the next few years of peace is

that with the passing of the present crisis, so few military officers are capable of carrying on the mental researches. I fear that some other nation may take up the mental analyses where we left them when the emergency ceased, and may develop a real military psychology that will be more deadly than 42 cm. guns. Our efforts, however excellent and however valuable, are only the first crude beginnings of such a military psychology.

"In reviewing my war work correspondence, it is interesting and instructive to note that while the practicable means of attack was often delayed for months, and the practical occasion for developing the problem was often quite unconnected with the inquiry from the Research Council, every one of my leads for service came first from the Research Council. Furthermore, in spite of some moments of personal discouragement and misgiving, not one of the inquiries that came to me in this way proved either useless or impracticable. It seems to me that this is a very high tribute to the wisdom of the guidance of our chairman and his colleagues of the Research Council. The only serious difficulty in the path of scientific service was the initial lack of confidence of the authorities. When confidence was once established, their faith in our ability to turn the desired tricks became an embarrassment, and the only limit to service was the limit of human endurance.

"The months that I devoted exclusively to national service seem in retrospect a kind of scientific excursion. The shadow of sorrow at their close comes from the fact that with the rotation of military service the old chiefs for whom we worked and would have followed to the death if necessary are being replaced by strangers who know nothing of our limitations nor our scientific potentialities.

"Shortly after the Committee on Psychology was organized, Chairman Yerkes referred to me as an inquiry from Dr. Mendenhall of the National Research Council the question whether we could recommend tests to select gun-pointers for merchantmen. The records of our efforts to answer that inquiry and of the vastly greater efforts to get our answer across

are intensely human documents. The first was a relatively simple problem of analysis of reactions, adaptation of approved laboratory practice, and construction of a suitable recording device. Four days after the receipt of the inquiry I outlined the instrument that we used later, and I guaranteed its success. The second was a problem of salesmanship. Every gunnery officer who saw the instrument work was enthusiastic from the start, but it was months later before the official wall of suspicion and red tape was passed.

"On June 11, I reported the construction and successful trial of an instrument to test gun pointers. It gave a series of graphic records of the fundamental processes involved in training a gun on a moving target, under as nearly service conditions as possible. Simple inspection of these graphic records, without elaborate computation, showed (1) the latency of beginning the training of the gun to a movement of the target; (2) the accuracy with which the actual movement of the target was followed; (3) the latent time of reacting to a change in the apparent motion of the target; (4) the effect on the aim of pressing the firing key; (5) the accuracy of the aim at the moment of discharge.

"This instrument was set up and tested on expert marksmen and inexperienced recruits of the U. S. S. *Georgia* and the U. S. S. *Pennsylvania*. The records show marked individual differences. The best gun-pointers gave the best records; the untrained recruits the poorest. But among the partially trained, some consistently followed the moving target with short reaction time and considerable satisfactoriness; some showed promising improvement within the five trials permitted.

"The instrument consists of an aluminum recording drum mounted on a horizontal axis. This is turned at an even rate through an arc of approximately ninety degrees by a weighted piston falling in a cylinder of oil with a predetermined by-pass. The weighted piston as it falls also moves the target in a horizontal plane. Thus the target starts when the drum starts and stops when the drum stops. A long recording lever (one meter) writes on the drum with a lead pencil. This lever is moved by the person to



be tested in the endeavor to keep a hair line in the sighting telescope, which is mounted on the recording lever, on the center of the target. The reaction latency of the person to be tested is shown by the distance that the target and drum move before the recording lever starts. His accuracy is shown by the approximation of his record to a normal line which is recorded when the lever which carries the target is temporarily clamped to the recording lever. The moment of firing is indicated by the perforation of the record by a jump spark.

"The practical utilization of this instrument remained confined to the U. S. S. *Georgia* and U. S. S. *Pennsylvania* where it was tried out. It never got by the Office of Gunnery Exercises. Considerably later, I submitted a plan for a robust training model of this instrument to Captain E. L. Bennett of the Training Section of the Bureau of Navigation. The instrument was intended for shore training to offset the lack of dotter equipment in the naval training stations. Constructed by authority and at the expense of the Committee on Classification of Personnel, this instrument was set up in the Armed Guard Camp of the New York Navy Yard. It proved an instant success. Its use was developed to greatest efficiency at this place under Lieutenant Norton. Under orders from the Bureau of Navigation a number of replicas were built by the Armed Guard Camp of the New York Navy Yard for other training stations.

"The instrument consists of a battery of four skeleton guns, with training or pointing gear, a mechanism for giving the targets a series of harmonic wave motions of great variety and complexity, a recording device, that shows each movement of the target and the corresponding movements of the gun as the pointer or trainer tries to follow it, and a firing device, that not only indicates the accuracy of the pointing but also the effect of the effort to fire on the pointing coördinations.

"The object of the instrument is to furnish land conditions for teaching a recruit the coördinations of hand and eye essential to pointing and firing at a moving target at sea. It was designed as a robust practice instrument which could be repro-

duced in any desired quantity at relatively small expense, and without demanding the services of expert machinists for construction. The graphic record was added to show the pointer and his instructor the relative adequacy of the pointing at each moment. The movements of the target, the sights, and the training mechanism were designed to duplicate actual service conditions as closely as practicable in simple construction.

"Successive records of the same recruit furnish a record of his progress in acquiring the coördinations necessary for gun-pointing. Gross incapacity or natural aptitude will be apparent in such a series. The instrument thus serves the double purpose of a shore-training device and a test by which marked natural inaptitude may be discovered with a minimum loss of time.

"Since the instrument is quite robust it is practicable for interested recruits to practice gun-pointing with it at odd times by themselves and without supervision. Experience with the instrument shows that the recruits do take an interest in it and use it practically continuously when permitted to do so.

"The base of the apparatus consists of a rectangular pipe framework, four feet high, six feet long, and two and one half feet wide. Each corner post carries an adjustable seat for the gun-pointer, and a skeleton gun. The latter is really a peep sight built into an iron pipe. This may be trained by a two-handed belt gear. At its muzzle end each skeleton gun carries a light but rigid recording lever, armed on its distal end with a soft lead pencil for making the records.

"The targets for the four guns are carried on a platform that moves across their line of action. The device for giving the targets an irregular series of harmonic wave motions corresponding to the movements of a floating gun-platform consists of a motor which drives a set of three wooden pulleys. The first pulley serves merely as a reducing gear. The second and third pulleys carry eccentric attachments to pull the target platform a greater or shorter distance, at greater or less velocity, according as they work together or in more or less complete opposition. The slack of the eccentrics is taken up by a long spring, which is attached to the target plat-

form and works in opposition to the pull of the motor. As it worked out, this series of wave motions has been commented on most favorably by all the experienced pointers who have observed it. Without exception they have said that it was the best artificial target motion that they had ever seen on a dotter. By shifting the driving belt on the reducing pulleys, it is possible to imitate the movements of a slowly moving gun-platform or a rapidly moving one at will. Observers of experience name these speeds battleship roll, merchantman roll, and destroyer roll.

"Instead of moving the target platform directly across the frame, the track on which it moves is laid at an angle of forty-five degrees to the main frame. This makes the target travel the hypotenuse of a right isosceles triangle, of which the transverse leg would give the apparent displacement of the target from the standpoint of the trainer. The other leg in the direction of the axis of the gun furnishes the conditions for the record of correlated movements of target and gun. Thus a line traced by the recording lever which is attached to each gun and writes on a sheet of paper laid on the target platform, will represent point for point each movement of the target and the gun. If the gun remains stationary while the target moves, this record line will be an oblique line at an angle of forty-five degrees to the gun axis. If the gun moves while the target is stationary, the record will show a straight line transverse to the axis of the gun. If the gun moves exactly with the target, that is, if absolutely accurate pointing is maintained throughout the movement of the target, the record will be a straight line in the axis of the gun. This latter is an impossible ideal of perfect pointing. All the records show a greater or less number of irregularities incident to the reaction time of the pointer and the imperfections of his coördinations. Improvement of the pointer in the essentials shows itself in the gradual reduction of the lateral displacements of the record from the theoretically perfect record of a single straight line. The record of each performance may be analyzed for the benefit of the recruit immediately after each trial. For this purpose, it is probably expedient not to have the practice records too long. Short records are less complicated and may be more easily analyzed. Moreover, the recruit can remember better what he did if the record is not too long.

"Following the recommendations of Lieutenant Norton, U. S. N., the instrument was adapted so that the targets could be given a vertical motion for practice in pointing as

well as a horizontal motion for training. This was done optically by introducing a total reflection prism into the eye pieces of two of the skeleton guns. To reproduce for the gun-pointer the errors of his training mate, so that he could get practice in watching the vertical wire while firing, an artificial training error was introduced mechanically at fixed points of the motion of the target platform. At first thought this would seem to give the training error such a mechanical constancy that it could be predicted and therefore ignored. This is absolutely not the case. It follows the non-predictable character of the main displacement of the target, because the arbitrary training error will appear at every phase of the more or less extended movements of the targets, while on the records they will appear only in homologous parts of the curves.

"Firing records are introduced in the pointing records by pressing an electric button, which closes a circuit operating a small electric bell, only instead of hitting the bell, the buzzing armature hits the pencil holder. This makes a dot on the records when the gun is standing still, and a waved line instead of a simple line when the gun is moving. The firing will be perfect only when these dots and waves appear on a straight line along the theoretically perfect pointing line, at those parts of the record that are free from the artificial training errors.

"The artificial training error is produced by placing irregularities on the track of the target platform, over which a friction wheel glides. This is attached to a series of levers that add a vertical motion to the main horizontal movements of the targets. Since the optical system transforms the horizontal motion into a vertical one for the pointer, it transforms these slight vertical movements of the target into errors of training.

"The actual uses of the pointing instrument will probably vary according to the needs and inclinations of the groups for which they are available. Quite tentatively, I suggested the following: Entirely raw recruits should probably be introduced only to the training mechanisms, without recording all

their trials. The instructor can see all the main features of the training errors. Occasional regular records should be taken at this stage only to show the rate of progress or as the mechanical proof of the judgment of the instructor that the recruit was or was not fitted to go on. Probably the men who make the best progress, or some proportion of them, should then be introduced to the firing pointer's more complex coördinations. Each group should be encouraged to practice by themselves without records. The formal records of each group should be posted to stimulate competition. Each record should have the benefit of individual criticism. The records of the trainers will show if the recruit's attention lapsed; if he got mixed up in the manipulation of his gears; if he started too slowly; if he tried to beat the roll; if he was irregular and jerky—as well as the accuracy of his training. The records of the pointers will show similar faults in the pointing coördinations, and in addition, if he fired when the vertical wire was off; if he stopped pointing to press the firing button; if he failed to fire when on; if he fired off; as well as whether he failed to "follow through" sufficiently long after pressing his firing key.

"Undoubtedly others besides raw recruits would profit by occasional graphic records and their criticism, especially those whose work at actual target practice failed to give evidence of adequate coördinations.

"The following letter from Rear Admiral L. C. Palmer was a very welcome summary of the naval estimate of the instrument:

NAVY DEPARTMENT,  
BUREAU OF NAVIGATION,  
WASHINGTON, D. C., March 4, 1918

*My dear Professor Dodge:* This Bureau is in receipt of an official report from the Commanding Officer of the Armed Draft Detail at the Navy Yard, New York, containing a description of an instrument devised by you for the primary purpose of selecting from among recruits those who are naturally fitted for training as gun-pointers.

The report indicates that in addition to fulfilling its primary purpose, the instrument has proven of great value as a device for

training selected men, both as gun pointers and gun trainers. In this connection the report says in part:

'This instrument has been in constant use now for over a month. During this time it has been found to be of great value, not only for classification of pointers, but further, for their actual training. Despite the almost constant use of this machine by different persons, no parts have become worn out or broken. It further has the most important advantage of being popular with the personnel who are being trained, and it has been found that the enlisted personnel make use of this instrument upon their own volition outside of drill periods. The motion of the target, derived by the design of this instrument, is by far the best the Commanding Officer has ever seen, and this opinion is supported by various other officers who have experimented with it. The diagram which is obtained from each pointer or trainer tells precisely how close to the target the man has kept during his period and further whether or not he fired when on. The records of these pointers or trainers are kept from day to day and one is soon able to tell whether or not the individuals will ever pick up the necessary requisites for efficiency as such.'

You may be further interested to know that the Bureau has taken steps to have the experimental instrument reproduced and furnished as a standard training device for recruits at all large training camps.

I beg to take advantage of this opportunity to express to you our sincere appreciation of the value of what you have done and are doing to assist in the very heavy burden imposed upon the Bureau in the present emergency; and I may add that we are also deeply sensible of your spirit of unselfish devotion to the cause which we all have so deeply at heart. You have given us most freely of your valuable time and have, I suspect, made other personal sacrifices of moment, and have declined to consider any method of compensation. I hope that this wholly unsolicited expression of the Bureau's gratitude will not be quite unwelcome.

I am taking the liberty of sending a copy of this letter to the President of Wesleyan University.

Again thanking you, I am

Sincerely yours,

(Signed) LEIGH C. PALMER,

*Rear Admiral, U. S. Navy,  
Chief of Bureau*

"While we were trying out our instrument for testing gun-pointers it became increasingly evident that the problem of picking prospective gun-pointers was regarded by most naval officers with whom I came in contact as only one of a group of interrelated problems of picking the most suitable men for the various tasks of the Navy. This was first emphasized by Lieutenant Mayo of the U. S. S. *Georgia*; then by Captain Plunkett of the Office of Gunnery Exercises, and by Commanders Bingham and McClintic, and Lieutenant Shannon of the U. S. S. *Pennsylvania*.

"In view of these reiterated suggestions, and in view of the wide scope of the permission granted me by the Honorable Secretary of the Navy to visit the fleet for analysis of the naval tasks, I undertook to do for the plotting room what I had done for gun-pointing. After observing the various tasks of the plotting room, I tried to reduce them to their simplest psychological terms, then to devise corresponding test methods, and finally to combine them in a single form or blank that would disclose at a glance, without elaborate computation, the relative fitness of the several recruits for plotting room service.

"The tests finally recommended were: the ability to repeat clearly by telephone a series of ordinary commands that were received by telephone, the ability to remember and repeat numerals, to read a circular scale, to read a plotting scale and to lay off distances to scale, together with neatness and accuracy in drawing and subdividing simple geometrical figures. All these data, except the telephone test were arranged on a single blank which could be estimated at a glance as good, medium, and poor.

"It proved impracticable to follow the history of this test, to work up the correlations between test and performance, or to modify the test according to experience. I was informed later that the tests, substantially as originally submitted, were adopted throughout the Atlantic Fleet for the selection of men for the plotting room, and that they were saving a great deal of time and trouble.

"A plan was submitted at the same time for a more ex-

haustive series of tests for a considerable variety of naval tasks based on an analysis of the tasks. This plan would probably have been carried out to some degree at least, if the armistice had not cut short our naval 'career.'

"The connection between the subcommittee on vision and the Listeners' School is not altogether clear even to myself. But we were not fastidious in the selection of tasks provided they would help win the war.

"One of the minor but necessary tasks of the Training Section of the Bureau of Navigation was to find properly equipped men for the new Listeners' School without robbing other training schools of their regular quotas. It was a relatively simple problem in the economy of human material and personnel, but one for which no data were available. At the request of Captain Bennett, U. S. N., Chief of the Training Section, I analyzed the requirements of the Listeners' School.

"On the basis of that analysis, I elaborated a series of tests for candidates for the Listeners' School and was sent to various training stations to pick students from the enlisted personnel. After correcting the tests from the school experience with the first few quotas, I was able to make a detailed recommendation for the examination of candidates. With the cordial assistance of Naval medical officers in the several Districts, these tests afforded the Listeners' School a selected student personnel from which 80 per cent. to 95 per cent. of each class passed the course, all without seriously affecting the supply of suitable men for other naval schools.

"Without divulging anything that might be regarded as a military secret, the general plan of the tests is indicated in the following statement which has already been published by authority of the Bureau of Navigation.

1. Candidates for the Listeners' School were selected by the following processes:

- (a) Their Navy record in the Training Station.
- (b) Their education (or other indication of intelligence).
- (c) A special medical examination.
- (d) A group elimination examination.
- (e) An individual stethoscope test.
- (f) A final compensator test.



2. All candidates for examination are selected by their officers. Candidates must have:

- (a) The general training of a seaman, especially military discipline, care of a seaman's belongings and person, and the ability to handle firearms effectively.
- (b) General intelligence sufficient to profit by an intensive course of school work.
- (c) Loyalty to and interest in Naval work.
- (d) Enough self-reliance and initiative to carry the responsibility of the Listeners' task.
- (e) Enough team-spirit to make him livable in cramped quarters.

3. In the absence of other clear indications of the requisite general intelligence to profit by the intensive course of school training required of Listeners (2b), it is required that candidates have at least two years of high school. Other indications of superior intelligence would be a record of success at some electrical trade, or some business where the candidate had given evidence of superior mental ability.

4. The medical examination of candidates emphasizes the integrity and normal functioning of the auditory apparatus, with less emphasis on the visual equipment.

5. The group elimination examination aims to eliminate from the group selected by their officer those men who are least fitted for the special tasks of the listener.

The ideal examination would probably be to try out every candidate with the special apparatus used under the actual conditions of service. Since that seems to be impracticable, a set of tests had to be developed that would correspond to those tasks as closely as possible. After a considerable experience with the men selected by these tests, consequent to which the tests have been checked and corrected, it seems important to give them as uniformly as possible in all Naval Districts, and to make no intentional change either in substance or method until that change has been fully considered by the Bureau of Navigation and checked by correlation with the actual work of the men at the school.

6. The mental test consists of four parts.

- (a) The memory span for 4-, 5-, and 6-place digits.
- (b) The ability to read a circular scale to an error of one degree.
- (c) The ability to locate the source of sounds from behind a long horizontal screen.
- (d) The ability to discriminate between softly spoken or whispered syllables and nonsense words.

7. Candidates who reach a grade of 75 per cent. in this elimination test are given an individual examination in their ability to discriminate phase difference, with a stethoscope attached to a rubber tube. This individual test also serves the following purposes: (a) To check other sources of information covering the candidate's personal qualifications. (b) To estimate the quickness and certainty of his reactions.

8. The final process in the selection of candidates for the Listeners' School was conducted by myself. In this examination it is assumed that all candidates have the minimum qualifications detailed above. No regular effort is made to check these qualifications except in the matter of schooling. Before examining a candidate a brief typewritten statement of instructions is given to him. Failure to understand or to follow these instructions leads to a more careful review of the candidate's general intelligence.

9. The purposes of the final examination are:

- (a) To serve as a check on the preliminary elimination in the several Naval Districts and to standardize the results. That is: it sometimes happens that in filling its required quota one District is compelled to include some men of a relatively low grade. By comparison of all the candidates at one place it is possible to apply a common standard irrespective of the Naval Station from which they came.
- (b) To estimate a number of factors that could not easily be tested without special training and apparatus.

10. The instrument used for making the final examination is a simplified compensator designed for the purpose and is constructed as follows:

On a suitable standard two concentric wooden wheels are mounted. One wheel facing the examinee measures thirty cm. in diameter. This is his compensating wheel. The compensating wheel is operated by the examinee as though it were a real compensator. The other wheel just behind the standard and hidden from the view of the examinee measures 26 cm. The position of this wheel determines the difference in wave phase. It carries on its periphery the middle third of a rubber tube which is two meters long. The ends of the tube are carried through the hollow axis of the wheels to a stethoscope. The exact middle of the tube is plainly marked on the operator's side, and the tube is cemented firmly in place on the wheel. The concentric wheels can be turned independently to prevent secondary identification marks. Slight

friction in the bearings prevents any accidental lag. A noise roughly approximating the revolution of a submarine propeller is produced by rubbing the tube lightly with a bare lead-pencil lead. If a circular motion is given to the lead the number of revolutions as well as the noise quality can be imitated.

11. The candidate after reading his instructions sits at a table opposite the operator with the simplified compensator between them while the operator rubs the rubber tube at some arbitrary height above the table with the naked lead of a lead pencil. The candidate rotates his compensator wheel until the sound seems to be dead ahead. If the localization is perfect the center of the tube should be directly under the operator's pencil.

12. Preliminary trials must always be insisted on until the examinee has a clear idea of his task. If there is any indication of failure to understand the task the candidate is verbally instructed by the operator. But if inability to understand the task, inability to learn the direction in which the wheel must be rotated, or if gross faults of manipulation are persistent, the candidate is eliminated. The practice is to permit any failing candidate to repeat his examination at any later examination period as long as he is still eager to do so. Eagerness is a good indication. Unfortunately only about one candidate in twenty is able to better his record materially.

13. If a candidate locates the center within an average error of one cm., quickly, consistently, and without gross variations, he is passed by the examiner. If there is any persistent difficulty a static localization test is given, in which the following points are noted in four grades of excellence:

- (a) Right or left displacement of the center.
- (b) Breadth of the center.
- (c) Consistency of the reactions.
- (d) Quickness and sureness of reaction.

If the candidate passes this test he is then given another active compensation test.

14. A strict watch is kept by the operator for disqualifying personal qualities. No grouches, boneheads, discontented or notably immature candidates are knowingly sent on to the School.

"The success of this selective process has a rather interesting theoretical bearing. The task obviously lay outside my proper field, as I do not pretend to know the psychology of audition. Perhaps this was not a serious handicap since

if we had depended on our psychological traditions of the localization of sound we would have missed the point of the new localizing apparatus. As a matter of fact, I followed here as elsewhere the general principle of using a replica of the actual task for test purposes instead of any presumptively correlated tasks. The results seemed to justify the method. I believe that it is theoretically sound.

"In the spring of 1918 the Chemical Warfare Service of the Bureau of Mines made specific inquiry through the Research Council as to the importance of certain visual limitations of the standard production mask.<sup>1</sup> The matter was referred to me, and after consultation with various members of the Service, I undertook to answer the inquiry, and was appointed Consulting Psychologist. From the moment that I put on a mask it became obvious that the visual conditions of tenancy interacted with various psychological and physiological conditions.

"My report on the visual factors which are involved in continuous tenancy of gas masks aimed to summarize the relevant traditions of physiological optics, laying especial emphasis on the military advantages of peripheral vision, and discussing the relative faults of various windows, with respect to both material and position. Experimental investigation took a more general form.

"Relative to the total complex of tenancy problems, I suggested a combined investigation of the respiratory, metabolic, neuro-muscular, visual, and psychological effects of the gas mask. The obvious place for such an investigation was the Nutrition Laboratory of the Carnegie Institution, Boston. The plan met the cordial approval of the Chemical Warfare Service. In spite of probable interference with other forms of patriotic service, the Director of the Nutrition Laboratory generously gave the investigation the hospitality of the Laboratory, and authorized such help as his collaborators and assistants found it practicable to give. Very unfortunately, as I believe, the coöperative investigation failed to get started.

<sup>1</sup> This statement of Lieutenant Commander Dodge is supplemented by the data in section 15, p. 141, of this report.

But in spite of many difficulties the psychological program was initiated and carried through a month of intensive work.

"Throughout the investigation the subject was myself. A number of circumstances made this somewhat questionable procedure necessary. In the first place, I needed first hand information of the effects of the mask. Moreover, in my own case there was available abundant data on the normal variability of my responses. So I could evaluate a few experiments on myself better than on an unknown subject. Finally, the lack of available assistance made it impossible to begin in any other way.

"The observations were of two sorts: (1) A serial record of personal impressions. While this was quite inexact and liable to distortion by personal bias, it was the only criterion in matters of relative comfort and discomfort. Moreover, for an observer who is trained to avoid the commoner illusions of self-observation, the method will give considerable insight into the presumptive facts over a wide field. (2) In addition to these personal impressions, quantitative measurements were made of a group of selected processes that seemed likely to be significant on psycho-physiological or military grounds.

"When the investigation began there was no available tradition of the probable effects of gas-mask tenancy. Based on the previous studies of fatigue and of the psychological effects of alcohol, I selected the following processes for measurement:

1. The duration of the pulse and respiration cycles during rest, measured work, and the recovery after work. Pulse is the best-known indicator of general physiological condition. Our pulse records were electrocardiograms.

2. Visual acuity as measured by the grating test object. The relation of visual acuity to military efficiency needs no comment.

3. Simple reaction of the hand to a visual signal. This was undoubtedly too simple a form of reaction, but no suitable military task seemed available on short notice.

4. Eye reactions were photographed by the usual technique. They are involved in every adjustment of a soldier to events that he can see. It is a more complicated form of reaction than the simple finger reaction, is more universally practiced, and probably more significant in its changes.

5. The velocity of eye-movement. This is also a universal factor in adjustment to seen events. But it seemed important for other reasons. Dependent on the nice coördination of the orbital muscles, it is very sensitive to disturbances of the central nervous system.

6. The speed of the oscillations of the fingers. Like the eye-movements, it is a kind of coördination test with simple technique, and a flat practice curve.

7. The hand dynamometer. Frankly this was a makeshift. We expected to discard it for the Martin strength tests as soon as Lieutenant Richmond arrived.<sup>1</sup>

8. Continuous addition, a simple form of continuous work test with controlled association.

"The most consistent and largest effect of gas-mask tenancy was decrease of visual acuity, an average of 20 per cent. Addition was slowed 7 per cent. Eye-reactions were longer by 9 per cent. Eye-movements were 7 per cent. slower. In lesser degree the finger reactions, finger movements, and dynamometer strength tests were adversely affected, three, two, and one per cent. respectively.

Inspection of the curves for the several days shows a progressive adaptation to mask conditions that corroborates personal impression. This adaptation was both general and specific and seems to be of the utmost military importance.

"Of vastly greater importance than the fractional falling off in efficiency of the various processes was the effect of improperly made or improperly fitted head gear. Within one hour I had reached a degree of discomfort from an ill-fitting head gear where in spite of experimental interest in the task, in spite of patriotic sentiment, and all the scientific pride I could muster, I could stand the punishment no longer and simply took the mask off. The extreme military importance of such a condition of mind seems clear. A properly constructed and properly fitted mask can be worn almost indefinitely, after adaptation.

"It was officially reported that our study and the recom-

<sup>1</sup> This officer of the Sanitary Corps, psychological service, had been ordered to report to Lieutenant Commander Dodge for duty as assistant in the gas-mask investigation. He received his orders too late to be of service.

mendations that grew directly out of it were of substantial help in developing the modern mask. For military reasons I have deleted all reference to the several types of mask and their relative advantages as measured by our tests. I would record in this formal way the help of my assistant, Mr. Vincent B. Coffin, and the helpful counsel of Dr. Walter R. Miles.

"The last problem that was taken up concerned the condition of effective anti-submarine lookout service. This was one of the first questions that was suggested in the spring of 1917. But it was not until the fall of 1918 that official recognition of its importance made a practicable opening for taking it up seriously. When Commander Coffey, U. S. N., was assigned to command of the Eagle Boats he requested authorization of a School for Lookouts in connection with the training of the officers and men of those boats. After a preliminary survey a satisfactory plan was drawn up and the school was authorized by the Bureau of Navigation. It was to facilitate psychological work in connection with this School that I was commissioned in the Naval Reserve Force. There followed in succession, a first-hand analysis of the tasks of a lookout, authorization of a plan to collect the most approved naval practices of our own and foreign forces, organization to investigate aspects of the task on which naval tradition and scientific doctrine seemed least adequate, the elaboration of a course of instruction and the preparation of a manual on the School of the Lookout, and the development of new training devices and apparatus. All these processes had reached substantial development before the U-boat warfare suddenly stopped at the signing of the armistice, and I was released from active duty to resume my academic work. The open publication of any of this work is obviously impossible for reasons of military expediency.

"By request of members of the National Research Council or Naval officers, memoranda were prepared on: (1) the relative advantages of binocular and monocular glasses for lookouts; (2) differences in the vulnerability of different kinds of firing reactions to emotional disturbance; (3) an examination for the admission of candidates to the Pay Officers' School;

(4) the training of college men for the Naval Reserve Force, and other topics."

7. *Committee on Pedagogical and Psychological Problems of Military Training and Discipline.*—When this committee was authorized by the Council of the Psychological Association. Charles H. Judd was designated as chairman. He subsequently resigned, without organizing the committee, and on request of the Psychology Committee of the Research Council, William C. Bagley accepted the chairmanship. He secured the coöperation of A. Caswell Ellis and C. H. Judd as members of the committee.

In March, 1918, Bagley obtained from the Carnegie Foundation a grant of five hundred dollars for the study of conditions and methods of military training in typical camps. Truman L. Kelley and William R. Harper assisted the chairman in the conduct of this initial survey.

Preliminary reports from two camps indicated the desirability of scientific observation and practical assistance in connection with military training. It was thereupon arranged that Major Karl T. Waugh, psychologist at Camp Gordon, Georgia, should coöperate with the committee in an attempt to improve the procedure of military training and discipline. His work led directly to practical results, since officers of the line appreciated the existence of psychological and pedagogical problems and the value of expert advice for the improvement of training.

Unfortunately the work of this committee was so long delayed by failure to achieve satisfactory coöperative relations with the War Department and to obtain adequate financial support, that nothing was accomplished aside from the observational work in the above mentioned training camps and the holding of two conferences which proved to be of notable importance to the work of physical and mental reconstruction.

By request, Major M. E. Haggerty has prepared an account of these conferences as a part of his summary report on psychological service in reconstruction, which is printed below.

"In February, 1918, Major Haggerty reported to the Surgeon General for duty and was assigned to the Division of



Special Hospitals and Physical Reconstruction to organize psychological service for the reëducation of disabled soldiers.

"At his request W. C. Bagley arranged a conference on psychological problems of reconstruction, under the auspices of the National Research Council. The conference, which was held March 11 to 16, was attended by W. C. Bagley, J. W. Baird, B. T. Baldwin, Mabel R. Fernald, S. I. Franz, F. N. Freeman, M. E. Haggerty, E. K. Strong, Jr., R. M. Yerkes, and Helen T. Woolley.

"To this conference Major Haggerty presented a memorandum on psychological problems in the reëducation of disabled men. The principal problems designated were: (a) the problem of mental attitude; (b) the problem of functional reëducation; (c) problems involving social, vocational, educational adjustment; and (d) general problems of learning involved in educational work for disabled men. These groups of problems were assigned to subcommittees, with the understanding that reports should be submitted promptly. The reports proved valuable for the organization and development of psychological work in reconstruction.

"For further discussion of the problems of mental attitude a second conference was called by Chairman Bagley at the National Research Council April 8 to 11. The members of the conference were W. C. Bagley, J. W. Baird, Ida Cannon, B. T. Baldwin, T. H. Haines, M. E. Haggerty, E. G. Brackett, and Kendall Emerson.

"As a result of these two conferences a program was formulated and presented to the Surgeon General. This led directly to the authorization by the Surgeon General of the assignment of a psychologist to the Walter Reed General Hospital. Major B. T. Baldwin reported for duty at that hospital April 17, 1918.

"This was the beginning of psychological service in military hospitals. In the course of work at Walter Reed and other General and Base Hospitals where psychologists have since been assigned, the original program formulated in the conferences has been considerably altered. One of the most important developments is the organization of the psycho-

logical service as an integral part of the educational work of the hospital. This new organization, known as the Educational Service of Military Hospitals, includes practically all extra-medical and extra-surgical activities carried on in the institution for the benefit of patients. In many instances even civilian agencies, such as the Red Cross, work through the Education Department.

"As now definitely organized, the Educational Service includes a Psychological and Statistical Division. The personnel of this division in most instances consists of from one to four commissioned officers, from two to ten enlisted men, and one or more reconstruction aides. The functions of the psychological service as defined by the Educational Officer's Handbook are as follows: (1) to have primary responsibility for psychological and educational surveys of individual patients; (2) to coöperate with other educational and medical officers in the study of special learning problems encountered under the curative workshop schedule; (3) to have coöperative responsibility for all measures intended primarily to direct and control the mental attitude of patients; (4) to have responsibility for all statistical studies of procedure under the curative workshop schedule; (5) to have coöperative responsibility for professional training and advancement of educational staff and of teachers in training.

"There are at present engaged in this service in forty-three different hospitals, 66 officers of the Sanitary Corps, 72 enlisted men and approximately 10 reconstruction aides. Twenty-one of the commissioned officers are serving as Chief Educational Officers.

"Thus far the greater part of the work of this personnel has been the interviewing of patients who are subjects for the educational work of the hospitals. The facts obtained in these interviews are made the basis of all educational work prescribed for patients. They are reported on the Physical Reconstruction Register known as Form 58, which is transmitted to the Office of the Surgeon General on discharge of the patient.

"The essential features of this register are as follow: (a)

diagnosis of patient's ability and a medical officer's prescription for curative work; (b) educational and occupational history of the patient; (c) psychological examination covering general intelligence, special aptitudes, mental attitude and ability to learn; (d) the interviewer's recommendation as to future occupation and immediate training; (e) assignments to curative work and data on the progress of the case.

"The program for practical work on mental attitudes as formulated in conference has been developed and modified in accordance with the following scheme, the characteristics of which are indicated below by the principal lines of endeavor: (1) development of objective attitude, (2) development of attitude of self-confidence, (3) development of the attitude of individual responsibility, (4) development of the attitude of social acceptability, and (5) developing the rationalizing attitude.

"As matters have developed, the most important single agency for the direction and control of the mental attitude of hospital patients has been the Reconstruction Aide in Occupational Therapy. Approximately one thousand of these aides, all of whom are women, are now engaged in domestic and overseas hospitals.

"Originally the occupational aides devoted themselves to teaching handicrafts and the work began with what seemed to many individuals trivial forms of occupation, but as it progressed, new forms have been developed and important changes have occurred. The following is a list of the chief groups of craft work now taught in military hospitals: (1) work with textile materials, as, for example, in weaving, knitting, crocheting, netting, knotting, embroidery, and rug making; (2) reed, cane and fiber work, as in basketry, chair caning, etc.; (3) woodworking, as in carving, toy making, joinery, etc.; (4) cardboard construction and binding, as in book-binding, novelty box work, paper folding and pasting, etc.; (5) work in applied pattern; (6) metal work; (7) work in plastic materials; (8) leather work; and (9) work in drawing, lettering and designing.

"Reconstruction aides are now engaged, in addition to

the teaching of crafts work, as teachers of commercial arithmetic and technical subjects. The chief value of the crafts work in military hospitals is psychological. The other subjects have strictly educational values.

"In this connection there should be mentioned the services of psychologists in measuring the progress of improvement in voluntary movement of disabled joints. This work, which was initiated originally at the Walter Reed General Hospital under the immediate direction of the late Professor Baird, has been introduced in several other military hospitals.

"A recent addition to the personnel of the Educational Service is the medical social worker, who has been brought into the hospital through the Psychological Division as a direct result of the second conference called by Chairman Bagley. The position of the social worker is now definitely established and her duties defined by a circular letter of instructions issued by the Surgeon General on January 18, 1919. From this letter the following sentences are quoted to indicate the scope of her service: 'It is thought that Reconstruction Aides operating as medical social workers may be available: (1) in assisting to coördinate the various educational and related activities within the hospital so that they may serve a larger number of patients more efficiently; (2) by bringing to the attention of outside agencies, such as the Home Service of the Red Cross, the Y. M. C. A., the Knights of Columbus, the Jewish Welfare Association, the Federal Board, etc., the cases of soldiers who are in need of the types of service which these agencies are prepared to render; (3) in assisting medical officers as desired to secure such personal and social data about the patient as will assist in accurate diagnosis; and (4) in rendering such other services as may be assigned to them . . . '"

8. *Committee on Psychological Problems of Incapacity, including those of Shell-shock and Reëducation.*—The Council of the American Psychological Association originally authorized this committee under the chairmanship of S. I. Franz, and also a Committee on "Psychological problems of vocational characteristics and vocational advice" with J. B. Wat-

son as chairman. By request of the chairmen, these two committees were combined under the above title and the following membership was arranged: K. S. Lashley, J. B. Watson, and S. I. Franz, chairman.

The activities of the committee continued to August 4, 1918, when it was reorganized with the same membership as a subcommittee on "Problems of Reëducation."

Assistance was afforded the War Department by this committee in the following directions: (a) Systematic instruction of neuro-psychiatric groups in the work of rehabilitation by Franz at St. Elizabeths Hospital; (b) the development and application of methods for the reëducation of aphasics and paralytics (report on this subject is being prepared for publication by Franz); (c) conduct of investigations on the effect of injury to the brain, with special reference to the relation of experiments on monkeys to cerebral war injuries; (d) arrangement with the Medical Department of the Army for the preparation of motion pictures of animal and human subjects to illustrate varieties of incapacity; (e) preparation of a program for reconstructional experimentation which was subsequently presented to the Chief of the Division of Special Hospitals, Office of the Surgeon General; (f) Coöperation with Major Haggerty of the Division of Special Hospitals in the formulation of plans and the preparation of methods for psychological service in reëducation; (g) Lashley of the committee extended Franz's reëducational studies to crippled children and also conducted investigations on animal reëducation after cerebral extirpation and the reacquisition of muscular control after cerebral lesions.

No one of the above lines of service or research can be adequately reported here, but it is understood that suitable accounts of the work will be published in scientific periodicals.

9. *Committee on Problems of Emotional Stability, Fear and Self-control.*—As originally organized this committee of the American Psychological Association consisted of W. B. Cannon, G. S. Hall, J. B. Morgan, J. F. Shepard, and R. S. Woodworth, chairman. In August, 1918, it was reorganized as the subcommittee on problems of emotional fitness with the fol-

lowing membership: E. G. Boring, H. L. Hollingworth, and R. S. Woodworth, chairman. Important assistance was rendered the committee by Captain A. T. Poffenberger, Major J. W. Hayes, and Dr. Josephine Curtis Foster.

This committee set itself the peculiarly difficult task of devising means which should aid in the prompt discovery and segregation of recruits whose emotional characteristics indicated unfitness for military service. A Personal Data Questionnaire was devised. Subjects, examined in large groups, were required to answer by "yes" or "no" a series of personal questions intended to disclose psychopathic or neuropathic tendencies and such emotional instability as might result in breakdown under the strain of warfare. Men answering a considerable number of these questions in the atypical way were to be referred for intensive neuro-psychiatric examination. No single unusual answer to the questionnaire was to be interpreted as indicative of psychoneurosis or neurosis, since each manifestation inquired about is reported by an appreciable proportion of presumably normal individuals. But the larger the number of such manifestations the more probable would it be that the individual has serious neurotic tendencies.

The correctness of this initial assumption was established by careful trial of the preliminary form of the questionnaire. One hundred and sixteen questions of the original list survived a thoroughgoing inquiry by a joint conference of psychiatrists and psychologists who passed on the statistical validity and genuine practicability and value of each question as shown in the preliminary trial on one thousand men at Camp Upton and on various groups of psychoneurotic and psychotic subjects. For example, any question was eliminated on the ground of doubtful significance which failed to separate individuals into a large majority, presumably of normals, and a small minority of atypicals, insofar as the characteristic under consideration was concerned. Judged by this criterion the following questions were among those found significant: Do you feel sad or low-spirited most of the time? Are you ever bothered with the feeling that people are read-

ing your thoughts? Did you ever walk in your sleep? And the following were for like reason rejected: As a child, did you have dreams that frightened you? Have you ever been engaged to a girl? Do you like to be the leader in whatever is going on?

The conference of psychiatrists and psychologists, at which the questionnaire was revised, authorized the printing of twelve thousand copies which were to have been placed in immediate use at several camps. Cancellation of drafts and the termination of psychological service in the field prevented the complete execution of this plan. Important results were obtained from the examination of five hundred negro recruits, from about five hundred soldiers invalided home from overseas for nervous disturbances and examined at General Hospital number 30, and from neuro-circulatory asthenics at Camp Upton, as well as from normal subjects.

Work on the Personal Data Questionary will continue, at first in the direction of determination of norms, since the method undoubtedly possesses considerable value as a means of discovering and segregating subjects who should be given special neuro-psychiatric examinations. The chairman of the subcommittee, who has been chiefly responsible for this work, will prepare an adequate account of the method and its results for publication in a suitable periodical.

10. *Committee on "Propaganda Behind the German Lines."*—This subcommittee, consisting of Scott and Angell, chairman, was appointed in December, 1917, by the Psychology Committee of the Research Council on motion of Dodge. This action followed urgent requests of Hall of the Psychology Committee that psychological assistance be rendered the War Department in connection with problems of military and civilian morale.

Prior arrangements of the War Department rendered it impossible for the subcommittee to accomplish anything in the direction originally indicated by the committee's discussion of needs, but subsequently members of the Psychology Committee were able to focus the attention of various officers of the General Staff on psychological aspects of the morale problem.

The first noteworthy event in the control of military morale traceable indirectly to the influence of psychology in the army, was the preparation in March, 1918, by Colonel E. L. Munson, of the Medical Corps, of a memorandum entitled "Need for a systematic plan for the psychological stimulation of troops in promoting fighting efficiency." This memorandum, which was prepared for the Surgeon General, was referred to the Division of Psychology, and after endorsement by that Division forwarded to the General Staff for consideration.

While Colonel Munson's memorandum was under advisement, representatives of the Psychology Committee were able to further the interests of psychological morale through numerous conferences with interested army officers and through the organizing at the suggestion of Dr. F. H. Keppel, Third Assistant Secretary of War, of two group conferences for the discussion of the morale problem and of ways of meeting the military need for enhanced morale.

These conferences were followed by favorable action with reference to Colonel Munson's recommendations, and there was ultimately established in the War Department the Morale Branch of the General Staff with Brigadier General E. L. Munson in charge.

Thus indirectly the Psychology Committee succeeded in stimulating military interest and in acquainting the War Department with morale needs to a degree which shortly led to most important official action.

In August, 1918, the Psychology Committee voted to organize a subcommittee on Morale under the chairmanship of G. Stanley Hall. Unfortunately he was unable to assume this responsibility and the committee was never appointed.

A considerable number of men trained in military psychology were also trained at the Medical Department Training Camp, Fort Oglethorpe, Georgia, for practical work in military morale. Conspicuously important among the officers engaged in this work were Major William S. Foster, Captain Elliot P. Frost, and Lieutenant H. D. Fryer. At various times as many as twenty-five trained psychologists were en-



gaged in this type of work. Still later several psychologists were placed on duty with the Morale Branch of the General Staff. Major Foster and Captain Frost rendered important service in this connection by organizing morale work in various camps. Major Foster also directed the study of military offences from the psychological standpoint, with a view to discovering their chief causes in order that appropriate steps might be taken by the War Department to eliminate or control them.

Morale is one of the subjects in which several members of the Psychology Committee, notably Hall and Dodge, were keenly interested from the onset of the military emergency. It is also a field of service in which the direct outcome of committee action is seemingly of trivial importance. Indirectly, however, the Committee through the interest aroused in War Department officials has achieved important service.

II. *Committee on Acoustic Problems of Military Importance*, consisting of R. M. Ogden, C. A. Ruckmich, Daniel Starch, Raymond Dodge, and C. E. Seashore, chairman, was not called upon to perform war work through committee organization. The chairman reports as follows:

"Immediately after the outbreak of the war, the chairman interviewed the officers in charge of the Training School for Listeners at New London and observed the types of acoustic instruments in use, the methods of using these instruments in locating submarines, and the method of selecting listeners for the training school. On the basis of these observations, a report was made to the Psychology Committee embodying suggestions for improving certain instruments then in use and for modifying methods and means of selecting listeners. The chairman later presented, through the Psychology Committee, a report upon preliminary experiments for the various methods that had been recommended for use in the selecting of listeners and submitted instruments which have been adapted for this service. One of these instruments was an audiometer designed in coöperation with Professor A. H. Ford. Two research assistants, Mr. H. M. Halverson and Mr. C. C. Bunch, were employed in the laboratory of the chairman for the dura-

tion of the war, devoting their attention largely to these problems.

"Perhaps the most significant contribution is an instrument and a method of measuring acuity of hearing at all pitch levels very quickly and accurately. This is a problem on which Mr. Bunch was working in coöperation with the chairman, Dr. L. W. Dean, and Professor A. H. Ford. A report on this instrument was transmitted by the chairman of the Psychology Committee to the Surgeon General of the Army.

"Another problem undertaken in coöperation with Professor Ford, in charge of the school for radio-telegraphers, was the development of a series of tests to diagnose fitness for the radio service before the training of the cadet was undertaken. A report showing the operation of these tests was submitted to the Psychology Committee.

"The chairman has worked in close coöperation with Professor G. W. Stewart, who has presented a report to the Research Council on the selection of listeners for the detection of aëroplanes."

12. *Committee on Tests for Deception.*—John F. Shepard, chairman, assisted by H. W. Crane and Mabel Goudge. This subcommittee was appointed to make inquiry concerning the reliability and practicability of certain procedures proposed by William M. Marston for the detection of deception.

By invitation of the chairman, Marston presented to the Psychology Committee a summary report on his methods and the results obtained in laboratory experiments. The purpose of his report appears in the initial sentence: "I respectfully submit that three psycho-physiological deception tests (association-reaction, breathing, and blood pressure) are of sufficient proven value to warrant practical application."

In response to request by Marston that the Committee arrange for adequate trial of his methods, either by the War Department or by the Department of Justice, he was asked to make application of his methods to a number of cases of actual crime, and to report the results to the Committee. This was promptly done, with positive results, and the Com-

mittee thereupon appointed a subcommittee consisting of Angell and Yerkes to attempt to make provision for the tentative application of the Marston procedures by the Department of Military Intelligence.

Initial efforts to effect suitable arrangements through the War Department failed. Thereupon attention was directed to the Department of Justice, but early favorable response failed to lead to definite arrangements.

Finally Marston was commissioned in the Sanitary Corps for psychological service and while in training at Camp Greenleaf, Georgia, was able still further to supplement his earlier laboratory and court observations. The method involves the measurement of blood pressure during systematic cross examining. Ordinarily the Tycos sphygmometer is used and accurate record is kept of the subject's verbal responses so that variations in blood pressure as read at intervals or as recorded continuously may be studied in relation to the verbal responses.

The percentage of correct judgments, with reference to guilt or innocence, reported by Marston is extremely high. Thus in the case of the Greenleaf experiments, of thirty-five men tested, nineteen without knowledge of the examiner had chosen to steal, while sixteen were innocent and told the truth under cross examination. On the basis of blood pressure curves, Marston made thirty-four correct judgments, a percentage of 97.1. These findings are supported by those of previous carefully controlled laboratory experiments and of several instances of actual crime.

The subcommittee appointed to thoroughly investigate the Marston procedures has not been able to complete its task and no report is available for summary or publication.

The deception tests, because of their applicational history, did not command the confidence of all members of the Psychology Committee. To this fact may be attributed the conservative position taken, which involved first the desire for trial under the conditions of real life, as contrasted with the experimental laboratory setting, and finally, the recommendation of tentative trial by the Government. It is ex-

tremely regrettable that such trial could not have been made, for the high percentage of correct judgments reported by Marston strongly suggests the probability of the practical serviceableness of blood pressure tests in connection with trial for military offence.

13. *Committee on Adaptation of Psychological Instruction to Military Educational Needs.*—Late in August, 1918, the Chairman of the Psychology Committee received information from the Chairman of the Section on Relations with Educational Institutions that psychology would be listed as an "allied subject" of instruction in the Students' Army Training Corps. This information carried with it the suggestion that a training course in psychology suitable for use in the S. A. T. C. be prepared.

Following a conference on this subject, attended by members of the Committee on Education and Special Training of the War Department, members of the Psychology Committee of the National Research Council, and representatives of the American Psychological Association, the Chairman of the Psychology Committee appointed the following subcommittee to prepare appropriate methods for the Committee on Education and Special Training: J. W. Baird, E. K. Strong, Jr., L. M. Terman, E. L. Thorndike, G. M. Whipple, and Raymond Dodge, chairman.

This committee faced the task of arranging immediately for a training course in psychology which should conform to the general policies of the War Department committee.

Members of this subcommittee were promptly summoned to Washington for conference and it was decided to recommend the following three courses: I. The study of human action; II. Educational psychology; III. The psychology of reason. Each course was planned for a term of twelve weeks with three recitations and six hours of study per week.

The course entitled the "Study of human action" was carefully planned by the Committee in accordance with the experience and opinions concerning military needs set forth below.

"It is believed that the psychological principles which

underlie established military practice as well as the principles on which the recent mental examinations and tests are based, should be brought together in a course in the study of human action for the S. A. T. C. and made available for all prospective officers including those who will spend only one quarter year in college. That is the idea of Course I.

"It is consequently recommended that the elementary course in 'the study of human action' be given in institutions which have the necessary equipment, omitting as far as possible all questions of a purely speculative or theoretical nature and concentrating on the relevant questions of applied psychology.

"While it is desired to leave each instructor the greatest freedom for personal initiative, it is recommended that reading assignments, formal reports, and recitation discussions be used rather than lectures, and that wherever practicable concrete military problems be used for illustration.

"It is further recommended that in contrast with the common plan of elementary courses in psychology, the emotions and the will be emphasized instead of the intellectual processes, and that the course be applied psychology rather than systematic.

"The subjects which are regarded as essential to such an elementary course in human action may be outlined under six sections.

1. The general characteristics of personal action and the conditions of effective reaction to new situations.
2. Individual differences and their military exploitation.
3. The learning process. General characteristics of habit formation. Plasticity. Fluctuation. Improvement and its limits. Fatigue.
4. Motivation and morale.
5. Principles of leadership.
6. The sources and critical estimate of information. The psychology of observation and report."

An arrangement was further made that materials for a fundamental course in human action as outlined above be prepared and made available for teachers in the Students'

Army Training Corps as promptly as possible. The sections of this course were assigned as follows: Section 1, Dodge; Section 2, Thorndike, Terman, and Strong; Section 3, Strong; Section 4, Hall; Section 5, Lindley; Section 6, Whipple. The editor of the *Psychological Bulletin* agreed to publish this text-book material. Sections 1, 2, and 6 have already appeared. It is planned to complete the preparation of this material and, if the demand justifies it, to reprint the entire course as a volume.

The plans and materials for this course represent in a most interesting and valuable way the results of contact with psychological military problems and the attempt of psychologists to adapt themselves to a new and unexpected practical demand.

14. *Psychological Service Rendered the Division of Military Intelligence.*—Request for psychological assistance in the selection and training of scouts and observers was received both by the Division of Psychology and also by Whipple of the Psychology Committee from the Chief Intelligence Officer of an Army Division. In response to this request, advice and definite suggestions concerning methods were offered by Whipple, and the chairman of the Psychology Committee conferred with the intelligence officers of the War Department in order to formulate the chief psychological problems of the Division and arrange for their solution.

The Division of Military Intelligence later requested the assignment of a psychologist, in the military service, to prepare training tests to be used in the schools for intelligence officers. Major Watson and Captain Bentley were assigned to the task. They jointly prepared special tests which have been fully described in a chapter of the "Combat Intelligence Manual" of the War Department.

The materials of this chapter, including the tests, were selected and arranged for general instruction, special training, and as a basis for selection. The purpose of the several exercises presented is to test and develop visual, aural, and mental acuity.

The first few paragraphs of this chapter will serve to indi-

cate both the scope and the purpose of the psychological labors undertaken by Watson and Bentley.

"An important function of a school of Military Intelligence is the training of the men to be *generally alert* and wide awake; to teach them to use their brains as well as their eyes and ears; and finally to show them that, even when they are doing their very best, they are liable to certain errors in observation, which arise partly through their own eagerness and expectation, and partly through slight defects in the sense organs themselves. The present simple course of training is designed to bring out these points and to give both training in the use of the eye and the ear, and in the noting and correct reporting of small details in observation. The exercises are all simple to give and they require neither extensive equipment nor a burdensome amount of work on the part of the group under instruction. The most important thing which can be said to the instructor is that he should thoroughly familiarize himself with such exercises before he attempts to instruct the group. The work can then be carried out in a dignified and impressive way. It is safe to predict that if the men are taken through all of the exercises in anything like a satisfactory way they will come out with a very much quickened ability to observe and a very much better estimate of their own abilities and weaknesses. They should be then in a much better frame of mind for receiving their more practical field instructions.

"The opening pages are devoted to some of the uses and *peculiarities of vision*. The intelligence scout or observer on duty at night depends to a great extent upon vision. It seems only fair to acquaint him with some of the more common functions of the eye.

"After this lecture has been given the instructor may proceed as rapidly as he can with Training Tests A, B, C, and D, which are designed both to test *general alertness* and to *train* the men to make close and detailed *observations*.

"Training Test E is designed to discover and to improve the men's ability to *find their way* and to *keep their bearings* in the dark.

"Training Test F deals with hearing. It is designed to train the men to detect and localize faint sounds and to guard them against purely imaginary sounds."

The several tests referred to above deserve characterization:

Test for A is preliminary training in noting and naming various combinations of colored lights. It involves the use of a series of lamps in connection with a keyboard. The chief purposes of the test are to cultivate alertness, accuracy, and speed in reporting observations; to test and improve immediate memory and in general those functions which are most important in correctly observing and reporting the combinations of enemy rocket signals.

Test B is an adapted form of the army Picture Completion test. It is presented by means of lantern slides and its purpose is to increase the rapidity with which men notice details and detect obscure objects.

Test C is an adaptation of the army analogies or logical relations test. It also is presented by means of lantern slides and its chief purpose is to increase mental alertness. At the same time it is supposed to improve ability to think and to draw correct inferences.

Test D deals with estimates of numbers. Groups of men are presented by means of lantern slides and the subject is required to estimate quickly the number of individuals when position, attitude, and background are varied.

Test E has to do with orientation. It is at once a measure of the man's ability, and a means of training him to hold his directions and to find his way under varied conditions.

Test F is planned as a measure of ability to detect and localize sounds (whistle and whisper). It is used also to train men to detect and localize sounds in the dark.

The services of Watson and Bentley were much appreciated by the Division, and the tests constitute an interesting contribution to the practical literature on the training of intelligence officers. It is probable also that the test materials will prove convenient and of value in various other types of situation.



An additional psychological contribution to the training of intelligence officers was made by Carl Rahn, wholly independently of the Psychology Committee. He prepared a series of lectures and exercises for the men in the Intelligence Section at Camp Grant. Each of the lessons included (1) the statement of some important psychological principle; (2) an illustration to clinch the point; (3) a statement of applicability to scout and patrol; and (4) an exercise based upon the practical work of the detail. The officers in charge of this course at Camp Grant were of the opinion that the psychological work could be incorporated largely in the training course for men in the Intelligence Service.

The Rahn materials were transmitted to the Psychology Committee of the Research Council through James R. Angell and the Chairman of the Committee forwarded them to Watson, who at the time was in the service of the Division of Military Intelligence.

15. *Psychological Assistance to the Chemical Warfare Service.*—Dodge, Baird, and Dunlap assisted the Chemical Warfare Service by a special investigation of problems referred by that service and the preparation of reports and recommendations.

This work was initiated in response to an appeal from an officer of the Chemical Warfare Service for assistance with certain psychological problems connected with the tenancy of the gas mask. The chairman of the Psychology Committee referred the matter to the subcommittee on Visual Problems. Dodge, of that committee, after preliminary analysis of the situation, conducted certain special investigations and submitted reports, the chief recommendations of which have been embodied in the latest form of mask. The following introductory statements indicate the important characteristics of the practical situation and the scope of Dodge's inquiry:<sup>1</sup>

"Except for a few sporadic experiments, the mental and neuro-muscular effects of wearing a gas mask have not been investigated. We know neither the effects of the mask as a

<sup>1</sup> See also Dodge's account of the gas mask inquiry, p. 120, above.

whole nor the effects of its several parts. There are consequently no traditions to guide us either in the selection of the processes that are likely to be most affected, or in the order of the experiments. Even military traditions are relatively scant. There are no standard experiments that correspond to definite military tasks. There is no way of telling whether a given experimental disturbance is or is not of military consequence. Furthermore, there are no standard masks, fittings, or harness. There is no adequate analysis of the psychological and physiological problems that are involved.

"A serial investigation of the neural and mental effects of each part of the various masks now in use, or proposed for use, would be an investigation of some magnitude. It is doubtful if there are any available methods by which it could be satisfactorily completed. At present such an investigation is apparently particularly impracticable. The situation demands immediate results, not exhaustive programs. We lack available laboratory facilities and personnel as well as time and techniques.

"In view of these considerations, I proposed and have initiated an investigation consisting of two parts, calculated to give the most important information in the least possible time.

"Part 1. A preliminary experimental exploration of the effects of as many different types of mask as may be called for, using a single observer, techniques that have proved themselves useful in previous investigations of neuro-muscular condition, and sufficient repetition to avoid gross chance errors.

"Such an exploration should discover: (1) the direction of any gross disturbances; (2) the kinds of measurement and procedure that are likely to prove useful in squad experiments; and (3) more or less probable indications of especially desirable and undesirable features of the various masks.

"Part 2. From the exploration, it should be possible with military assistance to devise definitive military group tests of the relative satisfactoriness of available masks."

In addition to noting the effects of the mask on various forms of reaction, Dodge made special study of head air space, characteristics of eye pieces, ventilation of the face, adaptation and tolerance, recovery of capacity after removal of the mask, side inspirational tubes, the relation between neuromuscular depression and the ability to endure the mask, regulation of the pressure of the head-gear, gas mask sizes, the most economical development of tolerance to gas mask tenancy of long duration, the relative military value of different mask types for long tenancy, and the effects of resistance on tenancy and the performance of military tasks.

Major Knight Dunlap was assigned to duty with the Chemical Warfare Service to continue and extend the investigations on tenancy of the gas mask initiated by Dodge. His attention was confined in the main to measurement of the visual fields of different types of gas masks by an original method which dispensed with the head-gears and also to the development of a pneumatic scheme for measuring relative pressures on the head at different parts of the mask. This scheme, perfected technically by the Goodrich Company, was subsequently applied to the comparison of different types of mask. It served to show that the ——— mask, even when so adapted as to be leak proof, exerted less pressure, and also showed a more even distribution of pressure, than the other masks previously used, although the latter were not free from leaks even with the higher pressures employed.

Just prior to the signing of the armistice, Dunlap had perfected a procedure for determining the effects of different types of masks on the efficiency of the wearer. He proposes to continue this inquiry in his own laboratory in view of the practical value of a perfected mask in certain civilian occupations.

The Chemical Warfare Service further sought the services of the Psychology Committee in connection with the problem of camouflaging odors, and as a result of a conference with Captain Carleton, Vice-chairman Baird of the Psychology Committee prepared a report in which he summarized pertinent physiological and psychological literature on the

classification of odors, relationship of odors, and the relation between odor and chemical composition of substance.

Despite the fact that complete cancellation seemingly does not occur, Baird concludes that the intensity of an odor is so far reduced by compensation as to effect valuable camouflage. On the basis of this practical inference, he recommends the use of the methods of fatigue and of cancellation in camouflaging.

The work done by Dodge, Baird, and Dunlap demonstrated the existence of important psychological and physiological problems within the Chemical Warfare Service and the ability of psychologists to speedily achieve practical solution of such problems in the interests of military efficiency.

16. *Psychological Service to the Committee on Education and Special Training of the War Department.*—Conferences for considering suitable methods for selecting, classifying, and placing men for instruction in Students' Army Training Corps institutions were attended by representatives of the Psychology Committee, the Division of Psychology, Committee on Education and Special Training, and the Committee on Classification of Personnel. Thorndike, Terman, and Yerkes acting jointly, decided to suggest to the Committee on Education and Special Training that the army mental tests be adapted to meet the special requirements of the Students' Army Training Corps.

In a memorandum addressed to Charles R. Mann of the Committee on Education and Special Training, it was recommended that L. M. Terman be designated to adapt methods and to prepare plans for their administration in connection with the S. A. T. C. It was further suggested that the army group examination for literates (the Alpha examination) be administered in adapted form by the Committee on Education and Special Training with the advisory assistance of the Division of Psychology, Medical Department of the Army.

This recommendation was favorably received by the educational committee, and Terman was promptly assigned to duty with it. He immediately prepared for A. C. Vinal, Director of educational measurements for the Committee,

an adaptation of the Alpha examination in which, aside from differences in directions, the principal modification was the omission of the reading by the examiner of detailed instructions for the several tests. This lessened the time available for each test by requiring the subject to read the directions for himself during the period allowed in the army form of the examination for actual performance.

Having completed the adaptation of procedure, Terman presented army mental test methods to members of the educational committee and also discussed their practical results. This demonstration and exposition led to the decision that the Alpha examination should be universally used for the examination of the members of the Students' Army Training Corps.

It had been definitely planned and anticipated that the introduction of the methods would be directed by a few competent individuals employed by the Committee on Education, and that the actual conduct of the tests would be left to a member of each institution designated by the President. Everything was in readiness for the administration of tests to the first group of students in the Students' Army Training Corps when the signing of the armistice interrupted the work of the Committee.

Numerous institutions had meantime directed requests to nearby army training camps that their examining staffs representing the Division of Psychology, Surgeon General's Office, make psychological examination of their men. This was impossible because the Division of Psychology had delegated responsibility for the conduct of mental tests to the Committee on Education and Special Training.

After the signing of the armistice, because of the widespread demand for further information concerning army mental tests and opportunity to use them in connection with the Students' Army Training Corps, the Committee on Education arranged through the Division of Psychology of the Surgeon General's Office, to supply copies of the Alpha examination blank to such institutions as desired them. About one hundred institutions signified their desire to use the examination.

Except for numerous delays, the Alpha examination would have been administered to all members of the Students' Army Training Corps during the initial quarter, and thereafter either to applicants or to students already enrolled, for assistance in connection with classification and instruction.

The work of Terman would have been of increasing practical value had the Students' Army Training Corps continued in existence; but despite the failure to bring the tests into actual use, it is clear that important service was rendered by acquainting the educational committee, many additional army officers, and members of upwards of five hundred educational institutions, with this method of mental classification.

17. *Miscellaneous Work for Various Military and Civilian Agencies.*—L. L. Thurstone of the Carnegie Institute of Technology carried on an important investigation in the selection and training of telegraphers. At the outset two facts were discovered which proved of far-reaching significance. First, analysis of the progress of drafted men in Class 1a who were preparing to enter the service with some knowledge of telegraphy showed that many well-educated men did not succeed in learning telegraphy as well as might have been expected from their general intelligence and education. Second, the converse also proved true, that some men of very modest education and general ability found it easy to learn the telegraphic code. From these two facts was drawn the initial assumption that ability to learn the telegraphic code is a special ability. A series of tests were devised to predict the presence or absence of this ability, in order that future selections of telegraphers might be to the best advantage of the service and the men. A report on these tests will be published in the *Journal of Applied Psychology*.

Herbert S. Langfeld, during the summer of 1918, made an official trip to France to observe from the point of view of a psychologist the personnel of the Y. M. C. A. His excellent opportunities to observe American soldiers in rest and leave areas, in training schools, and in trenches, supplied materials for a valuable report which, on his return to America, was transmitted to the Psychology Committee of the National Research Council.

The principal subjects of this report are: psychological problems of morale, the aviation service, reconstruction, shell-shock, and camouflage. In each of these subjects Langfeld made interesting and important observations upon which he bases suggestions for practical service. Thus, for example, in connection with camouflage work, he states that one of the most interesting problems, with important psychological aspects is the camouflage of motion, especially in relation to aëroplanes. He further reports that the assistance of psychologists was greatly desired by the officers engaged in camouflage.

Had the war continued, Langfeld's observation of conditions and his suggestions would undoubtedly have led to important developments of psychological service overseas.

In the summer of 1918 an American psychologist who had observed conditions in Paris reported to the Psychology Committee that assistance might, in his judgment, be rendered the Y. M. C. A. in selecting suitable personnel for service overseas. The facts gathered by this observer were transmitted to the Office of the Secretary of War and as a result of careful consideration of the matter, the Y. M. C. A., in October, 1918, introduced the army mental tests (Group Examination Alpha) in all of its special training schools as a supplementary means of eliminating undesirable applicants. The Medical Department of the Army coöperated with these training schools by supplying the necessary materials for examinations and such standards of judgment and other information as might prove of assistance to the examining officers.

This work was instituted at such a late date that results of statistical value could not be obtained. Psychological examiners in the army were able, however, to render assistance to the Y. M. C. A. in training camps and also in a few of the special Y. M. C. A. training schools. Arrangements had been made by the New York headquarters of the Y. M. C. A. for the conduct of psychological examination of candidates for overseas service by Robert S. Woodworth of Columbia University. Approximately five hundred examinations had been made before the armistice cut short this work.

Of special interest is the fact that the executive officers of this organization, after trying other methods of selection to improve the Y. M. C. A. personnel, decided that the army procedure for measuring mental alertness should be introduced.

#### IV. FUTURE RELATIONS OF PSYCHOLOGY TO THE NATIONAL RESEARCH COUNCIL.

The eager and effective coöperation of psychologists in professional war work has enabled the Psychology Committee to win the confidence and the hearty support of the several scientific groups which together constitute the Research Council. Largely because of the way in which it responded to the practical demands and the opportunities of the military emergency, psychology today occupies a place among the natural sciences which is newly achieved, eminently desirable, and highly gratifying to the profession. An immediate result of this improved status is the desire of the Executive Board of the Research Council to have psychology adequately represented in the permanent national organization.

A conference of psychologists called for the discussion of the relations of psychology to the organization of the Research Council, unanimously approved the inclusion of psychology in the national organization and formulated the following as a tentative statement of the functions of the psychological section of the Council:

"1. Service, in an advisory capacity, to the Government and its various agencies in connection, for example, with psychological problems of education, immigration, civil service, military organization, public health, and labor.

2. The stimulation and facilitation, coördination, conduct of psychological research.

"3. The furtherance of the international relations of psychology and psychologists in the interests alike of research and effective instruction.

"4. The study and improvement of the status of the relations of psychology as science and as technology.

"5. Concern with such generally important matters of psychological personnel as the qualifications of individuals for research tasks and measures for maintaining and improving the quality of the professional personnel."



It is proposed to associate psychology with anthropology in a Division whose chairman and vice-chairman shall be chosen alternately from the two sciences, a chairman from anthropology serving with a psychologist as vice-chairman and *vice versa*.

If psychology is to meet successfully the now rapidly increasing practical demands by which it is challenged, it must organize for coöperative endeavor in a way not thought of prior to the war. On the one hand is the imperative need of highly developed and specialized methods; on the other, the need for largely increased and adequately trained personnel. The war activities of the Psychology Committee have revealed or created opportunities whose scientific and practical significance cannot be estimated. Two years ago mental engineering was the dream of a few visionaries. Today it is a branch of technology, which, although created by the war, is evidently to be perpetuated and fostered by education and industry.

Psychology needs therefore as never before in its history intimate association with the more exact natural sciences, as well as with the biological sciences which are more nearly related to it. The support and coöperation of other scientists and especially their intelligent interest, are indispensable.

For the speedy and sound development of psychology as science and as technology, the National Research Council should prove the most important of agencies. It is earnestly to be desired that the psychologists of the country may unite in their support of this national organization for the promotion of scientific research, its practical applications, and the profitable relations of sciences and of scientists.

# CHROMATIC THRESHOLDS OF SENSATION FROM CENTER TO PERIPHERY OF THE RETINA AND THEIR BEARING ON COLOR THEORY—PART II

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As was stated in Part I., one of the incentives to this investigation was to clear up two points in relation to color theory. These points are as follows:

1. The claim has been made by followers of the Hering theory that the sensitivity of the retina to the pairs of colors: red and green, and blue and yellow, falls off in a constant ratio from the center to the periphery of the retina. This claim, it will be remembered, was made first by Hess (1) on the grounds of an investigation of the relative limits of sensitivity with colors equalized both in cancelling power and brightness; and was given a great deal of importance by Hering (2) in a companion article in refutation of a revision of the Young-Helmholtz theory made independently by Fick (3) and Leber (4) to explain the color blindness of the peripheral retina. Fick, for example, assumed that from the middle towards the periphery of the retina the relative excitability of the three nerve fibers to lights of the various wave-lengths constantly alters in such a way that at a certain distance from the fovea, namely, in the zone called by Helmholtz red-blind, the red sensing fibers possess the same excitability as the green sensing fibers towards lights of all wave-lengths; and that further towards the extreme periphery all differences between the relative excitability of the three fibers diminish and finally disappear. In the red-blind zone, then, the intensity curves for the red and green sensing fibers coincide, and in the totally color-blind zone, the curves for all three coincide. Curves drawn in accord with these assumptions will, it is contended by Fick, explain the types

of color-blindness found in the peripheral retina without violating any of the fundamental principles of the Young-Helmholtz theory. Helmholtz accepts the essential points of this modification and incorporates them in his theory in his later edition of the 'Physiologische Optik' (5). With stimuli equalized in cancelling power and brightness, however, Hess claimed to find a coincidence of the limits for the pairs of colors used, and contended therefrom that the sensitivity to the pairs of antagonistic colors falls off uniformly from the center to the periphery of the retina or that a constant ratio of sensitivity to these colors obtains throughout the retina. (See also in this connection the papers of Bull (6), Hegg (7), and Baird (8).)

Prior to the presentation of a direct disproof of Hess's conclusion that a constant ratio of sensitivity to the paired colors obtains throughout the retina, in the form of results obtained in a detailed investigation of sensitivity from center to periphery, we had pointed out in a previous paper (9) that his conclusion was not warranted by the work and results on which it was based. It was based, it will be remembered, on the twofold assumption that if in passing from center to periphery sensitivity ends at the same point of the retina for two stimuli which have equal power to arouse sensation at the center, (a) they must still have equal power to arouse sensation at the periphery and (b) sensitivity must have fallen off as much for the one as for the other and evenly and uniformly from point to point. This assumption in the first place begins with a fallacy, for the stimuli were not equalized in power to arouse sensation but in cancelling power. Cancelling power and the power to arouse sensation are, as we have already pointed out, not at all equivalent (10). In the second place the assumption is itself incorrect; for because of the abrupt decrease in sensitivity with stimuli of medium and high intensities as the limits are approached, the relative sensitivity to the two colors may have changed greatly, even assuming an even grading in the loss of sensitivity for each color from the center out, and still the deviations from equal sensitivity not make a difference of as much as 1 de-

gree in the limits for the two colors. We have found, for example, that working with pigment colors of good saturation under an illumination of 390 foot-candles, it takes, varying with the color and the meridian investigated, 90 to 120 degrees of color mixed with a gray of the brightness of the color to make a difference of 1 degree in the limits. But an even grading in the loss of sensitivity can not be assumed as the results given in this study show; hence even if it could be demonstrated that the same ratio holds at the limits or at any point well removed from the center, as at the center, the conclusion could not be drawn that constancy of ratio obtains between these points. On this question it is obvious that a conclusion is not warranted unless a point to point investigation is made, and such an investigation shows that striking irregularity and not constancy and uniformity characterizes the changes in sensitivity from the center to the periphery of the retina. And in the third place, when the results of Bull, Hess and Baird who all claimed coextensive limits are examined in detail, it is found that they show the same sort of deviation from coincidence from meridian to meridian as were obtained by Kirschmann and by us, who have made a point of lack of coincidence when stimuli of the same order of intensity are employed. Baird, for example, who determined the limits for red, green, blue, and yellow stimuli in eight meridians in the dark room by means of a perimeter, concludes: "The results show that the zone of stable red is coincident with that of stable green and that the zone of stable yellow is coextensive with that of stable blue" (11). An inspection of his results shows, however, that the coincidence is extremely rough. In case of the results for every observer it is found that in some meridians the green field is narrower than the red by 1, 2 or 3 degrees; in other meridians there is coincidence of limits; and in still other meridians the red field is narrower than the green by 1, 2 or 3 degrees. The same is true of blue and yellow, the deviations from coincidence ranging from 1°-5°. Hess and Bull's results show similar variations, in some cases even greater in amount. It seems probable from their conclusions concerning the coincidence of limits, that

they regarded these variations as insignificant. But it should be borne in mind that 2 or 3 degrees of difference in limits is not insignificant when conclusions with regard to the relative sensitivity of the peripheral retina to the complementary colors are to be drawn from the results. Because of the abrupt falling off in sensitivity before the limits are reached with stimuli of medium and high intensities, a difference of 2 or 3 degrees in the limits represents quite a large difference in sensitivity. For example, according to our results with the Hering standard papers under 390 foot-candles of illumination (vertical component), a difference of 2 degrees in the limits represents a difference in sensitivity sufficient to raise the threshold for yellow 120 degrees; for green, 100 degrees; for red, 160 degrees; and for blue 160 degrees. And a difference of 3 degrees represents sufficient difference in sensitivity to raise the threshold for yellow 210 degrees; for green, 215 degrees; for red, 210 degrees; and for blue, 215 degrees. Our results with the more intense spectrum lights show, as might be expected, that a difference of 2 or 3 degrees in the limits represents a still greater difference of sensitivity. This should be quite obvious from the curves we have given in Part I. It is scarcely needful to note in this connection also that the weaker are the stimuli employed, the nearer will the limits be to the center of the retina; and it should be clear from the curves we have given that the nearer the limits are to the center of the retina, the closer will be the approximation to coincidence. Bull, Hegg, Hess and Baird in their attempts to equalize their stimuli both in cancelling power and brightness must have worked with colors of comparatively low saturation, hence with stimuli unduly favorable to coincidence of limits. Their results, therefore, are the product of a special method of working rather than are representative of the relations of sensitivity actually existing in the more remote periphery of the retina even so far as these relations can be judged from a determination of limits alone. Unfortunately no specification of the intensity of their stimuli was given, but the narrowness of the zones of sensitivity obtained indicate that stimuli of low color arousing power were used.

It may be of some interest also to note in this connection that the present writers have never been able to secure red, green, blue and yellow stimuli of spectrum purity, all equal in brightness and at the same time to have the pairs of complementary colors in cancelling proportions. To conceive that the spectrum colors can be equalized at one and the same time with regard to these two independent variables would seem *a priori* to be a logical impossibility; and the task of making this twofold equalization has as yet proved too difficult for us as a practical problem. It might perhaps be done if a variable weighting factor, namely, colorless light, were introduced in the right proportions into the composition of a part or all of the stimuli, but that would scarcely be compatible with the purpose of the investigation. It is, in fact, difficult to understand why such an equation should ever have been attempted in the first place in an investigation of chromatic sensitivities. An equation in the power to arouse the chromatic response is, so far as we can see, the only subjective equation that could be rightfully given a place in the determination of the relative limits of chromatic sensitivity, and this only in a determination of whether or not the same ratio of sensitivity holds for the limits as for the center or other point at which the equation was made. That is, if it does hold, the limits would be coincident, and if it does not hold, they would not be coincident; but no definite knowledge would be gained of the amount of deviation from equality of ratio, nor would any inference be justified with regard to the relative values of the ratio between the point at which the equation was made and the limits. Just what would be accomplished by an equation in cancelling power which neither equalizes the stimuli in intensity nor in power to arouse the chromatic response, is far from clear. Had the object of the investigation been a determination of whether or not constancy in cancelling proportions holds for all parts of the retina, the verdict would be different. For one type of investigation of this sort, then, the equation would be of service, but for an investigation of constancy of ratio of sensitivity, it is obviously irrelevant. There seems also to

be no more experimental justification and little if any more *a priori* plausibility for the equation in brightness for a determination of the relative limits of chromatic sensitivity; for (a) it does not equate the stimuli in power to arouse the chromatic response, (b) neither does it equate them in intensity (the equation is merely of the very selective achromatic response to the stimuli), and (c) so far as the effect of the achromatic on the chromatic component of the excitation is concerned (the final variable factor that might be considered), it has already been shown by one of us in a previous article (12) that there is not enough difference in this effect for the colors used to change the limits of sensitivity by a detectable amount. However, the irrelevancy and the positive disadvantage of such equations, as they appear to us, have been discussed in detail in the previous paper. The question is raised again here only because the results of our point to point investigation throw additional light on the effect that the attempt to treat the stimuli in this way would have on determinations of the type under consideration, namely, compelling the use of stimuli of such low color arousing power as to make the conditions unduly favorable for a coincidence of limits.

But of much greater importance than all of this as a general consideration, is the realization that the determination of the relative or apparent limits for the purpose of ascertaining whether or not a constant ratio of sensitivity to the paired colors obtains throughout the retina, falls far short of its objective. The information sought can be obtained only by a point to point investigation of sensitivity. With the passing then of the belief in any especial significance of the determination of the relative limits, which after all is only a very inadequate way of comparing sensitivities at a limited number of points and which was given undue importance in relation to theory by the failure of Hess and his followers to realize that great irregularity and not uniformity characterizes the decrease of sensitivity from the center to the periphery of the retina, will doubtless pass also any feeling of need to be concerned about the reasons that may have influenced these writers to treat their stimuli as they did.

2. The point to point investigation also has an important bearing on the question of stability of color tone. The results given in Part I make it easy to understand why it is not possible to find a red and a green stimulus that are invariable in color tone from the center to the periphery of the retina in all meridians. That is, the conception of a red and a green that are stable in tone presupposes a regularity in the relative rate of decrease in sensitivity to red and green on the one hand and to yellow on the other which the point to point investigation shows is very far removed from fact.

The claim to a stable red and a stable green was first made by Bull and later by Hess, Hegg and Baird in investigations of the relative limits of sensitivity to the paired colors. Working with pigment papers Bull added blue to his red and green stimuli in order that he might get colors that would not be sensed as yellowish in the peripheral retina.<sup>1</sup> His purpose in doing this, he states, was to find the physiologically pure red and green. Passing over the fact that the addition of blue in sufficient amounts to cancel the peripheral yellow gives an excess of blue in the more central portions of the retina (even outside of the macula) which is scarcely compatible with the tenet of introspective simplicity, this method of obtaining a stable red and green would presuppose, as we have already stated, a regularity in the relative rates of decrease of sensitivity to red and green on the one hand and to yellow on the other in the different meridians of the retina which is far removed from fact. For example, if the sensitivity to red fell off at the same rate in all meridians of the retina and the sensitivity to red and yellow in a constant ratio, the amount of blue which is required in a given meridian to neutralize the yellow component in sensation would suffice for this purpose in all meridians. Since neither of these essential conditions is present in the relative distribution of sensitivities from center to periphery of the retina in the different meridians, the futility of the search for the stable red and the stable green by the method proposed by Bull is

<sup>1</sup> Speaking of the gelatines used for his red stimulus, Baird (*op. cit.*, p. 60) says: "The red stimulus transmitted no part of the visible spectrum."



obvious. Moreover, it is perhaps just as obvious that stability of tone throughout the retina is by no means a necessary corollary of a four-color theory of the type proposed by Hering, and therefore that its use by the aforementioned writers for the purpose of searching out or isolating the four physiological processes was questionable even on *a priori* grounds. That is, the assumption that our color processes are conditioned by four physiological processes, the action of any one of which alone would give a sensation which is introspectively simple, should not by any means carry with it also the assumption that stimuli can be found to which one alone of these processes responds. For in the first place, such a narrowness of selectivity of response is not needed to explain our experience of the introspectively simple sensation; secondly, it is not as a general case characteristic of selectiveness of action; and thirdly, it is quite out of keeping with the change of tone of red and green in passing from the center to the periphery of the retina. The explanation of this phenomenon seems to have given not a little concern to the followers of the Hering theory who have apparently, in some cases at least, thought that if corresponding to the four simple sensations there are four simple physiological processes, it should be possible to find stimuli that would arouse one of these processes alone, which stimuli should of course be invariable in color tone for all parts of the retina. But, as we have already pointed out, this is neither a necessary nor perhaps even a plausible corollary of the fundamental assumption of four processes, the action of any one of which alone should give a simple sensation, and besides detracts needlessly from its explanatory value.

Hering's own criteria for the selection of the *Urfarben* were (1) introspective simplicity at or around the maximal saturation (13) and (2) following the lead of Fick, no change of color tone with change of intensity of the stimulus light (14). Whether or not constancy of tone can be expected for all intensities of light would again seem to depend on whether constant ratios of chromatic sensitivity obtain for all intensities of light; in other words, upon whether or not the select-

iveness of the chromatic response of the eye varies with the intensity of light, as does the achromatic response. If it does, it is too much to expect that this second criterion of Hering's will be of any especial service for the purpose for which he used it; for, depending upon the variations of ratio of sensitivity or relative amounts of the selectiveness of the chromatic response, one spectrum band may give the introspectively simple sensation at one intensity and a mixed sensation at a different intensity. Also the effect of the varying strength of the achromatic component on the color tone of the sensation aroused can not be left out of consideration. The attempt, therefore, to label, so to speak, the simple physiological process with a wave-length or spectrum specification, presupposes a simplicity in the eye's reactions which very probably does not exist. The investigation of the selectiveness of the chromatic response of the eye in relation to intensity of light is, as we have already stated, now in progress in this laboratory.

To explain the experience of introspectively simple red and green at the center of the retina and its change in tone in passing towards the periphery in terms of four physiological processes, the action of any one of which alone would give a simple sensation, it is necessary only to call attention to three factual considerations<sup>1</sup> the application of which to the point in ques-

<sup>1</sup> With regard to the first of these considerations it may be of interest to note that Hering did not himself seem to regard the simple physiological processes as narrowly selective in their response to wave-length, and that he recognized, implicitly at least, that all of the color processes exert an inhibitive action on each other. This conclusion may be derived from the following passages and from others in the article (15) from which they are quoted: "Da alle sechs Prozesse fortwährend gleichzeitig, wenn auch mit sehr verschiedener Stärke in der Sehsubstanz stattfinden, so sind auch immer alle sechs Grundempfindungen, gleichzeitig gegeben. Jede Gesichtsempfindung ist daher eigentlich ein Gemisch aus den sechs Grundempfindungen, doch sind darin immer nur einige von den Grundempfindungen deutlich, die andern unter der Schwelle. Die Deutlichkeit, mit welcher die eine oder die andere der Grundempfindungen sich in der Gesamtempfindung zeigt, hängt von dem Verhältniss ab, in welchem die Stärke des, dieser Empfindung correlaten Processes zur Stärke der fünf übrigen steht. Ist z.B. der schwarze Process sehr stark im Vergleich zu allen andern, so tritt die schwarze Empfindung mit besonderer Deutlichkeit hervor, wobei die fünf übrigen so undeutlich werden können, dass sie nicht mehr einzeln wahrnehmbar oder, wie man zu pflegt, unter der Schwelle sind. Wir nennen dann die Gesamtempfindung schwarz. Sind z.B. die beiden Grundempfindungen Grün und Blau besonders deutlich, so nennen wir die Empfindung grünblau u.s.w. . . ."

tion apparently has not always been clearly kept in mind. (1) The selectiveness of the eye's response to wave-length is not complete. Apparently it ranges from a minimum to a maximum. In case of the red and yellow processes, for example, the maximum is reached respectively at certain points in the red and yellow portions of the spectrum and the minimum at a point in the orange. (2) There is an inhibitive action of the non-complementary colors<sup>1</sup> on each other as well as of the complementary colors. That is, the threshold of one of these colors in another is high and its value differs from color to color. And (3) the distribution of sensitivity is very irregular from point to point over the retina. That is, a certain range of wave-lengths in the red portion of the spectrum acting on the center of the retina arouses both the red and the yellow processes, the red strongly and the yellow weakly. The yellow does not come to sensation because it is below the threshold of yellow in red. When, however, the same stimulus acts on the peripheral retina at points where the red process is relatively undeveloped as compared with the yellow, the yellow is no longer subliminal in the red but becomes a component of the sensation of a value depending upon the ratio of sensitivity to red and yellow at the points in question, the

On pp. 79-81 he continues: "Ausser der weissen Valenz, welche allen Lichtstrahlen gemeinsam ist, kommen nun den einzelnen Strahlenarten verschiedene farbige Valenzen zu. Alle Strahlen von äussersten Roth oder vom *Anfange* des Spectrums bis zu jenem im Tone reinen Grün, welches eine Grundfarbe ist und welches wir das Ugrün nennen wollen, haben eine gelbe, alle Strahlen vom Ugrün bis zum violetten *Ende* des Spectrums eine blaue Valenz. Demnach theilen wir das Spectrum in eine *gelbwerthige* und eine *blauwerthige* Hälfte, wenn auch beide nicht gleich lang erschienen. Am Anfange des Spectrums ist die gelbe Empfindung so schwach, dass sie gegenüber der deutlicheren rothen unter der Schwelle bleibt; ebenso tritt sie in der Nähe des Ugrün wieder mehr und mehr hinter der grünen Empfindung zurück. Nur in einem schmalen Streifen erscheint uns das tonreine Gelb oder das Ugelb, welches der Grundfarbe entspricht. Analoges gilt vom Urblau, welches nach dem Ugrün hin immer mehr gegenüber der grünen Empfindung zurück tritt, nach dem Ende des Spectrums hin aber sich mehr und mehr mit rother Empfindung mischt.

<sup>1</sup> We do not wish to be understood as suggesting here that the inhibitive action of the non-complementary colors on each other requires the same mechanism for its explanation as the complementary or even that it takes place at the same functional level. We are inclined rather to believe that the inhibitive action of the achromatic on the chromatic excitation and possibly also of the non-complementary colors on each other takes place at a level posterior to the inhibiting action of the complementary colors.

mutually inhibitive actions of the excitations upon each other, etc. A similar explanation holds for the green. Why an analogous phenomenon of change of tone due to these factors should not occur in case of blue and yellow is obvious. That is, even though a subliminal red or green excitation were aroused in the center of the retina by the blue or yellow wave-lengths, it would become still more subliminal as the periphery of the retina is approached because of the more rapid decrease in sensitivity to red and green, and would not come to sensation. The changes that do take place in the color tone of yellow in passing from the center to the periphery of the retina we have already explained as an effect of the achromatic upon the chromatic component of the excitation. The demonstration of the validity of this explanation will be the work of a later paper.

#### COMMENTS

The discussion relative to color theory, so far as we wish to consider theory at this time, may perhaps be summed up in the following comments.

1. An explanation of the color changes of red and green in passing from the center to the periphery of the retina may be found in three factual considerations: (a) the absence of complete selectivity of response of the eye to the red and green wave-lengths of light; (b) the inhibitive action of the non-complementary colors on each other; and (c) the relative distribution of sensitivity to red and green on the one hand and to yellow on the other in the periphery of the retina. The color tone of red and green seems to be very little dependent on the achromatic conditions of stimulation in any part of the retina.

2. The changes in the color tone of yellow (also of blue, so far as they occur), in passing from the center to the periphery of the retina seem to be largely, if not entirely, an effect of the achromatic conditions,—in physiological terms an effect of the achromatic upon the chromatic component of the excitation, the state of achromatic adaptation of the eye, etc. Of the four principal colors, the color tone of blue and yellow is as a general case the most dependent on the

achromatic conditions. Moreover, given the same achromatic conditions there is a striking agreement in the effect in all parts of the retina. The relative distribution of chromatic sensitivities apparently plays little if any part in the changes of color tone of blue and yellow in passing from the center to the periphery of the retina.

3. The claim that a red, green, blue, and yellow stimulus may be found to which the eye will give a response invariable in color tone in all parts of the retina is based on a very incomplete and inadequate investigation of the eye's possibilities of response. Also the importance of the bearing of such a possibility on color theory seems to the present writers in many instances at least to have been very wrongly stressed. (Our own conclusions with regard to the possibility of obtaining stability of tone for these colors, for example, are based on a very minute investigation on a number of observers in sixteen meridians of the retina. Moreover, some of the more important findings of this investigation have been confirmed year by year in the work of the undergraduate laboratory.)

4. There is no basis of fact for a claim that a constant ratio of sensitivity to the pairs of colors red and green, and blue and yellow obtains in all parts of the retina; nor is it apparent that such a claim is of any considerable consequence to the fundamental postulates of theories of the Hering type. It is more important, for example, (*a*) that wherever one of these pairs of processes be found, the other shall also be found; and (*b*) that a constancy of ratio of cancelling proportions for the pairs of colors obtains in all parts of the retina. (The power to arouse sensation and the power to cancel the antagonistic or complementary color are not, as we have already shown, equivalent.) With regard to the first of these points it does seem to be of rather serious consequence that we have not been able to get even approximately coextensive zones of sensitivity to red and green, for we have no means of knowing where the color sensing substances are except by the responses aroused. With intensive stimuli, it will be remembered, we have found the limits of sensitivity to red, blue, and yellow to

coincide with the limits of white light vision, but the limits of sensitivity to green fall far short of this even with the greatest spectrum intensities we have as yet been able to obtain.

5. The constancy in the cancelling proportions of the paired colors from center to periphery of the retina in all meridians as contrasted with the great irregularity in the distribution of sensitivity, obviously presents a problem to theories of the Hering type; for while there is not and should not necessarily be in terms of theory an equivalence in the power to arouse sensation and to cancel the complementary color, some degree of constancy of relation between the two functions might be expected. Perhaps the easiest solution is to be found in the conception that more than one level of activity is involved in the process of arousing sensation and that the locus of the deficiencies which cause the irregularity in the distribution of sensitivity to the paired colors is posterior to the level at which the cancelling action takes place. While an explanation of this type meets with less inertia of acceptance, perhaps, for the occasional and sporadic deficiency such as the small areas of the Schumann type in the peripheral retina (16), than for deficiencies and anomalies of the order of magnitude here considered, still the need for it or some similar concept to explain these anomalies and deficiencies is no less insistent. Even the extensive deficiency in the sensitivity to green noted above is contradictory to the concept of paired processes only on the assumption that the deficiencies which affect sensitivity may occur at only one functional level; for again it may be that the deficiencies which prevent the green stimulus from arousing sensation are posterior to the level of the cancelling action. If this were true, the cancelling proportions between the paired colors could be constant from the center to the periphery of the retina even though the sensitivity to one of the colors had fallen off a great deal or disappeared entirely, as seems to be the case when either a partial or full spectrum gray is sensed as colorless from the center to the periphery of the retina without any change in the composition of the stimulus to compensate for the extensive deviation from regularity in

the distribution of sensitivities. As in the previous paper, however, our comments with reference to theory are meant to be only tentative and suggestive (17). Our purpose has been primarily to call attention to the lack of adequate explanatory concepts to meet the needs of our growing knowledge of the visual phenomena. Theories whose especial fitness is for the explanation of the fundamental facts of positive sensation, the after-image, and contrast can scarcely be considered as final and complete.

## BIBLIOGRAPHY

1. HESS, C. Ueber den Farbensinn bei indirectem Sehen. *A.f.O.*, 1889, 35 (4), pp. 1-62.
2. HERING, E. Ueber die Hypothesen zur Erklärung der peripheren Farbenblindheit. *A.f.O.*, 1889, 35 (4), pp. 63-83.
3. FICK, A. Zur Theorie der Farbenblindheit. *Arbeiten aus dem physiol. Laborat. der Würzburger Hochschule*, 1873, pp. 213-217.
4. LEBER, T. Ueber die Theorie des Farbenblindheit und über die Art und Weise, wie gewisse, der Untersuchung von Farbenblinden entnommene Einwände gegen die Young-Helmholtz'sche Theorie sich mit derselben vereinigen lassen. *Klin. Monatsblätter f. Augenheilk.*, 1873, 11, pp. 467-473.
5. HELMHOLTZ, H. *Handbuch der physiologischen Optik*, 2d Ed., 1896, p. 373.
6. BULL, O. Studien über Lichtsinn und Farbensinn. *A.f.O.*, 1881, 27, pp. 54-154.
7. HEGG, E. Zur Farbenperimetrie. *A.f.O.*, 1892, 38, (3), pp. 145-168.
8. BAIRD, J. W. The Color Sensitivity of the Peripheral Retina. *Carnegie Institution of Washington*, 1905, pp. 80.
9. RAND, G. The Factors the Influence the Sensitivity of the Retina to Color: a Quantitative Study and Methods of Standardizing, *Psychol. Rev. Monog.*, 1913, 15, pp. 28-31.
10. *Ibid.*, p. 65.
11. BAIRD, J. W. *Op. cit.*, p. 61.
12. RAND, G. *Op. cit.*, pp. 97-110.
13. HERING, E. *Zur Lehre vom Lichtsinne*. Wien, 1878, pp. 108-109.
14. HERING, E. *Zur Erklärung der Farbenblindheit aus der Theorie der Gegenfarben*. *Lotos, Jahrbuch für Naturwissenschaft*, 1880, 1, pp. 81-82.
15. *Ibid.*, p. 77.
16. FERREE, C. E., and RAND, G. Some Areas of Color Blindness of an Unusual Type in the Peripheral Retina. *Jour. of Exper. Psychol.*, 1917, 2, pp. 295-304.
17. *Ibid.*, pp. 300-304.





# THE PSYCHOLOGICAL REVIEW

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## A SCHEMATIC OUTLINE OF THE EMOTIONS<sup>1</sup>

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*What is an Emotion?* Hard and fast definitions are not possible in the psychology of emotion, but formulations are possible and sometimes help us to assemble our facts. A formulation which will fit a part of the emotional group of reactions may be stated as follows: *An emotion is an hereditary pattern-reaction involving profound changes of the bodily mechanism as a whole, but particularly of the visceral and glandular systems.*<sup>2</sup> By pattern-reaction we mean that the separate details of response appear with some constancy, with some regularity and in approximately the same sequential order each time the exciting stimulus is presented. It is obvious that if this formulation is to fit the facts, the general condition of the organism must be such that the stimulus can produce its effect. A child alone in a house on a stormy night with only a dim candle burning may display the reaction of fear at the mournful hoot of an owl. If the parents are at hand and the room is well lighted, the stimulus may pass

<sup>1</sup> The material here presented was not prepared primarily for presentation in a psychological journal. It is published in the hope that its main defects as an introductory presentation of the main observable facts about the emotions may be pointed out.

<sup>2</sup> Throughout this paper we have introduced physiological concepts into the behavior study of emotions. It is possible that we have given the impression that we are writing a physiology of the emotions. Such is not the case. It is perfectly possible for a student of behavior entirely ignorant of the sympathetic nervous system and of the glands and smooth muscles, or even of the central nervous system as a whole to write a thoroughly comprehensive and accurate study of the emotions—the types, their interrelations with habits, their rôle, etc. We have tried to connect emotional activity with physiological processes because it seems that such formulations are now practical and no longer purely speculative.

unreacted to. Stimulus then in this sense is used in a broad way to refer not only to the exciting object but also to the general setting. There is implied also the fact that the general state of the organism must be sensitive (capable of being stimulated) to this form of stimulus at the moment. This condition is very important. A young man may be extremely sensitive to the blandishments of every female he meets while in the unmarried state and may show considerable excitement and over-reaction on such occasions. In most cases, he becomes considerably less sensitive after being happily married. This formulation may seem somewhat roundabout—somewhat like saying that a stimulus is an emotional stimulus only when one gets the pattern-reaction, but this is very nearly the case. Possibly we can illustrate most easily what we mean by choosing an example from animal life. When the naturalist comes suddenly upon a young sooty tern under four days of age, it lies stock still (it is capable of very rapid locomotion): It can be pushed about or rolled over without explicit forms of response appearing. The moment the intruder moves away, the fledgling may hop to its feet and dash away or give one of its instinctive cries. The pattern-reaction, *i. e.*, the explicit observable pattern, is very simple indeed—a death feint or posture. Such a type of response is quite common in the animal world. In order to bring about such a tremendous variation in behavior in an animal usually so active there must be a profound modification of the organic processes. The locus of the effect (the implicit side), lies principally in the visceral system. Often, however, the skeletal musculature is involved in the pattern. A serviceable way to mark off an *emotional* reaction from an *instinctive* reaction is to include in the formulation of emotion a factor which may be stated as follows: The shock of an emotional stimulus throws the organism for the moment at least into a chaotic state.<sup>1</sup>

<sup>1</sup> It is most interesting that with many psychologists and with a good many physiologists and neurologists the newer conceptions of experimental zoölogy make slow progress. Experimental biologists and students of animal behavior have begun to put the emphasis upon accurate statements about what really happens in hereditary adjustments rather than to seek in them the exposition of the dogma that they exist

When in the state of shock the subject makes few adjustments to objects in his environment. In contrast to this stand the instincts. The subject under the influence of an instinctive stimulus usually does something: He throws his hand up for defense, blinks his eyes or ducks his head; he runs away; he bites, scratches, kicks and grasps whatever his hand touches. This distinction cannot be applied in every case of emotional activity, as we shall see in our next paragraph. In any event it cannot be pushed too far. We might express it in another way by saying that in emotion the radius of action lies within the individual's own organism; whereas in instinct the radius of action is enlarged to such an extent that the individual as a whole may make adjustments to the objects in his environment.

*Additional Formulations.*—The above formulation fits of course only the more stereotyped forms of emotional response. When we take into account the whole group of phenomena in which we see emotional manifestations in adults, a pronounced modification is necessary. Apparently the pattern as a whole gets broken up. At any rate it largely disappears (the parts never wholly disappear) except under unusual conditions, and there can be noted only a reinforcement or inhibition of the habit and instinctive (exaggerated and depressed reflexes) activities taking place at the moment. We mean to imply here only the generally observed facts typified by such popular expressions as "He is working at a low ebb today," "His tone is low," "He's a gloom;" in psychopathology when this phase is more marked, *depressions* are spoken of. The opposite picture is popularly portrayed by such expressions as "Jones is full of pep today," "He is excited," 'happy,' "He is working with a punch;" in psychopathology, the exaggerated type of this behavior is termed *manic*. It will be noted that these expressions refer to the activity level at which all of an individual's acts are accomplished, *i. e.*, they do not refer to the pattern type of

because they are useful or serviceable. No one who has watched animals display their hereditary forms of activity from birth to the adult stage could hold that more than a few, considering the thousands which exist, fit such a philosophic and really vitalistic mold. The case of the human infant is not different.

emotion. Only in pathological cases, or in the case of normals in periods of a cataclysmic nature such as war, earthquake, and the sudden death of loved ones, do we get a complete return to the original and more infantile type of emotional response.

Observation would seem to suggest the following formulation: Organized activity (hereditary and acquired) may go on and usually does go on at a given level. We may call the most usual, *the normal level*, or level of equilibrium. It varies with different individuals and one can determine it even with respect to a single individual only after observing him for a considerable time. We may note further that an individual at one time may exhibit more energy, push, or pep, than normal; we may call this the *excited level*. Again at times he works at a level lower than normal; we may call this the *depressed level*.

Without neurologizing too much, we may venture the assumption that in adults environmental factors have brought about the partial inhibition of the more external features of the primitive pattern types of emotion. The implicit, mainly glandular and smooth muscular side of the pattern, remains. The emotionally exciting object releases important internal secretions which, without initiating new part reactions, reinforce or inhibit those actually in progress.<sup>1</sup> This hypothesis would account for changes in level. Only in rare cases do we see mere changes in level. Usually when such changes occur certain auxiliary or additional part reactions appear such as we see in whistling while at work, keeping time with the feet, drumming on the table, biting the finger nails. These types of reaction are singled out and spoken of in some detail under the head *Emotional Outlets* (p. 184).

<sup>1</sup> If it is true that the thyroid, hypophysis, adrenal and sex glands accelerate metabolism and that the parathyroid, the pancreas and the thymus retard it as is sometimes stated, the observed changes in emotional level find easy explanation. If the emotional stimulus, either directly or through a conditioned reflex mechanism, influences the glands which accelerate metabolism, the excited emotional level will appear. On the other hand, if the stimulus influences the glands which retard metabolism, the depressed level will result. If neither group of glands is stimulated, there will be an absence of emotional tone. In other words, the individual will in such cases work at his normal level.

*The Genetic Study of Emotion in the Child.*—Unfortunately for the subject of psychology, few experiments have been made upon the emotional life of the child under anything like as favorable conditions as obtain in the study of animals. Our observations upon the child are similar to those which were made upon animals before Thorndike and Lloyd Morgan introduced the experimental method. Until very recently, in spite of volumes written upon it, it has been of the armchair variety. The superstition that the human infant is too fragile for study is giving way to a more sensible viewpoint. It has been proven practicable in some laboratories to take infants from birth and to study them from the same point of view that animals are studied, giving due consideration to those factors in behavior which do not appear in animal response. But unfortunately this work is handicapped because there are no facilities in maternity wards for keeping the mother and child under close observation for years, a condition which is indispensable for real systematic work.

*Summary of Positive Results, Early Types of Emotional Reactions.*—After observing a number of infants, especially during the first months of life, we suggest the following group of emotional reactions as belonging to the original and fundamental nature of man: *fear*, *rage* and *love* (using *love* in approximately the same sense that Freud uses *sex*).<sup>1</sup>

*Fear.*—What stimulus apart from all training will call out fear responses; what are these responses, and how early may they be called out? The principal situations which call out fear responses seem to be as follows: (1) To suddenly remove from the infant all means of support, as when one

<sup>1</sup> This list is identical with James's list of *coarser* emotions except for the omission of grief, which James puts first. Grief we look upon as being a *reactive state* (connected with love, really) in which the object or situation which usually calls out in the subject the reactions of love is suddenly removed. The state of grief must be looked upon as a mal-adjustment period, where the objects and situations which have usually called out both the original love responses and the conditioned reflexes built upon them are lacking. The state (in normal cases) disappears as soon as new objects are found or new conditioned reflexes have been entrained. We use these terms which are current in psychology with a good deal of hesitation. The student is asked to find nothing in them which is not fully statable in terms of situation and response. Indeed we should be willing to call them emotional reaction states, *X*, *Y* and *Z*. They are far more easily observable in animals than in infants.

drops it from the hands to be caught by an assistant. (In the experiment the child is held over a bed upon which has been placed a soft feather pillow); (2) by loud sounds; (3) occasionally when an infant is just falling asleep or is just ready to waken, a sudden push or a slight shake is an adequate stimulus; (4) when an infant is just falling asleep, occasionally the sudden pulling of the blanket upon which it is lying will produce the fear responses. (2) and (3) above may be looked upon as belonging under (1). The responses are a sudden catching of the breath, clutching randomly with the hands (the grasping reflex invariably appearing when the child is dropped), blinking of the eyelids, puckering of the lips, then crying; in older children possibly flight and hiding (not yet observed by us as 'original' reactions). In regard to the age at which fear responses first appear, we can state with some sureness that the above mentioned group of reactions appears at birth. It is often stated that children are instinctively afraid in the dark. While we shall advance our opinion with the greatest caution, we have not so far been able to gather any evidence to this effect (p. 173, 174). When such reactions to darkness do appear they are due to other causes; darkness comes to be associated with absence of customary stimulation, noises, etc. (They should be looked upon as conditioned fear reactions.) From time immemorial children have been 'scared' in the dark, either unintentionally or as a means of controlling them (this is especially true of children reared in the South).

*Rage.*—In a similar way the question arises as to what is the original situation which brings out the activities seen in rage. Observation seems to show that the *hampering of the infant's movements* is the factor which apart from all training brings out the movements characterized as rage. If the face or head is held, crying results, quickly followed by screaming. The body stiffens and fairly well coördinated slashing or striking movements of the hands and arms result; the feet and legs are drawn up and down; the breath is held until the child's face is flushed. In older children the slashing movements of the arms and legs are better coördinated and appear

as kicking, slapping, and pushing. These reactions continue until the irritating situation is relieved and sometimes do not cease then. Almost any child from birth can be thrown into a rage if its arms are held tightly to its sides; sometimes even if the elbow joint is clasped tightly between the fingers the response appears; at times just the placing of the head between cotton pads will produce it. This was noticed repeatedly when testing eye coördinations in infants under ten days of age. The slight constraint put upon the head by the soft pads would often result in a disturbance so great that the experiment had to be discontinued for a time.

*Love.*—The original situation which calls out the observable love responses seems to be the stroking or manipulation of some erogenous zone, tickling, shaking, gentle rocking, patting, and turning upon the stomach across the attendant's knee. The response varies—if the infant is crying, crying ceases, a smile may appear, attempts at gurgling, cooing and finally, in slightly older children, the extension of the arms which we should class as the forerunner of the embrace in the acts of courtship. The smile and the laugh which Freud connects with the release of repression (we are not denying in the case of adults that this may be true) we should thus class as original reaction tendencies intimately connected, in our opinion at least, from infancy with the stimulation of the erogenous zones.

These types fit fairly well the general formulation we gave on page 165. There is a reaction pattern, there is a definite stimulus which has its peculiarly exciting character (the reason for which must be sought in biology), the radius of action is small, no particular adjustment is made to any object in the environment. It is admitted however that the responses contain both explicit and implicit components, that is, involve the skeletal musculature, the visceral system, the smooth muscles and glands. It is probable though that if the exciting stimulus were sufficiently strong, *e. g.*, strong enough to produce 'shock,' or if continued for a sufficient length of time, the subject would tend to take on more and more the purely vegetative type of existence illustrated by

the example of the young tern. In rage the child becomes so stiff and holds its breath for such a long time that it is often necessary to soothe it. The final stage in any great emotion would seem to be paralysis or the 'death feint.' Approximations to this condition are seen in the paralysis of fear, in the fainting under strong emotional excitement, in the stereotyped reactions of the stoics and martyrs when they unflinchingly resist the torch. Individuals on the battlefield likewise are able to withstand operations, wounds and injuries without complaint. It must be admitted that there is a constant tendency for the organized habit response of the individual to disappear under the extremes of emotion. So far as we can see, this tendency towards stereotypy, paralysis or the death feint under the immediate effect of a strong emotional excitement has no biological or adaptive value (see p. 193, however, on post-emotional state). The organism exhibiting it is at the mercy of its enemies, whether on the battlefield or in the struggle for food among savage tribes, and is at a disadvantage in the race for a much-sought-after woman, or in the fight for business and scientific reputation.

*Negative Results of Experimental Study.*—Three babies from the Harriet Lane Hospital were put into various situations, the types of which are illustrated below, for the purpose of finding out whether there is a wider range of stimuli that may arouse an emotional reaction than the one we cited a moment ago. These babies represented splendid, healthy types. Their mothers were the wet nurses belonging to the hospital. They were 165, 126 and 124 days of age respectively. The first two, whose ages are given, were put through the more numerous tests. The experiments given below are interesting for the reason that the babies had never been out of the hospital and had never seen an animal. A summary of the tests on Thorne, a girl 165 days of age, is given below.

A very lively, friendly *black cat* was allowed to crawl near the baby. She reached for it with both hands at once. The cat was purring loudly. She touched its nose, playing with it with her fingers. It was shown three times. Each time she reached with both hands for it, the left hand being rather more active. She reached for it when it was placed on a lounge before her, but out of reach.

Then a *pigeon* in a paper bag was laid on the couch. The pigeon was struggling,



and moving the bag about on the couch and making a loud rattling noise. The baby watched it intently but did not reach for it. The pigeon was taken out of the bag on the couch before her, cooing and struggling in the experimenter's hands. She reached for it again and again, and failing of course to get hold of it put her hands in her mouth each time. She was allowed to touch its head. The pigeon moved its head about with quick, jerking movements. It was then held by its feet and allowed to flap its wings near the baby's face. She watched it intently, showing no tendency to avoid it, but did not reach for it. When the bird became quiet she reached for it and caught hold of its beak with her left hand.

*Test with a Rabbit.*—The animal was put on a couch in front of her. (The child was sitting on her mother's lap.) She watched it very intently, but did not reach for it until the experimenter held it in his hands close to her; then she reached for it immediately, catching one of its ears in her left hand, and attempted to put it into her mouth.

The last animal presented to her was a *white rat*. She paid little attention to it, only fixating it occasionally. She followed it with her eyes somewhat when it moved about the couch. When held out to her on the experimenter's arm, she turned her head away, no longer stimulated.

*172 Days Old.*—The baby was taken into a dark room with only an electric light behind her, not very bright (faint illumination). A stranger held the baby. The mother sat where she could not be seen. A dog was brought into the room and allowed to jump up on the couch beside her. The baby watched intently every move the dog made, but did not attempt to reach for it. Then she turned her head aside. The other light was turned up and the dog again exhibited. The infant watched very closely every move the dog and the experimenter made, but did not attempt to catch the dog. Exhibited no fear reactions, no matter how close the dog was made to come to her.

The *black cat* was then brought in (both lights on). The cat rubbed against the baby's feet and put her front paws in the baby's lap, touching its nose to her hand. The baby watched intently and reached for it with her left hand. The front light was then turned out. The experimenter held the cat closer to her and she reached for it with both hands.

*Rabbit.*—She reached for it with both hands as soon as the experimenter came into the room with it in his arms. The front lights were turned on. The rabbit was held out to her. She reached for it at once with both hands, trying to put her fingers in its eyes. She caught hold of a piece of fur above the rabbit's eye and pulled hard.

*Pigeon.*—The front light was turned out. She reached for the bird with her left hand before the experimenter was ready to present it to her. The pigeon's wings were released and it fluttered violently just in front of the baby's eyes. She continued to reach for it with both hands even when the wings brushed her face. When the bird was quiet it was presented to her again. She reached for it even more eagerly. She tried to take hold of the pigeon's beak with her left hand, but failed because the bird continually bobbed its head. The light was then turned on. The pigeon again flapped wildly. The baby looked at it intently with widely opened eyes, but this time did not reach. She showed no fear however. It was then held out to her again when it had become quiet. She reached for it at once with both hands, held the feathers and tried to put her fingers into its eyes.

*175 Days Old.*—The baby was placed in a small chair and tied in and put behind a screen so that she could not see any of the people in the room. The dog was allowed to walk suddenly around the screen in front of her. She showed no fear

when the dog rubbed against her legs. She did not reach for him however. While she was still in the same position, the experimenter held the pigeon in front of her and allowed it to flap its wings. She reached for it with both hands the moment it was presented to her and did not withdraw her hands while the bird was flapping its wings. She continued to reach as the bird was moved out of her range.

*The cat* was then brought around the screen and placed on the couch just in front of the baby's chair. She did not reach for it, but followed it with her eyes. It was held very close to her. She reached for it with her left hand and touched its head. The cat was then moved away, but she continued to reach for it. Then the cat put its front feet in her lap. She reached with her left hand and followed with her right, touching its ears.

*Rabbit*.—She reached with her left hand at once when the rabbit was still too far away to touch. When it came close to her she reached with her left hand and touched it.

She was then taken into the dark room with both lights turned out and seated in a small chair. A newspaper was lighted before her and allowed to burn in a large metal bucket. She watched it intently from the moment the match was struck until the flames died down. She showed no fear, but did not attempt to reach.<sup>1</sup>

While being tested in the large room for eye-hand coördination, the dog suddenly began to bark at someone entering the room. He was quite near the baby. He barked loudly and jumped about at the end of the leash. The baby became perfectly still, watching intently with widely opened eyes, blinked at the bark, but did not cry.

*179 Days Old*.—She was taken out to Druid Hill Park in an automobile for the first time in her life. She was wide awake the whole time. She was carried rather rapidly through the grounds of the small zoo at the park. The camel was braying and came up to the fence as we approached, rubbing rather violently against the fence, coming within a few feet of the baby. This produced no fear reaction and no constant fixation. She was then taken to the cages containing the cinnamon and black bears. She gazed at them from time to time, but with no constant fixation. We then took her into the monkey house which contained also a large number of parrots and other smaller birds. The monkeys came to the sides of the cage and from time to time attacked the wires. Three or four times they came up and made threatening movements and actually caught the experimenter by the arm. The child did not seem to be in the least afraid. The peacocks were making their rather uncanny sounds within twenty feet of her, but she did not turn her eyes towards the source of the sound. She was then taken back to the camel yard and the camel again 'performed' nicely. Two camels came up to each other and rubbed noses and put their heads over the dividing fence. The baby was within two or three inches of the camel's nose on several occasions, but while she followed the movements with her eyes, she showed no pronounced reactions of any kind. She was then taken to the Shetland pony, who put his nose through the wires and showed his teeth. She was within a few inches of his mouth. Outside of following movements of the eyes, no reactions were observable. She was taken near two zebras. They came to the edge of the fence, within a few inches of the baby. The zebras were possibly followed slightly more intently with the eyes, but there was no other observable reaction. While the baby was watching the zebras an ostrich came close to her and brought its head to the wire, but did not strike the wire violently. During approximately half of the experiment the baby was carried by her

<sup>1</sup> When tested seven days later with this she micturated, but no general fear reaction appeared (possibly normal bladder reflex).

mother and the rest of the time by the experimenter's secretary. She had never been carried by this individual before. At times the mother was kept out of the range of the baby's vision.

Baby Nixon, girl, 126 days of age, had just learned the eye-hand coördination. She was put through exactly the same series of situations. Slight differences appeared, e. g., when the cat rubbed its head against the baby's stomach, there was a distinct start, a tendency to stiffen. While the experimenter was out of the room getting the rabbit, three persons were left with the baby in the dark room (dim light). All were sitting very quietly. She was being held by a stranger. Suddenly the baby began to cry and had to be given to the mother for a few moments. She quieted down immediately. Again when the pigeon flapped its wings near the baby's face, she gave a distinct jump, but did not cry or show other signs of fear. When the dog was made to bark (lighted room), the baby blinked her eyes at every bark, but gave no other reaction. She smiled throughout most of the situations. She smiled all through the burning of the paper in the dark room.

It is thus seen that this unusual opportunity of testing children's reactions to their first sight of animals yielded few positive results. At least we can say that the older statements which maintain that violent emotions appear must be very greatly modified. Of course it is always possible that the children were too young, but this has not very much weight since we have tested children from birth through to 200 days. These children left the hospital shortly after the tests and further experimentation could not be made. As a control test, similar observations were made upon a colored baby girl (Lee) 200 days of age, who had been under observation from birth. She lived in the city under the usual environmental conditions. Exactly the same results were obtained. There was practically no evidence of fear.

*Conditioned Emotional Reflexes.*—This baby's reactions to darkness were tested at 115 days. Lee (as well as many others) had been tested many times in the dark room with negative results but she had not been tested for many days before the present observation was made. The following extracts bring out most of the points.

*115 Days Old.*—The baby was quiet. She was taken by the mother to the dark room and placed on a couch in the center of the room. The light was turned out. For the first two minutes she kept quiet, then grew fussy, and at the end of five minutes cried. The experimenter went in and turned up the light and left her there. She stopped crying, but at the end of two minutes was crying as lustily as when she was in the dark.

*122 Days Old.*—Taken to dark room with light left on. She began to fret

at the end of forty-five seconds, crying loudly at the end of seventy seconds. Cried loudly for a minute and forty-five seconds. She was then taken out and quieted and afterwards returned to the dark room. She began to cry when she was placed on the floor. After a test in a well-lighted room on the eye-hand coördination, she was taken back to the dark room with the lights on. She began to cry immediately while being placed on the floor.

*129 Days Old.*—She cried whenever she was left in the room alone.

*136 Days Old.*—The baby was left in the dark room with the light on. She began to fuss in one minute and to cry at the end of two minutes. The experimenter went in and stood in front of her without touching her. This did not stop her crying. Then the mother went in and stood in front of her without touching her, but this did not stop her. The moment the mother picked her up, crying ceased.

Although there is little new in this example that throws light upon the emotions, nevertheless it shows most clearly at how early an age the human infant learns to control the actions of its attendants. The conditioned reflex evidently has a genuine function.

*Are there other Original Emotional Patterns?*—It is thus seen that so far our attempts to bring out emotional patterns distinct from those enumerated on pages 169 ff have been barren of result. If it were possible to continue such experiments through a much longer span of a child's life, and if we could face him with a much larger number of situations that more nearly touch his daily life activities, it might be possible to extend the list. It is realized that we are working here with very young members of the human species. A good deal of organization and development takes place after two hundred days. Some very complex situations have yet to be faced, such as masturbation (and in boys especially, the first masturbation after puberty); the first menstruation period in girls; complex situations connected with family life, such as quarrels between the parents, corporal punishment, death of loved ones, all of which have to be met with for a first time. We know from later observation that these do become hitched up to emotional reactions; whether they are original or transferred does not appear from our studies. It would be especially desirable to study the reaction states we now designate by the names of shame and shyness, embarrassment, in this connection. We are of the opinion that most of the asserted emotions are of the consolidated

type (that is, emotion plus instinct, plus habit) or emotional attitudes. These are discussed on page 186.

Attention is called here to the limitations of the genetic method. As long as we can keep the baby under constant observation, a great deal of simplification can be obtained in the study of the emotions, but the human infant is a part of a social group and must sooner or later be returned to it. Things happen so fast then that a separate tabulation of events cannot be made. Under ordinary conditions, the emotions take care of themselves in a normal child, that is, society, including of course the parents and the family group, furnishes its own corrective for failure to react emotionally, for wrong emotional reaction and for over or under reactions. At times, however, due either to defective environment or to defective heredity, the emotions may go wrong. The genetic method is not of service. The emotional life of the individual must then be studied by the psychopathologist. Again, in business and professional life (especially in the Army and Navy), more and more emphasis is being placed upon what may be called emotional temperament. It is thus evident that the applied psychologist must have some means of making studies of emotional activity in adults. Finally, the scientific psychologist, for methodological and purely technical reasons, devises methods for the study of emotions in the hope that they will yield scientific results, or that his methods may prove of such value that they can be employed by the psychopathologist, by the criminologist and by the applied psychologist. A short account of the methods which can be used where the genetic method is not applicable follows:

*Methods Employed in the Detection of Implicit Emotional Response.*—The explicit portions of the pattern reaction in emotion are, as we have tried to indicate, usually the least important constituents. When they appear, systematic observation enables us to note them with sufficient scientific accuracy. In the study of criminals, of psychogenic disorders and of normal individuals, often all explicit emotional manifestations disappear. The exciting situation is complex. On

the one hand it inhibits overt vocal response, but on the other initiates a train of (visceral) implicit activity. Questioning the subject may reveal nothing. He may deny that the stimulus produced any reaction whatever, and yet the next moment he may drop his cigarette, bite his nails, or hesitate or stumble over a word. Popularly we speak in such cases of deceit, concealment of the emotion, 'repressions.' In many cases, however, the individual would report his observations upon himself correctly, if he could observe them, but the movements may be of such a fleeting character as to escape observation, or his intellectual level may be of such a low grade that he cannot make the observation. In such cases there are often so many disturbing factors that self-observation is not possible. Several methods are in use by means of which we can detect the implicit side of emotion.

1. *The Controlled Association Word Reaction.*—The subject is told to respond immediately with a word to a given visual or auditory word stimulus. The stimulus words are made up before the test. Some of the words are neutral, the others are the 'significant' words which refer to the emotional situation. The indicators of implicit response or tension obtained from the subject are unduly long reactions (with occasional appearance of explicit forms such as the giggle, dropping the eyes, a flush); significant response words, showing that the stimulus word was a part of the emotional setting; repetition of the same word; too rapid responses; low level responses; failure in responses (there are several variations in this method).

2. *The Free Association Method.*—The subject is started on any selected word, possibly a fragment from a dream, and told to 'speak the words as they come.' He begins. For a time the words come freely and then they fail. There is blockage. New associated lines are begun. Sooner or later, however, in disturbed cases all lines seem to converge and blockage occurs whatever the start may have been. The blockage seems to occur at the point where the words relating to the emotionally exciting object belong in the associated train of words.

3. Dream study and analysis often reveal emotional tension. They may be studied by the common sense method of questioning the patient now from one angle now from another, but they are often analyzed by employing the two methods described above singly or in combination. Dreams are a part of a person's total reaction. They are as good indicators of the nature of his personality, of his stresses and strains and emotional life generally, as are any of his other activities. We have already stated that we can judge the emotional level of an individual by watching his daily routine of activity. To make this statement complete, the dream activity in sleep and day-dreams must be taken into account. These are word reactions but not isolated reactions or reactions of the muscle twitch kind. They are connected and associated activity, fully as complete oftentimes as house-building, delivering a lecture, or putting through a big business deal. The study of dreams, since the dream language is extremely symbolic, requires individuals especially trained in that field.

4. The study of slips of word or pen, poor adjustments, over and under reactions, bodily postures and attitudes. These can be studied by general observation and by the methods which are employed in the study of dreams.

In discussing these methods, it should be stated that the psychologist busies himself with them principally from a methodological standpoint, that is, by determining the range of applicability, their reliability, the best technique, etc. The psychopathologist uses them for practical purposes. The reshaping and rebalancing of a personality often depends upon the finding of situations connected with an emotion, or upon finding out whether there is an emotion where normally there should be one. He uses all of the above methods, and in addition his common sense, combining it all with general observation of the patient's whole personality. In gathering his data, it is often necessary and desirable for him to question the patient upon the significant events of his life history; the things he is naturally inclined to do and inclined not to do (positive and negative reaction tendencies);

the books he has read, the way they affected him; the types of situation in real or dramatic life which influenced him most; his main emotional assets; the easiest way to get an emotional rise out of him; the trend of his daydreams and the types of aircastles he builds; what his chief lines of sensitiveness are; his conflicts and temptations, and the way he finds himself meeting these difficulties. A full discussion of these factors requires more space than we can give.

In addition to the above methods, several others are being developed:

5. The determination of increased sugar in the blood or urine before and after presentation of a stimulus when there is reason to infer that the stimulus is not without significance (page 189).

6. The emotional questionnaire of Woodworth, and the various character analysis outlines. The subject answers by 'yes' or 'no' a series of questions, such as: Were you considered a bad boy? Were you shy with other boys? Do you know of anybody who is trying to do you harm? Did you ever make love to a girl? Have you ever had any great 'mental' shock? Does it make you uneasy to have to cross a wide street or an open square? Did you ever feel a strong desire to steal things? Did you ever have the habit of biting your finger nails? Do your feelings keep changing from happy to sad and from sad to happy without any reason? Have you ever been afraid of going insane? If there is unstable emotional temperament, the fact is supposed to be revealed by the nature of the answers.

7. The so-called psycho-galvanic reflex. Here the subject sits in a quiet room with two non-polarizable electrodes upon two parts of the body. The electrodes are connected to a sensitive galvanometer. A definite deflection of the needle is obtained. Emotional stimuli are then given, and their effect noted by the deflection of the needle. So far in our laboratory this method has not been found serviceable. It is hoped, however, that with an improved technique, the action currents in the heart revealed by the string galvanometer can be made to yield serviceable results.



8. The so-called expressive methods. These consist of the recording of the respiratory changes, vaso-motor changes; automatic writing and drawing (planchette). Such methods in general have proven of slight value. The respiratory curve is a very sensitive indicator (showing conditioned reflexes quite clearly) but it is subject to so many influences that the significant changes are often obscured and their interpretation is made difficult. This is equally true of vaso-motor changes.

*Substitution of Stimulus: Attachments and Detachments:—* Under the action of environmental factors (habit influences) situations which originally did not call out emotional response come later to do so. This enlargement of the range of stimuli capable of calling out emotional activity is responsible largely for the complexity we see in the emotional life of the adult. We obtain some of the clearest and at the same time some of the simplest examples of stimulus substitutions of this type in the animal world. In 1905 the author while working with rats had a small trap door in the home alley in a maze. The animals in running the final lap would walk over the trap, throw it, and thus shut themselves off in the food box. The trap sank somewhat as the animals passed over it and made considerable noise when released (noise and lack of support, see page 169). After running over it once or twice, the animals showed every sign of fear—crouching, trembling, panting, defecating. They refused to eat. After two or three more trials, they began to jump the whole trap. The noise and the slight sinking of the trap which so terrified them was thus avoided, *but nevertheless the fear reaction remained*. Even after the trap had been removed and the floor made perfectly smooth, the rats continued for many trips to jump at the old position of the trap, springing over just as though the trap were actually present. Every evidence of fear remained. We see the same substitutions very clearly in the horse. If a horse is violently frightened at a certain point on the road by a terrifying object (a rolling paper in one observed case), it may exhibit the fear reaction when again passing over that part of the road although the

terrifying object is no longer present. A shaky bridge will make a sensitive horse terror-stricken, and this will endure long after the bridge has been made of concrete.

The same phenomenon is clearly observable in children. As was brought out above they show little fear of animals. If however one animal succeeds in arousing fear, any moving furry animal thereafter may arouse it. In one observed case a child at 180 days had a small dog tossed into its carriage. She became terrified and thereafter showed marked reactions not only to dogs but even to rapid mechanically moving toys. At 600 days she was placed on the floor near her mother and father and two children with whom she had been playing. A very tame white mouse was placed on the floor near her. She watched it for a moment, her lips puckered, she shook slowly from side to side, squirmed, retracted hands and arms, broke into a cry, scrambled to her feet and fell headlong into her father's arms.

The emotional transfers begin very early in life. The following diary of one of the infants under observation in the laboratory is clearly expressive of the process:

*Lee, 67, 80 and 87 Days of Age.*—When first laid on the couch (where grasping reflex was tested) she would smile and gurgle on all of the above dates, but after testing the grasping reflex, she would cry the moment she was put back on the couch. When picked up she would stop, and when put down she would start to cry. If left on the couch for any length of time, she would stop crying, but if the experimenter approached her or touched her hands with the grasping rod, she would immediately start to cry.

*101 Days of Age.*—She was laid on the couch by her mother. She gurgled and smiled. The mother then took her up and held her for a few minutes and again put her down. Again she smiled and gurgled. The experimenter then tried out the grasping reflex upon each hand. She cried loudly and struggled. As the experimenter first approached her with the rod to make this test she did not cry, but when the rod was put into her hand she began to whimper and actually cried before lifting was begun. After the test the mother took her up and held her until she became quiet. She was laid down, but immediately began to cry. The mother again took her up and quieted her and put her down with the same result. Repeated, with the same result.

*108 Days of Age.*—The above conditioned reflex did not carry over completely for the week. When her mother first laid her on the couch she did not cry. She was quite restless however. The first contact of the rod in the left hand caused only a whimper. This became stronger on touching her right hand. She cried outright as soon as the rod was raised and before she had supported very much of her weight.

*115 Days of Age.*—As soon as the mother was seated with the baby in her lap, the experimenter entered the room and tried to put a piece of candy in her hand (earlier

tests had been made upon the eye-hand coördination). She began immediately to whimper and then to cry. This in all probability was the carrying over of the conditioned reflex, *i. e.*, the visual stimulus of the experimenter was enough to set off the crying reflex.

The fear reactions we see in the dark, in graveyards at night, at lightning, and in many other definite situations, probably belong in the conditioned emotional reaction class. We would put all of the definite phobias (where the reaction is to a definite situation or object) in this class. Such reactions are more numerous in individuals of the unstable emotional type, and especially among frontier and primitive people where every crackling of a twig or cry of an animal or shaking of a bough may be fraught with danger.

Rage, likewise, is capable of being attached now to one object, now to another, in an ever widening series. That is, given an original situation that will arouse rage (page 170), attachments will occur whenever conditions are at hand for the arousal of conditioned reflexes. An individual hampers the use of the child's arms and legs, constrains it, or holds it badly when dressing it, (original condition for arousing rage). Soon the mere sight of that individual arouses the rage components. Finally an entire stranger whose appearance is even slightly similar to that of the first individual may set off the responses.<sup>1</sup>

The transfers or conditioning observed in love are seen to best advantage in the psychiatric clinic. However, such substitutions are seen in every day life in profusion. The mother who has lost a child may put the same loving care upon the child's crib, clothing or toys that she would put upon the child itself. The man who has lost his wife may exhibit toward his daughter much of the tender and respectful solicitude that he would shower upon his wife. We shall not attempt to enlarge further upon attachments of this type we see in love, since in recent years the subject has received sufficient attention at the hands of the psychoanalytic school.

<sup>1</sup> The conversation of adults often contains such expressions as the following: "I can't stand that person," "I have an instinctive aversion to one who looks like that." A good many such aversions (avoidance reactions) have their roots in such substitutions.

A great many of the so-called transfers we see in love probably belong under the vaguer type of behavior discussed below under 'emotional outlets.'

In general then it seems safe to say that when an emotionally exciting object stimulates the subject simultaneously with one not emotionally exciting, the latter may in time (often after one such joint stimulation) arouse the same emotional reaction as the former. It is probable that conditioned reflexes of the second, third and succeeding orders are also continually arising. In the process, the reaction pattern probably gets broken up to a large extent. Part reactions belonging to love, rage and fear might all appear in the reaction to such a substituted stimulus.

In addition to this sudden type of transfer or substitution which undoubtedly belongs in the class of conditioned reflexes, there are the 'attachments' and 'detachments' to persons, places and things which come by the slow process of association or habit connection. They probably do not differ in origin from the type just considered except for the increased length of time required for their formation.

*Emotional Outlets: Diffusion.*—On page 168 we spoke of changes in the general level of activity due to emotional disturbance. We spoke there of a normal, of a high and of a low level. Probably if an individual were perfectly balanced, the distribution of emotional activity would be uniform and all organized activity would share equally, *i. e.*, there would be a mere change in level. But few individuals possess that perfect balance which would make this possible. Furthermore society and one's own organization often make emotional outlets impossible along certain lines. When emotional expression is blocked in any one region, outlet seems to take place somewhere else. An illustration will make the point clear: *A* is insulted by a larger man, or by an older or a younger man, or by one from whom he is receiving his daily bread. The instinct and habit organization of *A* would lead to an attack, or at least to its equivalent—a strong verbal retort. But other features in the total situation (the fact that he is larger, older, younger) inhibit these outlets.

The emotional pressure however has been aroused. He may proceed to his office, fire his bookkeeper or office boy or terrorize his stenographer. One's family often suffers most in such cases. If a man's wife causes the emotional rise, the children are apt to suffer. The outlet, however, may not always be a harsh word or a blow. If the emotion partakes of the fear or rage components, the blow or harsh word is most frequent. If the thwarted emotion is of the love type, the final outlet may be exhibited by showering kind words or benefits upon someone other than the person calling out but thwarting the love emotion. If the thwarting is brought about by the death of the loved object, the outlet may be found in grief or suicide.

Human life is full of such outlets. If society as a whole puts on too many restrictions (rage) and the thwarted individual is not well-balanced, the outlet may be through burglary or vandalism. In balanced individuals it may have its outlet through swearing or in privately railing at the restrictions of society.

In certain individuals, either through inferior constitution or the narrowness or restrictiveness of their environment, no external outlet seems to be possible. The emotional drainage expresses itself in some form of attitude (page 186); by withdrawal or shrinkage from contact with fellow humans of any kind; in drink or drugs; in ruminations, day-dreams and air-castles—*i. e.*, there may be an implicit language outlet.

The point which rationalizes and gives a reason for all such behavior seems to be, that the individual by so reacting gets relaxation and freedom from emotional pressure. Popularly we speak of 'working off' the emotion, that 'one's rage is cooled' by this or that. The study of these various outlets when they assume pathological form and interfere with the remaining activities of the individual or with those organized functions which society demands of each individual, and the reshaping of such individuals, belong to psychiatry. We see however the same factors at work even in 'normal' individuals, and our training as psychologists is not complete until we are able to note the signs of emotional maladjustment.

We have not the evidence at hand to affirm the view that all of the phenomena seen in diffusion belong to the conditioned reflex realm. The activity seems to be too little stereotyped and entirely too complex to belong in that category. The attachment is not focalized. Probably the simplest way of stating the generally observed fact is that too great emotional pressure is drained off through whatever channel environmental (social) and hereditary factors make possible.

*Consolidation among Emotion, Instinct and Habit; Attitudes.*—Observation seems to show that combinations or integrations occur among emotional, instinctive and habit activities. Our discussion of these integrations will be handicapped to some extent by our not having had opportunity to study instinct and habit. Possibly the activities we see in 'anger' or its more active attitude 'fighting' best illustrate the points to be presented. Anger as we see it exhibited in the insect world probably remains on the emotional instinctive level (hereditary). Habit activities are at a minimum in these animals (though not wholly lacking). In the human race certainly the exciting stimulus is usually one which hampers, jostles, crowds or constrains the individual—the stimulus to rage. The instinctive factors are striking out with the arms and hands, grasping, running toward the object, probably biting it, the while unfleshing the lips. Defensive movements also occur of the instinctive kind. The habit factors express themselves in the scientific 'form' of attack and defense: the way the arms are held to avoid giving the enemy an opening, planting the blow on a vulnerable spot—the eyes or the solar plexus, and in the stance of the feet. The whole group is integrated, the part reactions work together. The individual becomes a fighting-defending, unitary action mass. If the environmental factors are such that actual fighting cannot occur, the subject assumes the 'defiant' attitude. All three factors are still present even in the attitude. Many of the emotion, instinct and habit action tendencies are constrained by social factors. The emphasis has then of course to fall back on the emotional component of the action mass.

In the above rage predominated as the emotional constituent, the hereditary attack and defense movement as the instinct and the trained activities as the habitual. Probably all other forms of emotion—those of the native or more fundamental type, as love and fear, and the broken up, combined and consolidated types which we get through substitution—show the types of combination shown above. To attempt to list these, to show their history and formation through the process of substitution and consolidation, would require a volume (and a very necessary one) of its own. Only a few will be touched upon here. The so-called submissive or inferiority attitude shows itself at once as having fear as the most prominent emotional element. The instinctive factor may not be clearly overt, but it is in general. It manifests itself in shrinking, submission and avoiding—sometimes with the body as a whole, sometimes with special organs as the lips and the eyes. The habitual factor shows itself especially in the language behavior of the adult—hastening to agree, avoiding an argument, and the hesitant voice.

In the sphere of love there are numerous attitudes as shown by the popular expressions 'lovelorn,' 'lovesick,' tenderness and sympathy; more fundamental and prominent attitudes are those of shyness, shame, embarrassment, jealousy, envy, hate, pride, suspicion, resentment, anguish and anxiety. There are many combinations of emotional habit and instinctive factors in all of these attitudes. They actually function by limiting the range of stimuli to which the person is sensitive. For the individual they are fundamental attributes of character, as much a part of him as his arms or legs or his method of attacking a new problem.

This very superficial analysis is not at all commensurate with the rôle these attitudes play in the life of the individual. In studying the life history of any person we can see how they have oftentimes furthered or hindered his life work and disturbed his personal balance. Shyness and the inferiority attitude may keep a man tied all his life to an accustomed, but unremunerative job. They have oftentimes prevented his

marriage or brought about a poorly adjusted marriage or kept him out of a wider social circle. On the other hand, in other cases too much aggressiveness has just as often made impossible a man's chances of making good business and social connections.

*Results of the Physiological Study of Emotions. A. Duct Glands and Smooth Muscles.*—The recent physiological work upon the duct glands of the mouth and stomach has brought out the fact that when the human or animal subject is under the influence of the stimulus of hunger (rhythmical contractions of stomach muscles) conditioned secretion reflexes occur when food (food positively reacted to) is allowed to stimulate the animal visually or olfactorily.

Under the influence of emotional stimuli these part activities are often blocked. This aspect of the phenomena of secretion and movement of the smooth muscles of the stomach is undoubtedly a part of the physiological study of emotion. A number of observers have shown that emotionally exciting situations do check the functioning of the glands. If a child with a gastric fistula is shown food and is then badgered by first handing it to him and then taking it away and then causing it to disappear from vision, crying and other definite signs of an emotional state appear. The secretions are checked. Similar conditions obtain in the case of dogs: if they are put in strange surroundings or if they are fastened in a holder, or finally if they are shown their natural enemy, the cat, the flow of secretion is checked. If the emotional state is long continued, in both man and animals even the unconditioned reflexes may fail for some time, *i. e.*, the actual contact of the substance may fail to arouse the flow of the gastric juices.

A similar phenomenon appears in connection with the peristaltic movements of the stomach, and indeed of the movements on the muscular layer of the whole alimentary canal. Restraining the animal, covering its mouth and nose with the finger, check the stomach contractions very quickly. But we have just seen that stimuli of this kind produce the emotion of rage. The same phenomena appear



in the case of man. People under the influence of fear and rage frequently do not digest their food (due to the checking of secretion) and the food remains in the stomach (due to lack of movements necessary to pass the contents of the canal along).

Excitation of the pain receptors has the same effect as emotional disturbance (probably is a stimulus to rage) both upon secretion and upon the stomach contractions. It is probable that any of the highly exciting emotions act in the same way as those discussed above. Sex emotions aroused by salacious photographs, and pictures, have a definite inhibitory effect upon the rate and amount of secretion of the parotid gland and upon certain reflexes (swallowing).

*B. Effect of Exciting Stimuli upon the Ductless Glands.*—Apparently one of the most important effects that emotional stimuli exert is the release of adrenin. The adrenin in turn liberates sugar from the stored supply in the liver, often in amounts greater than the body can consume. Glycosuria results, *i. e.*, the excess sugar passes over into the urine. This phenomenon often occurs in battle and in extreme emotional situations of any kind (depressing or exciting). Cannon states that young male cats when fastened in a holder become quite frantic, with eyes wide open and pupils dilated; the pulse is accelerated and the hairs of the tail become more or less erect; they snarl and growl as they try to free themselves. Whenever this excited condition occurs there is glycosuria (in from forty minutes to an hour and a half). When a small dog is allowed to bark at the cats, causing them to become excited, the glycosuria manifests itself. Similar results occur in the case of the human being. After hard examinations or exciting athletic contests, students show temporary glycosuria.

When glycosuria occurs, it is an indication of an increased supply of sugar in the blood, since so long as the kidneys are uninjured sugar cannot pass out into the urine until an excessive supply of sugar is at hand. Testing for sugar in the urine is really a very coarse method of detecting the emotional effect of a stimulus. Recently very sensitive

methods have been discovered for detecting the presence of increase of sugar in the blood. A large amount of material has collected in our laboratory as the result of blood sugar tests. It is unquestionably a very delicate indicator and revealer of emotional changes. It has been used in connection with the association word reaction method. This method may be operated as follows: One individual does a certain act and a second individual remains quietly in another room. The two return to the experimental room and the experimenter must decide from the word responses (hesitations, etc.) which one of the individuals performed the act in question. A small amount (few drops) of blood is obtained from both individuals both before the test is made and after and the percentage of blood sugar determined in all four specimens. The individual having committed the 'crime' shows as a result the greater increase in blood sugar. The blood sugar reaction can thus be used as a supplementary method of detecting 'guilt.'

The method is probably delicate enough to decide whether a given individual is emotionally aroused by the mere presence of another individual. These results were obtained by Dr. N. D. C. Lewis. They have not yet been published. It has been shown conclusively that if the adrenal glands are removed emotional stimuli will not cause this increase in sugar either in the blood or in the urine (Cannon and others). The conclusion is well sustained then that emotional stimuli through a reflex mechanism set free adrenin which in turn acts upon the supply of sugar in the liver and converts it into a form which can be used by the muscles after it gets into the blood stream.

In addition to its sugar conversion effect upon the liver, adrenin acts in conjunction with the sympathetic nerves and produces vaso-constriction and hence an increased blood pressure. It has been shown that when a given muscle is active, its blood vessels dilate, thus tending to decrease arterial pressure. If many muscles are called into action at any given moment, these dilated vessels may so reduce arterial pressure that the muscles fail to get their proper food.

Waste products also accumulate in the muscles. Adrenin because of its reinforcing effect upon the vaso-constrictor nerves produces heightened arterial pressure, which increases the food supply to the muscle and removes waste products. The blood is driven out of the vegetative organs of the interior into the skeletal muscles, which have to meet the extra demand when the animal is fighting and struggling to free itself.

*C. Specific Effect of Adrenin.*—There seems to be general agreement that the free adrenin in the blood acts directly upon the muscle in such a way as to neutralize fatigue products. "What rest will do only after an hour or more, adrenin will do in five minutes or less" (Cannon). This result is in addition to adrenin's function in producing a greater food supply to the muscle and increasing the amount of blood circulating through the muscle. After a muscle has been fatigued, *i. e.*, has lost its irritability, the injection of adrenin into the blood (or stimulation of the splanchnic nerve) will rapidly restore the muscle to its resting condition. Cannon also maintains that the presence of adrenin hastens clotting of the blood, which in wounded animals might be advantageous. His results in this respect have not been confirmed by other physiologists.

*Apparent Conflict between Formulations.*—There seems to be a conflict between our early statements about emotion and those gathered from the physiological studies just reported. We first expressed the view that if the emotional stimulus was strong enough or continued for a sufficient length of time paralysis or the death feint would occur. The state attained here is surely not adaptive. The result of the physiological study seemed to show that the organism under the influence of exciting stimuli often takes on a bettered state, one in which greater muscular activity and less fatigue is possible. The conflict can be harmonized. The 'improved' physiological state is apparently due to the action of the autacoid substances. Physiologists have shown that such substances act like drugs. If a small amount of a certain drug, say strychnine, is administered, increased

appetite and increased muscular activity ensue. A bettered general physiological condition may result. On the other hand, if too large an amount is given, the muscles may become rigid and the subject may die. Possibly a similar thing happens in the case of the autacoids. If the substances are set free in too large amounts, there is one type of action, namely, the paralyzing effect. If set free in physiologically serviceable amounts, their action may produce a combined series of reflexes, the total result of which may be a bettered physiological state.

The physiologists have unquestionably overemphasized the adaptive character in all of the major emotions. From Cannon's work it is easy to see how under the emotions of rage, fear and pain stimulation, the possibility of increased muscular effort might be of value, as in fighting, flight, etc. On the other hand it is difficult to see how this physiological state is of value unless the organism is in a situation where the increased muscular possibilities are to be used, but such situations are rare. A soldier in the army receives a letter telling him that his wife has gone off with another man. The news is undoubtedly a strong stimulus; depression takes place and examination shows the presence of sugar in the urine, and naturally an increased supply in the blood, but his routine of camp activity happens to be such that no great muscular demand is made upon him. We may grant Cannon's general position and yet maintain that it is not a very serviceable concept for the ordinary routine of daily life. We are no longer living in a frontier country, and outside of an occasional war, there is not much opportunity to bare our teeth and struggle for existence in the good old primitive way of our ancestors. Cannon's appeal to the biological serviceableness of the emotional reaction needs modification.

There would seem to be no question, but that the immediate effect of the exciting stimuli upon organized activity, as was brought out on page 166, is always disruptive. If an individual is preparing a lecture or writing a book or rendering a musical selection, any strong emotional stimulus at least temporarily disrupts and blocks the organized activity.

The same thing would occur if a group of officers were preparing plans to make an attack on the enemy the following day, and a shell were to burst and tear down a portion of the building in which they were working. It would thus seem necessary to state that the immediate effect of an exciting stimulus is unadaptive, disassociative and disruptive. The immediate effect may endure for an extremely short time, or for a longer time. We have found that the increased sugar in the blood may endure for several hours even after fairly slight emotional stimulation. There is thus a post-shock or post-emotional state. Apparently the post-emotional state may be of such a character that (1) the organism is left less well adjusted and less capable of carrying out organized activities. As an example of this, the death of a child may leave the mother in a depressed and apathetic condition which may endure for months. On the other hand (2) the post-emotional state may be of such a character that the organism is in a bettered physiological state; the activities going on before the emotional stimulus appeared may be resumed under a condition of facilitation and reinforcement. An example of this occurs when a parent punishes a child: there may be immediate improvement noticeable in his whole behavior (but the reverse may also happen; the child may be thrown into a sullen state which might endure for some time). As a less ambiguous example, take the case of an individual working at a low ebb. He receives a letter containing a check which, while it blocks his activity for the moment, has as its post-emotional effect a tremendous influence upon the speed and accuracy of his work for the remainder of the day or even for a longer period. In general we may assume that the effect of an emotion arousing stimulus upon the general level of activity may produce facilitation or the reverse; or it may leave the level unchanged.<sup>1</sup> What result will occur depends

<sup>1</sup> We are in genuine need of some experimental work of a detailed character upon the efficiency of the human organism in the post-emotional state: (1) of the ergographic type of experiment, (2) of the word association type, (3) of the learning type, (4) of the functioning of already well established activities such as typewriting (explicit motor), and (5) of the sub-vocal arithmetic type (well established implicit laryngeal motor habit). The work of H. T. Moore which was called to the writer's notice after the preparation of this paper in part supplies this need.

upon a great many factors: The nature of the exciting stimulus, the individual's character, his general bodily state, etc.

*Rôle of Emotion in Daily Life.*—The main fact about emotion seems to be that the human organism is built to react in emotional ways. We stated in the beginning that they are inherited modes of action. Consequently it is not incumbent upon us as psychologists to give any detailed statement as to their biological serviceableness in keeping the race alive. We should be content with describing the facts and pointing out the rôle that emotion plays in our development and in our daily life. Of course if one is terribly overawed by Darwin, one cannot react until one has pointed out in detail the utilitarian value of every reaction. We are inclined to believe that in both instinct and emotion there are many part reactions which are of no adaptive value to the organism whatsoever: if the organism possesses enough hereditary structures and modes of reaction to enable it to get along in its environment, the process of evolution (selection or elimination) allows it to possess many luxuries in the way of reaction possibilities.

We do not mean to assume by these precautionary remarks that emotions are without significance in daily life. We would emphasize the point that they can and do exist whether they are *always* useful, or useful only at times. (1) Even though they were mere luxuries, so far as biological fitness is concerned, they serve to remove the individual from the monotonous level of existing as a highly perfected biological machine. They give him his ups and downs, make the exact prediction of his acts more difficult (troubling the psychologist and psychiatrist thereby), and in general make him a more delightful personality with whom to work, fight and play. (2) As regards their effect upon the possibilities of the achievement of the individual, we are inclined to agree with William James in his 'Energies of Men' that in very exceptional cases, the heightened state which comes *after* a great emotional crisis may bring about a degree of achievement that could not be dreamed of at the ordinary working level of the individual—Poe, De Quincey, Byron, Goethe

and George Sand would probably never have produced their masterpieces under a humdrum regime. One can take selected cases and marshal an imposing array of such instances. On the other hand, one must preserve one's balance in making the assumption that because a few geniuses have produced great works under heightened emotional tension, such exalted states make for or produce genius. The point seems to be that occasionally under a great tension all part reactions hang together and mutually facilitate one another—every asset and every resource of the individual as long as the effect of the emotional state persists are marshalled for the work in hand. Such occasions are rare. The next emotional shock might as its after effect leave the individual trembling, enervated and flat; totally incapable of accomplishing anything except the merest routine. We all know from our own diaries of ourselves that under ordinary circumstances if we have a fine piece of work to do, a championship game to play, a delicate piece of apparatus to manipulate, a fine surgical operation to perform, we would not willingly expose ourselves to any strong emotional situation; and yet the brilliancy of our performance might be increased thereby. Certainly in history such achievements have been accomplished under such conditions. Possibly the sheltering which comes from civilization has built up an attitude of timidity, thereby lessening our readiness to take the chances which our predecessors had to take. Society more and more guards against the presence of strong emotional stimuli, since the weak and possibly even the individual of average ability cannot withstand their effects, however well the genius may thrive under their influence.

It is true that the illustrations in which we see the bad effects of emotional shock have been chosen from activities that demand the fine coördinations of delicate muscles. Would the case be different with more constructive activities? Would the plan of a great novel, the writing of a beautiful poem, the painting of a masterpiece, the composition of a great opera be facilitated, or the reverse, by producing in the artist some great emotion? The history of art apparently returns an

affirmative answer. (3) In observing the daily life of a great many individuals, we seem to see the following factors at work: One individual has reached a low level of adjustment; he can typewrite so many words a minute, or telegraph so many words a minute, or make so many entries in his journal. If this low level of adjustment gives the individual his daily bread, he does not depart from it. His social relations at home and on the outside are on the same dead level. His emotional attitudes are stereotyped: One takes the attitude of suffering at everything; another the religious attitude; still another the hard-done-by and the downtrodden attitude. There seems to be a wall around these people. Is there no way of breaking through this wall and getting the individual to reach a higher level of achievement? Emotionally exciting stimuli occasionally seem to accomplish it. The sudden accession of responsibility or wealth; the enforced demands which come with marriage and the rearing of a family; sometimes even a strong rage or fear may break through the stereotyped and habitual mode of response and arouse the individual to the point where he can accept and profit by intensive training (acquisition of greater skill in his field) and eliminate his errors, work longer hours, and plan his work in a more systematic manner.



# A CLASSIFICATION OF REFLEXES, INSTINCTS, AND EMOTIONAL PHENOMENA

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The problem of classifying human reflexes, instincts, and phenomena of the affective mental life arose in connection with a projected text on *Human Psychology*. The literature on reflexes proved disappointingly meager and unsystematic. Nowhere in the works consulted was there found a systematic classification of the human reflexes. Physiologists emphasize a few reflexes—notably the scratch and tail-wagging, which are not germane to the present inquiry; the rest are mentioned more or less casually.

The instincts and emotions have received better treatment. Under the inspiration of James several psychologists (notably Ribot, Thorndike, Angell, and McDougall) have listed and discussed both types of phenomena very fully. If criticism of their work may be offered it is that the instincts, and consequently the emotions also, have not been traced back to distinctive sources in the vital organic processes.

I. REFLEXES.—In making the classification of reflexes (Table I.) the procedure was wholly empirical. The literature was examined carefully. James's list of emotions was found to include many simple activities, such as biting, which are more properly classed as reflexes. Gould's *Medical Dictionary* contains a full list of diagnostic reflexes. The various muscles and parts of the body were examined for additional data. (Note the anomaly of the common term, 'pupillary reflex'!) Observation of self and others was practiced for some time to complete the list; shuddering and starting were added in this way. In some cases it was found impracticable to list the reflexes separately; thus a large group of distinct phenomena are listed together as 'myenteric reflexes.' Introspective evidence was used to determine the

## TABLE I

## HUMAN REFLEXES

*A. Purest—Least Subject to Central Modification in Adult*

"Pupillary" or iris reflex	Snoring
Ear twitching (controlled in some individuals)	Shuddering
Equilibration	Starting (to sudden noise, etc.)
Hand withdrawal (to heat and pain)	Trembling
Myenteric reflexes (operation of stomach and intestinal muscles in digestion)	Rhythmic contractions (in epilepsy, paralysis agitans, etc.)

*B. Largely Pure—Subject to Inhibition or Reinforcement*

Winking	Hand twitching (to dermal pain)
Accommodation, ciliary reflex	Plantar reflex (to stimulus on sole of foot)
Eye-fixation and convergence	Great toe reflex
Hiccoughing	Vasomotor changes (blushing, etc.)
Sneezing	Breathing changes (to specific stimuli and to onset of sleep)
Patellar reflex (knee-jerk)	Groaning
Dizziness reflexes	Laughing
Yawning	Cramp movements
Vomiting	Shivering
Facial reflexes (to bitter taste, etc.)	Squirming
Salivation	
Tickle reflexes	

*C. Occasionally Pure, more often Centrally Modified*

Coughing	Smiling
Swallowing and gulping	Wincing, etc.
Visceral discharge, etc.	Stretching
Functioning of sex organs	Convulsive contractions (to deep pressure and heat, to pricking and other dermal pains, and to visceral pain)
Nasal reflexes	
Gasping	
Weeping	
Sobbing	

*D. Pure in Infancy, Centrally Modified in the Adult*

Sucking	Tugging (wrist reflexes)
Biting	Clasping (elbow reflexes)
Spitting	Reaching (shoulder reflexes)
Hunger and thirst reflexes	Kicking (gluteal reflexes)
Vocal reflexes	Stepping (toe and ankle reflexes)
Reflexes to odors	Jumping (ankle reflexes)
Turning the head	Sitting up
Grasping (finger reflexes)	Bending forward
Tossing (elbow reflex)	Rising

*E. Posture Reflexes*

Holding head erect	Standing
Sitting	

extent of central or voluntary control to which each reflex is subject in the adult.

TABLE II  
HUMAN INSTINCTS

1. <i>Nutritive</i>	2. <i>Reproductive</i>
Metabolic expressions	Mating (sexual attraction, courtship)
Walking	Maternal
Feeding	Filial (of infancy)
Wandering [Hunting]	
Acquiring [Hoarding]	
Cleanliness	
3. <i>Defensive</i>	4. <i>Aggressive</i>
Flight	Fighting
Subjection	Resenting
Hiding	Domineering
Avoiding	Rivalry
Modesty [Shyness]	
Clothing [Covering]	
Constructing [Home-making]	
5. <i>Social Organization</i>	
Family (parental and filial)	
Tribal [Gregarious]	
'Apathetic'	
Sympathetic	
Antipathetic	
Coöperating	

2. INSTINCTS.—The evolution of the various instincts may be traced to the adjustment of relations between the organism and its environment. Some of the vital processes, such as organization, growth, and regulation, do not seem to have led to the evolution of any specific instinct. Other processes, such as *nutrition* and *reproduction*, are accomplished in part by reflex and instinctive activity. Furthermore, the general relations between the creature and his environment have led to certain types of instinct, which may be classed as *defensive* and *aggressive*. The social relations of mankind yield an additional group—the instincts of *social organization*. These five classes were adopted as bases of the present classification.

The method used in preparing the list of instincts (Table II.) was as follows:

1. The available lists were compared and duplicates rejected, the most appropriate term among alternatives being selected.

2. The terms so obtained were classed under the five headings.

3. Other instincts were noted empirically, from a study of human life-activities, and assigned to the appropriate class.

4. The terminology was critically revised by consulting lists of synonyms, such as Roget's *Thesaurus*, the aim being to adopt the most representative term in everyday use. Later the terms were still further revised to differentiate as nearly as possible according to general usage between instincts and emotions.

The aim in this and the following lists was to include only well differentiated phenomena. In a few cases important variants are given, but the finer shades are intentionally excluded. Primitive types of instincts are given in brackets after the more developed form; *e. g.*, wandering [hunting]. The term *apopathetic* is used to designate social instincts which occur as responses to the presence and attitudes of others, where the character of the action is neither sympathetic nor antipathetic (see Thorndike, *Educational Psychology*, ch. 3).

### TABLE III

#### INSTINCTIVE TENDENCIES OF MAN

Imitation	Dextrality (right-handedness)
Play	Esthetic expression
Curiosity	Communication

3. INSTINCTIVE TENDENCIES.—Instinctive tendencies (Table III.) were distinguished from instincts proper with reference to their reflex composition. An instinct was defined as a grouping of certain reflexes which vary in kind and degree according to variations in stimulation. An instinctive tendency, on the other hand, offers all sorts of different expressive manifestations. It includes a great variety of reflexes, and its unity as a separate class is apparent only through a broad general conception of the creature's life-

activities. Thus curiosity and play find most varied forms of expression. They do not constitute instincts under the definition here adopted; but they do represent typical forms of instinctive activity. Each constitutes a separate type of instinctive tendency.

TABLE IV  
HUMAN EMOTIONS

1. <i>Expressive (Nutritive)</i>		2. <i>Reproductive</i>	
<i>Emotion</i>	<i>Basis</i>	<i>Emotion</i>	<i>Instinct</i>
+Joy (Enthusiasm)	Diffused feeling	+Love	Mating
-Grief (Despair)	"	+Lust	"
-Shock	"	-Coyness	" (female)
+Mirth	"	-Jealousy	" (male)
+Ecstasy	"	+Tenderness	Maternal
Restiveness	"		
Exuberance	Play		
+Wonder	Curiosity		
3. <i>Defensive</i>		4. <i>Aggressive</i>	
<i>Emotion</i>	<i>Instinct</i>	<i>Emotion</i>	<i>Instinct</i>
-Fear	Flight and Hiding	-Anger (Passion)	Fighting
-Disgust	Avoiding	-Hatred	Resenting
-Timidity	Shyness	-Envy	Rivalry
(Embarrassment)		+Pride	Domineering
-Shame	Covering	+Exultation	"
+Awe	Subjection		
5. <i>Social</i>		6. <i>With Temporal Projection</i>	
<i>Emotion</i>	<i>Instinct</i>	Retrospective Reference:	
+Affection	Family	-Regret (Remorse)	
+Cordiality	Gregarious	+Satisfaction (Elation)	
-Pity	Sympathetic	Surprise	
+Gratitude	"	Prospective Reference:	
+Admiration	"	+Hope	
-Detestation	Antipathetic	-Dread	
-Revenge	"	Anxiety	
-Suspicion	"		
-Scorn	"		

4. EMOTIONS.—The method used in preparing the list of emotions (Table IV.) was generally the same as for instincts, with one additional step: Before the terminological revision the various terms suggested were tested introspectively to determine whether they represent (and call up) a true emotional experience.

The classification of emotions into pleasant (+) and unpleasant (−) was obtained by introspective examination. Introspection was also used in choosing between synonyms. It may be mentioned that Roget's manual was found exceedingly helpful in the revision of terms, though it proved of no value whatever in filling out the lists.

TABLE V  
HUMAN DISPOSITIONS

1. <i>Expressive</i>		2. <i>Reproductive</i>	
<i>Attitude</i>	<i>Emotion</i>	<i>Attitude</i>	<i>Emotion</i>
Cheerful	Joy	Affectionate	Love
Despondent	Grief	Lascivious	Lust
Dazed	Shock	Jealous	Jealousy
Frivolous	Mirth	Motherly	Tenderness
Zealous	Ecstasy		
Erratic	Restiveness		
Romantic	Exuberance		
Devout	Wonder		
3. <i>Defensive</i>		4. <i>Aggressive</i>	
Cowardly	Fear	Hostile	Anger
Courageous	"	Vindictive	Hatred
Of Aversion	Disgust	Malicious	Envy
Cautious	Timidity	Ambitious	Pride
Reserved	Shame	Arrogant	"
Servile	Awe	Bold	Exultation
5. <i>Social</i>		6. <i>Instinctive and Sentimental</i>	
		<i>Attitude</i>	<i>Basis</i>
Devoted	Affection	Miserly	Acquiring instinct
Friendly	Cordiality	(Avaricious)	
Compassionate	Pity	Orderly	Cleanliness
Of Attachment } {	Gratitude	Nomadic	Wandering instinct
Loyal } {	Admiration		
Antagonistic	Detestation	Credulous	Belief
Sullen	Revenge	Skeptical	Disbelief
Distrustful	Suspicion	Perplexed	Doubt
Supercilious	Scorn	Biased	Belief and Disbelief

5. DISPOSITIONS.—The emotional attitudes, or dispositions (Table V.), were derived directly from the emotions. Here the test by self-observation and observation of attitudes in others proved the most important part of the work. The indefiniteness of popular language and variations in common usage made the selection of terms in this field somewhat

arbitrary. Thus it may easily be asserted that the term *friendliness* represents more nearly an emotional state, and *cordiality* the corresponding disposition. In doubtful cases the writer was guided by his own 'feel' of the connotation of each term.

The three correlated groups—instincts, emotions, and dispositions—were critically compared with one another, and with other mental phenomena, which resulted in adding certain data to the lists. Thus the emotion of wonder appears to be derived not from an instinct but from the instinctive tendency of curiosity; the retrospective emotion of regret is differentiated by the memory coefficient in imagination and thought. Certain dispositions (such as avarice) appear to be traceable directly to instincts (the acquiring instinct), with no mediating emotion. Others (such as credulity and perplexity) are derived not from emotions but from sentiments.

So far as vocabulary and usage permit, instincts have been designated by present participles, emotions by nouns, and dispositions by adjectives.

The tendency to a cut-and-dried logical scheme was challenged to the utmost. It was recognized that certain instincts may not lead to corresponding emotions and that in some cases an instinct may give rise to two or more distinct emotions, or an emotion to several dispositions. The order of importance may be quite different in the three groups.

No attempt was made to connect the instincts with specific reflexes. The writer recognizes the extreme importance of this step in working out a systematic behavior psychology—or in any 'general' psychology, for that matter. But the problem requires a mass of experimental research, which no single laboratory can hope to accomplish alone.

The tables are offered for comment and criticism and as a possible working basis for future investigation. The opinion may be ventured that *no* satisfactory catalogue of the emotions and dispositions will be reached till we are able to measure qualitatively and quantitatively the various secretory processes and metabolic changes which occur in the human system. That this goal lies far ahead may readily be admitted.

## AFFECTIVE PSYCHOLOGY IN ANCIENT WRITERS AFTER ARISTOTLE

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Psychological interest in ancient doctrines of the affections—using the term broadly to denote what are popularly called the feelings, including emotions—naturally attaches chiefly to the teachings of Plato and Aristotle. Plato's doctrines of pain and pleasure as respectively disturbance and restoration of organic equilibrium, or, again, as phenomena of want and replenishment, and Aristotle's doctrine of pleasure as the concomitant and completion of unimpeded activity 'like the bloom on youth,' are among the most important contributions of antiquity to theory in this branch of psychology; their influence is traceable through all the centuries and is still potent. Similarly the significance of Aristotle's treatment of the emotions is evident in the extent to which the descriptive psychology of these and cognate phenomena, in aiming mainly at precise definitions and delimitations, has followed his model. Moreover, it is in the earlier period that we find the first broad outlines of a biological theory relating affective phenomena to organic welfare and the first beginnings of the attempt to connect them definitely with bodily conditions. Nevertheless, to the historical student, the discussions in this field in the post-Aristotelian schools are far from negligible. To begin with, there is no subject in psychology on which more came to be written. The direction taken by philosophy, which now seeks, characteristically, to find for the individual a way to happiness and security of soul in the midst of a changing and troubled world, brought with it an increased interest in his pleasures, pains and so-called passions. In literary output the lead is taken by the Stoics. Treatises on the passions are ascribed to Zeno, Chrysippus, Posidonius, Hecato, Herillus, Sphærus. Cleanthes wrote one on pleasure.



Dionysius Heracleota also wrote one on pleasure in four books and another in two books on freedom from passion (*περὶ ἀπαθίας*). Seneca composed a treatise in three books on the single passion of anger. All the Stoics deal with the passions in their ethics. Indirectly also they contributed largely to the discussion by arousing opposition. The principal controversy turned on the question whether the passions were, or were not, 'contrary to nature.' The Stoics—to speak broadly—said that they were and should therefore be extirpated; the Peripatetics denied this and required only that they should be controlled. Here, as throughout the ancient period, and, for that matter, throughout every period, the motive underlying the examination of the affections is practical. In post-Aristotelian philosophy the ethical interest, and along with this the religious, is everywhere paramount. The primary question relates to the worth and place of the affections in an ideal scheme of life; it is for the sake of this alone that any enquiry is undertaken as to their nature and conditions. This interest vitiates the scientific analysis and perplexes the psychological theory. But, in spite of this, an advance is made in several directions. The complexities of the phenomena are more fully realized; distinctions are recognized that had previously been overlooked; new aspects of mental life are brought to light in respect to the attitude the mind is capable of taking towards its affections; some new views are developed regarding their function; now for the first time a serious attempt is made to classify them; and the connections of the affections with the bodily processes, if not more satisfactorily explained than heretofore in detail, are in some respects more precisely conceived in theory.

An illustration of this advance may be found in the Epicurean doctrine of pleasure. Regarding pleasure as an affection proper to the animal nature and pain as alien thereto, Epicurus agreed with Aristippus, the founder of ancient hedonism, in holding that all pleasure is good and that to attain the greatest amount of pleasure is the supreme aim of life. But he observed, as bearing on this question of the greatest amount, what seemed to him an important difference

among pleasures. Admitting that all pleasures are alike in intrinsic quality, as pleasures, he called attention to the fact that they are not all alike in intensity or durability. Some are intense, but transitory, others tranquil, but more lasting. Aristippus, followed by Plato, had made all pleasure consist in motion. Epicurus finds two distinct kinds of pleasure; one consists in motion (ἡ κατὰ κίνησιν), but one is a state of rest (ἡ καταστηματική). To the former he reckoned bodily pleasures and the more boisterous emotions, to the latter the calmer pleasures of the mind.<sup>1</sup> This distinction is of the utmost importance for him in its bearing on the question of the best life. The Cyrenaics held that bodily pains were worse and bodily pleasures greater than mental. Epicurus maintains the opposite. He notes, in the first place, what Plato had made the basis of his condemnation of (bodily) pleasure as illusory, the contrast-effects in the experience of the bodily affections as tending to diminish the force of the argument for their intrinsic intensities. He notes, further, a difference in the time-relations of bodily and mental affections; bodily pain, he says, is acute, but transient, whereas the mind feels grief with reference not only to the present, but also to the past and future. And similarly of pleasures.<sup>2</sup> The mental, therefore, possesses the wider range as well as the greater independence. But his main argument against the Cyrenaics is derived from considerations regarding the varying relations of pleasure, pain and desire. Desire is want and want is painful; abolish or moderate the desire and you thereby abolish or moderate the pain. Now if pain is taken away we rejoice though no bodily pleasure follows, while if pleasure is removed, pain does not necessarily follow.<sup>3</sup> Again, it is only when we are pained at the absence of pleasure that we feel the need of pleasure, but when we feel no pain, we no longer need pleasure.<sup>4</sup> Herein lies the condemnation of unnecessary and inordinate desires. Desires which, if unsatisfied, do not lead to pain are unnecessary; if, on the other

<sup>1</sup> Diog. Laert., X., 136.

<sup>2</sup> *Ibid.*, 140.

<sup>3</sup> Cic., 'de fin.', I., 56.

<sup>4</sup> Diog. Laert., 128.

hand, the natural desires, failure to satisfy which is painful, are violent and obstinate, that is sure proof that they are mixed with false opinion.<sup>1</sup> All this points to an ideal of felicity conceived as consisting in freedom from pain and intemperate desire and all disturbing affections of the soul. This is the famous Epicurean imperturbability (*ἀταραξία*). "By pleasure," writes Epicurus in a letter to Menœceus, "we mean the absence of pain in the body and trouble in the soul. It is not an unbroken succession of drinking feasts and of revelry, not sexual love, not the enjoyment of fish and other delicacies of a luxurious table that produce a pleasant life; it is sober reasoning, searching out the grounds of every choice and avoidance, and banishing those beliefs through which the greatest tumults take possession of the soul."<sup>2</sup> Such pleasure is unaffected by time; infinite and finite time both have equal pleasure, if we measure the magnitude of pleasure by reason; the man who has found the true happiness would not have that happiness increased though he lived forever. Nor is it added to by the pleasures of the flesh; it is only diversified.<sup>3</sup>

The psychological significance of this teaching lies not in its attempt to measure the quantity of one kind of pleasure as compared with another, nor in its attempt to fix the limit to the variations of pleasure in intensity; it lies rather in its suggestion of a sort of generalized pleasure consisting in a habit or frame of mind. Aristippus had made the best life consist in a sum of pleasures strung along in a series; according to Epicurus it is not found in any sum or succession of pleasures, but in a stable disposition of soul which meets with the same cheerful serenity all the vicissitudes of life. This disposition is moulded by philosophy. Particularly interesting is the psychology involved in a famous application of this teaching. "Even in the hour of death, when ushered out of existence by circumstances, the mind does not fail to enjoy the best life," writes Epicurus; even if the wise man were put

<sup>1</sup> *Ibid.*, 149f. Maxims 30-32.

<sup>2</sup> *Ibid.*, 131f. Letter to Menœceus, transl. by R. D. Hicks, 'Stoic and Epicurean,' p. 172.

<sup>3</sup> Diog. Laert., 144f. Maxims 17, 19.

to the torture, he would still be happy.<sup>1</sup> Even in the bull of Phalaris, Cicero quotes him as saying, the wise man would exclaim, How sweet, how indifferent this is to me!<sup>2</sup> Something of this Epicurus himself claims to have experienced. Writing to a friend from his death-bed, he tells him that the disease—the acutely painful malady of renal calculus—had reached its height, but it is all outweighed, he says, by the mental pleasure derived from the memory of a life devoted to philosophy.<sup>3</sup> It is easy to accuse him of exaggeration, the doctrine at least is plain: the mind, habituated to reflection, has the power, by voluntarily dwelling on the memories of past pleasures, of detaching itself from present ills and thrusting them aside. As, according to another Epicurean doctrine, vulgar fears produce all manner of disturbances in the body and are only to be eradicated by enlightenment, so contrariwise the pains of the body may be surmounted and suppressed by the tranquil soul: the sage can enjoy felicity even on the rack. Cicero and others found in this doctrine a subject of pleasantries; modern psychology, better acquainted with the facts, is disposed to admit the experience among the possibilities of suggestion and the affective imagination.<sup>4</sup> We shall meet with a similar doctrine in the Stoics and in Plotinus, the latter of whom speaks for the mystics generally.

Regarding the physical basis and bodily symptoms of the affections Epicurus has nothing to say worth recording. He thought of the soul as an assemblage of atoms within the body, the irrational part distributed throughout the body, the rational more especially in the chest;<sup>5</sup> and this perhaps made it possible for him to conceive of the mind as rationally affected apart from the participation of the rest of the body. Lucretius gives a vivid description of the more striking symptoms of fear and anger, but his explanations do not go beyond a superficial use of the recognized Hippocratean principles.

<sup>1</sup> *Ibid.*, 118.

<sup>2</sup> 'Tusc. Disp.', II., 7.

<sup>3</sup> Usener, 'Epicurea,' p. 143.

<sup>4</sup> See V. Brochard, 'La morale d'Épicure,' *L'Année philos.*, 1903, pp. 8-12; cf. F. Pillon, 'Sur l'imagination affective,' *Rev. Philos.*, 1907, 63, pp. 240 ff.

<sup>5</sup> Diog. Laert., X., 66.

Of the other doctrines of Epicurus about the affections, the best known is that which places them among the criteria of truth; but this appears to amount only to the assertion that their presence points to a corresponding object to be approached or shunned. Less well known, but more significant, is his contribution to the theory of the origin of language, a subject in which the Greeks had for some time shown a lively interest. Was language 'natural' or 'conventional'? The question is discussed at length by Plato in the *Cratylus*. The view of Epicurus was that "the names of things were not originally due to convention, but in several tribes under the influence of special feelings and special presentations of sense primitive man uttered cries. The air thus emitted was moulded by their individual feelings or sense-presentations and differently according to the differences in the regions which the tribes inhabited. Subsequently whole tribes adopted their own special names" for convenience of communication.<sup>1</sup> This vague anticipation of modern teaching is in principle as far as any ancient writer on the subject ever went, but we do not know how far Epicurus carried it out, if at all, in detail.

We turn now to the Stoics. The peculiar theme of the Stoics is the passions. In reporting their doctrines it is even less possible than elsewhere to keep the psychological aspects free from the ethical, for a condemnation of the 'passions' is contained in their very definition. A 'passion,' namely, was defined, in general, as a perturbation of soul (πτοία ψυχῆς) which consisted in an excessive or overpowering impulse (ὁρμὴ πλεονάζουσα, ἐκφερομένη), such movement or process being irrational or insubordinate to reason (ἄλογος, ἀπειθής λογῶ) and contrary to nature (παρὰ φύσιν).<sup>2</sup> It is a phenomenon of unregulated impulse. That it was irrational and contrary to nature was implied in the Stoic conception of reason and nature, for this—the two are one—allowed of no excess.

<sup>1</sup> *Ibid.*, 75, Letter to Herodotus, transl. by Hicks, *op. cit.*, p. 275.

<sup>2</sup> Stob., 'Ecl.', II., 7, 1, p. 39, 5 W (quoting from Zeno); 7, 2, p. 44, 4; Clem. Alex., 'Strom.', II., p. 460, Pott.; Diog. L., VII., 110; Cic., 'Tusc. D.', IV., 11, 47. Cf. Aspasius in 'Arist. E. N.', p. 44, 12. Heylb., ὁρμὴν σφοδρὰν ἢ ὁρμὴν ἄλογον, λαμβάνοντες τὸ ὑεαντίον τῷ ὁρθῷ λογῶ.

The term here translated impulse (*ὁρμή*) nearly corresponds in the Stoic terminology to what Aristotle called *ὁρεξις* (which the Stoics made a species of *ὁρμή*) denoting all instinctive tendencies to action. In itself it is necessary, normal and natural.<sup>1</sup> But 'passion' is by definition excessive impulse, one not conformed to the natural ends of life, but a movement by which the subject of it is violently carried away. Hence it is contrary to that Logos, that rational order, or principle of order, which the Stoics regarded as constitutive of 'nature' generally and as the essence of reason in the conscious mind. A life according to nature or reason, they conceived, was free from its disturbance. The word by which they designated this freedom—'apathy' (*ἀπάθεια*)—must not be taken to mean what we mean by the term; it does not mean a pathological state devoid of all feeling. It meant rather a settled state of freedom from the perturbations which prevent the exercise of the normal functions in fulfilling the rational aims of human living. 'Apathy' is, therefore, not itself the end, but the negative condition of its attainment.

The question at issue between the Stoics and their opponents was not whether such perturbations were evil and freedom from them good, but whether the disturbances which they named as such—anger, fear, love, hate, etc.—were of such a nature that in order to the best life every disposition to these affections must be, as they held, eradicated, or whether by suitable discipline they might not be converted into occasions of virtue. The Stoic view was that the soul which is subject to 'passions' is in a condition of weakness, is lacking in *tone*. The 'passions' are diseases of the soul analogous to those of the body, and like the latter may be distinguished as to constitutional morbid propensity, the state of disease itself, and the incidental sickness or infirmity.<sup>2</sup> Freed from the 'passions' the soul is all strength, force, tone

<sup>1</sup> 'Schol. ad Lucian,' Jacobitz IV., p. 211, *ὁρμῆς μὲν κινητικὰ ὅσα κατὰ φύσιν*. Stob., 'Ecl.' II., 160, 162, *φορὰ ψυχῆς ἐπὶ τι τῶν ἐν τῷ πράττειν, φορὰ διανοίας*, quoted by Chaignet, 'Hist. de la psychol. des Grecs,' II., 140f. Cf. Cic., 'de fin.,' III., 16ff.

<sup>2</sup> On the distinction of *εὐεμπτώσια*, *νόσημα* and *ἀρρώστημα*, see Stob., 'Ecl.,' II., 93, 1; Diog. L., VII., 115; Cic., 'Tusc. D.,' IV., 12, 13, 26.

(*ισχύς, κράτος, τόνος*), in good tone (*εὐτονία*), mistress of itself (*ἐγκράτεια*).<sup>1</sup>

That the condemnation of the 'passions' did not involve the repudiation of every affection is seen in the admission by the Stoics of a class of 'good affections' (*εὐπαθείαι*, Cic., *constantiae*). They included under this term cheerfulness (*χαρά*), discreetness (*εὐλάβεια*) and a virtuous habit of will (*βούλησις*), each with subdivisions. Thus under virtuous will we have good-will or benevolence (*εὐνοία*), affability (*εὐμένεια*), cordiality (*ἀσπασμός*) and affectionateness (*ἀγάπησις*); under discreetness, shame or the fear of dishonor (*αἰδώς*) and purity (*ἀγνεία*); under cheerfulness, wholesome pleasure in the use of the higher senses (*τέρψις*), good-fellowship (*εὐφροσύνη*) and good temper (*εὐθυμία*).<sup>2</sup> These various affections and dispositions, regarded as habits or general modes of feeling and behavior, are set over against the turbulent 'passions' as species of quiet emotion befitting the wise.<sup>3</sup> In addition to these two classes of 'passions' and 'good affections,' the one remorselessly condemned, the other approved, the Stoics recognized two other classes of affections, the natural affections arising from kinship, companionship, etc., and physical pleasures and pains as distinguished from the elation or depression of mind attending them. The former, although not having the character of 'good affections' as dispositions of a virtuous will, were regarded by the Stoics as good, or at least as not in themselves evil, and the latter as at least necessary; but as to the goodness or badness of bodily pleasures and pains, opinions differed.<sup>4</sup>

<sup>1</sup> See Chaignet, *op. cit.*, p. 149.

<sup>2</sup> Diog. L., VII., 115. The English terms translating the Greek in the text are at best only approximately equivalent; in both languages the terms are equivocal. Alex. Aphr., 'Com. in Arist. Top.,' II., p. 96, Ald., p. 181, says that the first to distinguish *ἡδονή*, *χάρα*, *εὐφροσύνη* and *τέρψις* was the Sophist Prodicus. He himself regards the words as all meaning substantially the same thing. The Stoics, he says, distinguished *ἡδονή*, an irrational elation; *τέρψις*, pleasure from hearing; *εὐφροσύνη*, pleasure from discourse. Others, however, stated the distinctions somewhat differently; thus Andron., π. παθῶν (v. Arnim, 'Stoic. vet. frag.,' III., p. 97) defines *τέρψις* as 'pleasure from sight or hearing.' Cf. Sen., *Ep.*, 59, 2.

<sup>3</sup> Cic., 'Tusc. D.,' IV., 12f.

<sup>4</sup> On this difference of opinion see Stob., 'Ecl.,' II., 58, 3; Sext. Emp., XI., 73. On the fourfold distinction of affectional states made by the Stoics, cf. R. D. Hicks, *op. cit.*, p. 102.

Understanding, then, that the 'passions' formed only one class of affectional states, we return to their definition. As already described, they are regarded by the Stoics as commotions arising in unbridled, irrational impulse, and therefore contrary to nature. It was agreed on all hands that such ebullitions of impulse had a disturbing effect on the intellectual processes, and this was considered a reason why the wise man would seek to be rid of them.<sup>1</sup> But they not only disastrously affect judgment, they take their rise, according to the Stoics, in judgment, and from this point of view the various passions are described as kinds of judgment or opinion. The Stoic psychology thus recognizes in a 'passion' the three elements or aspects of judgment, feeling and impulse, without, however, sharply discriminating them. The aspect at first emphasized was the movement of the soul towards or away from the object, that is, the impulse; but this movement had to be accounted for, and the most obvious explanation was that the mind entertained a certain opinion concerning the good or evil nature of the object. At any rate they looked upon this opinion as an integral and essential part of the process. Accordingly the 'passion' itself came to be defined as primarily an opinion or judgment. Here is the origin of the intellectualistic theory of emotion which has prevailed in the schools even down to recent times.

This intellectualistic tendency was more especially developed by Chrysippus.<sup>2</sup> The 'passions,' according to him, are erroneous, ill-founded judgments (*κρίσεις*), in Cicero's phrase, *judicia levitatis*.<sup>3</sup> Another form of the doctrine spoke of the 'passion' as a 'sudden' or 'fresh'—meaning, perhaps, hasty—opinion (*δόξα πρόσφατος*; Cic., *opinio recens*).<sup>4</sup> But

<sup>1</sup> Themistius, 'de an.,' 90b, Spengel, II., 197, 24; Cic., 'Acad. Post.,' I., 38.

<sup>2</sup> In his work 'On the Passions,' numerous fragments of which are preserved in Galen, 'De Hippocratis et Platonis Decretis.'

<sup>3</sup> Cic., 'de fin.,' III., 31. Cf. Them., in 'Arist. de an.,' III., 5.

<sup>4</sup> Posidonius, whom Galen, *op. cit.*, V., p. 416, Kühn, follows, interprets the phrase as referring to the suddenness or imminence of the falsely opined good or evil; Cicero, 'Tusc. D.,' III., 75, interprets it as referring to the fresh vigor of the judgment. The text suggests that a 'snap' judgment would come near to expressing its meaning; it is at any rate an immature opinion, the opposite of the calm, reflective judgment of the wise man. Cf. Siebeck, 'Gesch. d. Psychol.,' I., 233.



Chrysippus went still further. Regarding the whole soul as rational, he refused to allow that the 'passions' were affections of the lower part of our nature which reason had to control, as Plato had taught, and boldly declared them to be diseases of the reason itself. As Plutarch states the doctrine, lust, anger, fear, etc., were perverse opinions and false judgments, not formed in some inferior faculty, but such motions back and forth, such operations and energies of the whole directive faculty, as are ready to be turned with the greatest ease this way or that, "like the sudden motions and irruptions in children, the violence and impetuosity whereof, by reason of their imbecility and weakness, are very fleeting and inconstant."<sup>1</sup>

A consequence of the Stoic conception of the 'passions' as judgments, or as essentially involving judgment, and as perversions of reason, or at least hostile to reason, was that 'passions' in the proper sense of the term were denied to brutes. "For," they argued, "all disorders and perturbations of the mind arise from a disregard of reason; hence they arise only in men; for though beasts act similarly, they are not similarly perturbed."<sup>2</sup> Seneca, in denying that brutes have anger while allowing them ferocity, put the general contention neatly by saying "dumb brutes do not have human affections, but have similar impulses (*similes illis quosdam impulsus*)."<sup>3</sup>

The doctrine of Chrysippus that the 'passions' are affections of the reason was vigorously criticized by Posidonius (130-46 B.C.), who represents the mediating tendency of the later Stoicism, which sought to reconcile the general spirit of the teachings of the Porch with those of the Academy and the Lyceum.<sup>4</sup> It seemed impossible to Posidonius to account on this theory for the origin of a passion. How can that which is irrational spring from reason? It is incredible that a

<sup>1</sup> Plut., 'de virt. mor.,' 7 ('Morals,' ed. Goodwin, III., p. 478); Cic., 'Tusc. D.,' IV., 22. Zeno seems to have been content with the traditional view of the faculties, distinguishing three according to Tertullian, 'de an.,' 14, eight according to Nemesius, 'de nat. hom.,' p. 96.

<sup>2</sup> Cic., 'Tusc. D.,' IV., 31.

<sup>3</sup> Sen., 'de ira,' I., 3.

<sup>4</sup> A full report of his arguments is given by Galen, *op. cit.*, IV., and V.; cf. Sext. Emp., 'adv. Math.,' VII., 93.

faculty should give rise to a movement whereby its own action is impeded. Moreover, the doctrine fails to explain the accepted facts; why, *e. g.*, ideas of even the greatest good and evil arouse no 'passions' in the wise, while fools are upset by the most trivial things; why the same or similar ideas excite different persons differently; why emotion should be so much influenced by habit. Why, further, should a 'recent' or 'sudden' opinion cause passion, and one old and familiar not? And how is it possible to suppose that reason at one and the same time knows a thing to be evil and yet falsely opines it to be good? For these and other reasons Posidonius reverted to the Platonic tripartite division of the soul, emphasizing the dualism of the rational and irrational faculties and ascribing the 'passions' to the latter. He regarded, moreover, the 'concupiscible' and 'irascible' susceptibilities from which the passions spring as determined by the constitution of the body. Thus the 'passions' are neither judgments nor necessary consequences of judgments; they often arise from a movement in the 'passional' side of our nature without any judgment at all. Posidonius has, therefore, no difficulty in ascribing passions to the lower animals. The basis of the passional disposition lies in the congenital constitution: the broad-chested and warm are courageous; the broad-hipped and cold, timid in men and in brutes. And similarly of the other passions.<sup>1</sup>

The tendency of the later Stoics to adopt the views of their opponents in explaining the operations of the 'passions' is well illustrated by a story told by Aulus Gellius.<sup>2</sup> He was once in the company of an eminent Stoic philosopher during a storm at sea. The passengers were much interested to see how a Stoic philosopher would behave under such circumstances, and even forgot their own peril as they watched him grow pale with fear. After the storm was over, one of the passengers, a luxurious Asiatic, asked him banteringly why he, a philosopher, had shown fear, while he himself had been unmoved. To this the philosopher replied by citing the

<sup>1</sup> Cf. Zeller, 'Phil. d. Griechen,' IV., p. 579ff; Siebeck, *op. cit.*, I., p. 234f.

<sup>2</sup> 'Noctes Atticæ,' XIX., 1.

answer given on a similar occasion to a questioner of similar character by Aristippus the Socratic: "You had no cause for anxiety for the life of a miserable profligate, but I had reason to be alarmed for the life of Aristippus." But when Aulus asked him seriously for the explanation, he replied by taking from his wallet a book of Epictetus in which he read that, according to the Stoics, certain impressions called *phantasiae* are made on the soul by certain objects and that it is not in our power to determine whether or when we shall be invaded by them, so that the soul, even of the wise man, may be moved for a time by grief or fear; but this does not imply that the soul accepts, approves, or consents to these impressions. It is within our power to refuse to do so, and the difference between the fool and the wise man lies precisely in this, that the one yields and consents to them and the other does not. Augustine, who repeats the tale, concludes that there is no difference, or next to none, between the opinion of the Stoics regarding the passions and that of other philosophers; for all agree, he says, in holding that the mind and reason of the wise man is not subject to them.<sup>1</sup>

There are, however, four ways at least in which this freedom from subjection to the passions may be conceived. (1) The wise man does not experience them at all; his whole mind is one perpetual calm. This was the orthodox Stoic view as popularly understood. (2) The perfect man experiences them, but always as expressions of his virtuous character, always at the right time, in the right way, towards the right objects. This represents the Peripatetic ideal. (3) The good man is temporarily affected, but quickly recovers and through the exercise of habits of control and considerations of reason moderates the force of the passion and shortens and deflects its course. This may be called the common-sense view; it is the one suggested on the surface by the story of Aulus Gellius. (4) The man both is and is not affected by the passion. The passion is a natural phenomenon which takes place in him and of which he is aware, while at the same time he himself, or some deep underlying part of himself,

<sup>1</sup> Aug., 'de civ. Dei,' IX., 5.

remains unmoved by it; in the midst of his fears he is fearless, his ebullitions of anger leave him calm. His attitude towards his emotions is detached. This may be called the mystic view. Epicurus maintained it, as we have seen, in respect to pain. Plotinus, as we are soon to see, maintains it on the basis of his metaphysical conception of the soul. It naturally connects itself with the dualism of the Platonic-Aristotelian philosophy in sharply separating the reason from the other faculties; so far as this separation was made by the Stoics, it fell in very well with certain of their tendencies also. There is a suggestion of it beneath the surface in the narrative of Aulus Gellius. Plotinus uses the very conception of the *phantasiæ* there referred to in explaining the turbulence of emotions such as fear. The view thus recurrent in the ancient world persists to be dealt with by modern psychology in the light thrown upon the facts chiefly, it must be admitted, from the region of the abnormal, though it does not follow that they are to be themselves regarded as pathological.

A great deal of ingenuity was expended by the Stoics on the classification of the 'passions.' This, along with the definitions they gave of the several kinds, was their most distinctive contribution to the psychology of the subject on its systematic-formal side. All the 'passions,' they held, were fundamentally four: appetite, or desire (*ἐπιθυμία*, Lat., *libido*), fear (*φόβος*, Lat., *metus*), pleasure, or delight (*ἡδονή*, Lat., *voluptas*, *lætitia*), and pain, or grief (*λύπη*, Lat., *aegritudo*). As irrational impulses (*ὁρμαὶ*) they were defined as follows: appetite (desire) is an irrational inclination towards (*ὄρεξις*); fear, an irrational recoil from (*ἐκκλισις*); pleasure (delight), an irrational expansion or elation of mind (*ἐπαρσις*); pain (grief), an irrational contraction or depression of mind (*συστολή*).<sup>1</sup> On the theory that the 'passions' were, or had their roots in, judgments or opinions regarding good and evil in their objects, they were defined, typically, in this way: appetite is an opinion of coming good, which, if already

<sup>1</sup> Andron. Rhod., *περὶ πάθων*, I, p. 11, Kreuttner; Stob., 'Ecl.' II., 90, 7W. The germs of this fourfold division are in Plato, 'Phileb.', 32B, 39C. It is unfortunate that the same words, *ἡδονή* and *λύπη*, here used for emotions, have also to do duty with the Stoics for simple sensory pleasure and pain.

present, would be of advantage; fear is an opinion of impending evil which seems intolerable; pleasure (delight, joy) is a 'recent' opinion of a present good wherein it seems right to the mind to be elated; pain (grief, sadness) is a 'recent' opinion of a present evil wherein it seems right to the mind to be dejected and contracted.<sup>1</sup> Over against three of these irrational 'passions' were set the three 'good affections' as rational feeling-tendencies: cheerfulness is opposed to pleasure as rational elation, discreetness to fear as rational disinclination, will to appetite as rational inclination. No good affection corresponded to grief.

Under the four fundamental 'passions' the Stoics included all manner of emotions and emotional dispositions which they disapproved. Andronicus of Rhodes enumerates twenty-seven kinds of appetite, thirteen of fear, five of joy and twenty-five of sadness or grief.<sup>2</sup> Other lists were shorter. We may take as representative the classification of Stobæus. Here under appetite are grouped anger and its varieties (resentment, rage, wrath, spite, bitterness), passionate love, longing, yearning, love of pleasure, wealth, honor, etc.; under fear, timidity, anxiety, consternation, shame, confusion, religious fear, awe and dread; under the pleasure-passion, malevolent pleasure, joy in unexpected good fortune, pleasure in magic tricks (*γοητεῖαι*), etc.; under the pain-passion, envy, emulation, jealousy, pity, mourning, heaviness of spirit, dumb grief, trouble, anguish, poignant grief, distress. Stobæus remarks that some of these terms, *e. g.*, pity, envy and malevolence, point to the source of the emotion, while others, like poignant grief and dread, refer to some peculiarity in the emotion itself.<sup>3</sup>

As already indicated, the definition of the class varies according as the 'passion' is conceived more prominently as impulse or as opinion. Thus fear is defined as "an opinion of impending evil that seems intolerable," but also as "an

<sup>1</sup> Cic., 'Tusc. D.', IV., 7, 14; cf. Stob., 'Ecl.', II., 88, 6W.

<sup>2</sup> v. Arnim, 'Stoic. vet. frag.', III., p. 96f. For other lists see Diog. L., VII., 111f., Cic., 'Tusc. D.', IV., 7, Nem., 'de nat. hom.', 19-21.

<sup>3</sup> Stob., 'Ecl.', II., 90, 7; 92, 18. The definitions help to suggest the corresponding English terms, but as already remarked there can be no thought of exact equivalence.

avoidance of evils without reason accompanied by a low and broken condition of the vital energies.”<sup>1</sup> Sometimes the two conceptions are combined as in the definition of fear as “a recoil by reason out of control arising from an opinion of impending evil.”<sup>2</sup> The different ‘passions’ included in each class are defined in general by adding to the class name the specific difference. Thus, to illustrate, envy is defined as grief at another’s prosperity, that prosperity causing no injury to the envious person; jealousy, as grief at another’s possessing what one has desired for oneself; pity, as grief at the misfortune of one whose suffering is undeserved; mourning, as grief at the death of one dear to us.<sup>3</sup> Such definitions, which follow in the main the model set by Aristotle, we feel to be largely verbal and only at the best the rough preliminaries of a psychology; and it must be admitted that the Stoics are more concerned with discriminating and fixing the meanings of terms than with the analysis of the states of mind the terms stand for. It was recognized, indeed, that the distinctions were made from different points of view, but the analysis in this direction is not carried very far.<sup>4</sup> Nevertheless, we should not underestimate the attempt to classify the emotions, to reduce them to a few fundamental forms, to group under each form its several varieties, and to take the terms used in common speech to denote the diversities of emotional experience and stamp them with a precise, technical meaning. Such an undertaking is an essential part of science. Even the discrimination of synonyms involves a certain amount of psychological analysis. Down to the end of the 17th century the classical writers on emotions all followed the lead of the Stoics in seeking to reduce, classify and logically define them. Nor is the problem wholly foreign to the psychology of our own time, though we are probably more keenly aware of the limitations of language to express the subtle shadings of feeling and find other aspects of the psychological problem both more important and more fruit-

<sup>1</sup> Both given by Cic., ‘Tusc. D.’ IV., 6, 13; 7, 15. Cf. Diog. L., VII., 116.

<sup>2</sup> Stob., ‘Ecl.’ II., 172. See Siebeck, *op. cit.*, I., p. 504.

<sup>3</sup> Cic., ‘Tusc. D.’ IV., 17f.

<sup>4</sup> Cf. Siebeck, *op. cit.*, I., 2, p. 233.

ful. Along with its obvious shortcomings it must, however, be regarded as a conspicuous merit of the first attempt of this sort that it selected as the basis of classification such fundamental characteristics of the emotional process as the antitheses of elation and depression and the opposed tendencies of approach and recoil rather than the simple qualities of pleasantness and unpleasantness.

Occasionally a single passion was made the subject of more extended discussion. The most famous work of this kind was the treatise by Seneca already mentioned in three books on anger.<sup>1</sup> Seneca, after giving other explanations, defines anger, with considerable psychological insight, as "a sudden and powerful agitation of the mind moving straight forward to the execution of vengeance, an agitation combined with will and judgment."<sup>2</sup> He disregards the distinctions drawn by Greek writers between the different kinds of anger because of the lack of corresponding Latin terms to express them, and skilfully marks off the differences himself by such epithets as bitter, sharp, peevish, clamorous, etc.<sup>3</sup> In the process itself he distinguishes the involuntary arousal of the impulse, the conscious idea that what has been felt or perceived demands punishment, and the arrest of the reason in the further development of the passion.<sup>4</sup> Along with these and other observations of permanent psychological value Seneca interestingly notes the manifestations of anger in the mob.<sup>5</sup>

The physiological side of the affections receives more or less explicit recognition in all the schools. On the basis of principles expounded in the medical writers of his time, Plato goes into details in the *Timæus*; illustrations abound in the pseudo-Aristotelean *Problemata*. Theophrastus, Strato, Alexander of Aphrodisia and other Peripatetic writers,

<sup>1</sup> Among other works on the same subject we have Plutarch's 'de cohibenda ira' and a treatise *περὶ ὀργῆς*, discovered at Herculaneum, the author of which was the Epicurean Philodemus, a contemporary of Cicero's.

<sup>2</sup> Sen., 'de ira,' II., 3.

<sup>3</sup> *Ibid.*, I., 4.

<sup>4</sup> *Ibid.*, II., 1ff. See Siebeck, *op. cit.*, I., 2, p. 233f.

<sup>5</sup> 'de ira,' III., 2.

while upholding the doctrines of the master in other respects against divergent views of Platonists and Stoics, follow and develop his teaching in this direction also.<sup>1</sup> The Stoics would seem to have the material for a more special development in the form peculiar to them of the doctrine of the *pneuma*. They held the *pneuma* to be the very soul and substance of the universe, a substance possessing the highest degree of vital energy, perfect in 'tone,' with power to expand spontaneously and disperse itself without loss. It is in nature akin to air and fire which form, as it were, the soul of the physical world as earth and water form its body. The soul of man consists of *pneuma*, an ethereal substance present in the generative seed and continually nourished by exhalations from the blood.<sup>2</sup> This *pneuma*-soul is not, as the Epicureans held the soul to be, merely contained within the body; it pervades the body and is the vitalizing principle which holds its parts together. Now the obvious application of these conceptions in a doctrine of the passions would be to say that, so far as the passions appear as disturbances in the soul, they are the transcription and counterpart of actual physical movements or of qualitative changes of the *pneuma*; then the doctrine might be called upon to point out the particular modifications of the *pneuma* corresponding to each kind of passion and the relation of these modifications of the *pneuma* to those states of the body which it pervades and controls. This programme is not carried out. What we find are merely such general indications as follows.

Chrysippus represented the modifications of the *pneuma* that take place in emotion as modifications of 'tone.' The

<sup>1</sup> For illustrations see Siebeck, *op. cit.*, I., 2, pp. 224ff.

<sup>2</sup> See Chalcid. in 'Tim.', c.220; Epiph., 'adv. haer.', III., 2, 9 (πολυχρόνιον πνεῦμα); Galen, 'de plac. Hippocr. et Plat.', II., 8, reporting the ascription by Diogenes the Babylonian of the view that the soul was *pneuma* and nourished by the blood to all three founders of the school; Long. ap. Euseb., 'præp. evang.', XV., 21, 3; 20, 2; and Theod., 'gr. aff. cur.', V., 2, for the soul-*pneuma* as a sensitive exhalation (ἀναθυμίασις), a view ascribed to Zeno and Cleanthes. Souls were supposed to be continually generated from moist substances, a conception connected apparently with the doctrine of Heraclitus of the 'way up and down.' Justus Lipsius in his 'Physiolog. Stoicorum,' III., Diss. VI. and IX. quotes numerous passages from Stoic writers and their doxographers which show that the identification of the soul with the material *pneuma* was a common doctrine of the Stoics in all periods of their history.



'tone' of the soul is either normal or defective. This must be taken literally; the reference is not merely to consciousness, but to the material, conscious pneuma. That is either in good tone or lacking in tone (*εὐτονία, ἀτονία*), just as the nerves or tendons are either tonic or relaxed. Chrysippus uses this analogy. There is that in the soul, he says, which resembles the nerves or tendons, so that we can say metaphorically (*κατὰ μεταφoράν*) that it is 'nerved up' or 'enervated.' Now when one is struck with terror or succumbs at the prospect of gain or loss, the soul, *i. e.*, the pneuma, is 'enervated,' it is in a state of weakness, and it is from this weakness that evil actions arise.<sup>1</sup> In another passage he draws the analogy from general bodily conditions. As in the body there is strength and weakness, good tone and poor tone, and, relative to these conditions, health and disease, so the soul is said to be strong or weak, firm or flabby, etc.<sup>2</sup> Elsewhere comparison is made with the balance of the constituent elements of the body. As health consists in a normal, disease in a disproportionate relation of the hot, cold, moist and dry elements, so the soul is fair or foul according as the reason is thus or thus disposed with reference to its proper parts.<sup>3</sup> This conception of the soul's 'tone' is perfectly general; it neither tells us what particular differences of tone correspond to the different passions nor how these differences are brought about. When, going beyond this, Stoic writers treat of the physical phenomena of emotion, they, like the writers of other schools, make use of common observation and current medical theory.

Seneca in a passage which sums up much of the ancient speculation and which seems to be the source of a great deal of the later writing on the subject, connects the dispositions to specific emotional reactions with the body's temperament and the various periods and conditions of life. As is the prevailing temperament of the body, so is the character of the man. Heat in excess makes men irascible, cold disposes them to timidity. Dealing with the passion of anger, Seneca

<sup>1</sup> Galen, *op. cit.*, IV., 6 (Kühn, V., p. 404; v. Arnim, *op. cit.*, III., p. 123).

<sup>2</sup> *Ibid.*, V., 2 (K., p. 438; v. Arn., p. 120).

<sup>3</sup> *Ibid.*, V., 2 (K., p. 444; v. Arn., p. 121f.).

remarks that the common opinion that anger is excited by effervescence of the blood about the heart is due simply to the fact that the breast is of all parts of the body the hottest. Where the moist element abounds, anger develops gradually, time being required to generate the requisite heat. Thus in women and children anger is sharp, but of little consistency, at first, though it may swell in volume as the movement continues. In middle life—the dry period of life—anger is vehement and robust, but is not likely to be greatly augmented. The aged, sick and convalescent are rather irritable and querulous than angry, the heat being diminished by lassitude and loss of blood. Wine increases anger because it inflames; people of a florid complexion are irascible because of their heat.<sup>1</sup> Plutarch, eclectic, platonizing, writes in a similar vein, picturesquely describing also the visible effects of anger on gait and voice, on the color and aspect of the countenance, which it ‘doth swell and puff very indecently,’ on gesture and on speech.<sup>2</sup>

All this falls short of a thoroughgoing theory of the relation of the conscious affection to its bodily conditions, concomitants and expressions. Hot, cold, dry, moist and pneuma seem to be so many independent principles each of which is called upon in turn to play its part, but the intimate, organic connections of which, if any such exist, are not made plain. No doubt it lay in the spirit of Stoic metaphysics to regard these principles not as independent, but as differentiated modes of the all-pervading world-substance, and accordingly to view the affections as ultimately conscious modes of an organic cosmic process and more immediately of the bodily processes in the individual organism. But so far as appears no Stoic writer develops this doctrine or avails himself of the general conception in his observations and explanations in detail. That the emotions were in a sense physical must, of course, have been the opinion of all the philosophers who held the soul to be a material substance or composed of material

<sup>1</sup> Seneca, ‘de ira,’ II., 19.

<sup>2</sup> Plut., ‘de coh. ira,’ 6, 10. According to orthodox Stoic doctrine all the passions have their seat in the heart, the center of the psychic life. Nemesius, *op. cit.*, 19, 20, makes the organ of grief the orifice of the stomach.

substances, although if conceived, as the Epicureans conceived it, as merely contained within the body, it was always possible to think of its motions as arising spontaneously and as unaffected by general somatic influences. That all ordinary emotions took their rise in the body and had a bodily aspect, was a natural inference from Plato's doctrine of the mortal soul, was implied in Aristotle's doctrine of the soul as the body's entelechy and his explicit statement of the two-fold way in which an emotion may be viewed, and was expressly asserted by adherents of the Peripatetic school.<sup>1</sup> But the most pronounced formulation of the doctrine in antiquity is found in a writer in whom perhaps we should least expect to find it, namely, in Plotinus, who teaches emphatically and unqualifiedly that all ordinary affections of the soul are nothing but the soul's consciousness of the affections of its body and that an emotion, for example, was, in modern phrase, simply the consciousness of the bodily changes, or the emotion's 'expressions,' as they occur. In the way in which he connects this doctrine with a metaphysical conception of the soul, he is far enough removed from the metaphysical reserve commonly professed by modern physiological psychology, but his teaching is for that reason perhaps all the more illuminating. It is at any rate sufficiently interesting both in itself and as the final term of the historical development in the ancient world to attract our attention.

The interest of Plotinus in the affections is metaphysical and ethical; he seeks to vindicate in his theory his conception of the soul's essential independence and impassivity. The soul is in the body, he holds, not as a body is in space or an attribute in a substance or a part in the whole or the whole in a part or sensible form in sensible matter, but as the power exerted by an agent is in its instruments, as 'fire' (he says) is in the warmed and illuminated air. It energizes and animates the whole body and in specific ways the different parts of the body, giving to them thereby a 'trace' or 'impress' (*ἵχνος, ἰγδάσμα*) of itself. In virtue of its intimate connection with the body the soul is conscious of the body's affections and

<sup>1</sup> See Siebeck, *op. cit.*, p. 225.

may identify itself with them and with their tendencies; but we must not think of the soul as affected nor, on the other hand, suppose the body sensibly affected without the soul. On this basis Plotinus enunciates the following specific doctrine of pleasure and pain. "Pain," he says, "is awareness (*γνώσις*) of the recoil (*ἀπαγωγή*) of the body in the process of being deprived of the 'impress' of the soul; pleasure is the living being's awareness of the process of restoration of the harmony of the 'impress' of the soul in the body."<sup>1</sup> When, for instance, the body is wounded, the affection is perceived and localized, and we say, *e. g.*, that the finger feels pain and that the man feels pain, since it is the man's finger. But we cannot properly say, according to Plotinus, that the mere awareness of the sensation is pain. The consciousness is cognition having pain for its object, and the cognition, he urges, is not affected, otherwise it would not be able to give a true account of the affection. Pleasure and pain, then, are not affections of the soul, but of the living body. In the living body, similarly, arise the appetites which spring from these affections. This is shown by the varying character and strength they assume in the different periods of life. But although the soul is not in itself affected, still as the 'nature' operating in the process and seeking to direct the appetites, it is conscious of the body's limitations and identifies itself, as it were, with its longings. Plotinus follows Plato in making the seat of the bodily appetites the region about the liver and the seat of the nobler impulses of defence in the heart.<sup>2</sup>

Passions or emotions, defined broadly as states accompanied by pleasure or pain, originate, according to Plotinus, in two ways. Some arise in ideas (*ἐπὶ δόξαις*), *e. g.*, fear from the thought that one is at the point of death, joy from expectation of good fortune. But some arise independently of ideas and themselves take the lead, involuntarily exciting the opinion.<sup>3</sup> Idiopathic emotion is recognized by other

<sup>1</sup> 'Enn.,' IV., 4, 19. In 18 pain is referred to the unsuccessful struggle of the inferior nature to achieve a firm union with the superior.

<sup>2</sup> *Ibid.*, 21, 20, 28.

<sup>3</sup> *Ibid.*, III., 6, 4: τὰ δὲ ἐστὶν ὡς ἡγησάμενα αὐτὰ ἀπροαιρέτως ἐμποιεῖν ἐν τῷ πεφυκότι δοξάζειν τὴν δόξαν.

ancient writers; Posidonius, for example, makes use of it in criticizing the Stoics; but in no ancient writer perhaps is it so explicitly intimated as in Plotinus that the emotion thus arising independently of ideas goes on to complete itself by developing its own motive or justification. Plotinus insists, further, that an opinion is not an emotion in the faculty or process of thinking (*ἐπὶ τὸ δοξάζειν*); thought and emotion belong to quite distinct spheres (*ἐν ἄλλῳ . . . ἐν ἄλλῳ*). Nevertheless the two are intimately related in the emotional process. The idiopathic emotion, as we have seen, gives rise to an opinion and an opinion may be the starting point for an emotional disturbance which, when occurring, produces a certain consciousness of itself with which the opinion is conjoined.<sup>1</sup>

If now we ask, what is the cause of the feeling, *e. g.*, of fear, it is not enough to say, as is commonly alleged, disturbance arising from anticipated evil; for how can any disturbance arise directly from an opinion? An opinion, the representation (*φαντασία*), *e. g.*, that some evil is about to befall, is a pure act of cognition and is neither itself an emotion nor, apart from accessory conditions, capable of exciting one. Plotinus finds these accessory conditions in an unconscious or subconscious psychical process mediating certain changes in the body—a kind of obscure thought and indistinct imagination (*ἀμυδρὰ οἷον δόξα καὶ ἀνεπίκριτος φαντασία*) operating in the organism and to be compared with the blind force of Nature producing the particulars of existence. The modern psychologist, according to his predilection, will think here of associational processes, psychical or psychophysical dispositions, instincts, Freudian complexes, etc.; Plotinus contents himself with the simple general conception of a blindly working function of the psyche animating the body. What he urges is that the whole process thus far considered, the whole

<sup>1</sup> Cf. ὁ δ' ἐκτὸς τῆς δόξης φόβος ἐλθὼν ἄνωθεν αὐτὸ ἀπὸ τῆς δόξης οἷον σὺνησὶν τινα παρασχὼν τῷ λεγομένῳ τῆς ψυχῆς φοβεῖσθαι (ed. Müller, p. 223, 23). If ἄνωθεν κ. τ. λ. is taken with ἐλθὼν, the main assertion appears to mean merely that the πάθος is consciously taken note of; but if, as seems possible, the clause goes with παρασχὼν, we have the more interesting view indicated that the conscious emotion is determined and defined by the idea of its object as the exciting cause.

process short of the bodily changes, consists not of 'passions,' but of the soul's activities. But what follows (*τὸ δ' ἀπὸ τούτων*) is a complex of bodily affections—trembling, paling, speechlessness, etc. In the last resort it is not the soul which is perturbed, but the animated body. But the soul is immediately, sensibly aware of this perturbation and associates with it the idea of impending evil. And in this synthesis the emotional process is completed.<sup>1</sup>

A similar explanation is given of anger. We are angry, indeed, not only at our own body's sufferings, but also at the sufferings of our friends and at violations of the proprieties generally. Perception and understanding, therefore, play a part in the emotion, and we might consequently be tempted to look for the origin of the impulse outside of the merely vital energies of the organism. On the other hand, we cannot help noticing that the disposition to anger follows the constitution and condition of the body. In animals as in men the hot-blooded and bilious are quick to take offence, the cold-blooded and those deficient in bile, slow; and the very same person is more disposed to the emotion in sickness than in health, when hungry than when sated. These facts make it clear, in the view of Plotinus, that the beginnings of anger are due to the bodily constitution and that the liver and the blood are, as it were, the animating principles of its motions. When, therefore, the predisposed body is affected, the immediate effect is a stirring up of the blood and the bile. Now the soul perceives this, and the imagination which unites the soul with the state of its body, allows reason to become conscious of the pain—the breaking up of the natural harmony of the animated body—and the soul which, from its own superior point of view, looks on injustice or injury of any sort in a calmly rational manner and is not inclined of itself to enter into the affections of the body, is made the ally of the passion. This view of the origin of anger is confirmed by the observation that those who are little inclined to indulge their bodily appetites and, in general, pay slight attention to the body are relatively less disposed to this and other similar

<sup>1</sup> III., 6, 5: *καὶ συνέχεται τῇ ταραχῇ ἢ τοῦ προσδοκώμενου κακοῦ εἰκῶν.*

affections. Let it not be objected that if the principle of the passion is placed in the vital forces of the organism—in the terminology of Plotinus, the vegetative soul—then trees ought to feel anger: the obvious reply is that trees have neither blood nor bile; if they had, but were without sensation, there would only be an effervescence of these humors, but if sensation were added, then doubtless there would also be an impulse to repel the injury.<sup>1</sup>

Although thus referring bodily pleasures and pains and emotions like fear and anger to the physical organism, Plotinus admits spiritual feelings and impulses, such as the longing for knowledge and pleasure in its possession, excited by the mind itself.<sup>2</sup> The rational soul is thus not absolutely impassive. In particular he contends for a state of happiness (τὸ εὐδαιμονεῖν) possible to one who finds pleasure in the good. He who has within himself the unchangeable good will not be moved by common griefs, happiness will not fail him even in the bull of Phalaris.<sup>3</sup> This doctrine of a source of feeling independent of the body is common to many ancient writers, from whom it passes over to the Church Fathers, being, we may say, a practically indispensable component of any orthodox interpretation of Christian doctrine. Also the doctrine of the power latent in such a transcendent source of feeling of resisting ordinary afflictions and annulling the force of physical pain plays a not inconsiderable part in the psychology of the mystics and in thinkers generally who, from Plato and Aristotle down, make a sharp cleavage between the rational or spiritual part of our nature and the sensible. We have already noticed the part it plays in the teachings of Epicurus and the Stoics.

In concluding this study of affective psychology in writers after Aristotle it may not be inappropriate to sum up briefly the impressions left by a survey of the work of Greek writers generally in this field. The whole movement, as already observed, is controlled by practical interests and the results

<sup>1</sup> *Ibid.*, IV., 4, 28.

<sup>2</sup> See Siebeck, *op. cit.*, p. 328f.

<sup>3</sup> 'Enn.,' I., 4. Cf. Dionys. Areop., 'de div. nom.,' II., 9, quoted by Thomas Aquinas, 'S.T.,' q. XXII., a.3: non solum discens, sed etiam patiens, divina.

are conditioned in no small degree by ethical and metaphysical considerations which to the modern mind lie outside of psychology, but which, nevertheless, are constantly tending, explicitly or implicitly, to intrude themselves. In ancient writers the intrusion is explicit; it furnishes the motive, sustains the enquiry. In spite of this we have here the fruitful beginnings of a science. Human nature is subjected to observation and analysis, theories regarding human affections are developed of wide-reaching influence. The conspicuous illustrations are the doctrines of Plato and Aristotle concerning pleasure and pain. Sir William Hamilton held that all theories of pleasure and pain were modifications of these two, and much might be said for this opinion.<sup>1</sup> Even Kant held with Plato that all sense-pleasure is conditioned on pain;<sup>2</sup> the affinity of modern biological theories of pleasure and pain to the views of Aristotle is so obvious as to have become a commonplace. On the formal, logical side the emotions are treated by the Greeks with unusual fulness, not altogether to advantage. This is particularly conspicuous in the endeavor of the Stoics to define and classify them. On the other hand, much attention is given to the physical phenomena, the tendency being to regard the bodily commotion as of the very essence of the emotional 'affect.' And although the Hippocratean physiology employed to explain the more striking phenomena of the coarser emotions is now antiquated, the influence of conceptions like those of the body's humors, vapors and temperatures, and particularly of the 'pneuma,' is seen throughout the whole of the Middle Ages, in the psychology of the Renaissance, in the 'animal spirits' of Descartes and Malebranche, and in still later writers, as well as in current forms of popular speech. Finally, there appears in these ancient writers a growing sense of the complexity of the problems: states of feeling are mixed and compounded, pass into one another, variously influence and are in turn influenced by thought and action, develop into habits and dispositions, inhibit one another, grow out of and react on

<sup>1</sup> Hamilton, 'Lectures on Metaphysics,' 5th ed., I., p. 444f.

<sup>2</sup> Kant, 'Anthrop.,' 58.



bodily conditions and are no less manifestly related to conditions in the social environment. The facts are noticed and partly accounted for. Some of them, as, *e. g.*, the apparent detachment of mystical feeling, to a marked degree, and all of them to a certain extent, evidently contain problems that point to the future, to a more developed psychology and to a more developed physiology. But in comparing the actual achievements of modern psychology in this field with the ancient it may very well be doubted whether the advance is in any degree proportionate to the vast amount of time that has elapsed; it is certainly not such as to justify any high degree of pride in our own attainments or of contempt for those who first made them possible.

# THE NATURE OF MENTALITY

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## MENTALITY AS ADJUSTMENT

Recent investigations of the unconscious phases of the mind have brought forth much valuable material; but they have sometimes suffered from what appear to us to be inaccurate concepts of mentality. We would like to approach these valuable findings by way of a preliminary statement of the nature of mentality as we wish to have it understood.

Mind or mentality means a certain mode of doing things on the part of an organism. An organism has other ways of doing things besides that way which constitutes mentality. The building up of waste tissue by way of the blood is not a mental process, nor the healing of a wound, nor organic growth in any form. Mentality on the other hand appears in writing a poem, building a house, mowing a lawn or organizing a social campaign. What then is the difference between these mental activities and that sort of behavior which is not mental? We must take a concrete example in order clearly to define the nature of mind.

When I endeavor to sketch the likeness of a man my whole mind is absorbed in the undertaking, for, while I am no artist whatsoever, I am very earnestly desirous of making it as perfect a representation as possible. Making such a sketch is a very clear case of mental operation. Of what does this mental action consist? It consists in the first place of tentative efforts to draw the man before I venture to mark the paper. I am actually moving my muscles and adjusting my whole nervous system in such a way as to make very slight movements, so slight as to be imperceptible to the naked eye, but movements, nevertheless, by which an imaginary outline is made supposedly representative of the man, before ever I

put pencil to paper. If a delicate instrument were attached to my hand in such a way as to record these movements, the tentative picture-making efforts would be revealed, although they are so slight that the naked eye cannot see them. Mental effort is nothing else than organic effort of this most delicate and tentative form. Mentality is that process by which we adjust the physical organism preparatory to some overt action upon the environment. When the organism is already adjusted by reason of some innate mechanism, hence no preparatory adjustments being required, there is no mentality involved. Such is the heart beat, the respiration ordinarily, digestion, the knee jerk, etc.

When the words "Draw that man" first fall upon my ears a number of physiological processes are stimulated to activity. Any one of these several tendencies, if isolated from the others, might be executed without the exercise of conscious mentality. But since they are simultaneously aroused they hold one another in abeyance because of their disorganized condition. They interfere with one another. In order to be executed they must be adjusted to one another in such a way as to constitute an orderly consecutive scheme of operation.

But our analysis is not yet complete. Thus far it would seem that the words "Draw that man" simply aroused a chaotic medley of tendencies. There is nothing inherent in such a chaos that would tend to reduce it to a definite scheme of procedure. If our analysis stopped at this point the inevitable conclusion would be that mind was some sort of mysterious entity that worked upon this chaos from without, reducing it to order. Yet this is precisely the conclusion that scientific psychology wishes to avoid.

As a matter of fact the operation of drawing the man does not require of me that I organize a system out of chaos. Previous mental operations of mine have long since established a certain system of tendencies which, in the form of an established system, is excited when the words "Draw that man" fall upon my ears. This system is more or less indefinite to be sure. In order to be executed on any particular occasion,

new tendencies, which may be aroused for the first time on that occasion, must be assimilated to it, and other tendencies which have previously inhered in it must be excluded or re-adjusted. Nevertheless this preëstablished indefinite system of tendencies constitutes a dominant propensity which serves as a governing agency in the process of drawing the man. The process of adjustment which constitutes mentality is the process by which numerous newly aroused tendencies are assimilated into this propensity, and the propensity itself modified, to the end that it may be fulfilled or executed.

Such a mental process stands in marked contrast to a non-mental one, such as that of a healing wound or metabolism. In the latter operations there is no organization of tendencies into a novel scheme which has never before been executed. On the contrary there is only one established and relatively invariable system of operation. In case of mind there are many different activities which tend quite as much to frustrate as to supplement the original propensity until they are adjusted to one another and organized into a single system of procedure. In the non-mental there are no frustrative tendencies, nothing requiring adjustment, the organization of activities being firmly established and operating automatically.

But in drawing the man we may have noted that we could pick up the pencil without giving our attention to it. In a sense it was an unconscious act; nevertheless it was a mental act. How are we to distinguish it from the non-mental acts? How can we have an unconscious mental act?

Picking up the pencil is a manifestation of mind because it is an operation which we had to learn. Just as we are now learning how to draw a man, so at one time we had to learn to pick up a pencil. At that time in our early infancy we had to make a great many tentative efforts, a great many movements had to be pruned down, gradually adjusted to one another and finally organized into a coördinated system of behavior by which the pencil could be firmly grasped and directed to the point desired. A great many reflex movements or previously acquired movements were put together

into that new combination which constituted picking up the pencil. Any such combination of movements which at some time in the past experience of the individual has been put together into an orderly system of behavior, is a part of the mind. Whatever has been put together under the control of some ruling propensity, can be taken to pieces again and put together into some other system under the control of some other ruling propensity. This taking to pieces of systems of tendencies and rearranging them and reorganizing them into new systems, is what is meant by mentality.

Many previously organized systems may be incorporated into the system which is in process of formation. In drawing the man we were not required to organize the system of activities by which the pencil was picked up. We found that system already established in the form of a habit and simply appropriated it as a subordinate part of the larger system we were forming. So it is that we are always forming larger systems of action, and in doing so are always assimilating older systems previously formed, some of which must be reorganized in order to adjust them to the new system in process of formation, others of which can be taken over without modification. Mind then consists of all systems of behavior which have been organized at some time during the life of the individual, or are now being organized, out of more elementary modes of behavior.

The prime essential in this systematizing of uncoördinated tendencies which constitutes mentality, is the preëxistence of some established system; for the entire organizing process is simply the process by which this original system of tendencies attains fulfillment. When any system of tendencies thus operates as the central nucleus in forming a new system we call it a ruling propensity. It is plain that the propensity itself is modified in the process of organizing the new system, and this modification may be so great as to transform it quite completely. Or again the process of modification and organization may continue indefinitely without attaining any final fulfillment. In such case the ruling propensity at any given moment in the process is the system evolved out of that

portion of the process immediately preceding. But in any case the existence of a preëstablished system or propensity as a governing motive is indispensable to that organization which constitutes mentality.

If we trace this organizing process of mentality back to infancy we find that in the beginning certain innately established systems or propensities served to initiate the process of mentality. These innate systems are not mental but the process by which they assimilate new tendencies and hence develop into larger systems is mental. These rudimentary combinations of reflexes are the nuclei of instincts. The instincts themselves, however, pass through a more or less elaborate process of development after birth and are organized and reorganized to suit the demands of an ever changing environment; hence they form an essential part of the mind. Indeed they are probably the most important part in as much as they constitute those original propensities to action which force the individual to organize and assimilate a great many diverse movements into a coördinated system in order to carry the propensity to fulfillment.

The manner in which such a propensity forces the individual into the mental process of organizing a multiplicity of movements into a coördinated system has been excellently demonstrated by an experiment described by E. L. Thorndike.<sup>1</sup>

A kitten is placed in a box made of slats between which she can look out. The door is shut and is fastened with a small piece of wood falling into a slot. So far as the kitten's physical ability is concerned she could open the door with ease, had she sufficient mentality. When she becomes hungry a piece of fish is placed in front of the box beyond her reach. Immediately the food-getting propensity is aroused. That means that certain glands are excited so that the gastric juices begin to pour into the stomach, the salivary glands begin to secrete, the swallowing apparatus acquires a certain tonus or readiness to act, the seizing apparatus of claws, fore-

<sup>1</sup> E. L. Thorndike, 'Educational Psychology,' Vol. II, 'The Psychology of Learning,' p. 9. Cf. also R. B. Perry, 'Docility and Purposiveness,' *PSYCHOL. REV.*, 1918, 25, pp. 1-10.

legs and jaws are slightly innervated, etc. This is the food-getting activity at the initial stage of operation. This is what is meant by the excitation of an original propensity.

But this propensity cannot get beyond this initial stage of futile excitation as long as the box door intervenes between the cat and the fish. Hence the propensity must continue in a state of excitation and the energy which is released by it must stimulate many other movements. Excitation of these other activities appears in the form of restless movements to and fro, clawing at the cage in all manner of ways, biting, rubbing, mewling, pushing etc. All these represent subordinate tendencies which have not been previously established as an essential part of that system constituting the food-getting propensity, but which are aroused now because of the accumulation of stimuli to which the kitten is exposed when she cannot reach the fish which lies so closely in front of her. Finally, quite by chance, the kitten happens to claw the latch in such a way as to open the door and gets the food.

She is again confined when hungry and goes through the same process, and likewise again and again. But the number of scratchings and other movements, which are carried out before the door is opened, rapidly become less and less. After a time, when so confined, she promptly puts her paw upon the stick, opens the door without any such superfluous movements as random clawing and restless darting to and fro. She has organized a definite system of movements by which she is able to procure the fish. She would never have been able to accomplish this organization but for the fact that: (1) She was impelled to seize the food by the original food-getting propensity (instinct); (2) Other secondary activities such as clawing, walking about, etc., were aroused, in part independently of the food-getting impulse by reason of stimuli which acted directly upon them, in part dependently upon the food-getting impulse by reason of more or less indefinite relations to that system; (3) This original propensity remained in a state of excitation until it had assimilated into a single system with itself all those movements which were necessary to its satisfaction and let fall into desuetude

all those which were not necessary, and thereby developed a system which included just those activities which open the door and no others.

Had the kitten a higher mentality the subordinate tendencies, such as mewling, scratching, moving about, might have been so numerous and diverse, or have been aroused so nearly at the same time, as to prevent one another from issuing in overt action. In such case they would have been maintained in the form of slight physiological processes, producing a labile interplay of tentative adjustments and nerve discharges until some sort of arrangement of activities had been evolved which would serve to open the door. Not until this arrangement of activities had been organized as a motor set of her organism would she have exerted her strength upon the box to obtain release. This mode of behavior would have represented a higher degree of mentality than the actual procedure which the kitten did carry out. It would have been higher not merely because the heterogeneous movements were restrained from exerting their full physical effect upon the environment until the scheme was worked out, but because this restraint made possible (or resulted from) a much more rapid transition from one kind of tentative movement to another and consequently the arousal of a far greater number and diversity of innervations and adjustments. But in any case the original propensity, making all this possible, would be an instinct.

### SUBCONSCIOUSNESS

It is apparent that any system in process of formation may include a great deal which is unconscious. All subordinate systems which are included in the one being formed, but which have been organized in the past and require no further modification, would be unconscious portions of the evolving system. Such was the system of picking up the pencil, as a subordinate part of the larger system of drawing a man. These unconscious supporting systems are always far more extensive than the individual is aware. The focus of attention is never very extensive and always consists of those ac-



tivities which are being most rapidly and critically reorganized. There may be other portions of the system which are undergoing modification at the same time as that at the focus of attention, but at a less rapid rate and of less critical significance to the system as a whole. But the total system, including all the supporting systems which are semi-conscious, co-conscious and unconscious, is far more extensive than the individual ever imagines. To use Stanley Hall's analogy, the mind is like an iceberg, seven eighths invisible (unconscious), and the motive force which impels a man to act as he does is the total force of the entire system, only a small portion of which appears in consciousness. When we refer to any such entire system it will be understood that we mean to include all these unconscious portions, the preëstablished supporting systems which enter into it. How vastly extensive these unconscious portions are it would be difficult to exaggerate, reaching back as they do to the earliest experiences of infancy and comprehending elements as diverse as mathematics and love.

But the unconscious is even more extensive than the range of any total system which at its apex is being developed in consciousness. In all of us there are systems of activities or tendencies to act which are not subordinate to, and do not support, the propensity which rules consciousness. Independent and refractory systems such as these can only arise in a very extensive and complex mentality, hence perhaps never arise among the lower animals; but they are very common among human beings, and particularly among the more intelligent. They do not contribute to mental ability, quite the contrary, for they are a mark of mental disease; but it is a disease which can only appear in a relatively complex mentality.

To illustrate the nature of these independent systems of the mind we might hypothetically endow our kitten with a more highly developed mind than she actually had. Suppose, then, that the kitten at some earlier time in its career had been trained not to eat fish, which would be quite possible with any kitten. This training has established a cer-

tain system of tendencies which, when excited by the sight and smell of fish, impels the kitten to ignore or retreat from the fish, rather than to seek to eat it. Now in the presence of the fish two independent and antagonistic systems of behavior are excited, one controlled by the food-getting propensity, the other by the protective or retreating propensity. Perhaps in the simple mind of the ordinary kitten both systems could not be operative at the same time, but with the more elaborate mentality with which we have hypothetically endowed her, both could be active. The food-getting propensity might dominate over the other quite completely so that the kitten would strive most energetically to get out of the box, but the other system would be active nevertheless in the form of slight physiological processes which interfere with the kitten's efforts to reach the food. In consequence of such interference it requires much longer time for the kitten to get out; or if she does get out with great difficulty for one or two times, she will be much longer in organizing that system of activities which we call learning to open the door, all because of that latent system which strives away from the fish. Here then we have an unconscious system which antagonizes the dominant system. The dominant system is partially conscious, although large portions of it are unconscious. This antagonistic system may be entirely unconscious, and it interferes with the process of bringing the dominant system to a state of efficient organization such that the kitten may be able to open the door promptly. She is frustrated in her efforts by an unconscious system which is part of her mind.

This case of the kitten is altogether hypothetical, but we can take a case from real life, which has been presented by Jung in his 'Analytical Psychology.'<sup>1</sup>

A young lady with a group of friends was walking down the middle of the road when the beating hoofs of runaway horses were heard approaching from the rear. The others stepped to the side of the street and easily evaded the team, but the young lady seemed so frightened that she lost her

<sup>1</sup> C. G. Jung, 'Analytical Psychology,' pp. 359-364. Tr. C. E. Long.

wits and ran straight down the middle of the road directly in front of the horses. Only at the point of exhaustion did she cast herself at the side of the road. Upon investigation it was found that she was not ordinarily hysterical but quite the contrary. Some years before she had been in Russia during a revolution and in the midst of death and other dangers had shown most remarkable self-possession. Why was she so much less capable of escaping from the horses than were her friends? By means of the technique of psychoanalysis the unconscious systems of her mind were analyzed and a propensity was there discovered which had organized about itself a system of tendencies which acted independently of, and antagonistically to, the natural instinctive propensity to escape from the horses.

This antagonistic unconscious system was developed under the following circumstances. The party at the time of the accident was returning from the home of a man with whom the young lady was in love. Circumstances were such that this night was the one supreme occasion, perhaps the only occasion, that she could hope to receive a declaration of love from him. If hurt, she would be carried back to his house. She certainly was not conscious of trying to get hurt, and indeed was not hurt, but the independent antagonistic system which sought a meeting with her lover, was impelling her into one mode of behavior, which was frustrative to the self-saving system which impelled her to escape from the horses. This antagonistic system rendered the self-saving system inefficient.

Freudian literature is full of such cases illustrative of the 'unconscious conflict' between systems of behavior which have become so fixated that they will not yield to that reorganizing process of mentality by which all established systems as well as all newly arising tendencies are assimilated into one comprehensive system under the control of a single ruling propensity. Any such inertia of unconscious systems means a diminution of mentality in as much as mentality is precisely this process of organizing the total mass of physiological processes into one system to the end of fulfilling the

ruling propensity. This process of organization or reorganization requires the transformation in some degree of all the preestablished systems which enter into the process. Hence any tendency or system of tendencies which resists that degree of modification which is required for adjustment in the newly evolving system becomes the source of inefficiency and of all those phenomena of which the Freudians treat.

However it should be emphasized that conflict per se is not inimical to the most wholesome and highly developed mentality. Indeed quite the contrary is true. There could be no mentality at all, as we have defined it, if there were no antagonism or maladjustment between operative systems of behavior requiring their readjustment and reorganization, for mentality is precisely this process of reorganization and readjustment and nothing else. Conflict, or maladjustment, is the indispensable pre-condition of that readjustment which constitutes mentality. Conflict diminishes mentality and causes misery and inefficiency only when the antagonistic systems are so rigid or inert that they will not yield to the process of reorganization. Mentality is diminished or rendered abnormal not by the antagonism of systems of behavior but by the automatism of such systems. Mentality thrives upon uncoordinated behavior, providing the uncoordinated elements are sufficiently plastic. If every tendency of the organism were perfectly adjusted to every other, mentality would disappear.

### CREATIVE MENTALITY

This relation between the process of mentality and coordinated behavior introduces us to a concept of prime importance. It arises in the answer to this question: Is mentality purely an instrumental process the sole function of which is to achieve coordinated behavior? Behaviorism all too frequently has treated mentality as though this were its sole function. But certainly the question must be answered in the negative.

The inherent function of any given tendency can be nothing else than its own fulfillment. It is not essentially a means

to any end unless that end is its normal fulfillment. The inherent function of growing grass, for instance, is certainly not to provide food for cattle. The inherent function of mentality is the achievement of coördinated behavior only in case such behavior is its normal fulfillment. Undoubtedly this is the normal fulfillment of a certain type of mentality. It might seem to be the only fulfillment in so far as we have treated mentality thus far. But we now wish to distinguish a second type of mentality.

Human beings manifest not only the process of organizing tendencies into coördinated systems, but they also display a process of organization which never produces a completed and final coördination, but continues indefinitely seeming to find no other fulfillment than the progressive evolution of a system which is never completed and never sufficiently coördinated to issue in overt action. This is the second type of mentality to which we refer.

Hence our analysis reveals two types of mental process. Orthodox behaviorism has restricted itself almost entirely to the study of one of these types only. But the second type is also quite in accord with its principles. The first type may be distinguished as a process which fulfills itself in the organization of some ultimate coördination of tendencies. It always culminates in some specific mode of interaction with the environment. The second type of mentality is a process which continues indefinitely, evolving some system of behavior, or series of systems, which never results in any ultimate adjustment. Adjustments may be made, issuing in overt action, but they are incidental to the further development of the given system; they are not ultimate adjustments.

In the first type of mentality, which we may designate as instrumental, we have seen that that mechanism by which the organizing process is controlled is some definite propensity which tends to bring itself to fulfillment by this very process of assimilating to itself all other tendencies and systems. We must now analyze the second type of mentality, which we shall designate the creative, and ascertain the mechanism by which the organizing process is directed.

This second type of mentality, the creative, arises out of an equilibrated conflict between two or more modifiable systems of behavior which keep one another from fulfillment and by reason of their mutual frustration constitute a continuous stimulus to the progressive outgrowth of new systems or the continuous evolution of some given system. But such a statement is hardly clear without an illustration.

Professor E. B. Holt, in his 'Freudian Wish,'<sup>1</sup> has made use of an excellent illustration for a different purpose. But it is especially adapted to our needs because Professor Holt has analyzed it solely in the interest of what we have called instrumental mentality. If we can show that the same data might also give rise to creative mentality, the two types of mentality will be set over against one another in clear cut distinction against the same background.

A man who is fond of mushrooms finds some while walking in the woods. But he recalls that some species are poisonous. He then experiences a conflict between two propensities, the one to eat what tastes good, the other to avoid poison. Shall he go on past the plants, casting backward many longing reluctant glances, or shall he sit down to eat with constant qualms and tremors of fear which quite destroy the enjoyment of the repast. If he follows either one of these two courses we have a case of conflict between two independent but maladjusted systems of behavior, the one seeking mushrooms, the other retreating from them.

Up to this point there has been only that slight degree of mentality involved in organizing each of the two independent systems. A much more complicated process will be required to adjust the two to one another. There are two possible methods of adjustment, illustrating respectively the instrumental and the creative types of mentality. As we have indicated, Professor Holt recognizes only the instrumental, although he does not use our terminology.

The adjustment which reveals instrumental mentality consists in dissociating the two systems and preserving them as independent and automatic propensities having no rela-

<sup>1</sup> E. B. Holt, 'The Freudian Wish,' pp. 125-8.

tion to one another. This is accomplished by learning from some infallible authority the precise marks of distinction between good and bad mushrooms. Thereafter every mushroom will be either good or bad. If the mushroom is seen to be edible, the food-getting propensity will be aroused and that only. The retreating propensity will not be excited and hence will not interfere with the eating propensity. If the mushrooms are plainly poisonous the retreating propensity will operate without interference from the antagonistic system, because the latter will not be active.

It is plain that this method of adjustment, the method of dissociation, does not involve any extensive reorganization of the two systems. Only that slight degree of reorganization is exercised which is necessary to bring about complete independence and automatism of the two systems. With this method mentality is exercised only to that minimum degree necessary to satisfy the original propensities by eliminating the conflict between them, and as soon as that is accomplished mentality ceases except as it may be exercised in eating or retreating. Conflict, the stimulus to mentality, is regarded as the great evil from which escape is sought.

But there is another method of adjustment which could be applied. This alternative method would consist in developing the antagonistic reactions to mushrooms until the systems of tendencies began to organize themselves about this plant, constituting, say, the scientific interest of botany. This new system of behavior, botanical investigation, instead of serving merely to dissociate the two original systems of fear and desire, becomes itself the predominant system comprehending these two original ones as subordinate functions of itself. Instead of mentality serving to satisfy fear and desire and then ceasing, fear and desire serve as motives to a progressive process which quite completely subordinates the original propensities. (Have not all the sciences arisen in some such fashion as this?)

Or again, the new and comprehensive system which arises out of the mutual frustration of the two original propensities may be that of psychological investigation. One may ob-

serve his reactions to mushrooms and find positive satisfaction in the conflict of tendencies because they serve to enlarge his system of psychological comprehension.

Or again, the new system which arises out of the reorganization of the primitive propensities may be that of humor. The whole experience with mushrooms reveals its humorous aspect. The two original reactions may remain, perhaps in a state of mutual frustration, but as constituent features of a humorous episode. (Here again may we not ask if all wit is not some such spontaneous reorganization of two antagonistic tendencies into a new system which arises so suddenly as to seem to flash into existence by magic, and which subordinates the otherwise meaningless and hopeless conflict to the function of creating this new system. We believe Freud's own interpretation of wit could be developed very readily into this theory.)

Still again, the new system arising out of the two opposing reactions to the mushrooms may be that of an ethical problem, the conflict serving to work out the features of an ethical theory.

So we might elaborate the possibilities of creative mentality arising out of these two conflicting propensities toward mushrooms. Whatever might be its form, and however it might be designated, as botany, psychology, wit, ethics, art, friendship or religion, it would be creative mentality rather than instrumental if it continuously brought forth new systems, or progressively modified some given system, without regard to the fulfillment of any particular propensity or completion of any final system. It is creative because it is directed to no other goal than the bringing forth of new systems for the sake of the process rather than for the sake of a final system.

This type of mentality seems to arise out of the very nature of the mind when two conditions are given. These two conditions are (1) complexity of the mental systems, and (2) their plasticity. In consequence of these two conditions no single system can operate as a ruling propensity, organizing the other systems of the mind under its control to the end of



its fulfillment. On the contrary the dominant system, by reason of its plasticity and the number, strength and diversity of the other systems, is itself modified so continuously and radically as to preclude its issuance in the form of a definite and completed scheme of conduct. In such case the subordinate systems are not passive beneath the control of the dominant system, but the readjustment is reciprocal among them all, the dominant system being transformed as much as they, hence there is no propensity which persistently seeks fulfillment in the form of a final and completed action, such as eating mushrooms or killing the foe or the like. On the contrary the process of reciprocal modification between the dominant system and the subordinate ones goes on indefinitely, producing a process of evolution which seems to have no teleology save the continuous elaboration of the dominant system or of a series of systems.

Of course it is understood that no human being exercises creative mentality alone. Any thing which serves to fixate the dominant system and transform it into a ruling propensity, halting the process of its continuous modification and making it the nucleus about which a definite and final system is organized and executed, gives rise to instrumental mentality. Many things may serve to give such rigidity to a mental system. Any strong and persistent stimulus such as hunger or fear will do it. Weariness renders the mental systems inert and so brings the creative mental process to an end. So also does long established habit. Age also removes that plasticity of behavior which is an essential pre-condition for this type of mentality. Among the lower animals the systems of behavior are perhaps never sufficiently plastic for this process of mind. Creative mentality may be the prerogative of human beings.

*Summary.*—Mentality is the process by which various stimulated tendencies of the organism are adjusted to the execution of a series of movements resulting in adaptation to the environment. Its mode of operation is the organization of diffusive tendencies into a definite system under the control of some dominant propensity having an instinctive

basis. This propensity consists either of an innate series of reflexes or of some system of tendencies which has been organized in the past experience of the individual and which persists as an established physiological motor set.

But where the organized systems of tendencies of the organism are sufficiently numerous and modifiable the process of their reorganization may continue indefinitely without issuing in any final completed system of behavior. Where the process of organization does result in a final system which can be fulfilled in execution, we call the organizing process instrumental mentality. Where the process continues indefinitely, never developing any system which can attain final satisfaction and thereby bring itself to an end, we call the process creative mentality.

# THE PSYCHOLOGICAL REVIEW

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## THE EVOLUTION OF BEHAVIOR

BY H. HEATH BAWDEN

### I. BEGINNINGS OF BEHAVIOR

Natural selection is at present the only accredited theory of organic evolution. In accordance with this doctrine what we call life and mind are as much, and as little, accidents as the shape of a stone or the color of an autumn leaf. In our particularly lucky corner of the universe we have happened to occur in accordance with the principle of the origin of organic structures and functions by fortuitous variations and survival of the fittest in the struggle among these variations. That is all science so far has been able to say. It is agreed among scientific biologists and psychologists that the sensitivities, the activities and the attitudes of our ancestors, human and animal, were not merely stages through which they passed, leaving them forever behind, but growths that have somehow entered into the very structure of behavior itself. Far more important than the fossils in the rocks, for a paleontology of behavior, are the tropisms and instincts and emotions we find in ourselves, and the precipitate of the past in an altered environment—in custom and code, institution and tradition, myth and cult, language and literature. And if this inheritance is more noteworthy in so far as it is a human product, it is no less real in the ages which preceded man. Long before our anthropoid ancestors discovered fire and invented the implement and the weapon whereby to mould nature to his ends, animals and plants had been modifying each other and the environment in that process, sometimes competitive, sometimes coöperative, that we call evolution.

Whether the beginnings of behavior are to be carried back to the physical and chemical elements on this earth or whether organisms were imported from another planet, it is perhaps useless, in the present state of our knowledge, to inquire. Arrhenius has suggested that ultra-microscopic bacteria may be driven about the universe by the pressure of light as are the fine particles in the comet's tail, and may have reached our planet by that means. There are known to be species of these microscopic plants which survive any degree of temperature which can be artificially produced, so that the extreme cold of interstellar spaces would be no obstacle. But this, of course, only pushes the problem a step further back. It may be, on the other hand, that within the etheric matrix whence the initial gas-cloud or swarm of meteorites was born from which our earth was derived, the potentialities of life were also generated. Life may have been a spontaneous generation which could take place but once in the primal birth-throes of a planet. However that may be, there evidently was a time in the early history of the earth when only the simplest unicellular organisms could exist. And these were the common ancestors of both the animals and the plants. Allied forms existing today are known as zoöphytes.

In order to understand the evolution of behavior we must employ for comparison some fundamental activity or activities which man shares with the lowest organisms. Such are found in the nutritive and the reproductive functions. Plants and animals, from the lowest to the highest, are characterized by the ingestion, digestion, and assimilation of food, the exchange of oxygen and carbon dioxide with the surrounding medium of the water or the air, and the excretion of noxious matters resulting from the oxidation of the digestive and respiratory functions. They are characterized also by some form of the process of reproduction, either asexual or sexual. It is obvious that the behavior of the lower forms is determined almost entirely, if not wholly, by these two processes. But it is equally true that that complex of behavior patterns which we call the civilization of man rests upon this same twofold basis. The hunting and fighting patterns still

underlie the food and the sex quest. From the food impulse spring the economic, the military, the political, the vocational activities. From the sex impulse, at least in part, spring art, religion, and the home. Literature, education, philosophy, science serve both ends. The food process and the sex process both involve the seeking of the distant object, movement toward it, grappling with it, and appropriating it. The rational processes of the higher animals and of man are but refinements in details of these fundamental coördinations of pursuit, combat and mastery of means to ends.

## 2. PLANT AND ANIMAL

In their simplest forms plants and animals are indistinguishable, so that many of the single-celled forms are classified in both groups. A plant may be regarded as a sessile animal or an animal may be regarded as a plant which has developed the power of locomotion. The plant has motion but not locomotion. The Myxomycetes or Mycetozoa—Slime-moulds—by some regarded as plants and by others as animals, illustrate an intermediate state, since in their earlier stages they have the characters of free-moving Protozoans, while in their later development they resemble Fungi. Here we see a degeneracy, so far as motility is concerned, due to the assumption of a sessile life. Cope argued from this that all plants are degenerate descendants of Protozoan animals. Probably a truer view traces them to a common source. Animals are not to be regarded as having developed in a linear order from plants. The higher types of behavior as found in the animals evidently sprang, not from the sessile multicellular plant, but rather from the primitive free-moving unicellular animal-plants. The complex sessile plants and the complex animals capable of locomotion evolved in different directions from a common free-moving one-celled ancestor. Nor must we forget, that, while the plants do not develop locomotion, yet their growth processes involve movements. As a rule in the plant these movements are confined to a rhythmic gyration of its growing-points. The tips of the roots and the terminal twigs of the branches of the tree are

continually in motion in what has been called a spiral squirm. The rootlet is searching for moisture in accordance with its hydrotropic proclivities, while the tendrils and buds and leaves are positively phototropic. If these motions could be projected upon a great chart in the sky there would appear an infinitely intricate intertwined pattern of spiral curves, an expression of the fact that even the life of the plant is a never-ceasing movement, from the swelling germ in the seed to the final flowering, fruition and decay of the mature individual.

The plant possesses one function which is absent in most animals: that of deriving its sustenance directly from inorganic nature, especially that of utilizing energy directly from the sun's rays for the breaking up of the dioxides,  $\text{CO}_2$  and  $\text{H}_2\text{O}$ , and turning them into vegetable compounds such as starch. The life-process of the plant begins with the dioxides—the carbon dioxide, which is breathed in by the leaves from the air, and the water, laden with mineral salts, which is sucked up from the soil by the roots—disintegrates them, builds up their elements into starches and sugars and other carbohydrates, and into hydrocarbons such as the vegetable oils, and then in the growth processes of the plant breaks them down into carbon dioxide and water again. The same is true of all animal life, except that there is relatively greater expenditure, and less storing up of energy in the case of the animal. But for both the life-process is a cycle from dioxide to dioxide.

This is a wider statement than that which we commonly associate with Behavior. Such processes are usually explained by chemical formulæ. There is no reason, so far as method is concerned, why we should stop with the chlorophyll-bearing zoöphytes in our description of behavior; indeed, the molecule and the atom may some day come within the purview of a science of behavior, especially in the light of the recent discoveries of the complexity of the mechanisms within the cell and the play of electrons within the atom. But in view of the present limitations of our knowledge we begin with the aspect of the metabolic process which, in the animal,

is indubitably associated with an evolution of progressively complicated behavior patterns. This aspect is that of the expenditure of energy which, while common to both plants and animals, is proportionately greater in the latter. Of course all living organisms expend energy. Even the green plants must expend energy in resisting the wind, overcoming the pull of gravity, carrying the sap from the roots to the leaves and conversely, and in the building up of the hydrocarbons and carbohydrates in their tissues. And in so far as they expend energy in these ways, like the animals they breathe out carbon dioxide. But the energy which the green plants expend and the carbon dioxide which they expire is slight in comparison with the energy they store up and the amount of oxygen they dissociate from the carbon of carbon-dioxide. Like a grain-elevator, a little energy is expended to store up a great deal. Compared with the plant the animal, on the other hand, stores up energy to only a slight extent, depending mainly on the food taken in from day to day for the sources of its energy. There is a certain amount of storage in the liver, the muscles, and in adipose tissue, but the distinguishing characteristic of the animal is expenditure of energy rather than its storage.

Even in the case of the animal, not all phases of expenditure are directly correlated with an advancing behavior mechanism, but chiefly those concerned with the sensorimotor adjustments and locomotion—the setting free in the muscles of energy which is available for tentative and overt movements of all sorts. In a broad conception of the matter, the behavior of the animal implies not only the process of photosynthesis in the plant but the electromagnetic vibrations from the sun which we call light and many other conditions in the environment. But in a narrower view, which endeavors to isolate the psychological problem from those of the neighboring sciences, the evolution of behavior may be considered as beginning in the katabolism of the animal organism.

### 3. EARLY LIFE ON THE SEAFLOOR

The primitive organisms were undoubtedly pelagic forms. It must be remembered that in early geological times there

was relatively less land and more water, that there were no great mountains and therefore less depth of ocean. Today unicellular water-forms like diatoms exist under the simplest conditions of life that can well be conceived. On the surface of tropical seas and lakes and ponds where there is an even distribution of temperature and illumination such plant life exists in enormous quantities. Under such conditions there is no necessity for the development of organs which will enable the creature to perceive the distant object. All it needs is immediately about it so evenly distributed that no special organs of perception or manipulation are required. It is hard to see how the more complex types would ever have arisen if the environment had continued to be thus simple. Brooks thinks that the appearance of a habitable bottom of the ocean was the occasion for the first development of the higher forms. The first stretches of water to be habitable were probably surface areas. But upon the seafloor there would gradually accumulate a richer medium than was to be found upon the surface, as the detritus of organic waste, food-particles, etc., would be deposited there. Animal forms dependent upon this food deposit would not have to be free-moving like the forms on the surface, nor would they need to be chlorophyl-bearing. They would become sessile and build up into colonies, at first simply aggregates of unicellular forms cohering, as they subdivided, in more or less solid groups like the sponge and the coral. Then those at the centre of such groups would change in structure and we would have the first appearance of two layers of cells, an outer sensitive layer and an inner absorptive layer, which would take on the perceptive and assimilative functions respectively. Here, in this symbiosis of single-celled Protozoans we get the first appearance of the metazoa or many-celled animals.

The sea is truly the mother of all life. All cells, even those of the many-celled animals and plants, require a liquid medium in which to live. The cells which make up the structure of an ox or a man are as truly water-forms as the diatom or the amœba, since they are all embathed in lymph.



This is as true of bone-cells and muscle-cells and nerve-cells as it is of the red and white cells which swim in the blood. In the case of the many-celled animal the liquid medium has been folded into the interior of the body. This has been rudely schematized by saying that if we were to take all the cells of the body and spread them out over the surface of the ocean they would have to spread over an extent which would include as much nourishment as is represented in the liquids of the body. When the water-form became a land-form it carried its liquid environment with it in the form of the lymph of the blood. The most important distinction between the unicellular and the multicellular form is, therefore, that the multicellular form controls the medium which surrounds its cells, while the unicellular form merely moves through a medium over which it has no control, picking up such food as chance throws in its way. The multicellular form keeps its internal liquid medium at a certain temperature, restores it regularly by breathing in oxygen and by the digestion of food, while the unicellular form is at the mercy of the elements.

The most primitive forms which represent the transition from the colony to the pluricellular form are those which are simply a sphere of cells surrounding an inner cavity, like a rubber ball. This, in the development of the mammalian embryo, is called the blastula stage, where we apparently have a hint of what took place in the evolution of the race. The next stage corresponds to the poking in of one side of this rubber ball, thus producing the gastrula or stomach-like stage. This second cavity, resulting from the invagination of the blastula stage, is thus really a part of the outside world folded in to become an alimentary tract. By this method not only is the outer liquid medium transferred to what are now interior cavities of the organism, but other structures and functions originally on the exterior of the body are carried within to constitute the deep-lying sense-organs and nerves known as intero- and proprioceptors.

#### 4. BILATERAL SYMMETRY AND RESPONSE TO THE DISTANT OBJECT

With the development of the multicellular form we first encounter the differentiation of definite organs for the perception and appropriation of food and mates. Locomotion, likewise, emerges at the point where, in the evolution of the sessile animal, its demands exceed the capacity of the immediate environment to supply nourishment. It is the problem presented by this combination of conditions that determines the appearance of sensorimotor structures and functions answering to what has been called the recession of the stimulus. As the animal form becomes adapted to an increasingly wider and remoter environment, the receding stimulus is represented in behavior by the conditioning of the reflex: for every complication of the context the action-systems of the organism exhibit a corresponding cross-reference synaptic or protoplasmic. Thus we may conceive to have arisen the twin facts of bilateral symmetry and response to the distant object.

The route by which the free-moving multicellular animal form evolved from its free-moving unicellular ancestor is far from clear, but it may be conjectured that in part this took place by a detour through the sessile colony types or compound individuals whose original habitat was the seafloor. With the relative impoverishment of the liquid medium in which such forms subsist, the occasion would arise for the detachment of such forms and the resumption of some mode of locomotion which, teleologically interpreted, might be described as the pursuit of the wonted stimulus. The free-moving multicellular form returns once more to the surface in the search for food, solid and gaseous as well as liquid. The jelly-fish would seem to represent such a transitional type, since its first stages are sessile while its later stages are spent on or near the surface as a free-moving form.

Without presupposing any entelechy, or any resident principle other than such as are suggested by the facts of geotaxis and chemotropism, we may yet clearly conceive the possibility of elongation of the irregular shaped multicellular

mass into a concatenated and later into a metameric form, with clearly defined head and tail ends—due, on purely mechanical principles, to the influence of localized stimulations in the environment. The results of experiments with viscous substances externally propelled through a liquid or semi-liquid medium go far to support the conclusion that more rapid movement and continual adjustment of a plastic protoplasmic mechanism, in its progressive adaptations to the chance canalizations of stimulus in a fortuitous environment, would tend to produce the various types of bilateral symmetry which as a fact we do find.

Whether or not the existing radiates, such as the sea-urchin and star-fish, represent an arrested stage in this transformation, we are warranted in supposing that such spherical or radiate structures represent one of the abortive, while yet partially successful, solutions of this problem. The jelly-fish moves about slowly by simple rhythmic pulsations of its umbrella, without any structural characteristic to determine that it shall move in one direction rather than another. It is still relatively at the mercy of its environment. The more rapidly moving forms, on the other hand, take on an elongated shape, bilaterally symmetrical, and later, as definite organs for perceiving and manipulating the distant object develop, we have the appearance of the head and tail ends of the animal. At first any part of an organism will serve as a mouth, as in the case of the *amœba*. But with the development of the elongated form, such as the worm, the mouth is found in that part of the body which first comes in contact with the food-object. It would thus be the character of the objects toward which the animal is moving, and indeed the character of the environment in general, which would determine the appearance of the bilateral form.

The mode of origin of the segmented or metameric form is still shrouded in mystery, and here we encounter one of the most baffling and enticing of the gaps in our account. Without doubt here, as elsewhere, whole chapters remain to be written. But, on the other hand, nowhere are structure and function more obviously related: the exfoliation of receptors

irritable by the receding stimulus is accompanied point for point by an involvement of effectors for pursuit and capture of the end-object. Moreover, in each segment is found a double nerve-knot for transmitting and accelerating the response to the stimulus, while at the head end are the cerebral ganglia for controlling the responses of the segments in the interest of the organism as a whole. Thus a wave of motion passes from one end of the organism to the other, each segment acting more or less independently of its neighbors, and yet capable, when necessary, of coöperating in the more important functions segregated at the anterior end.

### 5. INVERTEBRATE AND VERTEBRATE

Evolution is not continuous from plant to animal; invertebrate to vertebrate; fish, reptile, bird, mammal to man. There is a concomitant evolution of different types. Evolution is like a tree with a branching trunk, not like a tree with one central stem running straight from tap-root to terminal bud. There are two types of development, as represented by the invertebrate and vertebrate. In each of these we find a relatively complex type of behavior, but based on a distinct principle of structure and function. In the one we find a defensive exoskeleton, a ventral nerve-chain, and remarkably specialized types of invariable response. In the other we find a supporting endoskeleton enclosing a second and new type of nervous system not represented in the former.

In the higher types of invertebrate there is a collapse of the metamerer, the ganglia of various segments being condensed or telescoped into a single double ganglion. The ganglia in the head-end remain much as in the worm, but in other parts of the body, such as the thorax and the abdomen, certain segments disappear or coalesce, and the different nutritive and reproductive processes and locomotion are segregated in these regions. The legs disappear at the head-end where they are transformed into mandibles, jaws, and become concentrated in the thorax, while the assimilative and reproductive systems come to be located in the abdomen. The advantage of this division of labor, as seen, for example,

in the crustaceans and insects, is found in the greater unity and power of direction of activity which such an organism has, contrasted with the unspecialized metameric form in which most of the functions are performed equally by all of the segments. Instead of all the segments moving and digesting and breathing and reproducing, certain ones are specialized for the perception of the distant object, others for movement, others for digestion, breathing, reproduction, etc. Contrast the spider or the bee or the ant with the worm, in this regard. In the bee or the spider the impulse does not have to pass down the whole series of metameres, as in the case of the earthworm, to produce a movement. It is fair to assume that the greater complexity of behavior of such forms is due to this centralization and distribution of functions, the specialization of different regions for different functions and the bringing of all the motor organs into more direct and effective relation with the controlling center in the cerebral ganglia.

In the vertebrate, on the other hand, the metameric form is preserved, as shown in the structure of the central nervous system with its succession of spinal and cranial nerves springing in pairs from the bilaterally symmetrical spinal cord and brain-stem. Here the great development is in the brain at the head-end, in striking contrast with the development as it takes place among the invertebrates. In the vertebrate the unity and direction of action is secured rather by the multiplication of conduction-pathways and synapses than by the telescoping of the metameres. It is as if nature had experimented with both methods of evolving greater variability of behavior but succeeded only with one, in the vertebrates; securing great complexity and precision of response in the case of the invertebrates but at the price of variability. They thus represent parallel, not serial, developments, each reaching a climax along a different line.

It must be remembered, however, that the vertebrate retains an equivalent of the invertebrate nervous system in the autonomic system which, like the ventral system of the invertebrates, consists of a mere ladder-like double-chain of nerve-knots. And since what we call our instinctive and

emotional life is correlated with this structure, it is not mere fantasy to suppose that our behavior is here linked with that of our distant kin among the articulates.

#### 6. CEPHALIZATION OF THE SENSE-ORGANS AND DEVELOPMENT OF THE BRAIN

Three facts characterize the head end of the organism: the aggregation of the sense-organs which recognize the distant object; the centralization in the brain of the nerve-centers which control the movements of locomotion toward the distant object; and the mouth with its mandibles, teeth, etc., for the manipulation, mastication, and ingestion of the food object when reached.

The function of the sense-organs is the recognition of the food or sex object. One of the most noteworthy facts of the structure of the evolving vertebrate is the crowding together in the head of the projicient receptors. What in the lower animals are present as chemical and mechanical receptors scattered more or less generally over the periphery of the body, are here brought together in a closely associated cluster of irritable end-organs. The retina is a group of glorified warm-spots, the cochlea a group of modified touch-spots, while tongue and the nose contain areas which represent an intensification of the chemical irritability still characteristic of the visceral epithelia.

The extraordinary development of the brain, and especially of the cortex, is the inevitable accompaniment of this cephalization of the distance-receptors, since it is through these that the organism becomes related to the remoter parts of the environment. The relative weights of the brain and body as a whole, for example, jumps from the ratio of 1 to 5,668, in the case of fishes, to the ration of 1 to 186 for mammals. And, as the structure of the cortex shows, the greater part of this advance in relative amount and complexity of structure is represented in the ganglia and conduction-pathways necessary to equate the activities of these highly specialized receptor organs. "Fossils show that while the average size of the mammals has diminished since the middle

Tertiary, the size of their brains has increased more than one hundred per cent." (Brooks). The range of possible accommodation to a variable environment increases, and behavior becomes less dependent upon definite heredity and more upon acquired habits of the individual. Cuning counts for more than size and rational deliberation for more than precision of invariable response.

All this specialization of receptors and multiplication of conductors is more or less immediately related to the process of finally bringing the distant object to the mouth, or at least, as in the case of sex, for the sake of manipulating it in such a way as to bring about, sooner or later, the fulfilment of the inherited impulses. And not only are the cephalized sensorimotor processes, represented in the distance-receptors, subservient to this end, but also all the processes of locomotion and manipulation. The leg was developed for the sake of the jaw. Everything in the baby's hand goes into its mouth. The leg and the wing and the fin are for flight or for the stalking of the prey. And practically all the minor motor organs were specialized to lure or to warn, from the curious fish that dangles a worm-like appendage to its head before its victim, to the cotton-tail rabbit or deer whose conspicuous upturned flag is an instant signal to the group. Nor, indeed, has the predatory device disappeared even from the food quest of man, since the more sagacious invent legal methods of defrauding their fellows of the full product of their labor.

## 7. TRANSITION TO THE LAND AND THE CONTROL OF THE ENVIRONMENT BY THE ANIMAL

The passage of certain animals from the water to the land was probably determined by very definite conditions. In the water there is comparatively little development of plant life and what there is is unicellular for the most part. The conditions are so uniform that there seems to be no occasion for the development of the higher forms of vegetable life. The animals either feed upon this unicellular plant life or feed upon other animals which in turn feed upon it. The

unicellular plant forms of the plankton, such as the Plantain that covers thousands of square miles in the Sargasso seas, find themselves surrounded by the essentials of their existence and nothing would be gained by the development of a multicellular form.

But along the margin of lake and pond and stream we see the gradual passage of both plants and animals to the land. The algæ, among the plants, we may suppose, crept from the sea to the shore and evolved into the moss and the fern. First the spore-bearing plants and the conifers and later the plants which blossom and mature their seeds in an ovary, establish the new habitat. And among the plants which propagate by cross-pollination of their pistils, obviously the anemophilous must precede the entomophilous, since pollination by the wind would be possible long before the floral organs of plants were modified to correspond with the habits of insects. The transition must have been due to successive changes in the environment. The continents were rising, the seafloor was settling, and the denser atmosphere of the earlier times was becoming rarified. The giant forests of the Carboniferous era, of which our California Redwoods and Sequoias are relics, sufficiently indicate the enormous quantities of carbon dioxide which in those days must have been present in the air to enable plant life to attain to such proportions.

The amphibian of today is a reminder of the transition on the animal side, as are also the Dipnoids or Lung-fishes—the lung originally developing from the air-bladder of the aquatic form. Among the reptiles we encounter the development of the three- and the four-chambered heart, the passage to a gaseous medium necessitating the separating of the arterial from the venous blood. The ancestors of the spiders and the insects, supposed to have been evolved from a tracheate worm, likewise took to a terrestrial existence. Amphibian, reptile, bird, mammal represent the order of evolution of the vertebrates on the land.

In general when the plant passes to the land it precedes the animal, for the animal must have the plant to feed upon.



The animal can go no further from the plant, in its excursions, than the energy taken from the plant in the form of food will allow. But the conditions of the plant on the land are very different from those in the water. The multicellular form now becomes a necessity in order to provide an internal liquid environment for the cells of which its tissues consist. Consequently, in passing to the land the migrating forms must either carry their liquid environment with them or, as in the case of bacteria, usurp the liquid environment of some other organism. The plant must protect itself against evaporation of its sap by the sun or the liquid environment of its cells will be dried up; hence the development of cellulose tissue for protective purposes. Moreover, on land the two sources of energy, air and water, upon which the plant is dependent, are separated, as they are not in the water. The carbon dioxide and sunlight are in the air while the nitrates and potash and phosphates and other mineral solutions are in the soil. The fibrovascular bundles of cellulose tissue not only protect the living layers of cells from evaporation but interconnect the leaves and roots which are in contact with the air and soil respectively. And this same cellulose tissue, in the form of dead bark and woody fiber, protects the cambium layer of living cells from the depredations of the animal.

The passage of the plant to the land, with its changed conditions, accordingly, presents new conditions for the life of the animal. The multicellular plant has erected a barrier; hence the terrestrial animal must develop in corresponding complexity in order to overcome this obstacle to getting its natural food supply. How difficult this was, merely as a physical and chemical problem, is seen in the difference of structure of the digestive organs of beasts of prey and cattle. Practically the entire energy of the ruminant, as represented in its series of stomachs, is devoted to breaking down this cellulose tissue of the plant wherein its nourishment lies. The ox has a gut which is thirty times its own length while the tiger or lion has a gut which is only eight times its own length. This, in a rough way, is an index of the amount of energy which is required for the digestion of the two kinds of food.

There is no necessity for the plant to develop the bilateral form, methods of locomotion or organs of any complexity for the recognition of the distant object. It finds its sources of food everywhere about it in the soil and the atmosphere. But the animal must develop such organs or perish. The main problem, as the animal passes from the water to the land, is to overcome the obstacle presented in getting at and digesting the cellulose green tissue entrenched behind the outer protective layers of woody fiber. Of course, this relation between the plant and the animal is one not only of conflict and struggle but also of coöperation. The grass grows faster for being cropped. Animals are an important means of distribution of the seeds of plants; and in return, we may say, the plant develops its fleshy fruits and edible seeds for the animal. There is both competition and coöperation in nature. And as we pass up the evolutionary scale we find that plant and animal life are increasingly interdependent. The dependence of the plant on animal life reaches its climax in the reaction of the human animal on the plant world in agriculture—a process the same in principle as that which operates on the lower levels.

## 8. THE BUILDING UP OF A SPATIAL AND TEMPORAL WORLD

The recognition of the distant object and the possibility of movement toward it is the basis on the part of the evolving organism of the building up of a spatial-temporal world—a world which can be stated in terms of motion and locomotion. This represents the going out of the animal beyond its bodily limits to elements which lie outside of itself. As Lotze pointed out, when you take a stick into your hand you enlarge your contact world by the length of the stick. When you place your eye to the telescope or microscope or your ear to the telephone or get aboard a car or ship or aëroplane you extend the function of eyes and ears and legs. This is an important step in the evolution of the animal form—this attempt of the organism to state the environment in terms of the activity, the functions, the behavior, of the organism itself. We are prone to think of the environment as fixed,

but of course there is an evolution of the environment as truly as of the organism, and in all stages evolution is really a progressive dynamic balance of interaction between two changing systems of activity.

In a certain sense we may speak of the animal as standing at the center of a sphere and of this sphere as increasing in diameter as the capacity of its distance-receptors and controlling effectors increases. The head-end with its sense-organs, of course, is directed toward the distant object. We may conceive the fundamental line of space to be the axial line of vision drawn from the theoretical cyclopean eye to the distant object. This line would represent, however, not the diameter, but the radius of the field of activity, since the animal is able to turn about. Space is a spheroid to us because the body is capable of revolving in every direction; and the eyes within their sockets by their rotating movements, in conjunction with the turning of the atlas upon the axis, further facilitates this. If one's eyes were fixed in their sockets, one's head on one's trunk, and one were unable to turn about at the waist or on one's feet, probably space would be a very different affair. It may vary for the sessile animal, for the quadruped, for the burrowing, the climbing, the swimming, the flying form.

But the existence of this axial line would be of importance to the animal only if it were able to move toward the distant object. There must therefore be an equation between the motor processes of locomotion and the length of the line. As the terrestrial animal arises from the earth and acquires organs of locomotion, either as a quadruped or as a biped, there arises the problem of the maintenance of balance while in motion—a problem which appears in a different form to a walking, a leaping, a swimming, and a flying form. This problem increases in complexity as we ascend the scale from the creeping myriapod with its fabled thousands of feet to the human being which has only two supports. The advantage of the fewer supports and the upright attitude of the biped is the possibility of more rapid movement and a wider sweep of vision. The statement of the environment

in terms of the maintaining of such a balance gives us another fundamental equation of the space-time world of the animal.

The distance between the organism and the distant object is measured in terms of the number of paces or other functions of effectors necessary to reach it, and the animal interprets the one in terms of the other. This is a fundamental equation in the building up of a space and time world. The possibility of analyzing this line in terms of the locomotor processes and conversely, is basic to all space measurements and time adjustments of living creatures. Inasmuch as the animal never has a single object only in the field of vision, this implies in some sense a selection of one object or situation as the objective point and the treatment of other objects in the field of vision either as obstacles to be overcome or as instruments to the end represented by this selection. All the stimulations are necessary to the fulfilment of the act as a whole—even those that are subordinated to the main activity; we define ourselves in terms of what we inhibit and reject as truly as in terms of what we attend to and select. The ground, for example, represents the resistance offered to the foot, but it represents also the means for walking or leaping forward. The trees and bushes may be used as screens for stalking the prey. The intelligence of the animal is in direct ratio to its capacity for transforming these negative obstructions into positive aids to what it is undertaking to do.

#### 9. THE SHIFT IN EVOLUTION WITH THE APPEARANCE OF MAN

The end toward which evolution moves in the animal kingdom seems to be control over the environment, especially over the vegetable world. The animal, like the plant, is confined to the circular chemical process from dioxide to dioxide; but the animals make the plants partially prepare their food for them, and some animals, the carnivores, make other animals still further prepare their food. This process exhibits higher and higher forms of behavior as there is more and more control over the expenditure side of this circle. When in civilized man we get an adequate control over the

vegetable and animal environment the evolutionary movement along this line reaches its climax and a certain degree of finality. Unless some great catastrophe transforms the face of nature it seems as if this stage of evolution, which Darwin and his followers so adequately grasped, would give place to a new type of development.

For one thing, evolution has shifted from the development of sensory and motor organs to a modification of the environment itself. Man does not evolve a better organ of vision but invents lenses to supplement his imperfect optical instruments. He supplements the ear by telephone and telegraph. He refines his tactile delicacy by instruments of precision. He supplements motor and locomotor organs by artificial means of power and transportation. Evolution shifts from the inside to the outside environment. Thus is opened up the era of extra-organic evolution—evolution in terms of implements and weapons and extensions of the sensory and motor functions by mechanical means. It is of course just as legitimate to speak of evolution as taking place in the pushing out of railroads on the frontier, the extension of commerce, the invention of the automobile and the *aéroplane*, as in the cephalization of sense-organs and the telescoping of the metameres.

The whole life period is estimated to have been anywhere from twenty-five to sixty-five millions of years, almost all of this having elapsed before the appearance of the Primate. Yet this comparatively ill-equipped and puny mammal has transformed the face of the earth, exterminating not only the more ferocious of the beasts, but many varieties of his own species. The polar bear or the cactus tree is relatively adapted to its environment. It must await secular changes to alter its form; and if these are too sudden it will perish. But certain creatures, among whom were man's ancestors, instituted what, looking back upon it from our present vantage-ground, we may call the human as contrasted with the natural economy. The creature controls and modifies the environment instead of being controlled by it. He turns meandering rivers into straight irrigation ditches; he plants

seeds and cultivates them instead of depending on the uncertain sowing by wind and insect; he domesticates animals for food and labor power; he fashions animal pelts and the fibers of plants into clothes and the wood of trees and materials from the quarry and claybank into shelter; he discovers or invents fire, tools and weapons; he is discovering means of immunization against the ravages of harmful parasites; and there only remain the problems of artificial production of protoplasm and interplanetary transit when he will have controlled the fact of death itself.

It was the greater range of control and variability of response which these supplementary extra-organic sensorimotor processes made possible which gave man his great lead over the other types. The insects developed complex forms of sensory and motor organs, sometimes of marvelous delicacy, but this only rendered them still more dependent upon fixed conditions in the environment, whereas the unique thing about the human variation was the use of one part of the environment (the weapon or the tool) to control another part, in the interests of the organism. As one writer says, "It was not to the fact that man possessed hands that he owed his mastery. It was because he used those hands to make an alteration in his environment" (Lane, 'Law of Social Motion,' 120). The higher types may be said to have organized more of the environment into themselves by reason of a more variable adjusting apparatus in the brain whereby an equation might be established among increasingly remote stimulations. Or, again, looking at it from the other point of view, the higher animal may be said to have widened the scope of his individuality or selfhood just to the degree that, by extra-organic means, he has increased his control over the environment by supplementary sensory and motor organs. If by natural selection is meant merely the non-deliberative method of survival in the sub-human stages of evolution, then natural selection is comparatively wasteful, arbitrary, rigid and blind, leading to the survival of the fittest in only a limited sense of that word; but if it is used in the widest sense as including all phases of this extra-organic evolution, then

there is every reason to believe that all the phenomena of human civilization and culture have persisted only because of their superior survival value.

It is conjectured that the comparatively sudden shift in the evolutionary process which we encounter with the appearance of man—sudden in comparison with other geological changes—took place because of the abrupt alteration of the environment when the ice-sheet began to descend from the north in the early part of the Quaternary age. There is evidence that in what are now temperate and arctic zones all animal and plant life has been at some time related to tropical conditions. Coal is found in Iceland and Greenland and in the north of Scotland. The cinnamon tree flourished in Iceland. At the beginning of the ice-age there was a migration of this tropical forest gradually southward. This can be followed in the North American fossils. The animals that were able to migrate with the forest did not change their forms; their descendants are found today in essentially the same forms in South America. The change in form took place in those animals that lagged behind the migrating forest and succeeded in adapting themselves to the changing conditions. There is evidence of this in other animal forms. Any animal which staid behind had either to grow in size, in order to secure the necessary amount of bodily heat, since the surface area increases in diminishing ratio with the volume, or it must develop a heavy coat of fur, or hibernate, or perhaps all of these at once.

But in the Asiatic and European fossils we find a different set of facts. The migration southward of the forest paralleled that in North America. Here too there is relatively little difference between the early forms and those in Africa today, although there is a greater difference than between North and South America. But at the Indian ocean and the Mediterranean sea, or the great inland ocean from which the present Mediterranean is derived, there was a check to the southward migration of the animal forms. In the case of America there was no great barrier to the south to offer obstruction, but in Asia and Europe the conditions were

present for what may be regarded as a crucial experiment in evolution. The North American animals, including the primates, migrated with the forest to the south; the monkey forms in South America today are essentially the same as the fossil forms in the North. But in the peninsulas of Southern Asia and Europe they were penned in, caught in a trap, as it were: they must either undergo a rapid adaptation or perish. The mastodon illustrates a case of adaptation among the lower animals. His great size and heavy coat of hair made it possible for him to remain in the frozen zone. But the primates were relatively small and highly organized and doubtless most of them perished.

If any of them were able to adapt themselves it would have been just where in fact is situated the traditional cradle of the race. On the Indian peninsula there would have been no possibility of further migration to the south, while the Himalayas towered to the north. Here would be the point of struggle; if a new type could develop at all, it would be here. It is conjectured that this is what in fact happened. The form to undergo such a transformation naturally would be one of the primates, since they were the most highly organized and at the same time the most unstable, and therefore the most plastic. We can imagine some one or more species that descended from the trees, gradually changing their diet from fruits and nuts to shell-fish and other forms that could be found on the beach, preying upon other weaker animals, and finally picking up the stone or the club as a weapon, perhaps discovering fire in the chipping of flints, and finally evolving the rude beginnings of clothing and shelter. There is evidence in our own anatomy of a partial change to a carnivorous diet.

Here the beam tips in the evolutionary process. Our ancestor was not equipped to meet such an emergency so far as his gross structure was concerned; many other forms were indeed better equipped than he. What determined his survival and his preponderant power ever since was the accident, if it was an accident, that he began to live by indirect means. The center of the struggle was shifted from



the organism itself to the environment. Extra-organic extensions of the organic functions so transformed the environment in the case of the Primates that they were able to survive where otherwise they would perish. The conquest of the environment by indirect means is the great mutation by which the evolutionary process leaped forward in man.

#### 10. THE HEGEMONY OF THE ACCESSORY MUSCLES

One cannot penetrate into the beginnings of behavior, especially as found in the action-systems which evidently underlie human conduct, without the query arising whether the emergence of that mirror-image of himself that man has come to dignify as a distinct entity under the word mind or spirit or the psychical, may not have been a quite accidental and incidental product of the extraordinary development of the accessory muscles, particularly those of speech. Up to the time when our anthropoid ancestor began to babble to some effect, his motor processes did not differ essentially from those of his brute associates. They involved the gross musculatures of the trunk and limbs and, on the whole, constituted a unified and continuous action-system. But when, in the midst of such fundamental adjustments of food and sex, the freedom of the arms and hands made possible the use of the weapon and the tool, and thus released the voice, measurably, from its strict servitude to the needs of the warning cry and the signal of distress, we may imagine that the mechanism of the larynx came into the service of social ends. With the multiplication of the new types of situation which would spring rapidly from the mastery of the environment through the use of one extra-organic process to control another, the need for some method of communication would arise, more precise than the primitive gamut of insufflations and grunts. What more natural than that this flexible mechanism of vocalization, relatively freed from the sterner demands made upon it in the past, should be modified and enriched to furnish the symbols of reference necessary to a more diversified group activity.

With the recession of the snout, the abbreviation of the

tusks or tearing teeth, and the substitution of the hand for the muzzle in the manipulation of the food object, the finer musculatures situated in the head and neck become liberated for this surrogative function in relation to a widening range of sensorimotor adaptations.

And it is chiefly noteworthy that the use of such an accessory musculature, under the conditions of an extra-organic evolution such as we have been supposing, would naturally represent, in the main, arrested or incipient (*i. e.*, controlled, or what in recent studies have been called conditioned) responses. Speech partakes of the character of an attitude rather than an act. The word or name comes to stand for the act. It is a tentative movement which serves as a kind of substitute for the completed performance. As the distinctively human aspect of behavior, in the course of time, passed over almost entirely into terms of the extra-organic adjustments, these intra-organic coördinations which have come to stand for them, would finally carry an increasing ratio of the meanings of life. And since such meanings are always conditioned, in the sense that they are symbolic of the deeper-lying and more pervasive activities of the fundamental muscles, and ever point to them and presuppose them, it is natural that there should appear a plane of cleavage between the two levels of behavior. This is the biological basis for that duplication of realms that has played such an important, and often disastrous, part in the evolution of psychological theory.

## II. THE FUNCTION OF THE LARYNX IN INDIRECT CONTROL

We have seen that the liberation of the larynx was a collateral effect of the eversion of the foot, the freeing of the arms and hands, the recession of the snout, and the consequent substitution—except for the mastication of food—of the manipulative functions of the hand for those of the mouth. This effect was of momentous importance in relation to the initiation of increasingly indirect methods of control. The substitution of the gesture for the completed act, of the spoken word for the gesture, of the written for the spoken

word, and then, within the confines of the now socialized behavior of the individual, of subvocal articulation for interlocutory discourse, introduced a technic for handling the remotest parts of the environment as the stimulus receded farther and farther from the response. No part of overt behavior escaped the effects of this introversion: every act, and every object and situation, representing possibilities of action, was destined to find its counterpart in some incipient innervation or tentative tintinnabulation of the articulo-motor apparatus, and to a less degree of the optico-motor and grapho-motor mechanisms involved in reading and writing.

It was no accident that the elaboration of symbols took place primarily in connection with the functions of the larynx and the ear rather than in connection with those of the eye and the hand. It was these which were first freed from the stress of the struggle for survival. The eye must still be at the service of the hand, but the ear was, comparatively, released from this necessity and could take up with the larynx the functions of speech. Moreover, in our ability to make sounds we have a mechanism which, unlike the signs which appeal to the eye, we have always at our service. In the midst of all other kinds of activity, in any position of the body, in darkness as well as in light, the larynx may keep up its kinæsthetic comment upon the other activities, with a check upon it, in turn, by the ear. This expansion of activities is reflected in the increasing complexity of the central conductors, chiefly in the cortex. It is an instructive fact that the center for language lies adjacent to the center for the hand and without doubt the fact that the language center is unilateral is to be correlated with the fact that man is normally right-handed.

Behavior is turned in upon itself in the form of arrested movements, this inhibitory process promoting, in turn, the multiplication of new conduction-pathways. Speech uses many of the same muscles as eating, but it has elaborated a far more complex system of synapses. The function of this new behavior-complex is what is called thought, which has been significantly described as interior speaking. The advent

of a larynx, converted to the uses of language, gave our ancestor a new machine by which his power of accumulating experience was increased. It is the power of operating at a distance by the use of symbols, which demarks man from the brutes and gives one man or one race superiority over another. Man is *homo sapiens*, the thinking animal, because he is the speaking animal; brutes are the dumb animals.

Brutes are capable of vocal expression, it is true, but the function of symbolization in such expression is minimal. There is a limited amount of expression for the sake of communication—the cry of distress, the call for help or for mates—but for the most part expression is for expression's sake. The animal grunts or whines or barks or roars or chirps or sings with very little precision of reference to what his fellows are doing. The vocalization of man, on the contrary, is characterized by its indicative and social reference. As Romanes said: "So a man means, it matters not by what system of signs he expresses his meaning: the distinction between him and the brutes consists in his ability to mean a proposition." The utility of symbols has been enormously augmented by the arts of writing and printing. Language and literature, libraries, museums, laboratories, all the significant institutions of civilization, are the precipitate of such premeditated acts. The emerging individual finds a vast amount of the work of catching up with the race already done for him. A great part of his learning may be done by proxy. In a certain sense each new generation may begin where the previous one left off. The human infant is much more helpless in its environment than a kitten or a puppy of the same age, but he has what they lack, a budding mechanism of vicarious response by which he soon immeasurably outstrips them.

We do not know precisely how language originated—probably in the cries and calls of animals in relation to food and sex, and the expression of such other fundamental trends as fear and anger and pain. We may observe its beginnings by a study of the child. Such a study gives us an insight into what is perhaps the most complex and closely interwrought

system of behavior-patterns which is anywhere to be found—the only basis of behavior upon which it is conceivable that the vast superstructure of human literature and science and philosophy and art could have been erected. The complexity of this system is apparent in an enumeration of the sensorimotor mechanisms which are involved. A word may be spoken, heard, written or seen, involving the intricate interplay of musculatures of the articulomotor, the auditory, the graphomotor, and the opticomotor apparatus, while, in the synaptic connections in the cortex and lower centers in the brain, are to be found a most complicated system of corresponding conduction-pathways. As in the history of the race, the child begins by hearing words spoken, gradually learning to speak them himself by reason of that extraordinary over-production of movements in vocalization which is the natural accompaniment of the abundant vitality and proliferation of new cells which characterize the growing organism.

Speech is a combination of the singing tone or vowel sounds produced by a column of air passing over the vocal cords and the whispering attitudes known as consonants produced by the various coördinations of the muscles of the mouth and throat. The endless experimental exploitation by the child of this vocal apparatus, in a stimulating context, which from our adult point of view we erroneously describe as imitation, is the basis of variable response from which is evolved in a remarkably short time his ability to speak and to thread his way verbally amid the maze of meanings among which the adults about him move with such apparent freedom and ease. That this ability is acquired in so short a time is only explicable by the fact that he is born into an environment of selected stimuli by which he is enabled to make innumerable short-cuts in a learning process in which his primitive forebears floundered for ages.

The limitations and the dangers of the symbol grow directly out of its usefulness. The very assistance which the word renders as a convenient handle to remoter objects and events and situations leads almost inevitably to a substitution of means for ends. Man gives a thing a name or

finds a word to mediate between an attitude and an act, and then uses the name as if it were the thing and the word as if it were the finality of response—forgetting that the name may be but the moment's rendering of the stimulus, that the word may be but the moment's embodiment of the response. Words come to be treated as the miser treats his coins, hoarded up and gloated over for their own sake, in disregard of the fact that they are but a medium of exchange, their value depending upon the concrete things they represent. Language is that portion of behavior which functions as a buffer between other parts of behavior, intermediary between the inaccessible tensions within the individual and the overt adjustments of social intercourse. It partakes of the character of both conduct and thought; it is less overt than what we call acts, but more overt than the tentative movements in the accessory muscles which we call thought. Its chief utility lies just in this ambiguous, this amphibious character. Theoretically a word alters its meaning every time it is used, since it is being employed to mediate factors in a situation different, to some extent, from any that has ever been encountered before; practically, the word becomes a compromise and a reducer of these differences to some common denominator of action. Here are both its great serviceability and its harmfulness in growth. In so far as this reduction of differences is subservient to the ends of an experimental expansion of experience, it makes for economy and efficiency in action; but when it becomes habituated to the point of functioning independently as a behavior-pattern, there is danger of the substitution of a system of abstract relations for the world of concrete individual facts.

It is clear, therefore, why language has been of so much value in building up what we call our intellectual life. A word can stand, not only for the extraorganic object or event remote in space or time, but also for the obscure intraorganic innervations and nascent movements for which an unarrived psychology has had no other descriptive terms than the vague popular terms feeling and thinking. It stands for these, and relates them in that total of overt activities which we

call the conduct of life. Language thus is a bridge between the inner citadel of the interoceptive and proprioceptive complex we call the self and that exteroceptive complex we call the outer world. Words are our 'innards' trying to find hands and feet; they are also the machinery by which we succeed in bringing an increasingly wider range of the environment under control, organizing it, in a very true sense, into the very substance of our selfhood.

A gesture, then, is an arrested act. A word is a substitute for a gesture. A thought is an incipient word. The image or idea or meaning or thought is but a name for the most-reduced of acts, the tentative partial performance which serves as a substitute for the deed in its overt entirety. Meaning is this indicative, this forward or backward referring, significance of such nascent responses. A meaning originally is a signal-fire, a notch cut, a mark made, a line drawn, to direct subsequent action. A monument, a cross, a badge, a label, a tally, a voucher, an autograph, an endorsement, a bill, credentials, insignia, a flag, an escutcheon, a password, a cipher, an epitaph has meaning because it records past or controls future behavior. The gradual reduction of these to the more abstract symbols of grammar, rhetoric, logic, mathematics, methodology is merely an accessory muscular refinement on the more fundamental motor adjustments.

## 12. THE ORGANIZATION OF A WORLD OF VALUES

We shall not have arrived at a comprehensive view of the evolution of behavior without calling attention again to a fact presupposed in all that has been said: namely, that all this elaboration of stimulus and ramification of response is ultimately and always for the sake of bringing fulfilment to certain inherited or acquired propensities. All this development of bilateral symmetry and the metameric form, this cephalization of the sense-organs and magnification of the brain, all the complications presupposed in the building up, through these, of spatial and temporal adjustments, and particularly of a world of incipient responses or symbolizations through the action of the accessory muscles—all this may

be said to be for the sake of finally reaching the distant object and its ingestion or manipulation in connection with food or sex. In other words, there is a final consummation of the means in the ends, of the instruments in the values, of life. This may be called the ultimate equation of a world of methods or means with a world of ideals or ends. In our human sphere it is the culmination of efficiency in culture, of science in art. In terms of the evolutionary process, it is a consummation of the function of the distance-receptors in that of the interoceptors and proprioceptors. The tactile-kinæsthetic imagery is the carrier of the meaning: an object, a situation, a world, seen, heard, smelled, is for the sake of a world touched, manipulated, enjoyed. It is the contact-values which are the goal of the pursuit of the distant object, and all of our economic and social institutions in human society are capable of interpretation from this point of view.



# THE PRINCIPLES OF SERIAL AND COMPLETE RESPONSE AS APPLIED TO LEARNING

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The advantage of studying the behavior of animals in a maze, as a means of arriving at the factors involved in the formation of motor habits, is due largely to the ease with which those movements which are most detrimental to the habit-formation can be observed. Those movements are the ones occurring in the blind alleys; and it is their separation from the movements within the true path which facilitates their observation. Inasmuch as the learning of a maze depends upon the suppression, or elimination, of useless movements with the retention of the successful ones, and inasmuch as useless movements other than the ones into blind alleys, such as hesitations and retracings within the true path, disappear as a rule early in the course of the learning, the main problem of the observer is to account for the suppression of the blind-alley movements.

It is of capital importance, in attempting to solve this problem, whether one holds that the habit of running the maze is made up of a series of stimulations with a corresponding series of responses; or that it consists of a complete response to a complete set of conditions, the selection of the successful movements having been due to "the entire conformation of the organism together with the present more or less stimulating conditions." The former view is held by Dr. Harvey Carr among others, and underlies his exposition of the principles of selection in animal learning;<sup>1</sup> and the latter has been brought forward by Dr. Joseph Peterson in his presentation of completeness of response as an explanation principle in learning.<sup>2</sup> The following quotations, in which

<sup>1</sup> PSYCHOL. REV., 1914, 21, pp. 157-165.

<sup>2</sup> PSYCHOL. REV., 1916, 23, pp. 153-162.

Dr. Carr refers to the problem-box and Dr. Peterson to the maze, will summarize the respective views:

"The animal does not react to this complex situation as a unitary whole, as a single stimulus. He reacts to it selectively, and as a series of stimuli. There is a circular interaction between the sensory stimuli and the animal's movements. Each act modifies the stimulus in some respect, and the change of stimulus in turn modifies the act"<sup>1</sup> (Carr).

"Thus by an actual overlapping of many tendencies to respond in diverse ways the erroneous tendencies are directed into the successful ones, and the latter are strengthened by reinforcement. Without such overlapping of various impulses in the same general response, the inhibiting effects of the successful upon the unsuccessful or irrelevant tendencies are incomprehensible. . . . It is a mistake to look upon these tendencies as separate *acts* each complete in itself and occupying the whole arena for the time being. . . . The selectiveness, then, is due finally to the entire conformation of the organism together with the present more or less stimulating conditions; more immediately it is due to the cumulative effect of various incomplete partial responses"<sup>2</sup> (Peterson).

The serial-stimulation theory is the one generally held by students in this field of research, and most of them probably would assent to the 'circular-interaction' addendum to it. It has been recognized also that there is a less specific response which may be termed the set of the animal, or its attitude, in the maze-situation. This does not necessitate any other assumption than that the instinct of flight, which asserts itself upon the animal's introduction to a new situation, is no longer operative, and that entrance into the maze and obtaining food have become associated in the animal's nervous system. It is not necessary to assume, as Dr. Peterson does, that many of the stimuli which come into play in the course of running the maze are actually present when the animal is introduced into the maze, some of them directly and some of them indirectly by association.<sup>3</sup>

<sup>1</sup> *Op. cit.*, p. 157.

<sup>2</sup> *Op. cit.*, pp. 156, 157.

<sup>3</sup> *Op. cit.*, p. 158.

The article of Dr. Peterson's does not expressly identify completeness of response with the sum-total of responses occurring in the maze. But the absence of any limitation, expressed or—so far as the writer can judge—implied, would indicate that these terms may be taken as synonymous. A similar observation may be made with reference to the term 'general response.' It would consequently seem that, according to this view, the responses which occur in the successful running of the maze are already at hand when the animal enters the maze, in virtue of a complete response to a complete situation; and successive stimulations may be said to call them forth only in the sense that they give them opportunity successively to manifest themselves.

It is undoubted that progress toward the solution of this problem has been made by the analytical method, with its recourse to the principle of a one-to-one relation between the series of stimuli and the series of responses, and to the principles of frequency and recency, even if it has been found that equal stress cannot be laid upon both these latter, nor upon the same one of them at all times. But those who have resorted to it have not claimed for it finality and perfection. In future inquiry, the analytical method may be adhered to, while new factors influencing selection may be sought; or this method may be considered to have yielded its modicum of explanation, and the student may turn to some new principle, whether it be completeness of response or some other; or, still again, investigation may be carried on according to more than one method. It is not the aim of this article to advocate exclusive reliance upon the analytical method, but rather to raise a doubt as to the availability of the principle of complete response to serve either as a substitute for, or as a supplement to, the analytical method.

Some of the difficulties connected with the principle of complete response will become apparent if we examine Dr. Peterson's account of the elimination of a blind alley; and these difficulties obtain whether the expression 'general response' denotes the tendencies pertaining to the maze as a whole, or those occurring only in a part of it. While a

rat is entering and leaving a blind alley, certain elements of the general response, according to this account, are still tending to drain into other alleys that have been recently passed. "Let us suppose that the correct path, *A*, has just been passed when the animal suddenly comes to the end of the cul-de-sac, *B*. The tendencies to respond to *A* are still surviving and now direct the impeded activity into this, the successful, path. If, on the other hand, the correct path had been chosen the first time the distracting impulses toward *B* would have become fainter and fainter as the animal proceeded into *A*, and would finally have faded away. The principle is not different when the complexity of the situation is increased. When the food is finally reached all the remaining delayed reactions, the tendencies, still persisting, to go into other alleys recently passed, are relaxed—the act as a whole is complete."<sup>1</sup>

There are three difficulties. The first arises out of the statement that, when the animal suddenly comes to the end of the blind alley, *B*, it is the tendency to respond to the true path which impels it along that path, the implication being that this tendency draws the animal out of the blind alley before it impels it along the true path. It may be that Dr. Peterson did not intend to deny that any stimulation within the blind alley, such as the animal's butting into the end of it, has any effect in causing its retreat; but the fact that he could express himself so elliptically, leaving out any mention of stimulation within the blind alley, shows what stress he lays upon the conflict of tendencies as compared with the interplay of the organism and the environment. Furthermore, the animal on emerging from the blind alley is differently oriented toward the true path from the way it was before entering. It is not safe to assume that the solicitation of the true path in the later orientation will be as strong as it was in the earlier. Then, too, the movement into and out of the cul-de-sac may exert a modifying influence on the tendency to follow the true path, and there is no certainty that this modification will leave that tendency as strong as it was before the animal's entrance.

<sup>1</sup> *Op. cit.*, pp. 155, 156.

The second difficulty is to make clear how, when the animal is drawn back into the true path, the erroneous tendency is directed into the successful tendency as the result of a collision between them. There is no reference to any effect resulting from stimulation arising from the true path and leading to the orientation of the animal. It was observed by Carr and Watson<sup>1</sup> that rats which had already learned a maze, when placed at random in the maze, moved back and forth a few times, and then started in the right direction. The interpretation which they placed upon this behavior was that the animal had obtained a 'cue,' probably a kinæsthetic one, which directed it aright. It is possible, in the case Dr. Peterson considers, that the animal's retreat from the blind alley and entrance into the true path in a direction contrary to the correct one might result in a confusion, wherein exploration and orientation similar to that described by Carr and Watson might come about.

The third difficulty is that of understanding how, on the grounds alleged, the tendency along the true path, which was not strong enough to keep the animal from entering the blind alley, could gain sufficient strength, once the animal had been in and come out of the blind alley, to prevent its reëntering it. How the coming together of an erroneous tendency and a correct one could produce anything other than a resultant lying somewhere between them, much less result always in the reinforcement of the tendency in the true direction and never in the reinforcement of a tendency in a wrong direction, is not apparent. There is no help in appealing to the 'general response,' or to any redistribution in the general tension brought about by the overlapping of many responses, for it could not be proved that the bare entrance into and exit from a blind alley would produce such a change in this general response as to reinforce the tendency along the true path.

Some experiments performed by the writer, and to be reported in a forthcoming monograph, have a bearing upon the matter under consideration. Groups of rats were given

<sup>1</sup> Carr and Watson, 'Orientation in the White Rat,' *Jour. of Comp. Neur. and Psychol.*, 1908, 18, pp. 29, 44.

two or more trials in one maze, *E*, and then were transferred to another maze, *D*, which they learned to run without an error. After this they were brought back to maze *E*, and allowed to complete the learning of it. The mazes between which the transfers took place were of the same material, the same size, the same color, and were placed side by side, the only difference being that the true paths and the blind alleys, although of the same dimensions in both mazes, were differently arranged.

It was found that some of the rats on their return to the former maze ran it without an error four times in succession, thus meeting the criterion of mastery adopted in the experiment. Of seven rats, whose learning of *E* had been interrupted after two trials in order to learn *D*, three made a perfect score upon their return to *E*, although none of these three rats had made an errorless run upon either of their two former trials. Of seven rats, whose learning had been interrupted for the same purpose after four trials, one made a perfect score upon its return and another made but one error in five trials; and none of these had made an errorless run in any of its four previous trials. Of ten whose learning had been interrupted after eight trials, six made a perfect score and one a single error in five trials, among these being one rat which had made one errorless run and another which had made two errorless runs during their eight former trials. Of eight whose learning was interrupted after sixteen trials, two made a perfect score, one of these having made one errorless run and the other none.

If as a consequence of learning the *D* maze the rats had stored up the tendencies to be used in running that maze, these tendencies overlapping and being elicitable, many if not all, at one time, it is difficult to believe that these tendencies should not display themselves upon the return to the *E* maze, seeing that the elements composing the true paths and the blind alleys of the mazes are similar, and differ only in their arrangement. But they could not thus display themselves without interfering with the learning of the *E* maze. Yet we have seen that many of the rats upon their return traversed the maze without an error.

It is not believable, moreover, that the rats which made perfect scores upon their return to the *E* maze after having learned to run the *D* maze were enabled to run the *D* maze perfectly with the aid of certain abilities and to run the *E* maze perfectly with the aid of other abilities. Whatever would account for their running the *D* maze perfectly would account for their running the *E* maze perfectly; and, since it could not have been a set of tendencies constituting a complete response acquired in *D* which enabled them to run *E* perfectly, it is extremely doubtful whether it was such a set of tendencies which enabled them to so run *D*. Whatever were the abilities enabling the rats to run *E* perfectly after their return from *D*, those abilities were acquired while mastering *D*, for they became immediately effective upon their transfer back to *E*. But obviously those abilities could not consist in a set of tendencies used in running *D*, for they would not fit *E* and if called forth in *E* would only retard the learning. We cannot assume that while learning *D* a double process was going on, one part of which enabled the animal to learn *D* and another part of which was simultaneously preparing it to run *E* perfectly.

The only alternatives, as possible explanations of the general problem of the selection of the successful movements, are not, as Dr. Peterson seems to hold, that of completeness of response and that of a serial response to a succession of stimuli, in which these responses constitute separate acts "each complete in itself and occupying the whole arena for the time being." There is still another alternative: the stimulations may be serial in form, but before the effect of one stimulus is complete the next stimulus may begin to produce its effect. The rejection of the complete-response theory, therefore, does not mean the acceptance of the serial-response theory in the form which Dr. Peterson gives to it. It may be observed in this connection that, if the immediately preceding stimulus may begin to operate before the effects of the present one have ceased, a past response or past responses may also affect the present response through a modification which they have left in the nervous system of the organism,

thus influencing the organism toward one runway rather than another. But the assumption of effects due to the modification of the organism through past responses is a different assumption from that of the presence of a number of overlapping and inter-conflicting tendencies, some urging the organism in one direction and some in another.

While it would not be warrantable to assert that exclusive trust must be placed in the analytical method of dealing with this problem, nevertheless it is the method which has proved helpful thus far, and the only one that in the judgment of some investigators is applicable. There is one more factor to account for the selection of the successful movements, and to which attention should be drawn. It depends upon the fact that, on account of the structure of the maze, a successful movement must be made in its entirety—*i. e.*, through the whole length of the particular runway in which the movement is made,—every time the animal passes through the maze to the end, while an unsuccessful movement need not extend through the whole length of the blind alley.

The analytical method has dealt thus far mainly with the factors making for the selection of the successful movements and only indirectly for suppression of erroneous ones; but the factor to which attention is here drawn acts directly upon the erroneous movements, bringing about a gradual shortening of the distance to which the animal penetrates the blind alley. As was pointed out above, the whole series of stimuli do not necessarily affect the organism one after another with a sharp and rigid separation, but while one is operative the next may come into play. Consequently, if the animal, on its first entrance into a blind alley, received at the end of the alley the stimulus prompting its turning, it is not necessary on its next entrance that it should travel to the same spot before it comes under the influence of this stimulus.

The difference between this condition in the blind alley and a similar condition in the true path is that, no matter at what part of the true path the stimulus to turn into the next runway becomes operative, the animal must reach the end of the runway before giving way to it. This difference is due, as has been



already noted, to the construction and pattern of the maze, there being no physical barrier to the animals' turning within the blind alley, while there is such a barrier in the case of the runway forming part of the true path. The fact, then, that the various successful movements must be made in their entirety, through the whole length of the maze, but that erroneous movements need not be, taken with the further fact that there is a tendency constantly operative and reducing the length of the erroneous movements, as may be observed in the behavior of the animals,—these facts constitute an additional explanatory principle to be included in the analytical account of motor learning, the principle of the completeness of the single successful movement.

The writer hastens to add that this principle is not advanced as an entirely new one. Holmes, for example, says: "In behavior of the trial and error type, success is attained, not by a direct adaptive reaction, but by checking or reversing all reactions except the right one;<sup>1</sup> and Carr says: ". . . acts are selected or eliminated according to whether the sensory consequences tend to facilitate and intensify them on the one hand, or to disrupt and suppress them on the other."<sup>2</sup> Holmes, in the chapter from which the above quotation is taken, refers the advantage enjoyed by the successful movement to its connection with congenital modes of response which are adapted to secure the welfare of the organism; and Carr shows how the result of the final successful movement, which is entrance into the food-box, will ensure that the innervation connected with this movement will not be interrupted, but will reach completion.

It is not the purpose of the present writer to question such statements as the foregoing, the validity of which he accepts. It has been his intention merely to show that, in the case of the maze, inasmuch as the successful movements must be made through the entire length of the runway while the erroneous movements need not be, and there is a constant tendency making for the shortening of the erroneous move-

<sup>1</sup> Holmes, S. J., 'Studies in Animal Behavior,' p. 158.

<sup>2</sup> Carr, H., *op. cit.*, p. 162.

ments, the arcs involved in the successful movements must undergo greater innervation than any of the others. This, as already noted, seems to justify placing the principle of the completeness of the single successful movement on the same footing with frequency and recency as explanatory principles in maze-learning. It seems warrantable, moreover, to assume that this principle holds in all learning by the trial and error method.

# THE INFLUENCE OF EXTRANEOUS CONTROLS IN THE LEARNING PROCESS

BY HARVEY CARR AND HELEN KOCH

*University of Chicago*

This paper attempts a preliminary comparison of the rate of learning when all possibility of error has been eliminated by means of some extraneous control, as opposed to the usual procedure of learning by the trial and error process. The two methods may be termed 'controlled' and 'free or un-directed' learning respectively. The nature of the problem may be more adequately comprehended after a descriptive account of the apparatus and the mode of procedure.

A diagram of the problem box is given in Fig. 1. Its dimensions are  $2\frac{3}{4} \times 5$  ft. It consists of a  $10 \times 13$  in. food box *A*, an initial 3 ft. runway *C*, into which the animals are pushed through the door *B*, which is then closed, and two diverging paths *R* and *L* which finally merge into a common path *D* which in turn leads to the food box by the door *E*. These return paths can be closed as desired by sliding doors at *X* and *Y*.

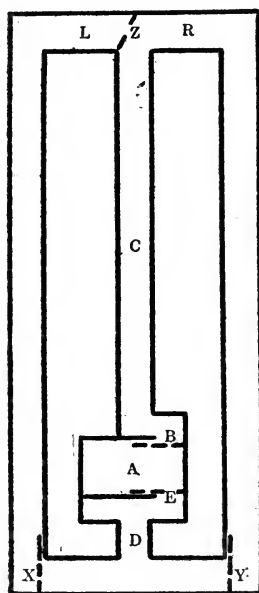


FIG. 1.—Diagram of problem box.

The problem to be mastered was the habit of alternating choices between the two paths *R* and *L* in the definite sequence of *R, L, R, L, R, L* for each day's test of 10 successive runs. In the controlled learning the animals were forced by the swinging door *Z* to choose according to the given sequence by alternately closing the entrance into each of the diverging paths *R* and *L*. But one path was open at a time; the

possibility of choice was eliminated; errors were impossible. The rat was forced to respond correctly in the desired sequence. In the test for free learning the door Z was removed and the rats were confronted with two paths at the end of alley C, of which only one gave access to the food box. One error was thus possible in each trial, and the animals were compelled to learn to eliminate these errors and to choose correctly,—to learn to select the proper sequence out of two possibilities.

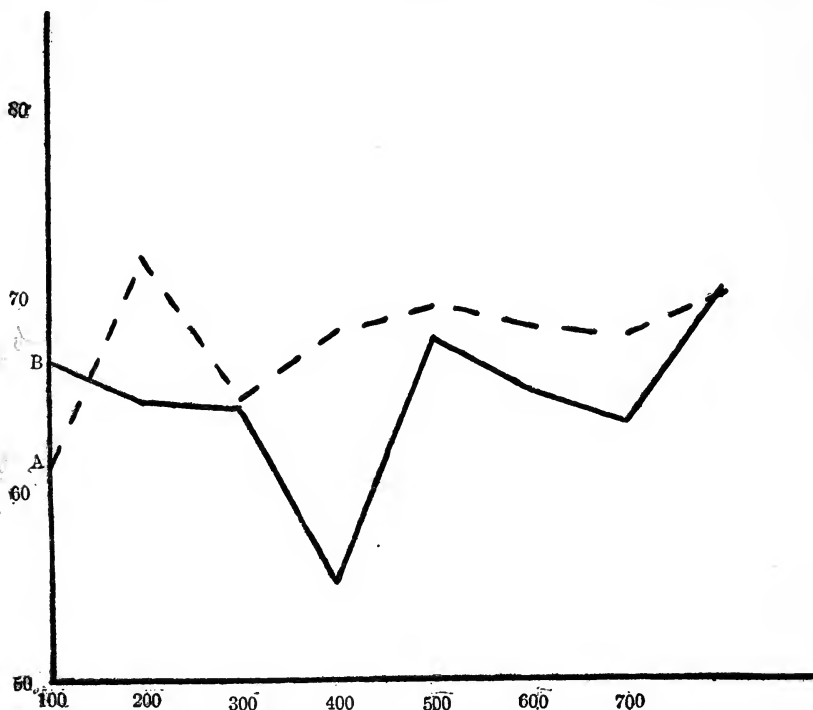


FIG. 2. Graphs representing percentage of correct choices.

The group employed in the free learning consisted of 12 rats, and they were given 10 successive trials daily. The record of this group is represented by graph A, Fig. 2. The values represented are the percentages of successful choices for successive 100 trials. Since but two opportunities of choice are given, it is obvious that the initial record of a group of rats will approximate 50 per cent. if the choices are distributed according to chance. As the problem is mastered the curve will rise from 50 per cent. to 100 per cent.

The controlled group contained 13 rats, and the previous conditions were duplicated in every respect with the exception that the responses were controlled by means of the swinging door Z. With this group training and test series were necessary. In the training series the responses were controlled as previously described. In order to determine the degree of mastery resulting from this mode of training, a test series was interpolated at regular intervals, in which the door Z was removed and the animal allowed to choose between the two paths as in the 'free' procedure. A test series was given every 5th day, or 10 test trials were given after each 40 training trials. The records representing the mastery of the problem with this procedure are thus based upon these test trials alone. The records for this group are represented by graph B, Fig. 2, in which the values are the percentages of correct choices for the 20 test trials given for each successive 100 trials. The two graphs thus represent the relative efficiency of 100 free trials as compared with 80 controlled and 20 free trials.

It is evident from an inspection of these graphs that neither group is likely to master the problem with any high degree of proficiency within 2,000-3,000 trials. Since we wished to study the relative effect of various combinations of free and controlled trials, it was soon apparent that the alternation problem was unsuited to our purpose because of the extended time necessary for its mastery. Consequently we decided to discontinue the experiment in its present form and employ some simpler types of problematical situation like the maze and latch box.

While the group graphs give but little indication of any possibility of ultimate mastery, yet an inspection of the individual records reveals the fact that seven of the 25 rats made considerable progress and gave every indication of finally attaining a high degree of proficiency. Each of these seven animals attained a record of 80 per cent. or better; three made records of 90 per cent. or better. Two other animals started with very high records but gradually decreased their scores throughout the experimentation. The

remaining 16 rats rapidly adjusted themselves to the problem at first but soon fell into a fixed mode of response and made no further progress.

There is but little difference between the records of the two groups, but such as it is favors the group with the free mode of learning. Of the seven animals that attained a high percentage of correct choices and gave evidence of an ability to master the problem, four belonged to the group whose learning was undirected. The values for the group given the free mode of learning are the higher throughout with the exception of the initial value. This initial record constitutes no valid exception to the superiority of this group, however, as this value is based upon the initial 100 trials, while the corresponding value for the group whose learning was controlled is based upon 20 trials after the animals had been subjected to 80 training trials. By computing the initial record of the group with the free mode of learning at a corresponding period in their training, a percentage of 72 is secured. With this correction, graph *A* represents the higher values throughout with the exception of the final value where the two records are approximately identical.

Although the 80 controlled trials were not as effective as a corresponding number of trials where choice was allowed, yet one can not maintain that these controlled runs were wholly without effect. It is hardly probable that the three animals of this group that attained a high degree of mastery of the problem would have been able to do so on the basis of the 160 free trials alone. Moreover the initial record of the group is 66.6 per cent., which is considerably above what would have been secured if these choices had been distributed according to chance. Two of the group made initial records of 90 per cent., and one a perfect record of 100 per cent.; moreover, two of these animals consistently made high scores throughout the experiment. Evidently the initial values for some rats at least have been effectively influenced by the controlled runs of the training series.

Our results indicate that the given proportion and distribution of the controlled trials was effective in mastering

the alternation problem especially during the early stages of the learning process, but that the efficiency of these runs is not as great as that of a corresponding number of free trials. The experiment can not, however, be regarded as a decisive test of the relative values of the two modes of procedure.

For other purposes the experiment with the group given free choices was continued for a total of 1,250 trials for each rat. At this point the group was given controlled and free trials on alternate days for 20 days,—a total of 200 controlled and free trials for each animal. The test was designed to determine the efficacy of an extraneous control in the later stages of mastery. The introduction of these 100 controlled runs produced no noticeable effect upon the course of the group graph. Neither was any effect apparent in the majority of the individual records. The scores of three rats may have been slightly bettered, and those of two slightly lowered. In general those animals that had developed strong position habits were certainly not affected. Those individuals that had developed no fixed scheme of response may have been affected to some extent. The new mode of procedure seemed to alter their normal mode of response, and this induced variation might be either advantageous or detrimental.

The influence of an external control in the learning process constitutes a problem of which 'learning by being put through' is a special case. Thorndike first employed this method and found that it was ineffective. Subsequent experimentation, with a few exceptions, has confirmed his results. In Thorndike's experiment, the animals were relatively passive,—the movements being both initiated and directed by the experimenter. Our conditions differ from his in two respects: our animals initiated their movements, and their activity was controlled by mechanical means. Thorndike concluded that the ineffectiveness of his method was due to the absence of a motor impulse to be associated with the sensory situation. He intimated that learning would have occurred if the animals had actively participated in the reaction and initiated their movements. Our results indicate that the presence of a

motor impulse does not necessarily constitute a very favorable condition of learning.

Our procedure is more nearly identical with certain educational methods of instruction in vogue in teaching such acts of skill as writing and dancing in which the subject is forcefully guided by manual means.

Our problem also raises the theoretical question of the value of errors or mistakes in the learning process,—a question which has never received any extended discussion in the literature on learning. It is usually assumed that errors have a value in an adaptive problem in that they aid the quick discovery of the proper means of solution. The greater the amount of random movements,—the greater the exploration, the sooner will the successful act be discovered. After the solution has been discovered, however, general opinion would probably contend that errors are no longer useful but actually detrimental, inasmuch as they represent tendencies to action which must be slowly eliminated by repeated trials.

Our provisional results do not support the above assumption that errors are invariably detrimental in the process of fixation; rather, they indicate that the process of fixing an association may be hastened by the inhibition of wrong responses. Given any problematical situation consisting of two alternatives, it is possible that an animal may not always react to these paths as two separate objects which have no relation to each other; they *may at times* react to two diverging paths as a unitary situation consisting of two related aspects. In the latter event it is possible that the correct choice is effective in part because the antagonistic tendency was inhibited, as well as because the animal performed the proper act. In such a situation, rejection and selection are relative terms and the process of rejection will emphasize the act of selection. The mere *doing* of a sequence of acts will tend to associate them to some extent, but their fixation will be further facilitated by the process of selection and rejection. The situation may be envisaged more easily in the problem of memorizing where mistakes when noted may be much more effective in establishing the desired association than a con-



siderable number of repetitions. The conception may also explain in part the effectiveness of an active attitude in memorizing; in the passive attitude the subject experiences the items to be associated as they are presented; with the active attitude the subject may either review the syllables previously exposed or attempt to anticipate the coming ones. In either case errors may be made and subsequently noted, and the effectiveness of the method may be due in part to this process of comparison of the proper associates as contrasted with the erroneous suggestions.

In this paper we wish to refrain from any general conclusions. Owing to the indecisiveness of our experiment, the factual results must be accepted with caution. Granted their validity, however, it is possible that an external control may be effective in one type of problem and not in another. The effectiveness of such a control may vary with the degree to which it is used during the learning process. It is also conceivable that an external control may be very effective when utilized only at certain critical stages in the development of a habit system. Miss Koch is now engaged in investigating these questions with the use of the maze and the latch box problem. She also plans to use human subjects with a pencil maze and employ various modes of control including that of verbal directions. This work has progressed to the point where we are able to say that a limited amount of control introduced at a certain stage of the learning is extremely effective in the mastery of the maze problem.

## MULTIPLE CHOICE EXPERIMENT APPLIED TO SCHOOL CHILDREN<sup>1</sup>

BY ELEANOR ROWLAND WEMBRIDGE AND PRISCILLA GABEL

The following test were designed as an application to human beings of the multiple choice method of testing, suggested by experiments which Major Yerkes once tried on pigs, crows and monkeys. In his account of his experiments,<sup>2</sup> he announced his intention of trying them upon human beings at a later date. But in the absence of any published data, we devised a choice experiment which seemed to embody the same principles as those employed in the animal work. Our experiment consisted merely in the choice of certain cards, instead of the choice of food boxes, as in the original experiments. These card-choices increased in difficulty, just as the series increased in difficulty in the Yerkes experiments. After applying the series to 100 children in the public schools, the resulting figures were correlated with the results of Binet intelligence tests. The Pearson coefficient was used.

It will be remembered that the Multiple Choice experiment was devised by Yerkes in order that a series of problems ranging from the simple to the complex might be applied to organisms of different types and conditions and at different stages of development. In the accounts published, Yerkes tried four different choices on three different types of animal. The animal was placed before a series of open mechanisms, and was induced to enter and to learn their relative positions, because he found that food was only in the correctly chosen

<sup>1</sup> From the Reed College Psychological Laboratory.

<sup>2</sup> Yerkes, R. M., and Coburn, C. A. 'A Study of the Behavior of the Pig, *Sus Scrofa*, by the Multiple Choice Method,' *J. of An. Beh.*, 1915, 5, 185-225.

Coburn, C. A., and Yerkes, R. M., 'A Study of the Behavior of the Crow, *Corvus Americanus* Aud., by the Multiple Choice Method,' *ibid.*, 75-114.

Yerkes, R. M., 'The Mental Life of Monkeys and Apes: A Study of Ideational Behavior,' *Beh. Monog.*, 1916, 3, No. 12, pp. 145.

box. He was taught to avoid the wrong choice, because food was not in any of the other boxes. He was entrapped by the door when he entered the wrong mechanism, and therefore unpleasant associations were established with the wrong choice.

The tests used were the following:

1. First mechanism to left of subject
2. Second mechanism to right of subject
3. First mechanism to left and right alternately
4. Middle of group

The animal tests of Yerkes are fully described in his own publications cited above, and will not be discussed further here except to say that the mental development of the three types of animal upon which he experimented was compared by their ability to grasp and remember the relation of the food boxes to each other, so that eventually the right box might always be chosen by its position.

Obviously in giving the same type of tests to human beings, some different variety of technique had to be devised. In the first place, the series had to be much enlarged. To this end, a series of fifteen choices was planned, of which the first four were the same as in the animal series of Yerkes.

Secondly, there was no necessity of rewards and punishments in order to make the subject take the tests, and wish to excel in them. Therefore, the choice was made from relative positions of cards arranged on a table before the subject, from which he was asked to select the 'right one.' His success in discovering which was the right one, and interpreting the choice scheme, was stimulus enough for interest, and for the attempt to do his best. The comparison was made of the number of trials needed to select the right card, rather than of length of time taken in choosing, for the length of time taken seemed to be more a temperamental factor, involving greater or less timidity, greater or less effort expended, etc., rather than difference in ability to perceive relationships. Our first problem was to increase the series of tests, from the four given by Yerkes, to a longer series, and to be certain that they increased gradually in difficulty.

Our second problem was to apply this graded series to enough subjects to make our figures in any way reliable. We devised eleven additional tests, and in order to place them in a series of increasing difficulty, we applied our series of fifteen tests to sixty children in the public schools. A norm was established by giving this list of fifteen tests, and forming a curve of increasing difficulty, as judged by the increased number of failures in solving the various tests.

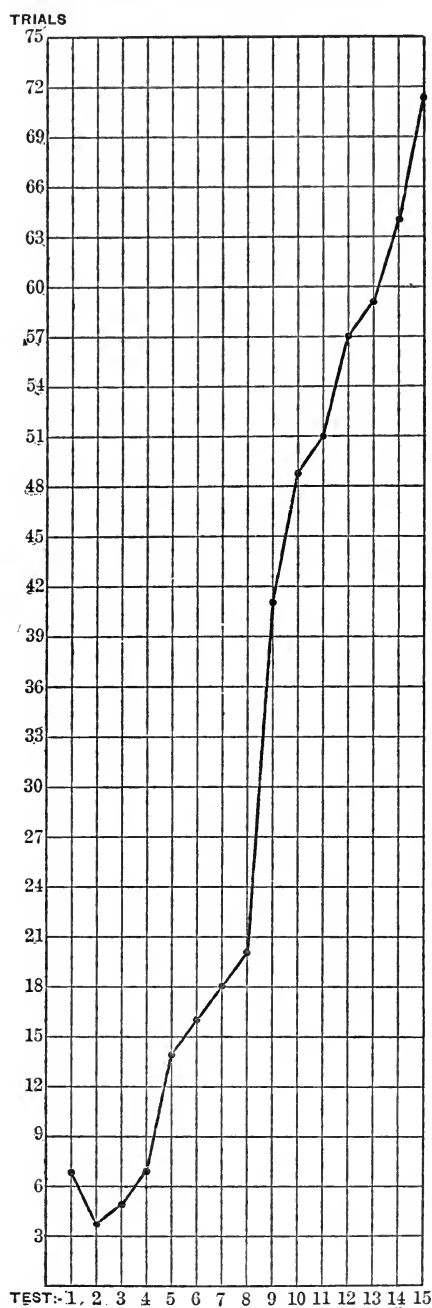
The list that was finally adopted was the following:

- 1-4. Identical with Yerkes tests.
5. Second card from each end alternately.
6. Third card from right of subject.
7. First and third; second and fourth cards from left alternately.
8. Second and fifth cards from left, alternately.
9. First card to right of middle (even number in series).
10. Fourth card from left and third card from right, alternately.
11. Fifth card from right.
12. Fifth card from left.
13. First card to left of middle (odd number in series).
14. Second card from left of the series and the middle card.
15. Third card from right, and fifth from the left; and fifth card from the right, and third from the left alternately.

The relative difficulty of the scale is shown by the curve plotted of the number of failures in each test. The whole scale was given to each child, and failure to solve the test in twenty-five trials was counted as failure.

The tests were given in the following manner. The series of nine cards (corresponding to the nine food boxes) was laid on the table. When the child entered the room, the experimenter addressed him, "I have some puzzles to show you. [Calling the tests 'puzzles' immediately aroused his interest.] Please choose one of these cards." Since the first choice was purely random, and probably wrong, he was asked to choose again, until the correct card was chosen.

The random choices until the card was first correctly



Graph showing relative difficulty of multiple choice tests judged by the number of failures.

chosen, were not counted. When once it had been selected, it was pushed a little above the others, in order to make its relative position more apparent to the subject. Then the series was changed to five cards, fourteen cards, ten cards and so on, and the child was asked to choose again, until he recognized its relation to the series, and chose correctly every time. The series was never longer than 15 cards. When it was certain that the child had solved a certain test, the one next in order was given, and the number of trials necessary to solve any of the tests after the first correct guess, was tallied by the experimenter. If in twenty-five trials the subject failed to solve a test, it was called a failure, and the following test was given. No reward was necessary for children. It was sufficient to tell them that their choice was correct, and their interest was so thorough, that they made every effort in most cases to make a good record.

In figuring the results an average was taken of the number of trials necessary for each subject to solve the fifteen puzzles. Each failure was counted arbitrarily as 30, whether the child actually tried thirty times or not. If it was evident that he could never solve the test, he was not wearied by random trials, but counted as 30. An average was then taken of the combined averages of the children in each age group, with the following result:

Average no. of Trials in				
7-yr. Group	8-yr. Group	9-yr. Group	10-yr. Group	11-yr. Group
18.6	17.0	14.5	14.2	12.4

Each of the hundred children who had taken the Multiple Choice test, was then given the Stanford revision of the Binet tests. The mental ages of the children, according to the Stanford revision, were then correlated with the average number of trials required by each child to solve the series of Multiple-Choice tests. The figure obtained by the Pearson coefficients was  $r = .48$ , which is significant as showing a high correlation between the standard mental test already in use, and this Multiple-Choice card test.

The correspondence seemed however to be least with the children of foreign parentage, who had trouble with the

language problems in the Binet tests. There were too few of these foreign children to make their correlation figure statistically significant from these experiments. We would however suggest that such a card test might prove valuable, as have some of the army psychological tests devised for illiterates, where the language factor makes some of the standard tests impossible to give. This might be tried out further by experimenters who have access to larger numbers of children of foreign extraction than did we.

# PRACTICE EFFECTS IN A TARGET TEST—A COMPARATIVE STUDY OF GROUPS VARYING IN INTELLIGENCE

BY BUFORD JOHNSON

*Bureau of Educational Experiments*

This investigation was made for the study of the comparative practice effects in a motor test given to groups varying in intelligence as measured by standardized scales. It has been generally supposed that those ranking as mental defectives would show less improvement by practice, be less able to profit by experience than those within the normal range. Very few studies have been made to obtain actual facts in given trials.

Ordahl and Ordahl<sup>1</sup> reported on the differences between levels of intelligence in feeble-minded. They took 30 cases of typical feeble-minded persons ranging in chronological age from fifteen to thirty-five, forming three groups of 10 each, with mental age respectively 6 years, 8 years, and 10 years, according to Binet ranking. They found that in learning a series of visual-motor coördinations, there was a marked difference in speed and accuracy for the three groups, ability increasing with mental age. Differences in initial ability were just as distinct, and the inability to establish control for the six year group would invalidate their results in a comparative learning test, though all groups have practice curves that are similar in form to those for normals.

Colvin<sup>2</sup> paired five normal subjects with five subnormal, using the Binet tests as a standard, and gave practice work in cancellation of A's. He reported that the normal child made greater improvement with less fluctuations. Complete data on this have not been available.

<sup>1</sup> Ordahl and Ordahl, 'Qualitative Differences Between Levels of Intelligence in Feeble-minded Children,' *Jour. Psycho-Asthenics, Monog. Sup.* 1., No. 2, p. 33.

<sup>2</sup> Proceedings of the American Psychological Association, *Psychol. Bull.*, 1915, 12, p. 67.



Kuhlman<sup>1</sup> made an early study of an aiming test with 9 subjects—6 imbeciles, 3 morons. He reports “their practice curve goes down after the second week through a decreasing interest in the work and rises again when that interest is artificially aroused. The regularity of their throw increases with practice. They miss the whole target less after a while and also hit the center less after a while than at the beginning.”

Woodrow<sup>2</sup> made an investigation upon practice effects and transference in normal and feeble-minded children, comparing the feeble-minded of average mental age nine, and average chronological age fourteen, with normal children of nine years of age. He found that the feeble-minded children improve with practice the same as normal children of like mental age, and that there is no significant difference in the amount of transference. His most striking conclusion is that there is an absence of correlation between capacity to learn and capacity to grow; that when practice is continued sufficiently long, there will be improvement due to growth rather than practice, and that then the normal children would outstrip the feeble-minded children of the same mental age. This conclusion is presented, however, as indicated by the data, and not established.

The subjects for the present investigation were inmates of the New York State Reformatory for Women at Bedford Hills, New York. They were selected according to mental age as determined by the Stanford Revision of the Binet and the Yerkes-Bridges point scale. Three groups of five each were formed, one group representing the upper mental level of the inmates; another the median reformatory type, who are just across the border-line in the inferior group of normals; a third, the lowest or clearly feeble-minded class. Records of two of the highest group are omitted because of defective vision and failure to obtain glasses for correction. One of the low group, as originally formed, moved to another cottage, and her record for the short period was discarded.

<sup>1</sup> ‘Experimental Studies in Mental Deficiency,’ *Amer. Jour. of Psychol.*, 1904, 15, p. 413.

<sup>2</sup> ‘Practice and Transference in Normal and Feeble-minded Children.’ Part I.: Practice,’ *Jour. Ed. Psychol.*, 1917, 8, 94.

Table I. shows the chronological age together with the mental ranking of each subject.

TABLE I

Group	Individual	Chron. Age			Stan. Rev.		Yerkes-Bridges	
					Men. Age		I. Q.	Score
		Yrs.	Mos.	Days	Yrs.	Mos.		
High	<i>a.</i> .....	18	9	9	15	3	95	92
	<i>b.</i> .....	21	6	9	15	4	96	88
	<i>c.</i> .....	28	5	6	17	2	107	95
Median	Average....	22	10	28	15	11	99.3	91.7
	<i>l.</i> .....	20	11	27	11	10	74	78
	<i>m.</i> .....	16	7	26	10	6	66	67
	<i>n.</i> .....	24	3	13	11	3	70	73
	<i>o.</i> .....	19	5	29	12	9	80	75
	<i>p.</i> .....	16	9	21	11	9	73	75
	Average....	19	7	23.2	11	7.4	72.6	73.6
Low	<i>w.</i> .....	17	0	25	8	9	55	54
	<i>x.</i> .....	21	4	25	10	6	66	61
	<i>y.</i> .....	17	10	27	8	2	51	54
	<i>z.</i> .....	24	4	25	8	2	51	58
	Average....	19	5	24	8	10.8	55.8	54.5

The individual variability and, in some cases, slight improvement raise a question of the significance of this form of test as a type of learning ability. Is ability to throw so nearly a learned process with adults that a few trials in adjusting this ability to a new situation give a relatively final capacity? To make a more intensive study of this test as one of learning ability, the scores of two other subjects, both rated efficient stenographers, are given. These subjects were given more total trials and more throws during a practice period.

#### APPARATUS AND PROCEDURE

The practice given was in an aiming test. A target board in the form of a circular piece of cork composition 2 feet in diameter, was placed in a vertical position upon a wooden base. This base was fastened between two uprights, along which it could be slid so that the target could be adjusted to the height of the subject. On this board three concentric circles were described in black ink, the lines of demarcation being  $\frac{3}{16}$  of an inch wide. The circumference of the circular board formed the fourth circle. The diameters of these circles were respectively 6, 12, 18, and 24 inches.

Bull's-eye in the center was a circle  $1\frac{1}{4}$  inches in diameter made solid black. The darts used were those manufactured by the Apex Dart Company, having a wooden handle  $4\frac{1}{2}$  inches long, and a metal point  $1\frac{1}{4}$  inches long. A length of 2 inches on the other end was feathered. An objection to the darts used was the difference in weight ranging from 2.6 to 2.13 ounces, apothecaries' weight. The only means used to meet this was the selection of paired groups of darts, each pair being approximately the same weight. A series of darts of the weight range mentioned was used, and if the point to one was bent, another of the same approximate weight was substituted. In this manner each subject had the same supply of darts, though the varying weight would doubtless interfere in a fine measure of increasing skill in the test.

Preliminary trials indicate 10 feet as the best distance from the board for adults, and this was adopted. Each subject stood at this distance from the plane of the board, and in such a position that his right arm when stretched horizontally in front of him was on a level with the bull's-eye. Similar position was taken for the left hand throwing. No special position of the feet was required except that one foot must touch the line and must not go over it, nor was there a controlled method of throwing the darts other than that in every case a straight overhand throw of varying trajectories and initial motions was used, and the chief difference observed was the manner of holding the dart. Some grasp it near the point, others much nearer the feathered end. One subject's manner of holding it or the motion given in the hurling, often made the dart turn completely over in the air and then stick into the board. At other times a partial turn was made, and the feathered end hit the board. She showed poor ability, especially with the left hand, and it was noticed that this peculiarity in throwing occurred more frequently with her left hand.

The subjects were brought to the laboratory in groups of five. Only the performer and observer were in the room where the apparatus was set up during the practice work.

This was given daily at the same time of day—between 10 and 12 in the morning—for four weeks. There were a few special cases, when a subject could not come in the morning, and the practice was given in the afternoon. There were necessarily many absences even in such a small group during so long a period. The effort was then made to give each one the same amount of practice, and the intervals between periods were noted. Two girls, X and Z, said that in an amusement park at Coney Island they had several times engaged in throwing at a target with darts like the ones used.

A comparison of the average scores for the first day's practice shows that the median and low groups have approximately the same initial ability, while the high group is superior with the right hand, but not with the left. So many factors enter into the first trial, especially the attitude of those who are rather unstable emotionally, as many of these are, that the results are not valid indications of initial ability.

A control group for each class was planned, and one day's practice given to seven. Varying causes prevented more than four subjects of this group taking the end practice. Since a single trial is not considered representative in a motor test, more practice periods should have been given at the beginning and end for valid measurement with control group. The very small number for end practice does not give sufficient representation in each group for a comparative measure. For these reasons no attempt is made at evaluation of practice effects as compared with a control group.

### QUANTITATIVE RESULTS

Group comparisons based upon the average scores are made. The comparisons of the initial and final records, the first five and the last five trials, initial score and total average score are also made.

*Distribution of Scores.*—Tables II., III., and IV. show the individual scores for each practice period and the average daily score for each group, also the individual averages for all trials. Where a small number is placed, it indicates an interval of that many days between practice periods. The

graphs show for the median group the usual form of learning curve. This group has decidedly less variability than the others. There is a similarity in the form of curves for the low and high groups, with marked fluctuations. The downward slope in fourth week for the low group might be taken as an indication of less interest or need of greater stimulation after so long a period of practice. For the high group this slump even below initial score, occurs earlier. These interferences may be due to the great degree of variability, or

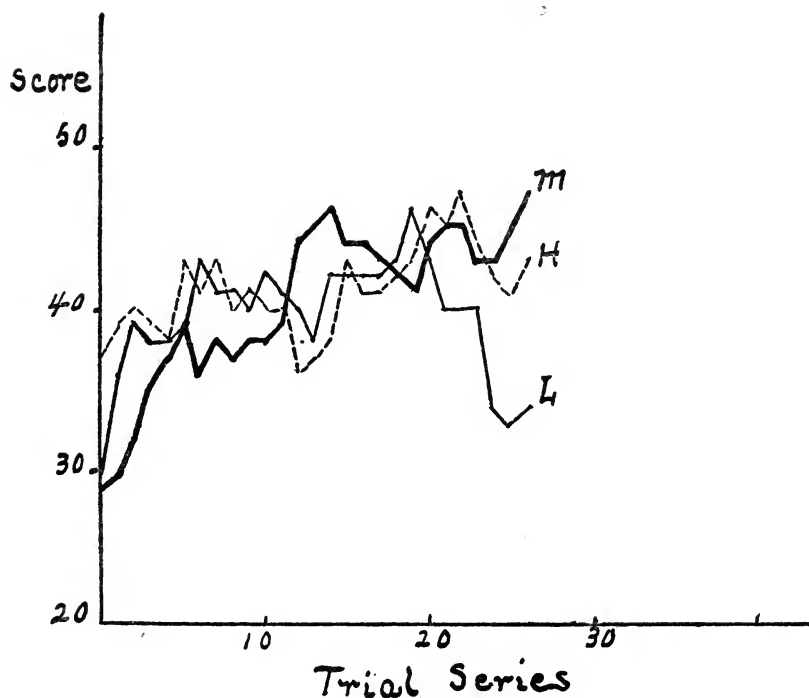


FIG. 1. Group Learning Curves, based on averages of each three successive daily scores made in 10 throws with Right hand. *M* is median, *H* is high, *L* is low.

perhaps to some emotional attitude, a factor to be discussed later.

Individual and group averages for left hand show superiority of right hand. In points gained the median group again leads in comparing first with final trial and first five with last five trials. They also have higher average score

by 1.4 points and are much more closely grouped together. The individuals maintained the same relative ranking as in R.H. scores with two exceptions, one in the low group and one in the median group.

The comparison of the curves for the low and high groups with that of the median indicates other differentiating factors than mental age as explanatory of the differences found.

The three subjects composing the high group show great individual inconsistency. *A*, who was decidedly the best thrower of all who have practiced in this test, making the

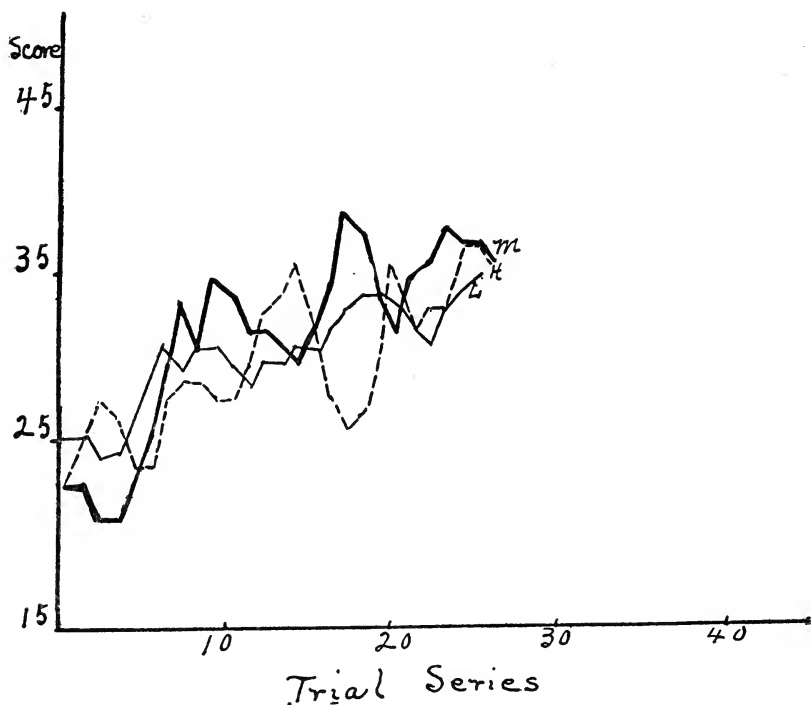


FIG. 2. Group Learning Curves, based on averages of each 3 successive daily scores with left hand. *M* is median, *H* is high, *L* is low.

highest final average and maximum score, was of a very emotional temperament, easily excited and dominated by moods. Her distribution curve of scores, shown in Fig. 3, is dominated by steeples, and her record was an overbalance in her small group. *C*, of highest mental ranking, showed

very poor motor coördination, seemed of the physically apathetic type, never having been interested in athletics or handicraft. Her final average was but a tenth of a point ahead of that of *Y*, the lowest in mental status. The graph for *C* shows the same predominance of marked fluctuations,

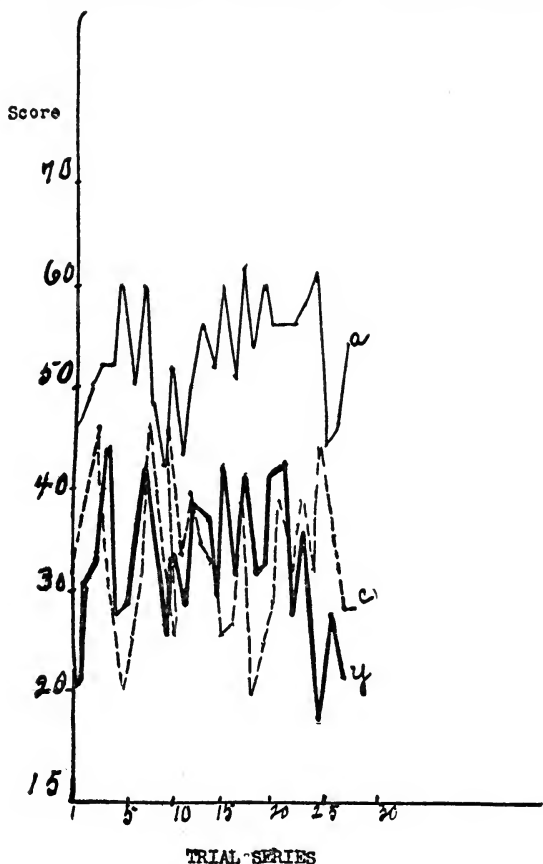


FIG. 3. Individual Learning Curves, based on daily averages of 10 throws with right hand. *A* and *C* belong to high group. *Y* belongs to low group.

but here they seemed to be caused by motor rather than emotional instability. Chance seemed the determining factor. There was little evidence of the development of a muscular memory in coördination with the fixation of the bull's-eye. She could never tell why nor when the dart would make a turn in the air. The arm muscles were held tense and the

attempt made to throw with the shoulder. With Y, of lowest intelligence ranking, the variability and final form of curve seem indicative of a lack of interest. Greater stimulation than the competitive spirit or observation of success would be necessary.

TABLE II  
INDIVIDUAL SCORES OF LOW GROUP

Practice Period	Right Hand				Daily Average	Left Hand				Daily Average
	w	x	y	z		w	x	y	z	
1.	7	46	20	33	27	20	30	16	33	25
2.	30	37	30	52	37	17	36	14	32	25
3	39	39	33	51	41	25	15	21	35	24
4.	21	38	43	40	36	24	17	13	42	24
5.	33	36	27	35	33	28	24	18	25	24
6.	43	41	28	54	42	34	28	36	31	31
7.	42	32	36	48	40	28	27	24	33	28
8.	41	37	42	55	44	30	24	24	44	30
9.	49	33	25	42	37	40	15	24	34	28
10.	35	33	33	54	39	38	9	28	50	31
11.	50	42	28	47	42	34	26	24	39	30
12.	46	40	38	48	43	33	17	22	32	26
13.	44	22	37	28	33	31	21	28	31	28
14.	49	41	29	48	42	32	19	42	33	32
15.	42	22	42	42	37	34	15	15	48	28
16.	54	40	31	47	43	43	9	35	35	30
17.	41	28	41	56	42	25	17	34	51	32
18.	35	25	31	58	37	28	12	27	41	27
19.	46	40	32	62	45	43	25	24	47	34
20.	48	38	41	48	44	35	16	43	40	34
21.	46	44	42	50	46	36	19	27	46	32
22.	40 <sup>1</sup>	29	27	46	36	32 <sup>1</sup>	22	34	40	32
23.	40	22	35	42	35	37	16	26	50	32
24.	54	42	38	52	47	34	19	25	42	30
25.	38	35	17	44	34	31	15	26	41	28
26.	33	29	27	58	37	53	17	38	48	39
27.	34	24	21	48	32	41	19	21	46	32
Total. ....	1,080	935	874	1,288	1,051	886	529	711	1,069	796
Average...	40	34.6	32.4	47.7	38.9	32.8	19.6	26.3	39.6	29.5

$$\sigma = 5.903, \sigma M_1 = 2.9515.$$

$$\sigma = 7.435, \sigma M_1 = 3.7175.$$

The low group included the two who spoke of previous practice, and who were also very alert physically. Psychographs showing their ranking in the entire series of tests given them at the laboratory, showed a marked contrast in ability for motor tests and for other tests primarily classed as mental.

A comparison of initial and final ability may be made from Table V. The median group shows greatest improve-



TABLE III  
INDIVIDUAL SCORES OF MEDIAN GROUP

Practice Period	Right Hand					Daily Average	Left Hand					Daily Average
	<i>l</i>	<i>m</i>	<i>n</i>	<i>o</i>	<i>p</i>		<i>l</i>	<i>m</i>	<i>n</i>	<i>o</i>	<i>p</i>	
1.	33	32	30	22	19	27	22	20	27	24	14	21
2.	27	18	44	30	52	34	9	25	36	21	29	24
3.	27	43	33	12	37	30	14	20	20	26	20	20
4.	31	33	46	25	31	33	22	14	19	15	16	17
5.	36	48	48	32	40	41	26	15	21	23	27	22
6.	38	42	42	35	23	36	35	19	25	31	31	28
7.	45	40	45	33	42	41	30	6	17	38	35	25
8.	38	38	30	25	30	32	37	19	31	48	35	34
9.	42	40	58	39	26	41	35	32	32	58	32	41
10.	38	37	40	41	30	37	32	23	43	42	23	33
11.	47	44	39	33	23	37	39	22	23	41	20	29
12.	43	39	48	46	27	41	33	52	29	44	24	36
13.	39	52	28	32	49	40	30	27	23	39	26	29
14.	39	52	50	47	48	47	29	31	34	22	31	29
15.	52	42	50	43	42	46	52	19	31	37	25	33
16.	47	31	44	42	40	41	37	28	39	6	22	26
17.	41	43	37	50	37	42	35	29	37	32	38	34
18.	50	43	42	50	46	46	48	50	41	39	31	42
19.	46	29	39	38	33	37	41	30	52	39	29	38
20.	39	30	54	46	36	41	40	17	42	38	12	30
21.	52	34	34	48	40	42	42	31	31	18	31	31
22.	50	42	50	37	48	45	34	31	29	18	41	31
23.	40	39	45	48	47	44	28	47	48	33	41	39
24.	38	44	39	44	46	42	34	32	37	23	48	35
25.	48	44	45	34	33	41	30	24	48	32	48	36
26.	40	45	48	44	33	42	22	30	47	41	40	36
27.	55	40	50	48	51	49	35	43	37	23	35	35
Total . . .	1,121	1,064	1,158	1,024	1,009	1,075	871	736	899	851	804	834
Average..	41.5	39.4	42.1	37.9	37.4	39.8	32.3	27.3	33.3	31.5	29.8	30.9

$$\sigma = 1.882, \sigma M_2 = 0.833.$$

$$\sigma = 1.5, \sigma M_2 = 0.6707.$$

ment, a gain of 22 points; the high group making 10 points; while the low group gains only 5 points. The same ranking results from a comparison of the first 5 trials and the last 5 trials. When the initial trial is compared with the final average score, the median group still makes greatest gain, but the low group is a close second. The initial ability of the high group was greater than that of the other two groups, and we should not be surprised at this negative correlation; while the high group attains a slightly higher final score.

For these groups, the members of which are in no sense homogeneous as to initial ability, past similar activities, or temperamental traits, the first five or ten trials do not give a measure valid for group comparisons. The low and high

groups which have the greater variability do maintain the same relative ranking with right hand; but for the left hand the high group does not catch up with the low group until

TABLE IV  
INDIVIDUAL SCORES OF HIGH GROUP

Practice Period	Right Hand				Left Hand			
	<i>a</i>	<i>b</i>	<i>c</i>	Average	<i>a</i>	<i>b</i>	<i>c</i>	Average
1.	46	26	31	34	32	21	14	22
2.	50	33	46	43	19	25	23	22
3.	52	38	30	40	42	23	19	28
4.	52	27	20	33	36	39	23	33
5.	60	39	25	42	8	31	8	16
6.	50	24	31	35	26	16	22	21
7.	60	31	46	46	37	22	33	31
8.	48	43	26	39	32	26	28	29
9.	42	34	46	41	36	21 <sub>1</sub>	17	25
10.	52	33	31	39	50	19	24	31
11.	43	39	39	40	50	21	23	27
12.	50	32	34	39	37	30	10	27
13.	56	26 <sub>1</sub>	32	38	42	21 <sub>1</sub>	28	32
14.	52	20	25 <sub>1</sub>	32	41	38	29 <sub>1</sub>	38
15.	60	40	26	42	46	35	39	30
16.	46	39	38	41	17	29	28	36
17.	62 <sub>1</sub>	48	19	43	50 <sub>1</sub>	27	15	24
18.	54	33	24	37	29	19	4	22
19.	60	29	28	39	32	22	30	28
20.	56	46	38	47	33	16	33	29
21.	56	36	31	41	38	19	24	32
22.	56 <sub>2</sub>	48	38	47	54 <sub>2</sub>	26	36	34
23.	58	43	31	44	39	19	38	28
24.	56	43	43	47	38	27	32	34
25.	44	35	35	38	44	28	40	35
26.	46	44	27	39	36	33	37	40
27.	54	42	37	44	51	36	27	32
Total....	1,421	971	877	1,090	995	689	684	786
Average..	52.63	35.96	32.48	40.37	36.85	25.53	25.33	29.11

$$\sigma = 5.388, \sigma M_3 = 3.11.$$

$$\sigma = 5.385, \sigma M_3 = 3.108.$$

after the first ten trials and only succeeds in equalling it in final average score. *C*, however, was most erratic in left hand performance and weighted the group heavily.

While it seems clear that a higher intelligence level makes for superiority in the target test, it is also evident that those who are ranked low in intelligence by a series of tests and scales and by judgmental ratings make great improvement. The graphs suggest that greater and different stimuli are necessary for such a group.

TABLE V  
GROUP COMPARISONS

	1st Trial	1st 5 Trials	Last 5 Trials	Final Trial	Average Score	Maximum Score
Right Hand:						
Low.....	27	34.8	37	32	38.9	47
Median.....	27	33	43.6	49	39.8	49
High.....	34.3	38.4	42.5	44.3	40.4	47.3
Left Hand:						
Low.....	25	24.4	32.2	32	29.5	39
Median.....	21	20.8	36.2	35	30.9	42
High.....	22	24.2	33.8	32	29.5	38

### INDIVIDUAL LEARNING CURVES

The individual differences and variations in scores, together with the observation of individual attitude and



FIG. 4. Learning Curves of Two Stenographers based on daily average scores of 25 throws with left hand.

technique, suggest the possibility of such a test as valuable for study of temperamental types. To follow this more closely and to test the test as one of learning ability, in the fall of 1917 two subjects were given thirty-four practice periods of twenty-five throws with each hand. This gives

TABLE VI

	<i>G</i>		<i>H</i>	
	Right Hand	Left Hand	Right Hand	Left Hand
1.	45 <sub>1</sub>	67 <sub>1</sub>	68 <sub>2</sub>	37 <sub>2</sub>
2.	* 32 <sub>1</sub>	73 <sub>1</sub>	62	67
3.	61 <sub>1</sub>	56 <sub>1</sub>	100 <sub>1</sub>	52 <sub>1</sub>
4.	111	37	41	72
5.	82	84	61	61
6.	111	78	52	67
7.	107 <sub>1</sub>	70 <sub>1</sub>	58 <sub>1</sub>	79 <sub>1</sub>
8.	118	93	78	68
9.	113	101	61	73
10.	107	88	73	78
11.	146	88	77 <sub>2</sub>	72 <sub>2</sub>
12.	118	112	70	62
13.	105 <sub>1</sub>	105 <sub>1</sub>	58	63
14.	133	119	64	62
15.	127	98	41	60
16.	137	94	57 <sub>2</sub>	52 <sub>2</sub>
17.	135	69	42	77
18.	127	99	30 <sub>4</sub>	65 <sub>4</sub>
19.	119 <sub>1</sub>	96 <sub>1</sub>	49 <sub>1</sub>	69 <sub>1</sub>
20.	137	85	55 <sub>2</sub>	69 <sub>2</sub>
21.	129 <sub>1</sub>	108 <sub>1</sub>	46	48
22.	137	101	80	41
23.	128	116	39	69
24.	127	106	38	77
25.	123	126	56 <sub>2</sub>	73 <sub>2</sub>
26.	127	101	61	79
27.	125 <sub>1</sub>	107 <sub>1</sub>	54	78
28.	125	122	88 <sub>1</sub>	104 <sub>1</sub>
29.	135	107	61 <sub>1</sub>	78 <sub>1</sub>
30.	110	114	69	110
31.	107	99	98	106
32.	135 <sub>1</sub>	141 <sub>1</sub>	101	118
33.	129	117	107	116
34.	117	141	91	77
Total.....	3,925	3,318	2,186	2,479
Average.....	115.4	97.6	64.3	72.9
$\sigma$ .....	22.8	21.46	19.14	18.92

\* Arm lame from taking up new physical exercises.

more than twice as much practice as the other groups had. Aside from the increased number of throws and more irregular intervals between practice periods, the method was the same as in other groups.

Both of these subjects were stenographers. *G* was a college graduate, while *H* had only completed high school course, but had more experience in typewriting, and had been more active in outdoor games. Table VI. shows their daily individual scores. The differences are clearly seen in the learning curves in Figs. 4 and 5. *H* seemed quite eager

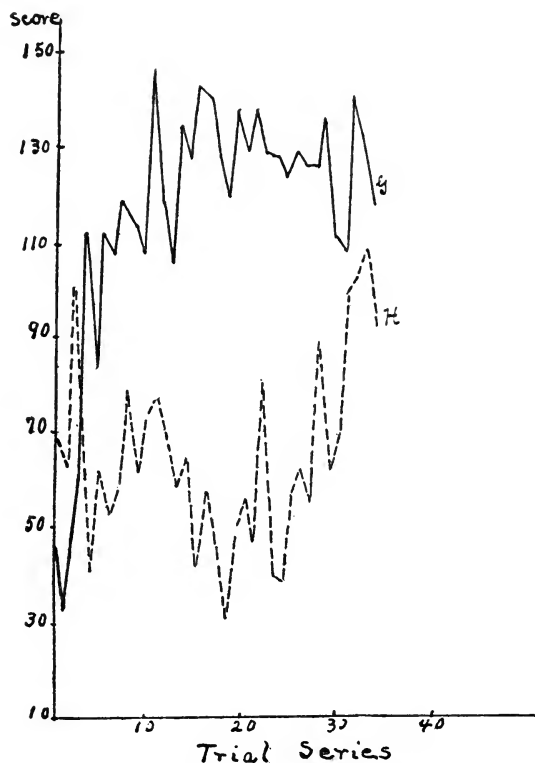


FIG. 5. Learning Curves of Two Stenographers, based on daily average scores of 25 throws with right hand.

to do well, and entered into all such games with a keen, competitive spirit. She knew that *G* was also practicing, but no definite comparisons of their scores were made until the close of the practice work.

*H*'s curve for the right hand shows irregular fluctuations, with marked downward slope in the later practice periods, and lower final capacity than *G*, though initial ability was

superior. The left-hand curve is more nearly normal in type. The curve of *G* is a typical learning curve.

Since there is innate capacity of *H* as evidenced by early trials, such a curve seems explicable only through physiological or temperamental factors. An inquiry was made as to physical well-being at different times during the practice work, and the assurance given that she felt quite well. The daily work as stenographer was maintained at the usual standard. While there is a superficial calm of manner, a marked hesitancy and confusion in speech and in general behavior indicate the highly nervous temperament. Sociological data as to heredity and periods of nervous excitability confirm this.

In a comparative study of the ability of adults and children to learn a maze, Perrin<sup>1</sup> says, "Other personal traits seemed to have influenced the different curves. While we are unable to correlate efficiency in the maze with such temperamental traits or with intelligence in any exact manner, a few general tendencies were noted. The maze taxes the patience and the quick thinker frets about the slow order of things. The individual given to analyzing and theorizing is not necessarily proficient in the maze."

#### METHODS OF ATTACK

Only gross variations in methods of throwing were evident. A slight shift in point of grasping the dart, an almost imperceptible motion given in hurling, the height of arm at moment of letting go, and the force with which the dart was thrown, would certainly cause varying performances. Observations of the methods used and the general attitude of subject, especially the noting of comments made during the practice period and immediately preceding a trial, suggest three general types of behavior. One analyzes the results, sometimes giving a reason for a poor throw, again expressing disappointment and failures to understand the cause. This would usually result in some change in procedure, more often a steadier

<sup>1</sup> "A Comparison of the Factors Involved in the Maze Learning of Human Adults and Children," *Jour. Ex. Psychol.*, 1916, p. 131.

fixating of the bull's-eye and a more accurate motor adjustment. A consequent success was an added stimulus, and this self-competition proved the best form of incentive. Another type, including 3 of the low group, showed a momentary interest in the result. There was an experience of pleasure over a good throw and sometimes one of dismay over a very unsuccessful attempt. Either might result in added force to the next throwing, which was as likely to fall wide of the mark as to hit it, but rarely in a critical appraisalment of the usual technique. A desire to make a better record than some other person seemed the best stimulus for this group, but did not hold from one day to another.

A third class seem intensely interested in achieving success, can scarcely wait for a dart to be withdrawn before throwing, but are overexcited for steady throwing. Failures aggravate the emotional reaction. Self-consciousness as to method and comparative score is a strong factor in their behavior. Members of this class are often of high intelligence, eager to try again, and at various times came back later in a day on which a poor score was made, asking for another trial and stating their belief that they could do better.

An analysis of the single scores during an individual's practice period might show interesting results as to effect of good or poor throws and also might give evidence of a warming-up process or of fatigue effects in later trials. That has not been done with the data yet. These subjects and others who have indulged in several practice periods show a tendency to wish to throw one dart immediately after the other. They give evidence of a physical displeasure over the arrested motion necessary in waiting for the withdrawing of dart by experimenter. There are suggestions of rhythmical reactions on the part of the subject. If the experimenter's reaction time was prolonged, as often happened when the dart was firmly imbedded in target or chanced to stick into the wood, the subject would sometimes throw ahead of time—a danger to be avoided by the observer.

On the other hand, when one throw immediately follows another, a dart will at times stick into the head of another,

again be veered farther away from the center by slight contact with another dart. The question also arises in this case of a segregation of darts forming a larger and more appealing target.

A study of these two forms of procedure and of the sequences of individual throws is being made on other subjects. The effect of an emotional disturbance, more particularly a form of stage fright, is also under observation.

#### SUMMARY

On the basis of group averages, the high group, representative of normal intelligence as compared with society at large, has greatest initial ability and highest final average. The median group ranks second. The low group, or those with marked mental limitations, has the lowest average. With the standard deviation as a criterion of unreliability, these average scores do not vary by significant amounts.

The low and high groups are characterized by wide variability, while the median group is closely grouped about the central tendency. Scarcity of numbers and the selection of individuals varying so widely as to previous conditioning factors and in daily performance, invalidate generalized comparisons based upon the statistical data.

The learning curves for the low and high groups are characterized by marked fluctuations and valleys rather than plateaus. These valleys occur at different stages of practice, indicating a differentiation of incentives for groups of varying levels of intelligence. The median group has the usual form of learning curve.

While the data indicate the effectiveness of superior intelligence in the acquisition of skill in the target test, there is evidence of great capacity to improve in the upper grade mental defective.

Individual learning curves are suggestive of temperamental types. The analysis of methods of attack and variability in performance, differentiates individual traits other than those dependent upon intellectual ability as it is ordinarily regarded.



# PLOTTING EQUATIONS OF THREE VARIABLES IN MENTAL MEASUREMENTS

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The possibility of using a series of curves, common and representative series values of a variable, to represent on ordinary plotting paper the variations of a dependant third variable of a mathematical equation, is not well known. The clearness with which these three-fold relationships can be visualized thereby, and the ease and accuracy with which such charts can be constructed for some of the simpler mathematical equations used by a clinical psychologist, would seem to recommend them to his use.

The two indices, commonly used by the clinician as comparative measures of mental ability, are cases in point. The first of these, the C.I.A. of the Yerkes-Bridges Point Scale, has the equation:

$$\text{C.I.A.} = \frac{\text{Score made by subject}}{\text{Norm for age of subject}} \quad (1)$$

And of the second, the I.Q. of the Terman or Stanford Revision of the Binet Scale:

$$\text{I.Q.} = \frac{\text{Mental age of subject}}{\text{Chronological age}} \cdot \quad (2)$$

Each of these equations is the result of a simple division of variables of the first degree. It is quite possible to represent all the relationships of each of these equations, each made up of three variables, by means of a simple series of straight line curves, thus eliminating the use of clumsy tables, logarithms, and the like. Ordinarily we are interested in plotting only representative values (in this case, mostly used values) of one of the variables. The I.Q.'s, for instance, are never plotted for values of chronological age above 16, nor below 3.

The principle used is that of proportionality of similar

right triangles. Let us refer to the dotted lines of Fig. 1. If on the ordinate-axis we lay off a series of decimal values, 0.00 to 1.00 inclusive, in any convenient scale, and then draw a line through the origin of the axes and intersecting the horizontal line which goes through the ordinate (1.00) at the point,  $a$ , we shall have the line,  $Oa$ . This line,  $Oa$ , serves to indicate the quotients of a whole continuous series of

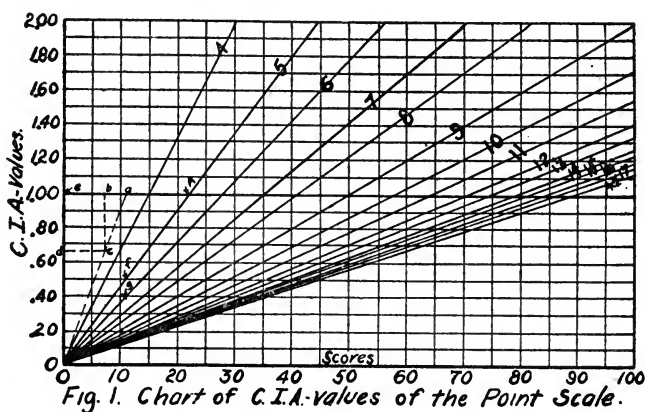


FIG. 1. Chart of C. I. A. values of the Point Scale.

divisions of a variable by a constant quantity. It indicates the quotients obtained by dividing every possible value between  $e$  and  $a$  successively by the quantity,  $ea$ . If we consider any given abscissa value,  $be$ , and from the point where the ordinate line of  $b$  cuts the line,  $Oa$ , project across to the ordinate-axis we find the value,  $Od$ , which is the quotient of  $be/ae$ . The proof of this and the applicability to mental coefficients will now be shown.

Let  $b$  be any point on the horizontal line through the ordinate, 1.00. Then, in the similar triangles,  $aOe$  and  $cOd$ ,  $Od/Oe = cd/ae$ , but since,  $Oe = 1.00$ , and  $cd = be$ ,  
Therefore,

$$Od = be/ae. \quad (3)$$

Or,  $Od$  is the quotient of  $be$  divided by  $ae$ . This holds true for any point,  $b$ , irrespective of its position on the abscissa-axis and also irrespective of the scale in which either ordinates or abscissas are plotted. By extension of the line,  $Oa$ , the

straight line curve serves to indicate quotients larger than unity for values of  $b$  greater than  $be$  as divided by the value,  $ae$ . Multiplying the quotients thus obtained by any constant quantity—say by 100 as is commonly done for these mental coefficients—has no effect on the principle involved. We may *label* the quotients whatever we wish so long as the *series* of labels is obtained from the quotients of the diagram by *multiplication by a constant quantity*.

Applying this to our C.I.A. values: If we let the abscissas represent all possible successive scores on the Point Scale from 0 to 100, and draw our line,  $OA$ , through the point whose abscissa is the norm for the age 5 and ordinate is 1.00, then the line,  $OA$ , will serve to indicate all possible C.I.A. values that might be made by a child five years old for all the scores possible for him to make, within the limits of the chart. Accordingly we label this line a 5-year line. The norm for this age is 22, and a 5-year-old who makes 11 points on this Scale (we see by tracing across the point,  $f$ , where the 11-ordinate cuts the line,  $OA$ ), has a C.I.A. of .50. A 6-year-old who makes the same score, 11, (we see by tracing across from  $g$ ) has a C.I.A. of .39. In similar manner, each age line is drawn through the score (with C.I.A. of 1.00) which is the norm for that age.<sup>1</sup>

The advantages of such a chart are many. It eliminates all computation and secures a distinct saving of time where quotients are needed quickly or where one has many to compute. The scale of the chart may be made large enough to secure any desired degree of accuracy. It can be constructed with the aid merely of pencil and straight edge. If the norms should be changed, the addition of an age line at the proper new norm would allow for the change, a thing not possible with a table of computed values. The interrelations of the coefficients are easily grasped. Ever recurring questions, such as, "What score would it have been necessary for the subject to make in order to have secured such-and-such-a coefficient" are answered by a glance at the

<sup>1</sup> Yerkes, R. M., and Wood, L., 'Methods of Expressing Results of Measurements of Intelligence,' *Jour. of Educ. Psych.*, Vol. VII., No. 10, 1916, pp. 593-606.

chart. We see also why there is so much more variation of C.I.A. values in the lower ages than in the higher ages; for, one point difference in score makes but a difference of little more than one point difference (0.01) in C.I.A. at the age 16, while at the age 4 it makes a difference in C.I.A. of almost 7 points (0.07). This is a great fault of the C.I.A. values. The almost horizontal angle of the higher age curves and the almost vertical angle of the lower age curves make for opposite extremes in their effect upon the C.I.A. values.

A chart constructed in like manner for I.Q. values has already been published.<sup>1</sup> Similar charts used for expressing the quotient of one variable of the first degree divided by another variable also of the first degree, which quotient may then be multiplied by any constant quantity, may as easily be constructed for all such equations.

A further extension of this principle may be made to other simple mathematical equations often used in the psychological clinic. Two examples will suffice to illustrate the application.

Let us consider the Spearman formula for rank correlation,

$$\rho = 1 - \frac{6\sum D^2}{n(n^2 - 1)}. \quad (4)$$

This may also be written,

$$\rho = 1 - \frac{6}{n^3 - n} \cdot \sum D^2. \quad (5)$$

Now,  $\sum D^2$  has a maximum value when there is a perfect inverse correlation (when  $\rho = -1$ ), or when

$$1 - \left( \frac{6}{n^3 - n} \right) \cdot \sum D^2 = -1.$$

That is (solving for  $\sum D^2$ ), when

$$\sum D^2 = \frac{n^3 - n}{3}. \quad (6)$$

The expression (6) is then an expression of the greatest possible deviation for all values of  $n$ .

<sup>1</sup> Toops, H. A., and Pintner, R., "A Chart for the Determination of I. Q. Values," *Journal of Delinquency*, Vol. III, No. 6, 1918, p. 272 + chart.

For any given single value of  $n$ , the expression,  $[6/(n^3 - n)]$  is a constant quantity and with  $\Sigma D^2$  values calculated, equation (5) is a straight line function which may be plotted on the coördinates—ordinates ( $\rho$ ) and abscissas ( $\Sigma D^2$ ). There will, of course, be a straight line curve for each value of  $n$ . In

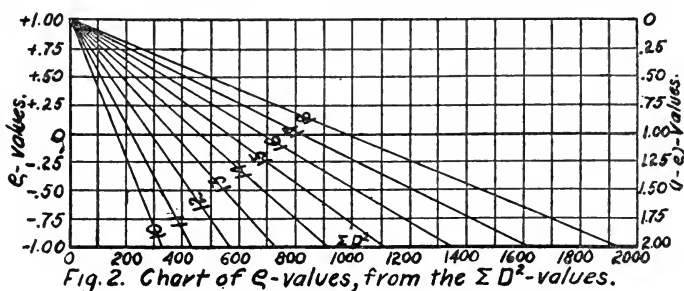


FIG. 2. Chart of  $\rho$ -values, from the  $\Sigma D^2$ -values.

Fig. 2 we have plotted several of these values. Any other curve for any particular value of  $n$  desired may be drawn with a ruler and pencil, taking care only that the line pass through the nodal point ( $\rho = +1.00$ ), and through the maximum  $\Sigma D^2$  on the line ( $\rho = -1$ ), through the  $[(n^3 - n)/3]$

TABLE I.

MAXIMUM  $\Sigma D^2$ -VALUES CORRESPONDING TO VALUES OF  $n$ .

$n$	Max. $\Sigma D^2$	$n$	Max. $\Sigma D^2$	$n$	Max. $\Sigma D^2$	$n$	Max. $\Sigma D^2$
9	240	23	4,048	37	16,872	51	44,200
10	330	24	4,600	38	18,278	52	46,852
11	440	25	5,200	39	19,760	53	49,608
12	572	26	5,850	40	21,320	54	52,470
13	728	27	6,552	41	22,960	55	55,440
14	910	28	7,308	42	24,682	56	58,520
15	1,120	29	8,120	43	26,488	57	61,712
16	1,360	30	8,990	44	28,380	58	65,052
17	1,632	31	9,920	45	30,360	59	68,440
18	1,938	32	10,912	46	32,430	60	71,980
19	2,280	33	11,968	47	34,592	61	75,640
20	2,660	34	13,090	48	36,848	62	79,422
21	3,080	35	14,280	49	39,200	63	83,328
22	3,542	36	15,540	50	41,650	64	87,360

value for the  $n$  considered. The maximum  $\Sigma D^2$  values for plotting the curves with other values of  $n$  are given in Table I.

We often desire to know just what is the effect upon the correlation of an apparently aberrant case. This may be

easily found, within certain definite limits to which it may be confined, from Fig. 2, as may be seen from the following considerations: Equation (5) may also be written,

$$(1 - \rho) = \left( \frac{6}{n^3 - n} \right) \cdot (\Sigma D^2). \quad (7)$$

These  $(1 - \rho)$  values may be used as ordinates with exactly the same set of curves as for equation (5). Any particular deviation in rank,  $h$ , will give to its own particular deviation of the formula a value,  $D^2 = h^2$ . It will, however, affect the other rankings also, as necessarily some of these are displaced by the aberrant ranking. The maximum effect of this displacement is when the lowest rank of ranking  $A$  becomes the highest rank of ranking  $B$ , or *vice versa*. Ordinarily, however, only part of the ranks are displaced by the aberrant rank; if the aberrant rank is shifted *down*, all those displaced measures *above* the aberrant measure are each shifted one point in rank, and *vice versa*. The maximum effect, when one extreme measurement of one ranking becomes the other extreme of the other, is thus represented by the expression,

$$D^2 = h^2 + (n - 1) \cdot (1) = (h^2 + n - 1). \quad (8)$$

On a purely chance proposition, the average value of  $(n - 1)$  of the above equation would be  $(n - 1)/2$ ; for practical purposes with  $n$  very large, the effect of  $(n - 1)$  may be disregarded.

We can best grasp the meaning of equation (8) by assuming that the correlation of any two series of measurements would be perfect if it were not for the deviations. Each deviation serves to reduce the correlation by an amount corresponding to a  $(1 - \rho)$  value for its representation in  $\Sigma D^2$ , which must be *at least*  $(h^2)$  and *not more than*  $(h^2 + n - 1)$ . The  $\rho$ -limits corresponding to these may be found on Fig. 2 by taking the  $(1 - \rho)$  value (righthand ordinates) corresponding to  $(h^2)$  and to  $(h^2 + n - 1)$  taken as  $\Sigma D^2$  values (abscissas). Aside from its theoretical interest, such a chart is especially valuable when one has a number of trial correlations to be calculated by this method.

Another more specialized application of the principle is shown by the equation of the P.E. of the above  $\rho$ -values.

$$\text{P.E.}\rho = (0.706) \cdot \frac{(1 - \rho^2)}{\sqrt{n}}. \quad (9)$$

Assuming that to be valid or acceptable, a correlation,  $\rho$ , must be  $a$  times the P.E. $\rho$ , we may construct a series of line curves to show the number of cases necessary with a given  $\rho$  to secure a greater reliability, or ratio of  $\rho/\text{P.E.}\rho$ .

Our assumed measure of reliability of  $\rho$  is:

$$a = \frac{\rho}{\text{P.E.}\rho}. \quad (10)$$

Then, in order to be valid, the P.E. $\rho$  must be no more than the value,  $\rho/a$ , or

$$\text{P.E.}\rho = \frac{\rho}{a} = (0.706) \cdot \left( \frac{1 - \rho^2}{\sqrt{n}} \right). \quad (11)$$

$$\text{Solving for } n, \rho \sqrt{n} = (0.706) \cdot (a) \cdot (1 - \rho^2)$$

$$n = (0.498) \cdot (a)^2 \cdot [(1 - \rho^2)/\rho]^2. \quad (12)$$

For any particular value of  $a$ , the expression  $(0.498a^2)$  is a constant quantity. Also the expression  $[(1 - \rho^2)/\rho]^2$  may be plotted as abscissas of a simple linear series of values and the appropriate  $\rho$ -values to which they refer (not an arithmetical series when plotted) so labeled. The straight line curves of Fig. 3 are labeled from the  $a$ -value from which they are derived, and not for the actual values used in plotting them. They can easily be plotted from the value of  $n$  when  $[(1 - \rho^2)/\rho]^2$  is taken to be 100 in case of each value of  $a$  taken. Then, by our principle already given, with the straight line through the origin of the axes and the point,

$$\left[ \left( \frac{1 - \rho^2}{\rho} \right)^2 = 100; n = n_{100} \right],$$

the value of  $n_{100/A}$  must be  $1/A \cdot n_{100}$ . Or, taking a concrete example,  $n_{50} = 1/2 \cdot n_{100}$ . The expression  $[(1 - \rho^2)/\rho]^2$  is thus treated always as the independent variable, and as though

it were a simple variable,  $x$ , so far as plotting is concerned. An example will best illustrate the use of Fig. 3.

With a correlation,  $\rho = 0.4$ , we find by tracing across to the left the point,  $h$ , where the  $(\rho = 0.4)$ -ordinate cuts the

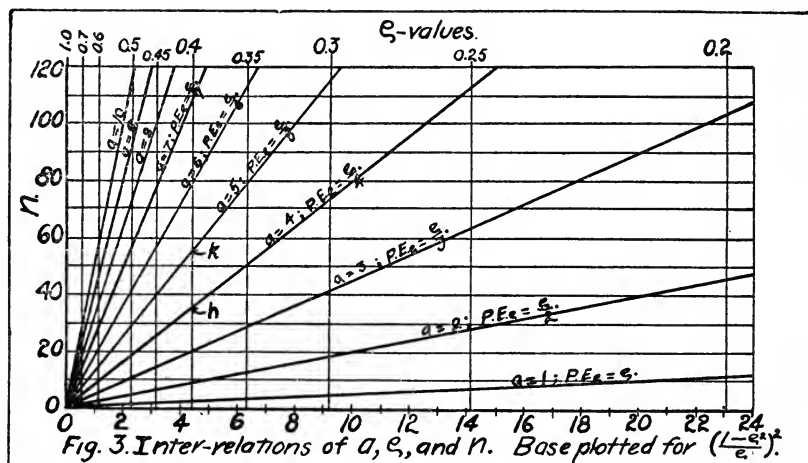


FIG. 3. Interrelations of  $a$ ,  $\rho$ , and  $n$ . Base plotted for  $[(1 - \rho^2)/\rho]^2$ . With a correlation  $\rho = 0.4$ , we find by tracing across to the left the point,  $h$ , that 35 cases are necessary in order that the P. E.  $\rho$  shall be only one fourth as large as  $\rho$ . In order to reduce the ratio so that the P. E.  $\rho$  shall be only  $1/5$  of  $\rho$ , we project across the point,  $k$ , and find that 55 cases will be necessary.

( $a = 4$ )-curve that 35 cases will be needed in order that the P.E. $\rho$  shall be only one fourth as large as  $\rho$ . Since the P.E. $\rho = \rho/a$ , in this case it is  $0.4/4 = 0.1$ . Now, if we wish to know how many cases we need with the same correlation (0.4) to secure an  $a$ -value of 5, we have but to project the point,  $k$ , across to the left, whereupon we find that 55 cases will be necessary. The P.E. $\rho$  here is 0.08.

By this procedure we have reduced an equation of three variables and of the second degree to a chart of straight line curves.

In like manner we may plot any equation of the general formula,  $y = A \cdot Z^v \cdot X^w$ , in which  $A$  is a constant quantity,  $Z^v$  is a variable of any degree of which commonly used and representative curve values are desired,  $X^w$  is either a complicated or simple expression which is to be treated in plotting



as a simple variable,  $x$ . When  $X^w$  is plotted along the lower base line of our plotting paper, we may then plot along the upper base line of the plotting paper as a geometrical variable a series of values (which may be computed for intermediate values), corresponding to the linear or arithmetical series below, the variations of the variable expression (not necessarily  $x$  as shown above) which produces  $X^w$ . The individual curves are then to be labeled from the  $(Z)^1$  value from which they were obtained. In all cases, the variations should be examined to see whether they may profitably be plotted as straight line curves. In general, it is unprofitable if  $w$  is larger than 3, unless only small ranges are taken of the variable expression which produces  $X^w$ ;  $v$  should also preferably be small or unity.

Fig. 4 illustrates a method of plotting ( $y = a \cdot x^2$ ) in such manner that not only common and representative values of

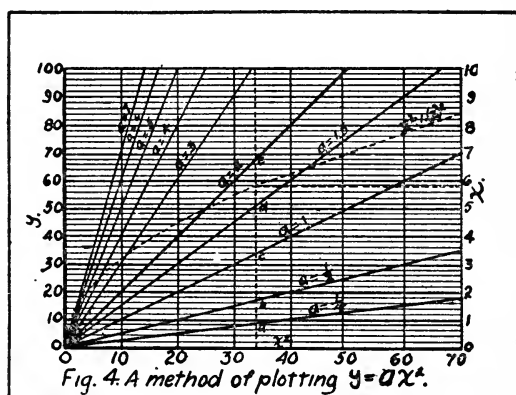


FIG. 4. A method of plotting  $y = ax^2$ . An  $x$ -value of 5.8, on the right-hand scale, traced across to the left until it hits the parabola and then projected downwards to the base gives us an  $x^2$ -value of 33.6. Projected upward until hitting the  $(a = 1/4)$ -line at  $a$  and then projected to the left we secure a  $y$ -value of 18.4. That is, for the equation,  $y = 1/4 x^2$ ; when  $x$  is 5.8, then  $y$  is 18.4.

$x$  may be used but also every possible value. This is secured by plotting the curve of  $x^2 = 1 \cdot (x)^2$  in such manner that the curve represents the square of the  $x$ -values which are plotted on the right-hand border, and thus being able to project the  $x^2$ -values to the base line. From the dotted lines

of the figure, we see that with an  $x$ -value of 5.8 we secure an  $x^2$ -value of 33.6; this ordinate intersects the ( $a = \frac{1}{4}$ ) straight line curve at the point,  $a$ , corresponding to a  $y$ -value, 8.4. This means that for the equation,  $y = \frac{1}{4} x^2$ , when  $x$  is 5.8, the value of  $y$  is 8.4. From the intersection of this ordinate with the other curves at points,  $b$ ,  $c$ ,  $d$ ,  $e$ , we can readily find the value of  $y$  when the value of  $a$  respectively is,  $\frac{1}{2}$ , 1, 1.5, and 2. The accuracy of the plotting of the curve,  $x^2 = 1 \cdot (x)^2$  can always be tested by comparing the  $x^2$ -values determined from it with the value of  $y_{(a-1)}$  determined by the intersection,  $c$ ; the line,  $a = 1$ , being very carefully plotted.

# THE PSYCHOLOGICAL REVIEW

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## THE MIND AND THE MAN-WITHIN

BY A. P. WEISS

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While those psychologists who call themselves behaviorists may consistently refuse to enter controversies about the nature of mind or consciousness, yet if their methods are adopted, it is necessary to show why the distinction between mental and physical has such prominence in the methodology and the fundamental assumptions underlying traditional psychology. In other words if, as would be maintained by a monistic behaviorist, a psychosis is only another form of neurosis, how did the conception "Every psychosis has a neurosis" ever originate among so many able and critical thinkers.

If the basis of all experience is action, then what is the nature of those actions to which men for two thousand years have given the name soul, ego, spirit, self, mind, consciousness, ideas, volition, awareness, etc. Is there such a thing as a dualistic prepossession and if so how did it originate? It seems reasonable to suppose that so thorough going a differentiation between the mental and the physical, should have a structural or functional basis in the human body. Such a basis the writer believes exists in the fact that *the nervous system of man is not supplied with sense organs or receptors for which the adequate stimulus is either a nervous impulse, or the neural changes induced in the nervous elements by the passage of a nervous excitation.* This makes it impossible for man to react as discriminatively to neural changes in the brain, spinal cord and nerve centers, as to changes in his environment; that is to say, man knows more about what is

going on outside of his body than within it. Therefore, in order to meet the practical requirements of adjustment to other persons, he personifies his own behavior mechanism.

However, before it is possible to show how this structural or anatomical limitation gave rise to the mind-body problem as found in traditional psychology, it is necessary to consider the behavioristic basis underlying the concepts of the natural and supernatural.

*Origin of the Natural-supernatural Prepossession.*—This prepossession, the writer believes, can be traced to the fact that *when an organism is acted upon by a variety of intense stimuli for which there is no adequate inherited or acquired behavior, the organism reacts in the manner that is appropriate in the most similar familiar situation.*

For a concrete illustration imagine a primitive man caught in a terrific storm.<sup>1</sup> The stimuli acting on him are so intense and varied that no one course of action is long continued. The blinding flashes of lightning, the crashing of trees, the howling of the wind,—for none of these does he have a specialized response. An event for which there is a more or less adequate form of behavior, and which resembles the storm conditions, is perhaps the occasion when primitive man expects to be overwhelmed by a powerful enemy. He cannot, however, fight the storm as he would his enemies because of the non-localizable character of the storm stimuli. Since the storm offers nothing to attack, he begs for mercy. The act of begging for mercy is the appropriate reaction when fighting the enemy is no longer possible.

However, in the case of the storm there is no person or group of persons toward whom he can direct his supplication. Primitive man is apparently begging for mercy even though the usual stimulus (enemy) is absent. In other words, he is reacting to a physical event as if it were a person; he has

<sup>1</sup> Some readers will wonder why the writer did not use the phenomena of death or dreams as illustrative material. In some respects these phenomena show the personifying process better than the storm situation that has been selected. The writer believes, however, that the dualism arising out of death or dreams represents a more advanced stage in the process of personification and should be regarded as corroborating rather than initiating the process.

personified the storm. It is not necessary to assume that this is the only form of behavior that could develop. Many other ways of reacting to the storm situation probably did develop, but personifying the storm has such decided advantages over other forms of behavior that it gradually displaced them. The process of personification may be regarded as developing through two stages:

1. When there is no adequate reaction to an impelling situation (storms, etc.) primitive man reacts in a manner that is appropriate (begging for mercy, etc.) in a more familiar situation (victorious enemy).

2. There is a gradual modification of the vicarious form of behavior so developed, into appropriate reactions to imaginary persons having attributes similar to the natural events so personified. Thus the storm god becomes a blustery, noisy individual. The god of winter is cold and cruel.

This process of personifying impelling natural events is to be regarded as a gradual evolution out of which the personifying reactions survive. It is not necessary to suppose that these reactions are the initial forms of behavior. When, however, a personifying reaction once occurs it soon displaces other forms of behavior because of the following advantages:

1. The individual reacts in the most resourceful manner consistent with his native and acquired endowments, because reactions to persons represent the most complex adjustments that primitive man must make.

2. The individual is given a definite plan according to which an analysis of the perplexing situation is immediately possible.

While the analysis of the storm into its apparent moods, passions, idiosyncrasies, does not at first seem valuable, yet when the alternatives are either doing this or giving way to baffled inaction, it is easy to see how an anthropomorphic analysis may lead to reactions that further effective adjustment. A simple rule for the guidance of primitive behavior might read as follows: When in doubt as to what to do, react to the situation as if it were a person; this will not only tell you what to do, but you will also be able to separate the situation into its important and unimportant parts.

It is well to point out here that this rule is only an unverified theory; a theory that gets its almost universal validity solely from the fact that it 'works.' The theory really has no factual foundation in the sense that the hidden-man is actually ever perceived. This logical inconsistency probably had very little effect in retarding its acceptance. It was the *practical* results that determined its adoption. Pragmat

It should not seem strange that a theory which actually has no factual basis should be adopted so generally. As social interaction becomes more complex many of the individual's reactions are directed to *absent* persons such as friends, enemies at a distance, chiefs or sovereigns who are actually seldom seen. It is therefore relatively easy to build up a set of adjustments to persons that are never seen or rather that are continually absent. Since the situations out of which the react-to-the-hidden-man theory arose were usually impelling or critical, the wishes and desires of the hidden-man as mediated through priests or medicine men carried with them the stamp of authority. The imperative character of the reactions to the hidden-man, their degree of permanence and constancy, the fact that the hidden-man was actually never perceived, were so different from the ordinary social reactions to other persons, or the food and shelter activities of ordinary life, that the distinction between natural and supernatural was finally made.

*The Hidden-Man Theory is Displaced by Natural Science.*—Even though the react-to-the-hidden-man theory could not be verified by observation, it was nevertheless effective in initiating the analysis of critical situations. This resulted in discriminative reactions which separated the essential from the non-essential. Gradually its range of application was extended to include not only critical and impelling situations but all the conditions under which man lived. Out of this, some system of religion gradually arose. While the hidden-man was never located, yet in hunting for him society learned many new ways of overcoming the hardships of nature. So valuable did some of these discriminations prove to be that some of the investigators of the attributes of the hidden-man

forgot the object of their search. It was found that many interesting and valuable forms of behavior could be developed without the hidden-man theory; indeed, in some instances the theory was even a hindrance. In this way the hidden-man theory became the instrument of its own elimination.

Finer and finer analyses were made and a methodology gradually arose in which the hidden-man theory was discarded and the emphasis placed on the classification and systematization of the discriminations that could actually be made with the sense organs. Then, instruments were devised by which the range and acuity of the sense organs were greatly increased. Formulæ and propositions were invented by the aid of which many discriminations could be predicted. Much of this now makes up what we call the body of natural science. In other words, as science develops, the gods disappear and they disappear first from those events in which the discriminations are most easily made.

There was at first considerable opposition from those in authority against discarding the hidden-man theory; indeed it is still the prevailing theory underlying human behavior, but by judiciously accepting the hidden-man theory at such times when it does not interfere with his investigations, the scientist is now left unmolested.

*The Hidden-Man becomes the Man-within.*—Where the nature of the investigation is such that the discriminations are not so dependent on the range and acuity of the senses, where the subject matter is hidden or obscure, or where it is dependent on records compiled by others as in the social sciences and psychology, the hidden-man theory is not so easily eliminated. A significant modification does however take place. The hidden-man becomes the man-within. The same principles that produced the hidden-man theory also produced the man-within theory of human behavior or conduct.

When man attempts to study human behavior in the same way that the geologist studies the earth, he immediately finds that one great section of his investigation is beyond the reach of his sense organs. He may discriminate to the finest detail

the nature of the stimulus; he may measure the reaction to the last degree of precision, but he cannot trace out the *path* of the nervous excitation over the nervous system which alone determines what the character of the reaction will be. The psychologist's inability to react to changes that take place in the nervous system of man is a problem of the same sort as that which confronted primitive man when he tried to react to the strange and mysterious forces of the storm. Primitive man solved his problem by personifying natural phenomena. The early student of human behavior solved his problem by personifying neural function.

For the early psychologist the man-within who directed the distribution of the nervous impulses was called the soul or ego. But the closest anatomical scrutiny failed to reveal either the soul or its administrative headquarters. It was therefore concluded by some that the soul or the man-within did not live in the body but merely escorted the nervous impulses, without influencing their distribution. By others it was concluded that while the soul did influence the distribution of the nervous impulses, it was impossible to say how it was done. A perplexing situation arose. Some psychologists invented new terms to take the place of the word soul. Some of these terms are volition, conative tendency, psychic force, vital impulsion, will to live, cosmic urge, subconscious, censor, libido, etc. Others decided that while there was not a really truly man-within, there was something else that functioned in the same way. This substitute for the man-within they called awareness, consciousness, mind, experience, meaning, intellect, mental function, satisfaction and annoyance, '*unanschaulichen Bewusstseinsinhalt*.'

Even this late modification of the man-within theory was of little help to those psychologists who were primarily concerned with human conduct. No explanation or description of the precise manner in which mind or consciousness or mental function influenced behavior was offered. The insistent demands proved annoying and to escape it some psychologists lost interest in human behavior and devoted themselves to a study of mind or 'consciousness as such.'



A new group of psychologists arose who decided to throw out the hidden-man and the man-within theories altogether. These psychologists maintain that if the nervous system, while in function, could be scrutinized as closely as the events occurring outside man's body, the need for a soul, or mind or consciousness, would be no more necessary than the hidden-man theory is necessary to account for the storm. Just as the hidden-man theory arose from the inadequate discrimination of the elements which made up natural events, so the man-within theory or the mind-body problem arose because man has no sense organs located in the nervous system by means of which the most important element in behavior, the nervous impulse, can be scrutinized and localized. That condition which is called awareness and which has always been surrounded by a halo of mystery, merely indicates that neural activity may take place and yet the individual may not be able to detect any bodily movements, or react discriminatively to the neural activity. That is to say, awareness means only that there is no detectable overt action, but nevertheless there is neural function which, if it could be observed, would have a specific configuration. These psychologists who are often called behaviorists further believe that the study of human behavior along the lines laid down by the natural sciences will not restrict it to a few mechanical reflexes but will in time include all activity even that of the introspecting psychologist or the philosophizing philosopher.

*Summary.*—The origin of the dualistic prepossession in traditional psychology that expresses itself as the mind-body problem, may be regarded as arising from the tendency of man to personify events for which no adequate form of behavior has been established. To react to a perplexing and impelling situation as if it were a person, releases the most effective inherited and acquired reactions and also furnishes a plan of analysis according to which the situation may be separated into important and unimportant parts. As the progressive perfection in the instruments of analysis fail to reveal the 'hidden-man' the tendency to personify gradually disappears.

Human behavior is determined by neural function but because there are no sense organs in the nervous system for which the adequate stimulus is neural function, it is impossible to discriminate the conditions which show the relation between the environment and behavior. To overcome this limitation in the study of human behavior, traditional psychology has personified neural function. The personification has passed through successive stages, from the frankly confessed personal soul or spirit, to the more restricted use of the term ego or self and finally to the guarded use of the conceptions mind or consciousness.

## LENGTH OF TIME INTERVAL IN SUCCESSIVE ASSOCIATION

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This paper presents data obtained by the use of animals in the alternation problem. Two diverging paths *R* and *L* were presented and the animals were required to learn to choose between them in the given temporal sequence of *R, L, R, L, R, L*, etc., for each day's series of trials. The mastery of such a sequence of choices involves the formation of an association between each of the two responses and a certain stimulus. Naturally the two stimuli must alternate in the given sequence. Conditions were arranged so that presumably the animal would be compelled to utilize for each choice the somatic (cutaneous, kinæsthetic, and organic) stimuli resulting from the preceding run in order to master the problem. The problem thus involves the formation of an association between two successively presented terms, each response and the sensory resultants of the preceding response. The resulting coördination may be regarded as somewhat similar to the act of walking, in which the sensations from each step constitute in part the stimulus for the succeeding step. The time allowed for eating between runs was varied for the different groups of animals. In this way the length of the interval between the two terms to be associated was varied, and our problem may be stated as the influence of the length of the time interval between two successively presented terms upon the speed of their association.

A plan of the problem box employed in the experiment is represented in Figure 1. Its dimensions are  $2\frac{3}{4} \times 5$  ft., with  $4 \times 4$  in. runways and a  $10 \times 13$  in. food box. With the exception of the food box *A*, the apparatus was covered with a glass top. An initial runway *C* opens from the food box by the opening *B* which can be closed by a sliding door. At

the end of this runway are two diverging paths *R* and *L* which finally merge into a common path *D* which in turn leads to the food box by the door *E*. These return paths to the food can be blocked as desired by the sliding doors *X* and *Y*.

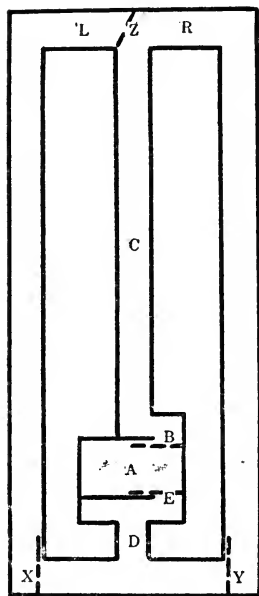


FIGURE 1. Problem Box.

The rats were required to master the habit of alternating their choices between the two diverging paths in the definite sequence of *R*, *L*, *R*, *L*, *R*, *L*, etc., for each day's series of trials. The animal was placed in the food box and forced through the door *B* which was then closed. In the first trial of each day's series of runs, the doors *X* and *Y* were arranged so that food could be obtained only by means of path *R*. In the succeeding trial the doors were rearranged so that path *L* was open and *R* closed. Between the successive runs for a day's series, the rats were allowed to eat for a definite time interval. This interval was measured by means of a metronome adjusted to ring a bell every

five seconds. The progress of the animals in mastering the association was measured by the percentage of successful choices in each successive 100 trials. Naturally the initial records will approximate 50 per cent., and the values will increase to 100 per cent. as the problem is mastered.

The sensory basis of the two responses must consist of two different stimuli which alternate in the given sequence. There are several stimuli which fulfill these conditions:

1. The responses may be based upon stimuli involved in the adjustment of the doors *X* and *Y*. This possibility was eliminated by control tests in which the adjustments were made after the animals had made their choice, or in which both doors were left open for a number of consecutive trials. The utilization of such stimuli would not, however, invalidate any conclusions concerning the relation of the time interval

to the rate of learning. It was the general practice throughout the experiment to adjust these doors immediately after the animal had entered the food box. As a consequence the two terms to be associated would be separated by the interval of time allowed for eating between runs.

2. The animals may have been guided by two different modes of handling while being inserted through the door *B*. To avoid this possibility the manner of handling and placing the rats in the initial runway was varied during the mastery of the problem and in control tests at the termination of the experiment.

3. The rats may have responded visually to two distinctive attitudes assumed by the experimenter while they were traversing runway *C*. It was soon observed that the experimenter naturally tended to act differently as the sequence of correct choices was alternated. The experimenter attempted to inhibit this tendency and maintain a constant position and attitude for all trials until the choice had been effected. The writer attempted to influence the choices of the rats by such means in one test. The orientation of the animal in relation to the experimenter while traversing the initial runway is such that the utilization of visual stimuli from the experimenter would presumably be difficult. The possibility was effectually eliminated in one experiment by a control test in which the initial runway was entirely covered so that visual stimuli from the experimenter were excluded until after the animals had entered one of the return paths.

4. Smell is also a possibility. One may assume that the odor left from the last run is stronger than that resulting from the run preceding it. Choice would thus be based upon a difference of olfactory strength of the two trails. Mastery of the problem would involve the selection of the weaker of the two olfactory stimuli. The pathways were thoroughly washed every two or three weeks during the course of the experimentation and this procedure disturbed in no way the ability to make successful choices on the succeeding day. If olfactory stimuli were utilized, choice would presumably

be impossible until the animal had reached the point of divergence at the end of alley *C*. As a matter of fact the selection of a path in the majority of cases had been determined before this point was reached. This fact was quite evident from the behavior of the animals and the phenomenon was noted and commented upon by several observers. Before reaching the end of the initial pathway, the animals generally manifested a bodily attitude adaptive and preparatory to the turn involved in entering the selected return path. In case of a leftward turn, the animals veered over to the left side of the alley with the body twisted and curved in that direction. It was quite evident that the whole organism was being adjusted for that sort of a turn, and in the majority of cases in the later stages of the experiment the observer could easily predict the path to be selected when the animal was from one to two feet away. A few rats at certain stages in the mastery of the problem apparently did not make their choice until the two diverging paths were reached; they ran the initial path in a desultory fashion and then exhibited hesitation and wavering between the two possibilities before a final selection was made. This type of behavior was the exception however.

5. Another possibility was suggested by Mr. E. S. Robinson. The hypothesis assumes that the rat first develops a tendency to repeat the previous choice. The effect of each run is retained during the interval and the tendency is awakened by the stimulus from the apparatus when the rat is inserted into the entrance alley. Such a tendency toward repetition is involved in the formation of position habits. Under our conditions this tendency to repeat the previous act will naturally result in an error or wrong choice. The rat now learns to inhibit this wrong response and choose the opposite path. The rat starts to turn in the wrong direction and this sensory attitude becomes the stimulus to the correct choice. On this hypothesis the production and elimination of errors are necessary stages in the mastery of the problem. Furthermore the two associated factors, the correct response and the somatic stimuli involved in the

tendency to enter the wrong path, are presented in immediate succession. The behavior described in the previous section would seem to eliminate this possibility. The assumed process of gradually inhibiting the wrong act and substituting the correct response in its place was not observed. Neither did the animals when placed in the initial runway first manifest an attitude antagonistic to their subsequent choice and then reverse it. The only observable attitude was one adapted to the succeeding choice.

6. The animal may assume and maintain two distinctive orientations in the food box during the interval allowed for eating. We were unable to observe such attitudes, however. On the contrary, each rat assumed a characteristic attitude which was constant for the majority of the trials. The construction of the apparatus minimizes this possibility inasmuch as both return routes finally merge into a common path and the animals are compelled to approach the food in an identical manner irrespective of which return route was chosen.

7. The remaining factor is the somatic sensitivity involved in the preceding run. The two responses necessarily entail two distinctive sets of somatic stimuli, and each of these may well constitute the basis of the subsequent choice. The fact that choice was apparently determined by an organic set manifested while the rat was in the initial runway is readily explicable on this hypothesis. Proof of the efficacy of this factor is primarily a matter of exclusion, the elimination of all other possibilities.

1. The first experiment was conducted by a graduate student, Miss Louisa Lewis. Three groups of eight rats each were employed, but several were eliminated by various causes during the experimentation. Complete records were obtained from five rats of group *A*, three of group *B* and seven of group *C*. The time intervals for groups *A*, *B*, and *C* were 15, 25, and 35 seconds respectively. Similar conditions obtained for the three groups throughout. Twenty trials per day were given each animal with occasional exceptions when the condition of the animals did not warrant that number.

The animals were tested daily with the exception of Sunday. The tests were omitted for a period of ten days at the end of 1,300 trials. To facilitate the progress of the experiment, two animals were generally tested simultaneously; one rat was allowed to run while the other was engaged in eating during the specified time interval. The experiment was continued until each rat attained a record of 90 per cent. of correct choices for 100 trials.

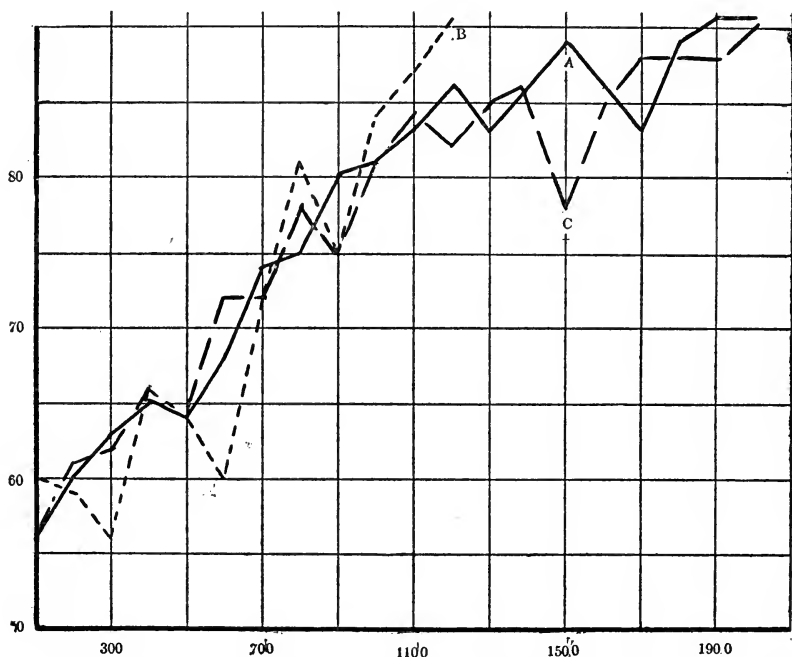


FIGURE 2. The graphs *A*, *B*, and *C* represent the average percentage of correct choices for successive 100 trials for the intervals of 15, 25, and 35 seconds respectively.

Graphs *A*, *B* and *C* of Figure 2 represent the records of the three groups. The values represented are the average number of correct choices per group for each successive 100 trials. All animals succeeded in attaining a record of 90 per cent. of correct choices for 100 trials. The three groups were subjected to the various control tests enumerated above with the exception of that for visual cues from the experimenter while traversing the initial path.



Several significant features of the results deserve comment: (1) All curves start slightly above the 50 per cent. level. The animals make some progress toward a mastery of the problem during the initial 100 trials. (2) Curves *A* and *C* exhibit no significant differences. The rate of learning is the same for the two intervals of 15 and 35 seconds. (3) All three curves are practically identical for the initial 1,000 trials. Evidently the rate of progress for the initial stages of learning does not depend upon the time intervals employed. (4) Curve *B* rises quite rapidly from the 1000th to the 1200th trial. This fact is the only evidence that the rate of learning may be a function of the length of interval. Other modes of explanation are possible. Unfortunately this group contained but three rats at the end of the experiment. The progress of any animal in this problem is highly erratic; a high record may be made consistently for a number of days, and this period may be followed by one in which extremely poor records are achieved. This sudden rise of the curve may be a chance spurt and the group might have been unable to maintain this level of excellence if the tests had been continued. The experiment should have been continued. Chance might still be a factor even if the group had been able to duplicate their records in succeeding trials. Individual success in such a problem is quite dependent upon chance factors. The experiment can hardly be regarded as a decisive test of the influence of the time interval upon the rate of learning. The evidence favors the assumption that the differences employed in the experiment were without effect.

2. A second experiment was conducted by the writer. Groups *A*, *B*, and *C*, each containing 6 rats, were tested for intervals of 5, 10 and 15 seconds respectively. Several conditions differed from those of the preceding experiment. The animals had had previous experience in a maze problem. Ten trials per day were given each animal for seven days of the week. But one rat was tested at a time. Owing to sickness no tests were given for a week at the end of the 900th trial. All control tests previously enumerated were given at the end of the experiment.

The group records are represented by the graphs of Figure 3. Again the initial values are greater than 50 per cent. From an inspection of the curves it is evident that no group probably would have attained a proficiency of 90 per cent.

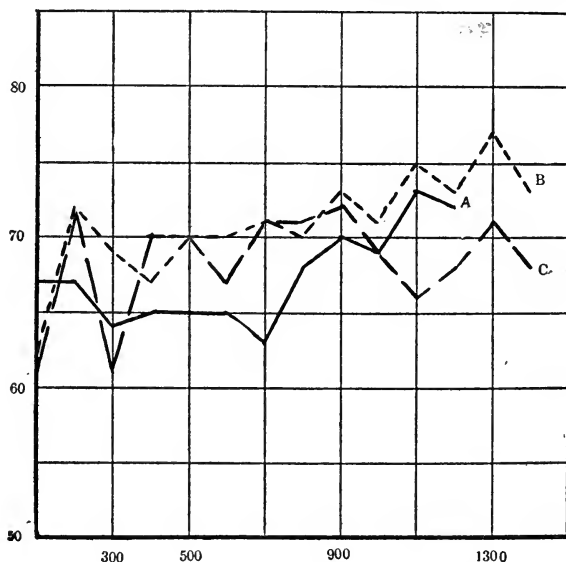


FIGURE 3. The graphs *A*, *B*, and *C* represent the average percentage of correct choices for successive 100 trials for the intervals of 5, 10 and 15 seconds respectively.

within 3,000 trials. For this reason the experiment was discontinued. However, some rats did attain this level of efficiency during the experimentation. Other animals were making progress and gave every indication of final success. Some rats soon fell into fixed position habits and gave no indication of further progress. Group *B* made the best records rather consistently. Their records are the highest in 11 of the 14 cases of comparison. Two rats of this group made scores of 90 per cent. of correct choices, while only one animal in each of the other groups attained this level of proficiency. There is but little difference between graphs *A* and *C*. Group *C* on the whole made the better initial records, while group *A* made the more rapid progress in the later stages of the experiment. As to the influence of the

time interval upon rate of mastery, the records indicate that 10 seconds may be the most favorable of the three intervals.

3. A third experiment was conducted by Miss Koch. She tested group *A*, consisting of seven animals, for a 10-second interval, and group *B* containing six rats for a 15-second interval. The conditions of the experiment were the same as those of experiment 2 with the following exception. By means of the swinging door *Z* (see Fig. 1), the entrances into the paths *R* and *L* were alternately closed and the animals were thus forced to run in the given sequence of *R, L, R, L*, etc., for each day's series of trials. But one path was open at a time; errors were impossible; the rat was compelled to respond correctly in the desired sequence. To obtain records of the rat's progress in mastering this coordination, test series were interpolated every fifth day, in which the door *Z* was removed and the animals were allowed to choose between the two paths as in the former experiments. The percentage values for each 100 total trials are thus based upon two test series of 10 trials each.

The scores for these two groups are represented by graphs *A* and *B* of Figure 4. The initial records are quite high and neither group gave much indication of further progress. Evidently progress was confined mainly to the initial 100 trials. Certain animals succeeded in attaining a proficiency of 90 per cent. of correct choices. The graphs gave so little indication of an early group mastery that the experiment was discontinued. The high degree of irregularity of the successive scores is due to the small number of trials upon which

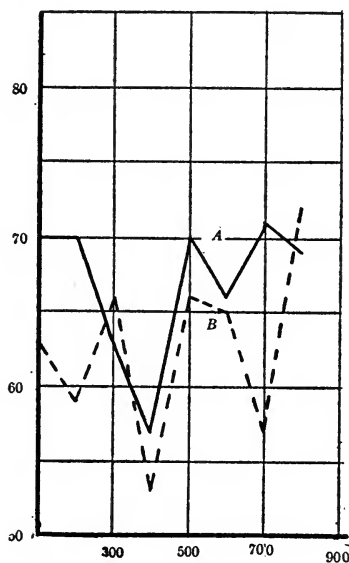


FIGURE 4. The graphs *A* and *B* represent the average percentage of correct choices for successive 100 trials for the intervals of 10 and 15 seconds respectively.

the values are based. Group *A* made the better record. Their scores were the higher in six of the eight comparisons. Three rats of this group made a record of 90 per cent. while but one rat of group *B* attained this degree of proficiency. The group difference is probably not very significant, but the results indicate that 10 seconds is a more favorable interval than 15 seconds.

The character of the curves in experiment 1 is radically different from the type of curve obtained in experiments 2 and 3. The latter curves exhibit the greater progress for the first 200 trials, and maintain a higher level for 500 trials; From this point on the curves of experiment 1 rise much more rapidly and maintain this ascendancy for the remainder of the experimentation. The explanation of this difference in rate of progress must be sought in the different conditions of the experiments. Length of interval is not a factor, as the 15 second interval was employed in all three experiments. The previous experience of the animals in experiment 2 may account for the greater initial progress of these groups, but this factor can hardly explain the poor records of these animals in the final stages of the experimentation. Moreover the rats used in experiment 3 lacked previous experience. The technique of the experimenters may have been a factor. The rats employed in experiment 1 may have learned to rely upon visual cues from the experimenter in making their choices, as this possibility was not eliminated by control tests. The number of trials per day seems the more probable cause, as 20 trials per day were given each rat in experiment 1 as compared to 10 trials in experiments 2 and 3. On this hypothesis 10 trials per day is the more effective distribution for the initial stages of mastery, while greater progress can be attained by the use of 20 trials after this early period of adjustment to the problem. Such a fact will constitute an exception to our present knowledge concerning the relative values of concentrated and distributed trials. This conception must be regarded as a tentative hypothesis to be tested by further experimentation.

Our experiment proves that animals can form an associa-

tion between two terms separated by a time interval as large as 35 seconds, provided that this interval is filled in with some rather constant type of activity such as eating. There is no conclusive evidence that the rate of learning is a function of the length of time interval employed. Certainly the rate of learning does not vary directly with the length of interval. There was some indication in two experiments that 10 seconds may be more favorable than either a 5- or 15-second interval. In the first experiment the 25-second group mastered the problem more readily than did either the 15- or 35-second groups. All of these differential results may well have been due to chance factors. In the absence of any conclusive evidence to the contrary, one must assume from our data that the speed of forming the association was not dependent upon the size of the intervals used. One is not justified, however, in extending this conclusion to other intervals. One must also admit that the method of the experiment is not particularly well adapted to the solution of the problem.

This problem of the relation of the rate of learning to the time interval has received but little attention from experimenters in either the animal or human field. Bergstrom and Froeberg have made excellent studies with the use of human subjects. In Bergstrom's<sup>1</sup> first experiment the subjects were required to memorize a list of 12 syllables which were visually presented in succession. Each syllable of the series was exposed to view for a period of  $82\sigma$ , and a given time interval was allowed to elapse between the successive exposures of the syllables. The problem thus required the association of a series of 12 terms successively exposed when the series of exposures were separated by a given time interval. Three subjects were tested. Each memorized 12 lists of syllables per day for four days. The subjects were requested to pronounce silently each syllable but once at the time of its exposure. After four repetitions of a list of syllables, the amount memorized was measured by the number of errors

<sup>1</sup> Bergstrom, John A., 'Effect of Changes in the Time Variables in Memorizing, together with some Discussions of the Technique of Memory Experimentation,' *Amer. J. of Psychol.*, 1907, 18, pp. 206-238.

made in an immediate written reproduction. The intervals between exposures were  $302\sigma$ ,  $686\sigma$  and  $1454\sigma$ , and the average number of errors for the three intervals respectively were 10.3, 8.9, and 7.5. In a second test 30 subjects were used and the material consisted of lists of 10 words and lists of 10 letters. This material was presented orally at a rate controlled by the beating of a metronome. Since the time necessary to pronounce each term of a series presumably approximated a constant, the differences of rate will represent differences of time interval between successive exposures. The time intervals employed were .5, 1, and 2 seconds. Each list was presented but once and the amount memorized was tested in terms of the errors made in an immediate written reproduction. The average percentages of error for the lists of words were 51.12, 36.5, and 23.9 for the .5, 1, and 2-second intervals respectively. The corresponding percentage values for the lists of letters were 44.09, 42.65, and 38.44. In both tests the amount learned increased as the interval was lengthened, and the author indicates his belief that this relation will obtain for much larger intervals than those employed in the experiment.

Froeberg's<sup>1</sup> subjects memorized a series of five pairs of syllables. Each term of the pairs was exposed for about one fourth of a second. The members of the pairs were exposed in succession and the interval between their exposures was varied from 0 to 5 seconds. The interval between successive pairs was a constant. Each series was given two repetitions as a rule, and the amount memorized was tested 10 seconds later. In the test for recall the stimulus syllables were presented in a new order and the subject was required to supply the missing member of each pair. The subjects were requested to inhibit the tendency to articulate the syllables and to refrain from all thought associations. The average percentages of correct responses for the group of seven subjects were 49, 45, 48, 51, 49, and 49 for the intervals of 0, 1, 2, 3, 4, and 5 seconds respectively. These figures indicate that the

<sup>1</sup> Froeberg, 'Simultaneous versus Successive Association,' *PSYCHOL. REV.*, 1918, 25, pp. 156-163.

rate of learning is not a function of the length of interval. The experiment was repeated with a group of four subjects with one change of condition. Numerals were visually exposed during the interval of time between the exposures of the members of a pair which were to be associated, and the subjects were asked to perceive and read these interpolated numerals during the interval. This procedure was designed to prevent a memory survival of the first member of a pair during the interval separating it from its associated member. With these conditions the average percentages of recall for the above intervals were 45, 34, 36, 40, 24, and 22 and the rate of learning decreased with the length of interval.

There are but two experiments on this topic in the field of animal psychology. Thorndike<sup>1</sup> taught cats to climb up three feet on the wire netting constituting the sides of their cage in order to obtain food held in his hand. The cats had thus learned to associate the act of climbing to a given spot with the visual stimuli of food in the hands of the experimenter. Thorndike now sat at a distance of 8 feet from the cage, clapped his hands four times and said 'I must feed those cats.' After a lapse of 10 seconds, he took food, walked over to the cage and fed them as usual. This procedure was repeated in order to determine if the animals could learn to associate the act of climbing with the visual and auditory stimuli involved in the clapping of the hands and repeating the phrase. Such an association was soon formed and the cats climbed up for the food as soon as the new stimulus was given without waiting for the experimenter to bring it in the usual manner. The animals thus learned to associate two terms which were at first experienced successively with an interval of 10 seconds between them. Thorndike did not vary this interval in order to determine the relation between its length and the rate of learning. The experiment merely proves that cats under these conditions can learn to associate two terms over an interval of 10 seconds.

In a previous experiment with the alternation problem,

<sup>1</sup> Thorndike, 'Animal Intelligence,' p. 111. The MacMillan Co.

the writer<sup>1</sup> employed an interval of 16.5 seconds, and a group of white rats mastered the problem with a high degree of proficiency. The length of the interval was not varied during the learning process. The experiment indicates again that rats can form an association over an interval of 16.5 seconds. The experiment was defective in that the apparatus permitted the maintenance during the interval of a motor attitude characteristic of the previous choice. It was thus possible that the two choices were based in part upon the two distinctive motor attitudes maintained during feeding. On this hypothesis the interval between stimulus and response would be much less than 16.5 seconds.

There are three possible types of explanation of the fact that association can bridge a considerable interval of time. (1) One may assume the memory survival of each term during the interval until the succeeding term is presented. The connection is thus established between two simultaneous activities—the perception of one term and a conscious representation of the preceding one. (2) The connection may be mediated by some third factor which is present during the interval. A series of syllables may be memorized by associating each with its position in a temporal sequence. Almost any kind of mental activity may serve such a mediating function. (3) The third possibility assumes that the two terms may be associated directly, that each activity becomes connected with the retained effects of the preceding activity. An inspection of the various statements concerning association in the texts and treatises on general psychology reveals the fact that simultaneous or successive presentation is regarded as an essential condition for the formation of an effective connection. The term ‘immediate’ is not explicitly defined. Apparently the authors imply one of two things: The two experiences must be in temporal contact with no interval or interpolated activity between them, or other experiences may intervene but the two terms to be connected must exist within the ‘specious present.’ On this basis only conscious

<sup>1</sup> Carr, Harvey, ‘The Alternation Problem,’ *J. of Animal Beh.*, 7, pp. 365-384.



activities can be associated, and our third possibility of connection is excluded.

Thorndike in his experiment with cats considered the first and third possibilities. He regarded a direct connection as highly improbable, and hence inferred that the association must have been mediated by a memory image of some sort. The experiment is thus presented as a proof of the existence of an ideational capacity in cats. The second possibility was ignored. Such a mediating factor may have been the visual perception of the experimenter. On this hypothesis the experience of the food became associated with the perception of the experimenter and this latter term was again associated with the hand clapping and the spoken words.

In Bergstrom's experiment memory connection was possible. Temporal association and other indirect thought connections may have been established. The character of the results indicates such modes of connection, for the longer the interval the greater is the possibility of a memory review and the development of effective thought connections.

In Froeberg's experiment temporal association was excluded by the method of paired associates. The temporal order in recall was different from that in presentation. In the first test the subjects were requested to refrain from developing thought connections and to inhibit the tendency to articulate the syllables. In spite of these instructions, the subjects reported that the association was mediated by memory. In the second test the subjects were required to read numerals during the interval. This activity was interpolated for the purpose of preventing any memory connection, and the introspective reports indicated that the procedure was successful. Froeberg thus concludes that associations can be established directly over an interval of at least 5 seconds, and that within these limits the rapidity of learning decreases with the length of interval.

There are three possible objections which can be urged against Froeberg's conclusions. (1) One may assume that all memory and thought connection was not wholly excluded. This hypothesis can not be regarded as wholly irrational as

such modes of connection are many and devious, while some are by no means capable of being easily and readily observed. Such an objection will apply to practically every experiment which employs human subjects. (2) One may assume that the associations were mediated by these interpolated activities themselves. With this conception the subjects were memorizing a series of terms presented in immediate succession,—a syllable, a numeral, a second numeral, and the final syllable. Owing to the instructions for memorizing and recall only the final term is reproduced. This explanation is similar to that suggested for Thorndike's experiment, viz., that the connection may have been mediated by the visual perception of the experimenter. (3) One may admit that the connection between the syllables was formed directly but deny the validity of Froeberg's factual correlation between rate of learning and length of interval on the ground that the interpolated activity constitutes a distraction.

It is not our purpose to maintain the validity of the above objections. Rather we wish to emphasize the difficulties involved in securing a decisive proof of the possibility of a direct associative connection with the use of human subjects, and to suggest that animal experiments offer certain advantages for the solution of the problem. Presumably indirect thought connections can be effectively excluded by this means. Few will assert that our animals mastered the alternation problem by means of a concept of temporal order, viz., that they developed an idea of the principle of alternation and kept track of the proper response by counting the number of trials. Neither is the employment of other conceptual devices very likely. The experiment was devised in order to exclude a connection by means of the interpolated activity. The animals form two distinct associations while the interpolated activity of eating was the same in both cases. It was assumed that one interpolated activity could not mediate two distinct associations. Further reflection, however, has induced some scepticism of the truth of this proposition.

Granted the elimination of all indirect modes of connec-

tion of the second type, one must choose in our experiment between the hypotheses of a memory survival or of a direct connection. As noted Thorndike regarded the former explanation as the more probable. The writer prefers the latter conception. On the one hand the memory hypothesis encounters many factual and theoretical difficulties which need not be enumerated at this time. Animals are preferable to human subjects in the attempt to avoid a memory connection. A direct connection, on the other hand, seems logically feasible on the basis of certain generally accepted explanatory principles.

Let us take the problem of associating the stimulus  $X$  with a certain movement  $Y$ . Originally the stimulus  $X$  will not evoke the act  $Y$ , and the problem consists of so connecting them that  $Y$  will become the natural motor outlet of the nervous impulse aroused by  $X$ . This connection is usually secured by the simultaneous functioning of the two terms, and the experimenter achieves this result by evoking the act  $Y$  by the use of some stimulus  $B$  at the same time that stimulus  $X$  is presented. The whole task of the experimenter consists of repeating this simultaneous presentation until the connection is effected; the rest of the process must be left to nature.

A usual type of explanation assumes that the nervous impulse aroused by  $X$  is deflected from its normal channels over to the motor outlet  $Y$  because of the lowered resistance or tension in that center due to its activity. This conception involves two assumptions: (1) The motor outlet of an incoming sensory impulse is in part a function of the relative resistance, tension, or susceptibility to response of the various motor centers. Each sensory impulse naturally tends to be drained off into that motor channel which for the time offers the minimum of resistance. (2) The conception assumes that the resistance or tension of any center is decreased by the functional activity of that center. The point which we wish to emphasize in this mode of explanation is the fact that the state of decreased resistance is the *essential condition* which mediates the desired connection. The induced ac-

tivity in the motor center is not important *per se*, it is but a means to an end. Theoretically an experimenter can establish an associative nexus between the given factors by any device which will effect a decrease in the resistance of the motor center.

The conception of resistance is also frequently applied to the phenomena of retention and habit. The hypothesis assumes that the repeated activity of a center permanently lowers its resistance or increases its susceptibility to activity. By sufficient practice any center may be brought into a rather permanent state of a high degree of readiness for response. With such a center, activity produces the maximum of effect, and this effect rather rapidly diminishes with the cessation of activity until the relatively permanent level of habit is reached. On the basis of this conception, there are no *a priori* reasons why an effective association can not be established over a considerable interval of time, provided that one of the factors, *e.g.*, the motor center *Y*, has been developed by practice to a state of readiness much greater than that of any other motor outlet. One should expect that simultaneous presentation will constitute the most favorable condition for the formation of an association but that the rate of learning will diminish as the time is increased up to a certain interval, after which further variations of interval will have but little effect.

Certain writers prefer to regard a state of activity in a center as a necessary condition of its lowered resistance or readiness for response. When the motor center *Y* is evoked to activity by the controlling stimulus *A*, it is assumed that this activity will persist in a subconscious and subliminal form after the stimulus is removed. Applying this conception to habit, a nervous activity when once aroused never completely subsides. A center when once active never attains a state of functional quiescence. Association over a time interval is as feasible on this basis as on that sketched above. The writer sees no reason why readiness for response can not be conditioned by a state of functional quiescence as well as by a state of subliminal excitement. The additional hypo-

thesis is thus gratuitous. While one can easily conceive of the persistence of an activity for a short interval of time on the principle of inertia, yet the further assumption of an indefinite continuance rather taxes one's credulity.

In conclusion, this paper advances the following propositions. An associative nexus can be established over a considerable interval of time. Our results do not permit of any statement that the rate of learning is a function of the intervals employed. With the possible exception of the interpolated activity, the connection was not mediated by some third factor. As an explanation one must choose between the hypotheses of direct connection or of memory survival. The choice must be determined largely by *a priori* considerations. We do not assert the validity of the resistance conception as applied to the phenomena of habit, retention, and the formation of associations. We do note the fact that such a mode of conception is quite generally accepted, and we do maintain that on the basis of these assumptions the hypothesis of a direct connection is logically permissible. One must either admit the possibility or deny the validity of the resistance conception.

# A STUDY OF ASSOCIATION IN NEGRO CHILDREN

BY IDA MITCHELL, ISABEL R. ROSANOFF, AND AARON J. ROSANOFF

*Kings Park, N. Y.*

The question of racial differences in mental capacity has always been of interest, but data which might make possible a scientific judgment concerning it are as yet scant. It is difficult to distinguish, in the performance of subjects, the parts to be attributed, respectively, to native mental capacity and to influence of environment.

It is obvious that in work with children the environmental factors cannot be so prominent as in work with adult subjects; and the present study, having been undertaken in the hope of contributing toward an answer to the question of racial differences, was therefore carried out on young subjects.

The material consists, in the main, of three hundred association test records. The subjects were negro children found in New York City schools, in age groups of twenty-five ranging from four to fifteen years, and about equally divided as to sex. In other respects the selection of subjects was a random one.

The technique of the investigation was that of the Kent-Rosanoff test. This test, as has been shown, furnishes results which are in close correlation with mental capacity and, up to the age of eleven years, with degree of maturity, while they seem to be influenced to but a slight extent, if any, by education.<sup>1</sup> This test, moreover, having previously been applied to many variously selected subjects—normal adults, children of different ages, insane, and feeble-minded subjects—affords an opportunity of comparing the results obtained in our group with those of various other groups.<sup>2</sup>

<sup>1</sup> Isabel R. Rosanoff and A. J. Rosanoff, 'A Study of Association in Children,' *PSYCHOL. REV.*, 1913, 20, 43-89.

<sup>2</sup> G. H. Kent and A. J. Rosanoff, 'A Study of Association in Insanity,' *Amer. J. of Insanity*, 1910, 67, 37-96, 317-390. F. C. Eastman and A. J. Rosanoff, 'Association in Feeble-Minded and Delinquent Children,' *Amer. J. of Insanity*, 1912, 69, 125-141.

In Table I. is presented in statistical form the showing made in the test by the negro children as a whole, together with figures for various other groups of subjects.

TABLE I

Subjects	Common Reactions		Doubtful Reactions %	Individual Reactions %	Failures of Reaction %
	Specific %	Non-specific %			
1,000 normal adults.....	85.5	6.2	1.5	6.8	—
247 insane adults.....	66.4	4.3	2.5	26.8	—
253 defective children aged over 9 yrs.....	75.2	8.2	2.1	13.0	1.5
125 normal white children 11-15 yrs.....	82.0	7.2	1.6	8.6	0.6
175 normal white children 4-10 yrs.....	62.7	4.2	3.2	18.8	11.1
125 normal negro children 11-15 yrs.....	75.3	7.2	2.5	14.9	0.1
175 normal negro children 4-10 yrs.....	54.1	3.5	2.5	33.2	6.7

It will be seen from the table how various groups of subjects differ from normal adults in associational tendency, especially in the tendency to respond with common specific reactions. Weakness of this tendency is usually due either to immaturity, or arrest of development, or psychotic constitution, and is characterized either by excessive proportion of individual reactions, or failures of reaction, or both.

Particular attention is invited to the showing of negro in comparison with white children: the former show the further departure from the normal adult standard.

Negro children have shown a greater tendency than white ones to give individual reactions, but they have furnished fewer failures of reaction. This is, perhaps, due to nothing more than a greater inclination to obey instructions.

In Table II. is given a classification of all the individual reactions given by the negro children in comparison with a similar classification of such reactions given by white children and by a selected group of normal adults. (86 normal adults; selected records, containing not over ten individual reactions.)

TABLE II

Types of Reaction <sup>1</sup>	86 Normal Adults %	300 White Children %	300 Negro Children %
Normal (by appendix).....	41.8	20.0	12.3
Derivatives of stimulus words.....	0.3	0.1	5.7
Partial dissociation <sup>2</sup> .....	8.0	11.1	16.7
Perseveration <sup>3</sup> .....	6.1	27.8	23.0
Neologisms (without sound relation).....	—	0.6	0.3
Unclassified.....	43.8	40.4	42.0

The relatively reduced percentage of normal reactions and the excessive percentage of derivatives of stimulus words are the most striking features in the results obtained from the negro children. Both, however, may be accounted for by their heightened tendency to furnish individual reactions rather than failures of reaction.

In Table III. the material is presented by ages, similar material from white children being also shown to facilitate comparison.

TABLE III

Group Ages in Years.	Common Reactions				Doubtful Reactions		Individual Reactions		Failures of Reaction	
	Specific		Non-specific		White %	Negro %	White %	Negro %	White %	Negro %
	White %	Negro %	White %	Negro %						
4.....	40.4	37.5	1.1	1.0	3.8	2.3	25.3	40.9	29.4	18.4
5.....	55.1	41.5	2.0	1.4	4.4	1.8	21.4	37.4	17.1	17.9
6.....	62.2	52.1	2.7	2.2	3.2	2.6	18.6	37.6	13.3	5.5
7.....	64.9	56.0	4.0	3.7	3.5	2.1	20.0	35.1	7.6	3.3
8.....	68.4	59.3	5.8	5.4	3.1	3.2	18.0	31.0	4.7	1.2
9.....	75.1	62.3	5.5	6.5	1.7	2.6	14.2	27.7	3.5	0.9
10.....	72.9	70.0	8.4	4.4	2.3	2.7	14.3	23.4	2.1	0.1
11.....	82.0	74.8	7.1	7.0	1.7	2.6	8.6	15.5	0.6	0.1
12.....	83.8	74.2	6.6	7.5	1.3	2.4	7.6	15.6	0.7	0.2
13.....	81.1	74.2	8.4	7.2	1.8	2.6	8.5	15.8	0.2	0.2
14.....	84.1	77.2	6.3	7.8	1.4	2.6	7.7	12.4	0.5	0.1
15.....	78.7	76.3	7.6	6.2	2.0	2.2	10.8	15.2	0.9	0.1

<sup>1</sup> The Kent-Rosanoff classification was used.

<sup>2</sup> Under this heading are included the following varieties of reactions: non-specific, sound (words and neologisms), word complements, and particles of speech.

<sup>3</sup> Under this heading are included the following varieties of reactions: association to preceding stimulus, association to preceding reaction, repetition of preceding stimulus, repetition of previous stimulus, repetition of preceding reaction, repetition of previous reaction, and reaction repeated five times.



It will be seen that at every age the showing of negro children is inferior to that of white children. It would seem, therefore, that the average mental capacity of negro children is inferior to that of white children.

65.7 per cent. of negro children made a showing inferior to the average for white children; and 21.3 per cent. made a showing inferior to the lowest for the corresponding age groups of white children.

However, there is much overlapping between the racial groups. 34.3 per cent. of negro children made a showing superior to the average for white children; and 1.3 per cent. made a showing superior to the highest for the corresponding age groups of white children.

Among negro children, as among white ones, there is a close correlation between teachers' estimates of mental capacity and the showing made in the test. This is demonstrated in Table IV.

TABLE IV

Teachers' Estimates	Common Specific Reactions %	Individual Reactions %	Failures of Reaction %
"Dull" or "very dull" group.....	61.1	27.7	3.2
"Bright" or "very bright" group.....	67.5	23.6	0.3

In collecting the material an attempt was made to distinguish, as far as seemed possible from the skin color, children who were of partly white descent from those who were without white admixture. The data for the two resulting groups are shown in Table V.

TABLE V

Racial Composition	Common Specific Reactions %	Individual Reactions %	Failures of Reaction %
Partly white.....	71.3	18.9	1.4
Pure negro.....	70.5	18.0	2.3

It will be seen that the showing of the two groups—partly white and pure negro—is practically the same. The white admixture, such as it has been, has not resulted in increase of mental capacity.

The opinion is often expressed that negroes residing in southern states are different in some ways from those who have taken up residence or were born in northern states. Some think that negroes of the south are, on the whole, superior; others that they are inferior. A difference of some sort, whether resulting from selection determining the migration to the north or from the influence of northern environments, is conceivable. In order to have something more than a mere impression concerning this question we secured test records from twenty-five twelve-year-old children in the Thomy Lafon school at New Orleans, La. This school is conducted exclusively for negro children and by negro teachers. The results, in comparison with those obtained in a similar group of northern negro children, are shown in Table VI.

TABLE VI

Subjects	Common Reactions		Doubtful Reactions %	Individual Reactions %	Failures of Reaction %
	Specific %	Non-specific %			
Northern negro children.....	74.2	7.5	2.4	15.6	0.2
Southern negro children.....	72.0	9.0	2.1	16.8	0.1

The table shows a slight, probably insignificant, difference in favor of the northern negro children.

*Summary.*—Negro children, on the whole, show further departure than white ones from the normal adult associational standard.

At every age, from four to fifteen years, the showing of negro children is, in the average case, inferior to that of white children. It would seem that the average mental capacity of negro children is inferior to that of white children.

However, there is much overlapping between the racial groups. 34.3 per cent. of negro children made a showing superior to the average for white children; and 1.3 per cent. made a showing superior to the highest for the corresponding age groups of white children.

An attempt was made to distinguish, as far as seemed

possible from the skin color, children who were of partly white descent from those who were without white admixture. The test showed that the white admixture, such as it had been, had not resulted in increase of mental capacity.

A group of children found in a school in New Orleans, La., made a showing practically the same as a similar group found in a New York City school.

## PSYCHOLOGICAL PARERGA

*From the Laboratory of McLean Hospital*

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### I. PSYCHOGALVANISM IN THE OBSERVATION OF STUPOROUS CONDITIONS

BY E. S. ABBOT AND F. L. WELLS

1. The instrument employed was a Leeds & Northrup Type H D'Arsonval galvanometer, sensibility 260 megohms, with various types of electrodes; two types of calomel electrodes, metal plates, carbon cylinders, platinum and carbon electrodes immersed in saline solution. The type of electrode used was governed by the demands of the experiment and the responsiveness of the subject.

2. Current from a Gordon Cell was used with all electrodes save a pair of balanced calomel electrodes. With the balanced electrodes body potential is measured; with the cell current, the resistance of the circuit under the conditions which the interposition of the body imposes. The 'reflex' is given in a change of either the above potential or resistance, in response to specific stimulus.

3. During the experiments, the subject reclined on a massage table. Two experimenters were usually present, one responsible for giving the stimuli, the other for the record of reaction.

4. Deflections were read from a millimeter scale. Time relations were measured with a stopwatch of high grade, operating practically without noise. Graphic registration was contemplated, but deemed unsuited to the purpose.

5. The sensory stimuli employed included the exhibition of a flash light, a red light, a blue light, as visual stimuli; a

TABLE I

	Normal Men Subjects										Normal Women Subjects									
	I	II	III	IV	V	VI	VII	VIII	IX	X	IX	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	XX
1. Total no. timed reactions	27	27	27	20	28	10, 11	9	13, 10	9, 12	10, 10	22	19	27	27	28	11	13, 13	28, 7	25	25
2. Their average time (0.2")	11.1	15.0	14.1	17.0	14.7	18.7, 17.0	19.8	17.6, 15.1	15.9, 14.7	10.3, 18.1	15.4	17.3	14.6	17.1	17.7	18.6	16.9, 13.7	15.6, 15.1	15.0	14.8
3. M.V. of this average	4.4	1.9	2.1	2.7	2.6	1.5, 1.6	6.6	1.9, 4.9	1.6, 3.7	3.5, 4.1	1.5	3.3	2.6	2.1	2.1	1.7	3.7, 5.3	1.7, 2.0	1.9	2.2
4. Average above reaction-deflections (mm.)	14.9	7.6	6.7	7.5	6.5	2.8, 3.5	8.6	7.7, 5.8	24.6, 23.8	6.4, 4.4	7.3	5.4	3.3	6.5	4.8	2.6	3.6, 2.8	2.5, 3.9	3.4	3.0
5. M.V. of this average	9.6	2.8	2.7	3.5	3.5	1.2, 1.9	4.9	3.5, 4.6	11.7, 15.8	5.8, 2.8	3.1	2.7	1.4	2.3	1.6	1.3	1.2, 2.0	1.5, 1.6	1.4	9
6. Per cent. of all stimuli given to which no certain reaction-deflection was recorded	13	4	13	38	12	45	68	19, 33	44, 8	41, 53	21	43	21	21	0	61	35, 35	7, 0	22	11
7. Average all positive movement, controls (mm.)	×	×	×	×	×	×	×	×	×	4, .7	1.0	1.6	1.3	1.0	.8	.5	1.2, .5	.1, .2	1.4	1.3
8. Per cent. which (4) is of average initial deflection	4.4	2.8	2.7	2.2	2.8	1.9, ×	6.7	3.9, ×	3.4, ×	2.5, 3.1	2.9	2.9	1.2	1.1	2.3	×	4.0	1.5, 2.0	1.3	1.2

loud sound-hammer stroke, a weak sound-hammer stroke, a whistle, and the dropping of a golf ball as auditory stimuli. Tactile and temperature stimuli were tried and discarded. Questions also were used with the pathological subjects.

6. The accompanying table gives results of twenty normal subjects, ten men and ten women. The control observations of item 7 were in that after each single observation, a control reading was recorded by marking a moment which would be suitable for giving another stimulus. Then was recorded the amount of any positive movement which took place within the latent time of the previous observation of reaction-deflections. Observations are reliable in proportion as these control readings are small or negative.<sup>1</sup> In Cases VI., VIII., IX., X., XVII., XVIII., changes of technique were necessary during an experiment. Results with the two procedures recorded are separated by commas.

7. Reaction deflections which one may expect to obtain with this apparatus, under the most favorable conditions of stimulation, average from 2 to 25 mm. according to the subject. There are large intercurrent changes of body resistance, during which, however, the same order of relation (item 8) is preserved between reaction deflection (item 4) and the base-line (initial deflection). No relation can be determined between these values and the subject's general tendency to emotional reaction; the deflections quoted are obtained under varying electro-physiological conditions, and not directly comparable.

8. In respect to latent time of reaction deflections, the data are thus comparable. It is generally understood that latent time of psychogalvanic responses varies between 2 and 5 seconds, and is governed by latent time of impulses through the sympathetic nervous system. In the present observations, latent times average often under 3 seconds, never over 4, with a median approximating 3.1. Definite responses after 5 seconds are very rare. The lower limit is necessarily less definite, being complicated by reactions from expectation.

<sup>1</sup> Such movement is in itself seldom to be confused with a true 'reflex,' being slower and more continuous.

TABLE II

Case.....	22 <sup>1</sup>		29 <sup>2</sup>		35 <sup>3</sup>		35 <sup>3</sup>		59 <sup>4</sup>		59 <sup>4</sup>		73 <sup>5</sup>	
	Sensory	Questions	Sensory	Questions	Sensory	Questions	Sensory	Questions	Sensory	Questions	Sensory	Questions	Sensory	Questions
1. Total number timed reactions..	9	10	16	8	19	×	12	10	5	5	3	4	15	9
2. Their average time (0.2'').....	14.3	20.1	15.4	18.3	23.0	×	15.3	18.1	19.2	18.2	18.0	20.0	16.9	17.4
3. M.V. of this average	4.1	1.9	2.6	1.3	5.3	×	1.3	1.6	4.6	4.3	2.0	2.0	2.7	2.2
4. Average of reaction deflections (mm.)	20.5	33.9	38.6	39.3	3.9	×	60.9	30.6	3.0	15.8	31.0	97.0	8.7	5.3
5. M.V. of this average	13.0	4.9	22.0	16.4	2.2	×	39.7	24.7	.4	7.0	23.0	27.0	8.7	1.7
6. Per cent. failures of reaction.....	36	0	24	11	57	×	14	0	83	0	78	50	46	10
7. Average positive movement, controls (mm.).....	.8	11.2	30.2	1.0	×	×	.3	0	0	1.6	2.0	1.3	.4	.5
8. Date of observations.....	7-26-12		11-6-12		7-22-12		10-15-12		7-25-12		11-11-12		7-30-12	

9. The accompanying table presents results with pathological subjects. The figures for sensory stimuli proper, and for questions, are separated. Some of the questions were of general character: *What day of the week is it; Are you married; How tall are you;* others were framed with special reference to the case history. The point at issue is how the psychogalvanic reflex is affected by the stuporous or semi-stuporous conditions in these subjects.

10. The latent times to sensory stimuli observed under stupor are within normal limits, save one not far beyond

<sup>1</sup> Man of twenty-three, with prolonged manic attack (May-Oct.) followed by confused depression with thinking difficulty, delusions and marked psychomotor retardation at time of experiment (100-7 in 4 min. 37 sec., 6 sec. to spell "constitution").

<sup>2</sup> Man of twenty-seven, manic depressive depression with considerable thinking difficulty and moderate psychic retardation (100-7 in 41 sec., in convalescence in 20 sec.).

<sup>3</sup> Man of twenty-four, alcoholic, with manic-depressive depression, greatly retarded and confused (100-7 in 2 min. 25 sec.) at first experiment, much improved at second experiment.

<sup>4</sup> Woman of nineteen, religious, overconscientious; at first experiment in mixed phase, manic stupor; little spontaneity, some impulsive episodes, generally mute. Much improved at second experiment.

<sup>5</sup> Woman of twenty-three in fourth depression, with marked psychomotor retardation. At time of experiment comparatively normal, only fatiguing easily.

(35a). This indicates a perception of the stimuli but little slower than the normal, not in proportion to the slowness of the subjects' ordinary behavior. Some slowing of the perceptive process is a possible inference from the shorter latencies which Cases 35 and 59 show after much improvement from the retarded condition.

11. The mention of latent times made in the extensive study of Gregor and Gorn (*Zt. f. d. Ges. Neur. u. Psychiat.*, 1913, 16, 1-105) appears consistent with present results.

12. It is presumed that the mental process involved in this reflex to the sensory stimuli is relatively simple, little associative elaboration being necessary to the organic processes of the reflex. The questions mentioned were asked the subject, to examine if greater perceptual retardation existed where associative processes of higher level were called into play.

13. It is necessary to consider if the reaction is to the perception of the question's content, or to its character as a sound stimulus merely. Evidence is found in the ordinary behavior of the subjects; the fact that they reacted relevantly if slowly to things said to them in their daily hospital surroundings. There are much fewer failures of reaction with the questions than with the sensory stimuli proper (item 6), indicating a more certain arousal of psychic response by the the sentences (their sensory quality was scarcely more coercive than the lights, etc.). Relevant answers might be made to the questions, these coming sometimes before, sometimes after the psychogalvanic response. It is indicated that the psychogalvanic responses to these questions represent a rational comprehension of the question, by the conscious of the main personality. That the comprehension is as complete or has as much associative elaboration as it would in a normal mind, is impossible to say. There is nothing in the records pointing otherwise.

14. Latent times to the questions are generally longer than to the sensory stimuli proper, but this is not regarded as a function of the psychosis. Reaction to more complicated stimulus takes longer in normal life; *cf.* also the records of 59a and 73.



15. Psychogalvanic reactions are reflex in that they are not directly governed by conscious volition; they are not induced or inhibited by will thereto. Their reflex character differs from that of the patellar or wink reflexes, in that they are governed less by the external stimulus and more by the internal elaboration of that stimulus. Among mental processes emotion is said to be the essential mental process which induces them and governs them much more closely than it does knee-jerk or the wink. Normally, consciousness of a mental event is a concomitant of emotional reaction attaching thereto. It is not necessarily so; psychogalvanic phenomena have been themselves invoked to demonstrate the presence of sympathetic reactions whose mental causes were not conscious (Sticker, Peterson and Prince). Their observations refer to hysterical types of dissociation, which have not been demonstrated in the manic-depressive group here involved. Conscious perception of the stimulus is positively indicated where relevant verbal response occurs as well as psychogalvanic reflex. It is doubted that the possibilities of coconscious perception have other than theoretical bearing on the present observations. Psychogalvanic response does not in either case take place without *some* perception of the stimulus, whether or not integrated with the personal consciousness.

16. It is indicated that the perceptive processes in conditions of manic-depressive stupor are but slightly lengthened from the normal. The associations in the present instances were formed with about the same rapidity as in health. The retardation may lie in a lessened complexity of the associations formed, or in the slower and feebler conversion of these associations into their motor expressions.

## II. PSYCHOTIC PERFORMANCE IN CANCELLATION AND DIRECTIONS TESTS

BY F. L. WELLS

17. In 1915 a series of choice reaction experiments was carried out to investigate how far the vital maladaptations of the psychoses are reflected on this more superficial level of the laboratory. The results of these experiments, which indicated that they are very slightly if at all so reflected, have been published.<sup>1</sup> Contemporaneously with these experiments, and with the subjects involved in them, observations were made with (a) a cancellation test, termed in this study test NC;<sup>2</sup> (b) a form of the Woodworth-Wells easy Directions Test, termed in this study test D1; (c) a form of the Woodworth-Wells easy Directions Test, termed in this study test D2; (d) the Woodworth-Wells hard Directions Test, termed in this study test D3.

18. The experimental instructions were in writing, and are quoted in the normal order of experiments. (See pp. 367-369.)

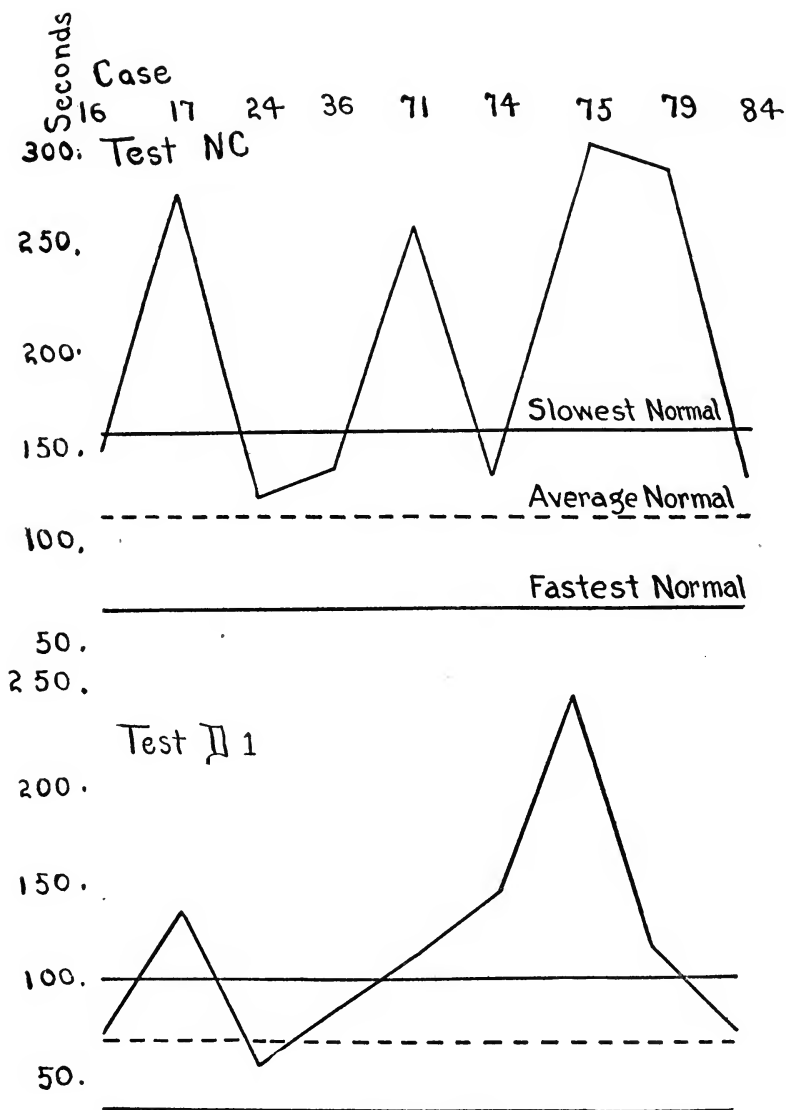
19. The score in each test is the total time of accomplishing it, increased by 10 per cent. of itself for each error made. Certain minor defects were scored as half errors.<sup>3</sup>

20. There are records from 8 normal and 9 pathological subjects, who are designated in the same manner as in the choice reaction tests mentioned. Results are presented *en profile* on the following pages, where the horizontal lines represent the fastest, average and slowest normal performance (corrected for errors), and the points of the curve represent the performances of the pathological subjects.

<sup>1</sup> Wells & Sturges, *Am. J. Ins.*, 1918, 75, 81-119.

<sup>2</sup> For description of tests in this study, cf. Woodworth and Wells, Association Tests, Psychological Monographs No. 57, 1911.

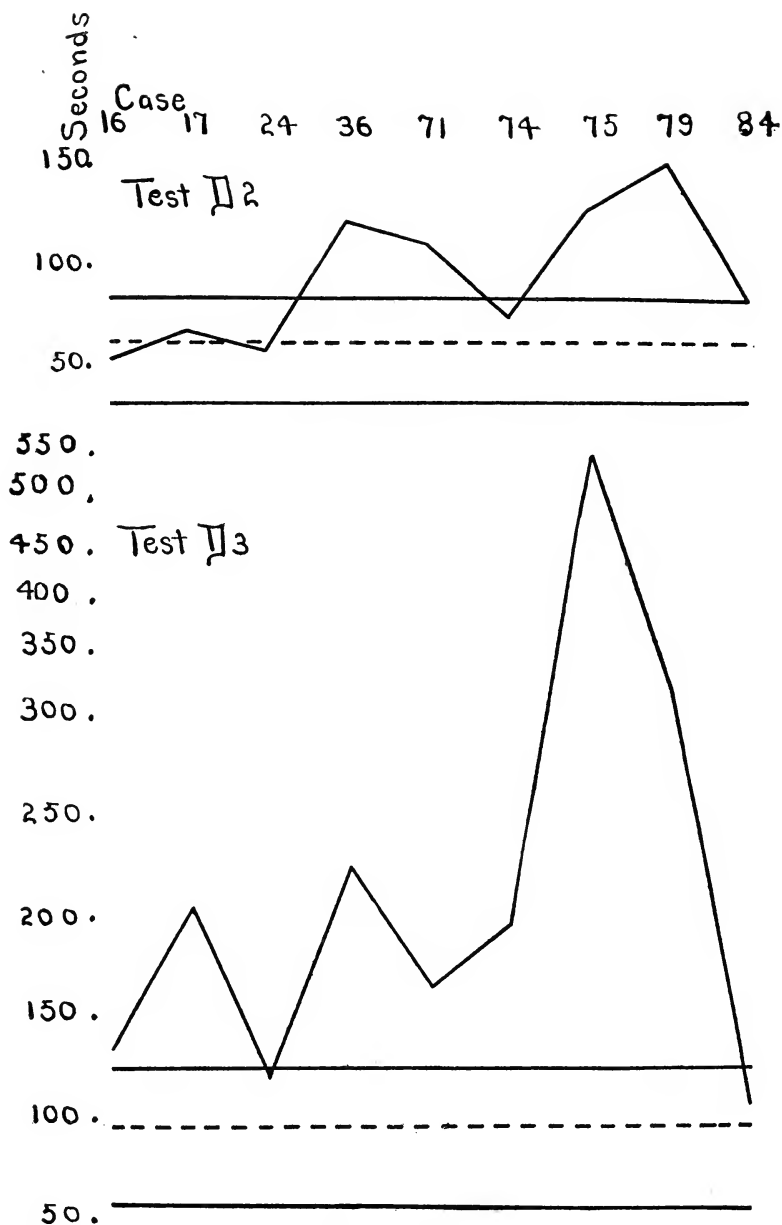
<sup>3</sup> Effort was made in the course of these experiments to satisfactorily record the time of the individual responses in the directions tests. Use was made of a pencil operating simply by gravity, to break electric contact during the writing stroke. The device is adapted only to intelligent and fully coöperative subjects. The plan generally followed was for the experimenter to press a key recording on a tape, in time with the reactions of the subject. These pressures can be coded so as to make the tapes easy to read.



(Test NC.) At the word 'go' take away the blank sheet in front of you from the paper that it covers, and you will see on this paper several lines of figures like this:

45879236017418605923596084231782130756494582976301  
76484395121947250836364570129865283940172376941850

Every line has five each of the figures. Begin at once to go through the lines of figures with your pencil and cross off every figure 0 that you come to. There will be 5 0's in each line, 100 in all. Do this as quickly as you can without missing any or crossing out a wrong figure.



(Test D1.) At the word 'go' take away the blank sheet in front of you from the paper that it covers, and you will see on this paper directions for different things you are to do with your pencil, such as 'Put a dot under the e in carpet,' or 'Make an x after the girl's name; John, Mary.' Begin at once to go through the list doing

21. The separation of normal from pathological subjects is greatest in D<sub>3</sub>, distinctly less in the remaining tests. Compared with this separation in the choice reaction experiments, tests NC, D<sub>1</sub>, and D<sub>2</sub> have as many cases within normal limits of speed, though they do not reach into these limits so far. In the present tests with four kinds of measures, there are two cases, in the same subject, where the normal average is surpassed. In the choice reaction experiments, with seven kinds of measures, this happens in nine cases with five subjects. In the choice reaction experiments the pathological cases did not make more errors than the normal. This is also the case with tests NC and D<sub>2</sub>; with tests D<sub>1</sub> and D<sub>3</sub> the psychotic cases make many more errors. D<sub>1</sub> and D<sub>2</sub> are intended to be of equal difficulty; here D<sub>2</sub> appears to be easier, perhaps owing to its coming immediately after D<sub>1</sub> and gaining its 'practice' effect.

22. The more complicated or 'synthetic' of these experiments accomplish better separation of normal from pathological subjects. The chief separation of the latter is indeed through the most synthetic of all tests, that of practical life. In a limited field, notably that of memory, the laboratory has been not unsuccessful in analyzing the mental failures of psychosis. But it is generally difficult to show psychotic deviations from the normal in the analytical procedures of the laboratory. With manic-depressive and schizophrenic conditions such as are here involved, analytic progress of the laboratory is slight. Here the laboratory separates normal from pathological rather through synthetic experiment involving the unanalyzed, damaged functions. The synthetic situation may be controlled experimentally as in this study. To do so has objective and quantitative advantages. It is important in the diagnosis of defect con-

each thing with your pencil as directed as quickly as you can without making mistakes. Thus in the cases above, you would make a dot under the e in carpet, and an x or a cross mark after the word "Mary," following the directions given.

(Test D<sub>2</sub>.) At the word 'go' uncover the sheet and do the same as last time. This is the same kind of test as before, but with another set of directions.

(Test D<sub>3</sub>.) At the word 'go' take away the cover sheet and do the same as last time. This is also the same kind of test but the directions are a little harder.

ditions, feeble-mindedness. The observational method more closely reconstructs the actual life synthesis in which the patient has broken down. For mental disease it is still a better criterion than psychological tests:

23. Qualitatively, the pathological subjects above are distinguished by the presence of 'overmarking,' *i.e.*, the use of more pencil strokes than necessary for compliance with instructions. Examples of this are the making of more than one stroke to check a zero in test NC; drawing a wavy line 'around the three dots' instead of the normal oval (D1); marking the 'line that looks most like a hill' (D2) by sketching an arrow pointing towards it; writing *in the east* to 'tell where the sun rises' (D3); writing script instead of Arabic numerals for number answers. The most overmarking is in the disturbed cases. The terminal dementia præcox 75, however, checked all his zeros with meticulous cross-marks.

24. A series with case 36 (manic-depressive excitement), made three weeks after the first, shows less excitement in manner of execution, and is also more rapid and correct. With case 71 (constitutional psychopathy), there are two series nearly three months apart; at the time of the first he appears to have been depressed, later more normal for him. The second records are more rapid, but contain mistakes absent in the first. There is much overmarking in both series; neither is as good as the normal average. Eighteen months later, while not under institutional care, he underwent the Stanford Revision series with an IQ of 1.18. Later he committed suicide. For case 75 (terminal dementia præcox) there are experiments on four days; three within a week, the fourth some five weeks later. The times at the beginning are very long; the later experiments show a great reduction, which is not explained. It is of course not paralleled by any change in the patient's mentality. Nor is it usual to see practice phenomena of this extent in pathological cases. Three series of experiments with case 84 (probable dementia præcox on psychopathic basis) again point to slower performance at times of greater excitement. His records are nearly errorless and reach unusual speeds.

25. For case 79 (circular manic-depressive), there are two complete series some ten weeks apart. Two weeks previous to the earlier of these a series was attempted, but the patient was in too great an excitement, and his adjustment broke down after accomplishing 11 lines of test NC. This record required 295 seconds for 11 lines with one error. The marking begins with single strokes; overmarking starts at the fifth line and increases to the termination of the record. In the two complete series, this excitement was subsiding. The last, less excited series is much more rapid than the earlier. Both show characteristic errors such as writing *g* for 'any letter except *g*'; *yes* for 'write *no* if 2 times 5 are 10.' It is suggested that tests of this kind be used for objective records of psychomotor excitement.

### III. ASSOCIATION TYPE AND PERSONALITY

BY F. L. WELLS

26. It is desired to complete the presentation of experiments on personality and the association test previously mentioned,<sup>1</sup> and of other data gathered in connection with them.

27. Consensus among investigators of its pathology is that association reactions are more closely related than other experimental procedures to the degree of conflict in the individual's mental trends. The indication of the present study is that such relationship cannot be objectively stated until the conception of mental conflict is better defined, and leads to a scoring of the test more efficient for the purpose.

28. Among the measurable functions of the free association test, reaction times serve chiefly in interpretation of single responses. The formula  $\frac{\text{av. react.-time}}{\text{med. react.-time}}$  has been suggested by Lang<sup>2</sup> as a criterion of the 'psychic resistance' of the subject to the experimenter. The higher the figure,

<sup>1</sup> 'Mental Adjustments' (D. Appleton & Co.), 1917, pp. 262-266.

<sup>2</sup> *Psychologische Abhandlungen*, 1914, 1, p. 2.

the greater the 'resistance.' Practically, a high resistance is thus indicated by reaction times that take excessively long in comparison with the median.

29. The *commonness* or *singularity* of responses is measured in their per cent. of occurrence among large numbers of subjects. This criterion is thoroughly objective, but is limited to the Kent-Rosanoff list of stimulus words, for which alone satisfactory comparative material exists. The frequency of each word is found, and the median of all such findings gives the subject's score in the function. It is designated by the letter *c* ('community' of response).

30. The five logical categories of responses have been formulated in an earlier paper.<sup>1</sup> The mechanism of the egocentric category is inversely related to that of community in response. Correlation (*r*) of  $-75$  is observed with 25 subjects.<sup>2</sup>

31. The supraordinate category has a moderately positive relation to community of response, the *r* being 29. It has correspondingly negative relation to the egocentric category, this *r* being  $-30$ .

32. The mechanism of contrast responses is directly related to that of community in response, their observed *r* being 61. Correspondingly, the *r* of the contrast and egocentric category is  $-62$ ; of the contrast and supraordinate categories,  $-34$ .

33. The *miscellaneous* category sustains with the community of responses an *r* of 25; with the egocentric category, *r* is  $-53$ . The egocentric and miscellaneous categories are a large part of the responses, and increase in one is at the expense of the other. The miscellaneous category is without clear relation to the supraordinate and contrast categories.

34. The speech habit category is uniformly too small to show significant relationship with these other functions of the experiment.

<sup>1</sup> Cf. PSYCHOL. REV., 1911, 18, 229-233.

<sup>2</sup> These include the ten selected individuals of paragraph 41 and are thus subject to a dilation of the *r*'s. However, the *r* between *c* and egocentric category is  $-74$  in two arrays of 14 unselected subjects. In an array of twenty-five subjects not thus selected the *r* of supraordinate and contrast categories is  $-51$ .



35. Relationships of present concern are the positive relation of the community index  $c$  with the contrast category, and the negative relation of these two to the egocentric category. On these lines are drawn the objective differences in association type. At one extreme is the type with low  $c$ , many egocentric and few contrast reactions. At the other extreme is the type showing (see paragraph 37) high  $c$ , few egocentric and many contrast reactions.

36. Superficial rating was obtained of fifteen persons in respect to such traits as intelligence, talkativeness, promptness, persistence, tendency to shape surroundings, tactfulness, conscientiousness, truthfulness, cheerfulness, depth of mood, self-appreciation, sensitiveness. When compared with above differences in the free association test, no separation of groups according to any of the ratings is indicated. Significant relation is shown neither with traits where previous experience has not indicated relationship, such as intelligence, promptness, self-appreciation, nor with traits where correlation is more to be expected, as depth of mood, sensitiveness, conscientiousness.

37. Quantitative idea of this situation is afforded in the following figures, which separate the subjects according to their records, more resembling the one or the other of the above types (paragraph 35). The average position of either

TABLE III

	Group of Lower $c$ More Ego-centrics, Fewer Contrasts		Group of Higher $c$ Fewer Ego-centrics, More Contrasts	
	Av.	M. V.	Av.	M. V.
Intelligence.....	8.8	4.7	7.4	3.3
Talkativeness.....	8.7	3.0	7.6	4.2
Promptness.....	8.2	4.5	7.9	3.2
Persistence.....	5.7	3.3	9.6	3.3
Self assertion.....	7.7	4.3	8.2	3.3
Tactfulness.....	7.7	4.0	8.2	3.5
Conscientiousness.....	6.5	3.8	9.0	3.5
Truthfulness.....	6.0	4.2	9.3	3.4
Cheerful mood.....	7.0	2.3	8.7	4.3
Depth of mood.....	6.2	3.5	9.2	3.5
Self-appreciation.....	8.5	3.2	7.7	4.3
Sensitiveness.....	7.0	4.0	8.7	3.7

(The *smaller* the Av. figure, the *more* marked is the trait)

group in a trait, and its mean variation, is compared with these figures for the other group.

38. Personality-type *A* from the group of paragraph 36 is an individual rated as especially intelligent, prompt, persistent, active (shaping surroundings), conscientious, truthful, deep of mood, sensitive, not at all loquacious. Personality-type *B* is an individual rated as especially intelligent, prompt, active, self-appreciative, loquacious, superficial in mood; rated low in truthfulness, conscientiousness and sensibility. As contrasted, *A* is quiet and *B* is loquacious; *A* is conscientious and truthful, *B* is much less so; *A*'s mood is deep and *B*'s is superficial; *A* is sensitive, *B* is insensitive.

39. Initial association tests of these two personalities compare as follows.

	Time	Middle Quartiles	<i>c</i>	Per Cent in Categories				
				Eg.	Su.	Co.	M.	S. H.
<i>A</i> .....	13.4	7.4	2.2	58	4	1	34	3
<i>B</i> .....	9.5	3.6	3.8	14	1	31	50	4
Norm. Av.....	12.1	6.2	8.6	27	9	16	46	2

The range of the middle quartiles is subject to about the same interpretation as Lang's 'coefficient of resistance.'

40. The record of *A* approximates the normal limit of individuality of response, egocentric and contrast categories; these functions standing in their normal relationships. *B* is near the opposite normal limit in respect to egocentric and contrast associations. The above recorded relation of *c* to these categories is not sustained in this subject. The associations are not so individual as *A*'s, but much more so than the average. The direction of all differences in the figures is consistent with the personality ratings of paragraph 36. Reaction time and logical categories represent the difference in good proportion; *c* fails to do so.

41. A selection was made, outside the experimenter's knowledge, of five new persons supposed to temperamentally resemble *A*, and five persons resembling *B*. Figures on which the subjoined comment is based are given herewith:

	Med. Assoc. Time.	Per Cent. which Av. is of the Med.	Fre- quency <i>c</i>	Eg.	Su.	Co.	M.	S. H.
<i>A</i> -Selection subjects: 1....	8.9	1.05	3.8	42	2	7	49	0
2....	11.6	1.22	3.9	52	10	0	37	1
3....	16.3	1.25	3.6	42	16	0	40	2
4....	11.2	1.05	6.7	32	14	2	49	3
5....	11.5	1.16	5.4	32	12	1	53	2
<i>B</i> -Selection subjects: 6....	9.7	1.27	5.4	41	9	1	47	2
7....	17.8	1.21	3.4	44	6	0	48	2
8....	17.2	1.18	14.3	18	21	16	43	2
9....	11.2	1.50	14.6	26	18	16	38	2
10....	9.7	1.04	15.5	9	7	29	53	2

42. Previously observed relation between reaction time and egocentric response is not absent from this material. Lang's criterion of psychic resistance also shows correspondence to egocentric reactions. Neither time function effects a separation of groups.

43. A control of the intellectual factor in association time as distinguished from affective and volitional ones was sought in giving an opposites test (Woodworth-Wells No. 1) in connection with the free association test. Nothing of significance for the problem was indicated in this direction.

44. The four words *high*, *white*, *slow* and *long* occur in both Kent-Rosanoff and opposites lists. A comparison is afforded of reactions to the same stimulus under free association and opposites 'mental sets.' For all subjects, the median reaction time in the free association set is 11.5, in the controlled association set it is 6.2. In nine cases a subject responded identically to a stimulus word in both forms of test. The median time of these cases is for free association 13, for opposites 6.

45. In respect to *c* there is cleavage between seven subjects having *c* of 6.7 or less, and three subjects having *c* of 14.3 or more. These three subjects are of the group selected as *B*. It follows that all the *A* selections and two of the *B* had low *c*.

46. In egocentric reactions the above three subjects have a per cent. of 26 or less, the remaining subjects a per cent. of 32 or more.

47. In respect to contrast reactions these three subjects have a per cent. of 16 or more, the remaining subjects a per cent. of 7 or less.

48. In the other categories there is no cleavage. Upon one side of the observed cleavage are all the *A* selections and two of the *B* selections.

49. Negative findings of the foregoing are open to interpretation that the scoring of the test is faulty. This is in part true. Different weights should probably be assigned to different sorts of egocentric reactions. While this is the chief improvement to be seen at present, it is not expected that it would change the situations essentially. The three factors of *c*, egocentric and contrast associations, are also to be weighted and combined. This process requires adjustment of the weights to give the best correlation with the trait to be measured. The weighting is not yet practicable because an independent and controlling measure of mental conflict has not been determined.

50. Failure in the mental rating of the subjects is the more important factor in the failure of correlations. Estimate of mental conflict in a personality, without special analysis, is subject to large error. Study of pathological material introduces a further consideration, the relation of the mental conflicts to consciousness.

51. The relation of the test to personality is hardly so simple as Jung, Pfister or Ferenczi state it. From the clinical record of the test, its fidelity to type, and its intercorrelations, it is improbable that this relation is too complex to formulate. Its practical application remains an art.

#### IV. AUTISTIC MECHANISMS IN ASSOCIATION REACTION

BY F. L. WELLS

52. This discussion is immediately based upon association records in the Kent-Rosanoff experiment upon 21 pathological cases, whose data are summarized in the accompanying table. Certain of the cases have designations identical with those in

the study on cancellation and directions tests. These allude to identical individuals.

TABLE IV

## ASSOCIATION REACTIONS IN PSYCHOTIC INDIVIDUALS

The ages given in the footnotes are those at the time of the single or first experiments

	16 <sup>1</sup>	17 <sup>2</sup>	21 <sup>3</sup>	23 <sup>4</sup>	24 <sup>5</sup>	29a	29b <sup>6</sup>	36a	36b <sup>7</sup>
Median reaction time (0.2'') . . . .	14.2	10.9	9.3	10.6	9.6	14.1	11.1	29.0	8.0
c, index of community . . . . .	1.0	9.8	9.3	11.0	4.4	.23	.29	11.5	6.5
Per cent. egocentric responses . . .	59	8	8	17	37	31	37	19	13
Per cent. supraordinate responses .	0	3	3	1	17	0	0	27	21
Per cent. contrast responses . . . .	1	30	30	28	0	0	0	13	7
Per cent. miscellaneous responses .	30	54	53	49	46	25	24	41	58
Per cent. speech habit responses .	10	5	6	5	0	44	38	0	1

	45 <sup>8</sup>	46 <sup>9</sup>	57a	57b <sup>10</sup>	59 <sup>11</sup>	62 <sup>12</sup>	66a	66b <sup>13</sup>	71 <sup>14</sup>
Median reaction time (0.2'') . . . .	16.5	36.6	50.5	47.6	17.4	16.0	22.7	10.8	13.2
c, index of community . . . . .	.62	3.0	2.0	.30	4.3	.87	3.0	10.0	.80
Per cent. egocentric responses . . .	51	48	39	50	7	43	14	8	46
Per cent. supraordinate responses .	7	1	22	10	0	4	28	14	0
Per cent. contrast responses . . . .	1	3	4	3	12	2	7	24	4
Per cent. miscellaneous responses .	37	47	33	27	79	44	48	48	42
Per cent. speech habit responses .	4	1	2	10	2	7	3	6	8

	73 <sup>15</sup>	74a	74b <sup>16</sup>	75a	75b <sup>17</sup>	79 <sup>18</sup>	84 <sup>19</sup>	93 <sup>20</sup>	98 <sup>21</sup>
Median reaction time (0.2'') . . . .	13.2	11.2	8.6	18.0	25.0	11.0	13.1	13.0	11.4
c, index of community . . . . .	2.1	9.5	14.0	1.3	.30	5.5	6.5	13.0	3.4
Per cent. egocentric responses . . .	72	10	5	43	53	24	16	19	23
Per cent. supraordinate responses .	5	8	5	2	0	6	2	14	5
Per cent. contrast responses . . . .	1	35	33	0	0	14	27	17	14
Per cent. miscellaneous responses .	22	45	56	51	47	51	55	47	53
Per cent. speech habit responses .	0	2	1	4	0	5	0	3	5

<sup>1</sup> Man, thirty-nine. Inferior personality. As a child "reticent, cold, hard to understand." Manic depressive, improvement from fifth attack (excitement). Disorder chiefly formal, without conscious trends.

<sup>2</sup> Man, thirty-six. Somewhat shut in personality. Tense, agitated depression. Other symptoms such as would be secondary to this. Trends from the unconscious not elicited. Dementia præcox outcome.

<sup>3</sup> Man, sixty. Average personality. Manic-depressive depression, second attack. Formal depression, somewhat agitated, without trends.

<sup>4</sup> Man, thirty-three, capable personality, history of syphilis. Manic-depressive depression with firm delusion of having General Paralysis as result of luetic infection. Depressive sexual ideas, otherwise no special autistic content.

<sup>5</sup> Man, forty-five. Religious, conscientious personality. One previous attack, precipitated by death of mother. Manic-depressive depression, colored by self-accusation for persistent masturbation. No other autistic content.

<sup>6</sup> Man, twenty-seven, competent personality, rather sensitive. Manic-depressive depression, with disproportionate thinking difficulty, or reticence, or both. Only marked trends are in written self-accusations. Tests are at height of psychosis, and on leaving "much improved."

53. In respect to association time, such separation from the normal as appears is secondary to other factors in the

<sup>7</sup> Man, twenty-one. Psychopathic personality, repeated manic-depressive attacks. Disorder chiefly formal, not exhibiting conscious trends. First record in excited, second in depressed phase.

<sup>8</sup> Woman, forty. Psychopathic heredity; capable personality, first attack at thirty-seven after strain, other attacks following. "Internal rebellion," self-description by patient. Evidence of sexual trends; *e.g.*, instructing younger brother, delusions of marriage.

<sup>9</sup> Man, thirty-four. Some heredity, capable personality. Manic-depressive depression, some general suspicion and ideas of reference, no elaborated delusions. Poor coöperation in test, made at beginning of recovery.

<sup>10</sup> Man, thirty. Normal personality. Typical dementia præcox. Scattering and mannerism evident from admission. Precipitating cause or trends not elicited. Good coöperation in both tests. Much more dilapidated at second test, six months after first.

<sup>11</sup> Woman, nineteen. Overconscientious, avoided opposite sex. Manic-depressive, repeated attacks. At test, recovering from confused phase. Indication of much autistic activity that reaches conscious expression but very fragmentarily.

<sup>12</sup> Woman, fifty. Circular manic-depressive attacks since eighteen. At period of test, mixed phase. Easily offended, demanding much attention, ideas of man concealed in room. Test made at beginning of improvement from this condition.

<sup>13</sup> Man, forty-six. Capable personality, married. First attack of depression, precipitated by worry over attachment to young woman. Fair insight. First test upon admission. Second test eight months later, shortly before discharge, "much improved."

<sup>14</sup> Man, twenty. Psychopathic personality (over-intellectual, imaginative, sensitive, self-centered) plus manic-depressive episodes. During these, some dissociation, and autistic elaborations. Later suicide.

<sup>15</sup> Woman, twenty-three. Religious, intellectual personality. Manic depressive psychosis, fourth attack, depression. Retardation, reticence. Some autistic thinking during psychosis, described retrospectively. Test after substantial recovery.

<sup>16</sup> Man, twenty-four. Psychopathic inferiority, manic-depressive episodes (circular). Psychosis of formal character, intellectual symptoms secondary to motor and emotional state. The first test is in excited phase; the second, four months later, at subsidence of the excitement.

<sup>17</sup> Man, thirty-three. Shut-in personality. Dementia præcox. Rich elaboration of autistic trends. A period of eighteen months, with gradual dilapidation, intervenes between the two tests.

<sup>18</sup> Man, twenty-one. Conscientious, sensitive personality. Manic-depressive mixed phase, second attack. Masturbation complex. No elaborated delusions or hallucinations.

<sup>19</sup> Man, thirty-two. Competent personality. Dementia præcox. Evidence of considerable autistic thinking, not especially systematized in its conscious expression.

<sup>20</sup> Woman, thirty-two. Retiring, overconscientious. Attack precipitated by death of man to whom had been attached. "Voices," ideas of influence, considerable conscious autism. Recovery after treatment of ear condition.

<sup>21</sup> Man, fifty-six. Competent personality. Manic-depressive depression, one previous attack. In this attack, accounts of hallucinations, with religious and marital delusions, subsided at time of test. Alcoholism involved.

cases (as in choice reaction performance, cf. *Am. J. Insanity*, 1918, 75, p. 117). Lengthened time is consistent with depressive retardation, manic distractibility, schizophrenic blocking or apathy.

54. In the above, lengthened time in the second experiment with case 75 marks the progressive deterioration, but fails to do so in the already much blocked 57. Case 36 is depressed in first experiment; second is in excited phase, and very much quicker. In cases 66 and 29 an improvement from depression is also accompanied by decrease in time. In case 74, the same accompanies a subsidence of excitement.

55. Decrease in  $c$  accompanies schizophrenic deterioration (75, 57). Increase in  $c$  accompanies improvements from manic-depressive states (74, 66, 29). Excitement shows lower  $c$  than depression in case 36.

56. Save in marked deteriorations, pathological subjects differ from the normal much less than normal subjects from each other. The difference of normal and pathological is much greater in respect to vital adaptations than in respect to association reactions. It is suggested that interpretation of the last named be sought in mechanisms common to both normal and pathological psychology, rather than in more distinctively pathological processes. The basal conceptions providing such interpretation will be passed in rapid review.

57. There are in every person's mind a number of trends which, when they govern the conduct of vital reactions, result in mental disease. These traits are included in Bleuler's concept of *autism* or *autistic thinking*. Where autistic trends meet those of logical or realistic thinking, a conflict is often set up. These conflicts are what is essentially understood in the conception of mental conflict. To the extent that the person is aware of these conflicts they are *conscious*. Of some there may be no awareness, and they belong to the *unconscious*.

58. Normal voluntary behavior is represented in consciousness; the psychic system that directs it is the 'conscious' personality, the 'main' personality. When various autistic trends normally unconscious or ineffective come to

expression in the consciousness of the personality or the conduct of its body, symptoms of mental disease ensue. The type of symptoms depends on the sort of expression these autistic trends establish. Autistic 'break-through' into the motor sphere is expressed in stupors and in manic or catatonic excitements. 'Break-through' in the affective sphere is shown in mood alterations not consistent with reality as the patient sees it. These are distinctly pathological forms of 'break-through.'

59. In the intellectual sphere, autistic *thinking* proper reaches the normal consciousness through dreams, wit, and various beliefs with rather benefit than harm to the personality. It is further represented in the normal consciousness through sundry mental attitudes, prejudices, intuitions. Deeper emotions, though with conscious rationalizations, have instinctive and autistic determinants. Pathologically, it is a source of delusions and of scattered ideation.

60. The associative stimulus and response is an arc of the 'third' level, normally involving consciousness. The relation between stimulus and response is one known to the subject. It is not necessary that the mediate elements in the association be consciously thought of, but it is within the subject's power to think of them. The indicated conception is that the association reaction is influenced not by mental conflict whether conscious or unconscious, but rather through the representation of the conflicting trends in consciousness. However severe the formal character of the psychoses (Cases 17, 21, 36, 74, 98) the association type is influenced only as the content of the conscious is affected.

61. The content of the conscious is thus affected in two ways. The normal presence or pathological break-through of autistic trends into the conscious, often results in mental conflicts that induce a special attitude of self-reference, somewhat as there is greater consciousness of a bodily organ when 'conflict' of its functions occurs. A natural expression of such attitude of self-reference, in the association experiment, is the egocentric-predicate type of reaction. The responses may be extremely unusual, though preserving a relevancy con-



sistent with normal experience.<sup>1</sup> Examples of this reaction type are the records of Cases 24, 45, 62, 71, 75. This type results from the conflicting interaction in consciousness of the autistic trends and those of logical, realistic thinking.

62. Autistic trends may be intruded on the main personality with little if any conscious relation to the trends of realistic thinking. The latter are simply displaced for the occasion. This is the mechanism of 'scattered ideation,' Association responses so determined are egocentric in that they are highly individual, but they cease to have a relevancy consistent with normal experience. This type is mostly seen in the schizophrenic forms of mental disease. Examples from the present material are the records of Case 57. Records exemplifying the above types are fully quoted in the monograph of Kent and Rosanoff.

63. Egocentric reactions are not a direct result of the autistic conflict. They express rather a usual concomitant of such conflict, as attitudes of self-reference (paragraph 61). In cases 23, 59, 79, 84, 93, some notable degree of pathological autism fails to so affect the association reactions. Though in the presence of grave mental disorders, not without constitutional factors, objective association types are produced. The egocentric type would be more consistent with the contents of these psychoses as they are recorded. It is not indicated what is the special relation of these autistic intrusions to the main personality, through which objective association type is preserved.

64. In case 16, the egocentric reaction type appears less a product of the psychosis as such than of the personal constitution. Either factor is such as to account for the reactions exhibited by cases like 71 or 75. The heaping up of speech habit responses in Case 29 is unexplained. These are often lengthened, giving the appearance of repression phenomena in line with the atypical reticence of this case. Otherwise the type is egocentric.

<sup>1</sup>Through such channels as described in the 'Appendix' to the Kent-Rosanoff frequency tables.

## V. EXPERIMENTS CONCERNING THE THRESHOLD OF CONSCIOUS LEARNING

BY F. L. WELLS

65. The technique is derived from that described by Benedict and Dodge for the study of memory.<sup>1</sup> Its special features here are sufficiently described in the instructions to the subject, which are as follows:

In the aperture before you, where you see the white paper now, there will appear gradually, and one by one, a series of one syllable English words, each word being followed by a single figure, like this: short 2, cream 4, court 1. As soon as you see the figure strike the corresponding key on the board at your side. For the figure 1 strike the first key with the thumb; for the figure 2 strike the second key with the forefinger; for 3 strike the third key with the middle finger; for 4 strike the fourth key with the fourth finger; for 5 strike the last key with the little finger. Thus in the first case above, short 2, you would strike the second key, with the forefinger.

These same words will then be repeated several times and each time one of the words appears, strike the key for the figure that follows it, as soon as possible, no matter whether the figure itself is where you can see it or not. Of course the first time you will not know what the figure is until you have seen it, but as the words come over and over again you will learn what figures correspond to what words, and will be able to strike the right key as soon as you have seen enough of the word to know what it is. Wait until you have seen at least the first letter of the word before you strike the key for it.

Do this as quickly as you can do it correctly. Strike each key with a quick, sharp motion, so as to make sure of a good contact, but letting it up again as soon as a good contact has been made (as a typewriter key would be struck).

66. The stimuli were printed on sheets of paper to fit the kymograph. Recording thereon was with the Dodge double marker for reaction and time line. The speed of the drum approximated 14 mm. per second. False reactions

<sup>1</sup> 'Psychological Effects of Alcohol,' Carnegie Institution of Washington, 1915, pp. 129-30.

were separately recorded on a ribbon register. Observations up to as many as 50 rounds have been thus made.

67. The subject is free to react as soon as he sees the first letter of the word. As such short reactions as these are scarcely evident in the records, it is indicated that time of reaction after a response has been learned, is governed by visual recognition of the word to which the number attaches, or of the number itself. It is still objectionable that effort may be made to memorize the succession of figures without reference to the words to which they attach. This would produce reactions in effect premature. It is owing to this situation that the work is not carried further than is here reported. It is desirable to vary the order of the stimuli, which is not practicable with the kymograph exposure method.

68. A set of cards was prepared, each of which bore a word cut from a kymograph stimulus sheet, without its number. At specific times in the experiments, these cards were shown in varied orders to the subject, who gave verbally the number attaching to each word as well as he could remember it. This gave information of the subject's verbal knowledge of the pairs at different points in the experiments. The observations with the cards were regularly made at the beginning of an experiment, after the first ten rounds with the kymograph, and at the conclusion of the experiment. The score in this portion of the experiment is the number of right answers minus the number of wrong answers.

69. Items of results presented are as follows:

1. Initial knowledge of pairs, *i.e.*, of numbers attaching to words presented on cards at the beginning of experiment, score right answers minus wrong answers.

2. Rounds 1-10, number of false responses as derived from ribbon record.

3. Rounds 1-10, number of delayed responses, *i.e.*, those responses which are longer than the initial response, indicating a loss of learning effect, through either immediate distraction or forgetting.

4. Rounds 1-10, number of anticipatory responses, *i.e.*, those which come before the full exposure of the stimulus.

As shown in the profiles, there is continuous gradation from these to the responses requiring full reaction time after the exposure. In the above anticipatory responses, correct response is evidence that the subject well knew what the response should be, before seeing the number representing it.

5. Knowledge after round 10 of the numbers attaching to the words, derived as for item 1.

6. Rounds 11-20, number of false responses, derived as for item 2.

7. Rounds 11-20, Number of responses delayed beyond the time of initial response in round 1.

8. Rounds 11-20, number of responses delayed beyond the time of response in round 11.

9. Number of anticipatory responses, derived as for item 4.

10. Knowledge of pairs after round 20, derived as for item 1.

70. Results are summarized in the accompanying table:

TABLE V

Subject.....	I				II			
	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>
Designation of Experiment.....								
Date .....	1-18-16	1-20-16	1-22-16	1-24-16	1-26-16	1-26-16	1-27-16	9-6-16
1. Initial verbal response (right minus wrong).....	X	3	16	20	-1	3	4	3
2. Rounds 1-10, no. false responses.....	5	9	4	0	4	7	5	0
3. " " no. delayed responses.....	39	14	19	35	54	27	36	24
4. " " no. anticipatory responses.....	30	29	101	129	0	7	19	3
5. Knowledge after rd. 10 (right minus wrong)....	1	10	20	20	-1	6	11	10
6. Rounds 11-20, no. false responses.....	0	2	5	5	3	5	8	2
7. " " no. responses delayed over rd. 1. 60?	16	41	30	5	7	29	12	
8. " " " " rd. 11 22?	16	56	64	36	35	64	68	
9. " " no. anticipatory responses..... 0?	29	84	122	0	18	44	16	
10. Knowledge after rd. 20 (right minus wrong)....	6	10	19	20	1	12	18	11

Notes: Experiment *a* with both subjects contains ten and five kymograph rounds; experiments *b*, *c*, and *d* contain ten and ten. The figures questioned in experiment *a* of Subject I. are anomalous.

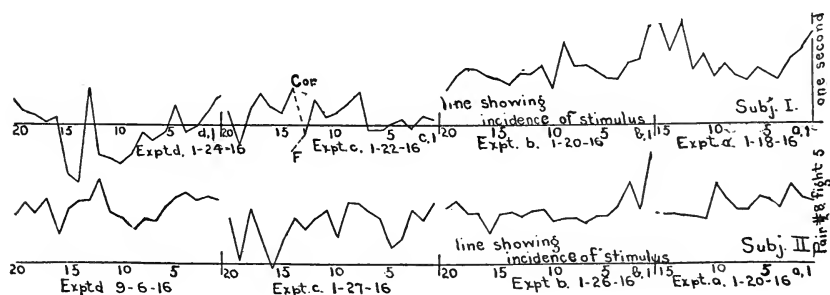
71. Learning in the experiment is measured in two ways, by the verbal responses to the cards and by the key responses to the kymograph exposures. The verbal responses above

show learning by their progressive increase in score (items 1, 5, 10). Progress of learning during one experiment, as well as loss of learning from the end of one experiment to the beginning of the next, are here illustrated. Subject I. makes practically perfect score after the tenth round on the third day (item 5). In the long interval between the third and fourth experiments with Subject II., more has been lost than is made up in the fourth experiment.

72. The kymograph exposures show learning through the increased number of anticipatory responses (items 4 and 9) and earlier time of their appearance, both progressive through the experiments. The last experiment with Subject II. again shows excessive loss from the long interval.

73. False reactions and delayed responses demonstrate neither progressive tendency nor individual difference.

74. Responses in these experiments are essentially voluntary, requiring mediation of consciousness. After the first stimulation-reaction, its pattern has been experienced, and may be established in consciousness. More usually it is not at once so established, but requires several repetitions, during which it moves up towards the threshold of consciousness, occasionally crossing it and being 'remembered,' then slipping below it and being 'forgotten,' before it is well established as part of the subject's knowledge. Movement across the threshold of conscious knowledge is here indicated in gradual and irregular shortening of the reaction times. The threshold is actually crossed where the response is anticipatory, coming before the stimulus is perceived.





75. The accompanying profiles illustrate this gradual and irregular shortening of reaction time through successive rounds of the kymograph. At the beginning, the subject did not know the number attaching to the word.

76. Attention is invited to records of individual stimuli, with reference (1) to the selective character of the learning process, (2) to the relation of the learning shown by verbal and by key responses. Information is offered in the accompanying table. (See p. 386.)

77. Responses at the ends of memory series are expected to be learned more readily than those in the middle. This partly but not generally characterizes the present material. Memory for verbal responses appears receptive to certain pairs as such; Subject I., pairs 10, 15, 16, 17; Subject II., pairs 5, 6, 16, 19, 20. It appears refractory to other pairs such as in Subject I., pairs 9, 13, 14, 18; Subject II., pairs 8, 9, 14. (Table VI., items 1 and 2.)

78. The most anticipatory responses are shown by Subject I. in pairs 5, 7, 1, 6; by Subject II. in pairs 2, 20, 5, 16. Fewest are shown by Subject I. in pairs 12, 13, 14, 18; seven pairs have no anticipatory responses by Subject II. (Item 3).

79. Above indicated lack of parallelism between memory for verbal responses and memory for key responses is further evident (Items 4 and 5). Pairs are quickly learned to anticipatory key response but correct verbal response is not assured at the next trial or until some trials later; Subject I., pairs 2, 5, 6, 9, 19; Subject II., pairs 1, 2. Again and more frequently, correct verbal responses are not associated with anticipatory key responses until somewhat later in the experiment. (But many responses not so listed are virtually anticipatory, since though not occurring before full exposure of stimulus, they occur less than true reaction time after such exposure. See profiles.)

80. Following observations are also pertinent to the above. In Subject I.: pair 6, though well known verbally at and after 20*b*, elicits but two anticipatory reactions during the first ten exposures of experiments *c* and *d* (at 9*c* and 8*d* respectively). In pair 15, verbal response fails at 20*b*, though

the key response at 20*b* is anticipatory. In Subject II.: pair 1, between two false verbal responses at 1*b* and 10*b*, anticipatory key response occurs at 9*b*. Pair 2, anticipatory responses occur at 9*b*, at 2*c*, 5*c*, 6*c*, 7*c*, 8*c*, and at 15*d*, with negative or false verbal responses both preceding and following them. Pair 7, at 20*d*, a false verbal response is given after three correct verbal responses preceding, and after 5 anticipatory key responses in the preceding ten rounds. On the other hand in pair 8, the sole correct verbal response given falls at 20*c*, after the sole anticipatory key response at 16*c*. (See plate, paragraph 75.)

81. Knowledge both for verbal responses and for anticipatory key responses fluctuates above and below the conscious threshold where it becomes effective for volition. When knowledge for verbal response is below the threshold of consciousness, negative or false responses are produced. When but slightly above it, doubtful responses are produced. When knowledge for key responses is below the threshold of consciousness, response requires true reaction time after exposure of the stimulus. As this threshold is approached and crossed, this time is shortened to anticipation. The conscious is continuous with the unconscious, the threshold an imaginary line.

82. It is not unreasonable to infer that all gains and losses of knowledge represented in these observations, are fluctuations of a completely associated knowledge of verbal response and of key response, which at any given time is equally effective for both. This cannot be checked, as verbal response and key response cannot be measured at the same moment without associating them irrelevantly to the purpose. The indication is of two partly dissociated sorts of knowledge, each capable of moving at different rates and in different directions, with reference to the conscious threshold.



## CHANGES OF APPRECIATION FOR COLOR COMBINATIONS

BY STEPHEN C. PEPPER

The present experiment<sup>1</sup> was prompted by some cases that have come under my observation of evident change in the appreciative attitude of certain persons towards colors and color combinations. Artists who in the schools could admire only combinations of unsaturated colors, have so changed in their attitude that now they admire not only combinations of saturated colors but combinations of saturated colors at short intervals apart, combinations which they would have abhorred in their younger days. That changes in persons' appreciative attitude towards musical harmonies takes place is a well-established historical fact that has received experimental confirmation through the investigations of H. T. Moore.<sup>2</sup> I wished if possible to find experimental confirmation for the changes I had observed in persons' appreciative attitude towards color combinations. I resolved, therefore, to stimulate subjects repeatedly with combinations of colors in an attempt to produce experimentally such changes.

### METHOD

The experiment was carried on in a dark room. The subject sat on a stool in the dark room facing a window which was covered with a black shutter. On the other side of the shutter, so arranged that it was the only object visible to the subject when the shutter was raised, was a light gray cardboard two feet square tipped at an angle of approximately 45° and illuminated by a daylight lamp swung overhead. In the middle of the cardboard an opening 3½ in. square was cut, behind which slides of colors could be slipped.

<sup>1</sup> Conducted in the Harvard Psychological Laboratory under the direction of Prof. H. S. Langfeld.

<sup>2</sup> Moore, Henry Thomas: 'The Genetic Aspects of Consonance and Dissonance,' *PSYCH. MONOG.*, No. 73, Sept., 1914.

The Bradley colors were used in the saturated hues, violet, red-violet, violet-red, and red, with a tint and a shade of each hue—twelve colors in all. The choice of violets and reds was the result of a hasty preliminary examination of my subjects in all the hues, which seemed to indicate a greater dislike for combinations of reds and violets than for those of any other narrow range.

#### PART ONE

The first part of the experiment had for its purpose the determination of the subjects' average consistency in judgments of color appreciation, and the establishment of a basis for the choice of combinations to be used in the second portion of the experiment.

This double purpose was served by causing the subjects to give two judgments for each combination of colors within the range chosen. Since there were 66 possible combinations within the range selected, each subject gave 132 judgments in the first part of the experiment. Two slides were slipped behind the opening in the cardboard and placed side by side, so that half of each color showed, one to the right and one to the left. There was no interval between the slides; the edge of one slide abutted the edge of the other. To avoid the space error, the position of the colors in combination were exchanged when presented a second time. And by setting the colors side by side, not one above another, the effect of apparent weights of colors was minimized (though even as it was some subjects were disturbed by this factor).

After entering the dark room, the subject was allowed about five minutes to become dark adapted before the colors were shown; and then was given the following instructions: "Grade the combinations of colors you see in accordance with your liking of them, A+ being highest, E— lowest, and C neutral. Do not compare combinations with one another, and make your judgment of the *combination* of the colors, not of the colors separately." The last phrase was inserted to focus the attention on the combination. Frequently subjects found they could make two distinct judgments which would vary greatly, the one upon the general impression the colors

produced, the other on their suitability for combination. Since it was the combination we were interested in, the former type of judgment was systematically rejected.

After these instructions were given, the shutter was raised and the colors were exposed for five seconds. Then the shutter was lowered and the subject gave his judgment. When a minute had elapsed (affording time for after-images to lose their intensity), another combination was exposed. And so on at the rate of a little less than a combination a minute. As many judgments were recorded as could be made in the course of an hour. No regular method of determining the order of combinations to be exposed was observed. The combinations were selected at random, effort being made only to avoid showing the same combination twice in close succession.

When all the judgments had been made, the average consistency of each subject was estimated and may be seen in the third column of the table. In these averages a unit means a grade, or the difference between B- and B. The average consistency of the subject was estimated as follows: As stated, each subject gave two judgments on each of the 66 combinations. These pairs of judgments were compared and the amount of deviation between them set down. Thus if a subject's first judgment on a combination were D+ and his second C, there would be a deviation of two points. These deviations were all set down and then added up. The total sum of deviations was then divided by 66, the number of combinations observed, and this gave the average deviation for a combination, or the subject's average consistency.

Also the general average judgment for all the combinations was estimated and may be seen in the second column of the table. The general average of a subject was obtained by reducing the letters representing his judgments to figures. Thus A+ = 1, A = 2, A- = 3, B+ = 4, and so on. Each judgment would in this way be represented by a number. The numbers representing all the judgments of a subject were added together, and this sum divided by the total

number of judgments given. The number so obtained would be the number of the subject's average judgment. This number was interpreted into its corresponding letter (*e. g.*, if it were 4, it would be interpreted as B +) and this letter would be the average grade of the subject.

In all cases but one, namely subject No. 2, the subjects were kept in ignorance of the purpose of the experiment.

## PART TWO

From all the combinations six were chosen for each subject to constitute the basis of experimentation in the second portion of the experiment. Whenever possible three of these combinations were D's, one an A, one an E, and one a B or a C. In all cases but one, three D's were obtainable, but in that one case (subject No. 1) there actually were no D judgments given. These selected combinations were then presented to the subject in the same manner as before. But the instructions were changed as follows: "Combinations of colors will be shown to you as before. But now as far as possible inhibit all judgments about them. Simply look at them as long as they are presented to the eye."

After four weeks the subjects' judgments on the combinations with which they had been stimulated were checked up. Since it was desirable the subject should not remember from time to time the judgments he had previously given on a combination, and also that he should make the judgments on the selected combinations as naturally as possible, the six selected combinations were distributed at random in a group of fifteen or twenty other combinations. All of these combinations were then exposed before the subject one after the other exactly as in the first part of the experiment. And the subject was given at the first of the hour the same instructions as in the first part of the experiment.

The new judgments for the selected combinations were recorded and compared with the old ones. In this manner the experiment was continued until the end of the year—four hours of exposure without judgment, then a checking up, again four hours of exposure, and again a checking up.

## RESULTS

More than a third of the judgments showed a change of appreciation at the end of the experiment over the beginning exceeding the average consistency of the subject, this change on the whole being toward neutrality. The total number of points gained or lost for each grade was as follows: The total gain for E's was 9; loss 0; for D's 21, loss 9; for C's 1, loss 3; for B's 2, loss 20; for A's 0, loss 13. While there is some evidence of a change in individual æsthetic judgments, the results are not sufficient to warrant a definite conclusion or more detailed analysis.

The experiment, however, brought out some other results of a more definite character. Chief among these is a movement of the *average* of æsthetic judgments (in contrast with a movement of individual judgments). The evidence for this movement is based on a comparison of the average judgments of the subjects considered in relation to their individual experience in color observation. The subjects fell naturally into two classes on the basis of their color experience—an experienced class and an inexperienced. The experienced class comprised an artist (subject No. 1), an æsthetician (subject No. 2), and an experienced layman (subject No. 3) who had somewhat more than average interest and experience in colors. Of these the artist had most experience with colors, the æsthetician next, and the experienced layman next. The other four subjects comprised the group of inexperienced subjects and so far as could be ascertained at the beginning of the experiment they were of about an equal grade of inexperience. In view of these individual differences the average grades of the subjects for all the combinations used in the first part of the experiment are exceedingly illuminating. The average of all the combinations for the artist is B, for the æsthetician C, for the experienced layman C. For the rest of the subjects it ranged from C— to D (two C—'s, a D+ and a D). No inexperienced subject had a grade as high as an experienced subject, and among the experienced subjects the artist who was the most experienced received the highest grade. In other words, the greater the experience of a

subject in color combinations the higher the average grade of appreciation. Upon continued stimulation with color combinations, a person's average æsthetic judgment for all combinations tends to be higher than before.

The same tendency may be seen from another point of view by examining the number of E's in the experienced group compared with that in the inexperienced group. No. 1 had no E's; No. 2 had five; No. 3 had four. These made up the experienced group totalling, nine E's with an average of three apiece. In the inexperienced group, No. 4 had five E's; No. 5, thirty-four; No. 6, twenty; and No. 7, seventeen—a total of seventy-six E's with an average of nineteen per person. The E's are evidently much scarcer for the experienced subjects.

It is noteworthy too that all the E's tested in the experienced group were raised: an E+ to a D+ in subject No. 2, and E to a D— in subject No. 3, both raises being more than the average consistency of the subjects. On the other hand, in the inexperienced group out of four E's tested two remained unchanged, one was raised slightly but not above the average consistency of the subject, and only one showed an unmistakable raise. The E level was in unstable equilibrium for subjects No. 2 and No. 3, while it was still firmly established in subjects Nos. 4, 5, 6, and 7.

If these facts indicate a law, then with experience the average grade of a subject should tend to go above neutral C. If the æsthetician and experienced layman had had more experience with colors, there is every reason to believe they would have averaged higher than a C.

The fact that they averaged a C in the particular range of color combinations experimented upon is evidence that they would have averaged considerably above a C in their appreciation of all color combinations. For it must be borne in mind that the particular range of color combinations chosen was selected because in a rough preliminary investigation that range seemed to be the one in the whole gamut of colors most disliked. Hence, if the experienced subjects averaged a C in this narrow range of color combinations, they would

have averaged above a C for all color combinations. It is evident, therefore, that experience in colors not only tends to raise the average appreciation of a subject, but to raise it above neutral C.

To avoid misunderstanding it must be emphasized that repeated stimulation is not the only factor operative in determining judgments of appreciation. A person's character, state of health, or condition of rest or fatigue, all have their influence on the æsthetic judgment, but above all association. A color associated with a violently unpleasant experience will carry the shadow of that unpleasantness into every combination into which it enters. The same is true of associations with pleasant experiences. A certain combination used in the first part of the experiment happened to be the colors of one of the subject's college fraternity. It was quite impossible for him to take a disinterested attitude towards the colors, so that his judgment on that combination was valueless. The effect of association, however, seemed to be minimized in this experiment by the range of colors that was chosen. The violets and reds are not so common in nature, dress, and decoration as most other colors, and consequently the experiment was not so much disturbed by association as would have been expected. It also appeared that the experienced subjects were less disturbed by associations than the inexperienced, as might have been foreseen since experienced subjects have noticed colors so often that whatever associations might once have formed about them would afterwards become blurred and neutralized, while inexperienced subjects notice colors so rarely that when once a color is noticed the whole context in which it appeared is likely to be recalled when next the color is seen.

A subsidiary point of some interest brought out in the course of the experiment was that the greater the subject's experience with colors the higher was his average consistency or assurance of judgment. All in the experienced group had an average consistency of less than 2, and in about the order of their experience. Subject No. 1 who had the greatest experience had a higher consistency than No. 3 who was less

experienced. Though subject No. 2 was less experienced than No. 1, that subject's exceptionally high consistency may be attributed to long introspective training, since he was a trained psychologist. All the inexperienced subjects except No. 4 had an average consistency of 2 or more. No. 4's average consistency was 1.8, which though high was not so high as the lowest of the experienced subjects. No. 4, it may be recalled, is the subject who had the small number of E judgments among the inexperienced subjects.

### SUMMARY OF RESULTS

- I. The average of æsthetic judgments of color combinations
  - (a) is higher for experienced than for inexperienced subjects,
  - (b) has a tendency to rise above neutral C.
- II. The influence of association on æsthetic judgments of color combinations is less prominent in experienced than in inexperienced subjects.
- III. The average consistency of æsthetic judgments of color combinations is higher in experienced than in inexperienced subjects.

TABLE

Subject	Average Grade	Average Consistency
No. 1	B	1.5
No. 2	C	1.2
No. 3	C	1.7
No. 4	D+	1.8
No. 5	D	2.0
No. 6	C-	2.6
No. 7	C-	2.6



## PATIENCE WORTH

BY CHARLES E. CORY

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The reader has probably had his attention called by the press to the writings of the author known as Patience Worth, and to the discussion that has arisen concerning them. During the past two years it has been possible for me to study the phenomenon at close range. Some of its features are of interest to psychology.

It should be said at once that the case is one upon which no satisfactory report can be made without the aid of hypnosis. Anything like a real explanation of the problems to be solved requires data that can be obtained in no other way. These problems are, as will be seen, (1) subconscious memory and perception, (2) subconscious thought. Without hypnosis these problems lie largely in obscurity. Hypnosis has been refused because of a fear that it might injure or destroy the ability to write, and not, I believe, through a desire to avoid a thorough investigation. It has seemed, however, worth while to report the case, and to show what the problems really are in the light of facts that have been verified. To this general statement I shall add, at the close, some reflections of a tentative character.

In describing the case it will make for clearness if the reader will understand that Patience Worth, the writer, is a subconscious personality of Mrs. John Curran, of St. Louis. About five years ago Mrs. Curran began to write, automatically, literature of an unusual character. Since that time novels, plays, and poems have appeared. Over fifteen hundred poems have been written. Two of the novels, 'The Sorry Tale' and 'Hope Trueblood,' have been published by Henry Holt & Co. Four additional novels are in various stages of completion. Most of this literature is conceded by critics to be of a very high order. No two of the larger works

are written in the same English. They range from a very old English to one that is in nearly all respects modern. *The New York Sun* said of 'Hope Trueblood': "It is a story that George Elliot might not have been ashamed to own up to." Of the 'Sorry Tale,' *The New York Times* in reviewing it, said: "The long intimate tale is constructed with the precision and accuracy of a master hand. It is a wonderful, a beautiful, and a noble book." And from the *Boston Transcript's* review of the same book I quote: "The thoughtful reader will marvel at its beauty, its poetry, its power. The impression is that it is the work of a literary artist." Mr. Reedy, the editor of *The Mirror*, and a distinguished critic, says of it: "This is the most remarkable piece of literature I have ever read. I have no hesitation in saying that this production (I ignore any religious claim for it, and I discount that adventitious interest in the manner of its appearance) is a world literary marvel."

Mrs. Curran is a woman thirty-five years of age. Her education did not extend beyond the grammar school. Her general reading has been meager and desultory. She has not been abroad, and has travelled but little in America. She has no experience or practice in writing, and never entertained any aspiration to authorship. The one ambition of her life has been to sing. She has a good voice, and until Patience Worth 'arrived,' her entire energy was given to its cultivation. No one could be more surprised and mystified by what has taken place than Mrs. Curran. It should not be inferred that Mrs. Curran is an unintelligent woman. On the contrary, she is very intelligent. Her quick intuitive understanding is recognized by all who know her well. A conversation with her, however, though based upon an extended acquaintance, does not give the impression that one is in the presence of the mind that wrote 'The Sorry Tale.' And so foreign to her entire life do Patience Worth and her writings seem, that both she and Mr. Curran have, from the beginning, looked upon her as a disembodied spirit. Toward her they hold an attitude of awe and reverence.

Before writing, Mrs. Curran appears to have no intima-

tion of what is coming. She shares with those present complete ignorance on this point. It may be one of several novels, poetry, or general discourse. Without delay the writing proceeds, and with a speed that frequently outstrips that of the most expert longhand writer. In the most difficult part of a novel she has written as high as five thousand words in an evening. The composition is final. A complete record has been kept of all that has been written. These records, running back to the first manifestations, afford a good outer history of the case. Aside from the literary output, the discussions and conversations fill many volumes.

A thing that gives special interest to this literature is that most of it reflects the life and manners of other times, and this it does with an intimacy that astonishes the reader. They presuppose upon the part of the author, a wealth of information, a richness of contact that is normally secured only through a prolonged study. 'The Sorry Tale' is a large and intricately woven novel dealing with Jewish and Roman life at the beginning of our era, involving an enormous mass of knowledge of the life and customs of that time. It is a powerful drama, full of subtle humor and seasoned wisdom. 'Telka,' an unpublished poem of seventy thousand words, has an English background. The language used is unlike that of her other works. It is an archaic English of different periods, and various localities. It is difficult to understand how it could be used as a medium of poetry by a modern writer. And the source of this language is a part of the general problem. 'Hope Trueblood' has an English setting of the early mid-nineteenth century. 'The Merry Tale' goes back to the days of the cross-bow. It is a humorous tale of rough tavern life. The language is not modern, and the general reader would find it difficult. Only a reading of the million and a half words that have been written can give an adequate idea of the great reservoir of knowledge that is accessible to this secondary personality. A careful survey of Mrs. Curran's reading from childhood leaves the problem of its source largely unsolved. What she may have heard, or rather, what may have been said in her presence, is another

matter, and, obviously, a far more difficult thing to determine. But a knowledge of the interests that have dominated her life forces the conclusion that most of this material did not pass into her mind through the channel of her conscious attention. What her total environment has been, and just how that environment has been appropriated, is the question. And the problem is complicated by the fact that there have been two selves, that is, two centers of apperception drawing upon that environment. Hence no history of Mrs. Curran in terms of an ordinary biography could hope to solve the problem. Not one, but two histories must be traced. So far only one has been followed. And its value, in view of the probability that the dissociation is of long standing, is, and could be, only indirect. It only defines the problem, marking off the residue to be explained by processes which did not enter her consciousness. And there is in the material thus left, much that is significant for the study of subconscious perception and memory.

But even more significant, it seems to me, is the bearing which the case has upon the problem of subconscious reflection processes, or those that are commonly so called. It offers a new answer to the question that is of growing interest, namely: What degree of rationality may the processes of a subconscious center attain? Here there is a product showing a mentality of a very high order. It is original, creative, possessing a delicate sense of beauty, a hardy rationality, and, above all, and perhaps most surprising, a moral and spiritual elevation. Patience Worth easily meets most tests that are applied to the normal personal consciousness. In conversation she displays a quickness of insight, a readiness of repartee that enables her to hold her own in the company of the learned. Mrs. Curran is an intelligent woman, but her mind is much inferior to that of Patience Worth. In short, here is a subconscious self far outstripping in power and range the primary consciousness. This is an indisputable fact, and it is a significant one for psychology. In some way the dissociation has resulted in the formation of a self with greatly increased caliber. It has not only given it access to

a much wider range of material, but it has given it a facile creative power amounting to genius. It is hard to give an adequate impression of the versatility of this mind. Intricate composition proceeds with astonishing ease. During an evening she may write alternately upon several novels, passing from one to the other without a moment's pause. And this work is accomplished without the aid of manuscripts. While writing Mrs. Curran does not have before her the finished portions of these works. A novel untouched for weeks or months will be resumed at just the point where it was broken off.

When writing, Mrs. Curran goes into no trance. In other words, the primary self is not displaced, or it would be more accurate to say that the modification that occurs does not amount to an alternation of personality. To the casual observer no change is noted. There is, however, some abstraction. This is more pronounced than it was formerly. When the writing began, it was read from the board; an *oudja* is used, in the usual manner. But years of practice have made it possible to write with only an occasional reference to the printed letters. A general movement of the hand is now sufficient to throw the letters into the consciousness of Mrs. Curran, and these are rapidly spoken as they appear. Not only do the letters appear but the entire panorama of the story seems to move before her, like pictures on a screen. Within the field of the primary consciousness there is a smaller field, and within this field the characters of the novels act their rôles and are seen as vividly as on a stage. Apparently the only effort required on the part of Mrs. Curran is that involved in passivity. With the proper abstraction she receives from the secondary self the letters and imagery. The meaning of what is written is, naturally enough, frequently not understood by her. Neither its form nor its substance is determined by her consciousness. They are apparently the creation of a self whose existence she is, for the most part, completely unaware of. And this self is no mere by-product of a more fully developed mind. Patience Worth is a personality of tremendous creative energy. And unlike most dissociated personalities she is morally sound.

Is there not here, then, material for a new answer as to what a subconscious mind is capable of doing? But one may ask, is this mind really submerged when it does this creative work? In other words, may it not all be done at the time of writing when Mrs. Curran's abstraction permits a slight emergence above the threshold? And is not this approach to the surface a condition of lucidity? A brief study of the case will eliminate that possibility. It is true that some of the poems are thought out as they are written, that is, they are improvised at the time of writing. And the actual composition of the larger works probably receives at that time its final form. But it is inconceivable that these elaborate and intricately wrought novels should not have been planned before they are so hastily written. And that they are the work of previous thought is confirmed by Patience Worth's own statement. This means that while Mrs. Curran goes about the cares of her household, the other self, unknown to her, may be deep in an English novel. The selves, to repeat, are not alternating but coexistent or co-conscious. Such co-conscious phenomena are now familiar enough to psychology. That which is peculiar to this case is the quality of the mentality of the secondary self. The passage from the primary to the secondary self is not one into a twilight zone or semi-darkness. On the contrary, there is met a mind of a higher order, a mind of decidedly greater power. And of the activity of this self, aside from the slight contact while writing, Mrs. Curran has no knowledge. There is evidence, however, that the secondary consciousness includes much, or possibly all, of the field of the primary consciousness. That is, while Mrs. Curran has no knowledge of Patience Worth, the latter is probably aware, in a way, of all that the former experiences, or may at any time become so.

It is evident that the term subconscious is misleading when used to describe the source of this literature. As generally used it would imply that these works are the product of marginal or submarginal tendencies. This they are, only with reference to the other field. With reference to the self that created them, they are distinctly within the conscious.

The term co-conscious, as used by Prince to describe somewhat similar cases, is helpful here. At all events, and this is the significant thing, these delicate and finely rational processes, these highly elaborate compositions, are performed apparently without the aid or knowledge of Mrs. Curran. Just how these works are composed, and just what the sources for much of the material are, this report has little that is definite to give. It is chiefly interested in attesting to the above facts.

Some reflections, however, have occurred to me in reviewing the case. I accept the judgment that Patience Worth is a genius of no mean order. And, perhaps, there is in the genius of this writer a concrete illustration of what freedom a mind may achieve when released from the inhibitions that clog and check the normal consciousness. She is a dissociated self, and this dissociation has taken place in such a way as to free her from the burdens and concerns of life, from all the claims that split the will and bind the fancy. And perhaps in this fact, and all that it implies, lies the condition of her genius. The division of the self has resulted in a division of labor. To Mrs. Curran falls the care of the needs of the body, and the interests of the social life. Their reactions and distractions are hers. From all this Patience Worth is free. Between her and the entire active phase of life stands the buffer consciousness of Mrs. Curran. In aloofness and abstraction she dwells. She is beyond the reach of perturbation and confusion, and therein lies her strength. Her mind seems to possess the effortless activity, and facility of a dream, a dream without chaos. The normal consciousness is forced constantly to divide its attention between the world of idea and the world of action. The imminence of action is never far removed. Consequently its moments of abstraction or thought are brief and fitful, sustained with effort against the solicitation and lure of sense. The organism requires constant orientation. This holds the attention and divides the energy. It is well that the imagination, in normal life, is weighted and somewhat inert. Action and its world have their claims, and these are, as they should be, strong.

But unless they are to some degree inhibited they tend to absorb consciousness. Fancy is crowded out or sustained with effort. Irrelevant it perishes.

But turn to this dissociated mind and the conditions have changed. The work of adjusting the organism to the environment being left to the other self, the inhibitions which perception places upon the imagination are removed. This sets free and unfettered the mind of Patience Worth. In the realm of the idea she lives, and there she sustains herself without effort. She acknowledges no tie or bond that might take her out of her dream. She is a dreamer that never awakens. And the conditions of this spell are, in a way, the condition of her genius. With her our moments of abstraction, moments that life affords us the luxury of thought and imagination, are prolonged indefinitely. They are, in fact, a fixed condition. In other words, she lives only in a world of thought. And so far she has shown no desire to displace the other self, and alternate with her in the rôle of action. To do so would result in essential modification of her consciousness, and put her under inhibitions from which she is now free.

It is clear that we are here dealing with a mind so constructed as to open up most interesting possibilities. Structurally, the type is so novel that it is hard to imagine either its range or its limitations. Its actual behavior is instructive from every angle, whether it be from that of memory, feeling or thought. In regard to all of these, interesting modifications of normal experience are observable. It is difficult to give an adequate impression of her composition, its ease and rapidity, and no less impressive are her feats of memory, such as reproducing immediately, upon request, an early chapter of 'The Sorry Tale' which had been mislaid, months having elapsed since the time of its writing.

A reference has been made to dreams. While surely very unlike dreams in most respects, there are, I believe, some points of resemblance. Some things are done that would suggest that in a mind of this type the processes have a tempo not normal, probably much accelerated. And since, as in



dreams, this mind is not correlated with action its tempo may resemble more that of dreams than that of normal life. Something like this I know to be definitely affirmed in a case of alternating personalities, that is, one of the selves insists that upon taking control or possession of the body she experiences a noticeable restriction and loss of freedom in her thought. The structural changes involved in such forms of dissociation would provide the explanation for these accounts, should they prove to be statements of fact.

The point in question is one upon which all available data should be brought to bear. The idea would admit significant alterations of normal experience. Processes of mediation, normally requiring considerable time, might approximate immediacy, as seems to be the case in certain dreams, when a highly complex experience presents within itself directly its meaning. Some of the performances of *Patience Worth* would suggest that she may have some advantage over the normal consciousness in this regard. Such a supposition would also throw some light on her power of orientation, as illustrated in her shifting without pause from one novel to another. In doing this she changes periods of history, and passes at once into another world of feeling and action, and clothes it in a different style and language. As in dreams the appropriate mood and background follows quickly any idea, unchecked by the stability of outward circumstances. Within the mind itself, however, all seems under a nice control.

I have spoken of the rationality and sanity of this strange author. This is the impression of hundreds that have talked to her. And I believe there are few writers that get nearer to the heart of human life. But one need not concede that she is a great genius in order to see the problems involved, and their interest to psychology. I have briefly sketched what they appear to me to be. At another time I hope to make more detailed statement of the case.

One more thing should be mentioned. The assertion was just made that *Patience Worth* was highly rational, sane. Upon one subject, however, this mind is under an illusion. It is well known that she insists that she is the discarnate

spirit of an English woman who lived in an age now long since passed. She not only insists upon it but she argues her claim at length, and with cleverness. And, to my mind, it is doubtful whether the S. P. R. has on hand better "evidence." That she is honest in this belief there is no reason to doubt. The full history of this illusion, this idea that she is a returned spirit, can be secured only by psycho-analysis. But it is worth noting that Patience Worth made her appearance after Mrs. Curran had spent many evenings with a friend, a confirmed spiritualist, with a view of getting a message from the spirit world. In the atmosphere of expectancy, of hope that a voice from the dead might be heard, she may be said to have been born, and it is more than possible that the idea became, at that time, a vital part of the dissociated self then developing. Thus in this self is found just that idea that would sever it most completely from the dominant personality of Mrs. Curran. This idea, although having, I believe, nothing to do with the real cause of the disintegration, has helped shape and mould her character. What is more she has lived in the atmosphere of the idea ever since the day of her appearance, those about her acting their part in sustaining the illusion. This chapter of the story is too large for this context, but it is an interesting one.

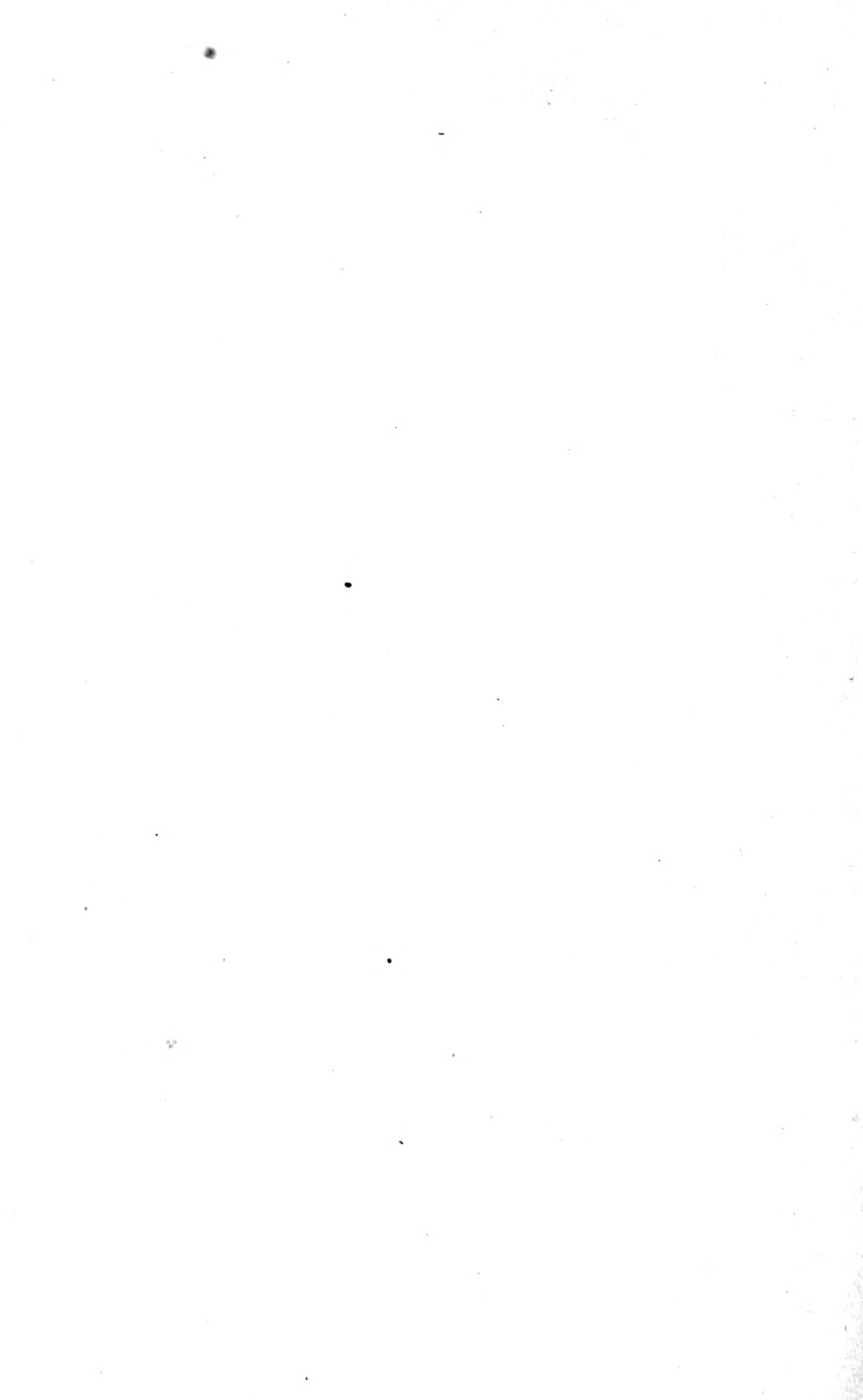
And there is a sense in which her claim to be a disembodied spirit is correct. A self incarnate is generally understood to be a self correlated with, and interested in, the biological needs of the organism, a self articulated with the perceptual and functional life of the body. Its life includes action and conduct. Patience Worth is not embodied in this sense. Back in the recesses of the subconscious she was born. Created in an ideal world, conceived in fancy, she has fashioned herself out of the stuff of the imagination, and there she remains, admitting no interests that would contradict the illusion. Such she believes, and understands herself to be, an English spinster of long ago.

But the thing that interests her most is not her own personality, but the religious and spiritual truth that she presents in her poetry and fiction. And in reading these

works, one will find no trace of abnormal tendencies, such as are clearly marked in much of the poetry and painting of Blake, whose experience was in many ways analogous to the case here described. Patience Worth is blessed with an abundance of fine and wholesome humor. And her poetry reflects a mind acquainted with the out of doors, one that has had an intimate contact with the life and moods of nature. These things are mentioned here because they go to make up one's feeling of her general sanity, her mental poise.

But it is to 'The Sorry Tale' that one must go to get the dimensions of her mind, its moral security, and dramatic power. And yet this mind that has plumbed so deeply human experience, and has touched with a sure hand its greatest tragedy, is a mind that is in error regarding its own origin and history. "Many moons ago I lived," she repeats, "Patience Worth my name." And concerning my effort to explain her, she recently made the following comment:

I am molten silver, running.  
Let man catch me within his cup.  
Let him proceed upon his labor,  
Smithing upon me.  
Let him with cunning smite  
My substance. Let him at his dream,  
Lending my stuff unto its creation.  
It shall be none the less me.



# THE PSYCHOLOGICAL REVIEW

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## EMOTION AND PERCEPTION FROM THE BEHAVIORIST STANDPOINT

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The future of behaviorist psychology will depend on the success with which it treats the specific phenomena of consciousness. To rest its case on the general theoretical advantages, important though they may be, of defining consciousness in terms of behavior, would be to forego the chief claim of any theory to scientific recognition: methodological fruitfulness. At present behaviorism is a program rather than an achievement; a method of approach rather than a theory possessing scientific credentials. Those of us who are most impressed by the importance of the philosophical advantages behaviorism offers, and who see, or think we see, great promise in its program of reconstruction, are impatient to see the program carried out. Why dally over the definition of consciousness? The progress of biological science has not depended on the formulation of an adequate definition of life. It has, indeed, depended on replacing the belief that a living being is a corporeal object endowed with a vegetable or animal soul, with the conception of it as an organized structure capable of maintaining a system of cyclical activities. The attainment of this conception has been absolutely essential to the development of biology. It has offered a program and a method of approach comparable to that which behaviorism claims to offer today. But the progress of biology has consisted in the exhibiting of the characteristic activities of the living being as elements in that cyclical system. Instead of defining life, biology has been occupied

in investigating the specific processes of living, and in tracing their mutual relations. To a similar occupation behaviorism must henceforth devote itself.

So far the behaviorist movement has had two distinct, if not wholly independent, sources. On the one hand, we find a group of experimental investigators of animal behavior, occupied with such problems as that of determining what mode of response, if any, is called out in a given species by a given physical stimulus; how given types of reaction are excited, and how they are modified. On the other hand, we find a group of philosophical behaviorists, who are chiefly concerned with the metaphysical aspects of the new doctrine and who devote themselves almost exclusively to the task of defining consciousness in terms of behavior. The two groups of thinkers find a common ground in their conviction that the study of mind and the study of behavior are not two things but one, and that the investigation of the so-called phenomena of consciousness can be fruitfully carried on only through the study of behavior.

But while the experimentalists are occupied with the reactions of the lower animals, and the philosophers with the definition of consciousness, the classical categories of psychology—sensation, perception, emotion, affection, volition, thought, etc.—are left almost wholly to the undisputed sway of the traditional psychology.<sup>1</sup> Has behaviorism no use for these categories? If it be true, as is sometimes implied, that the ideal of the science of behavior is the ability to predict what muscular contractions a given animal will make under given conditions, the psychological categories may well be neglected. But that ideal, at least in the case of the higher animals, is an unattainable one; and, what is more important, if it were attained it would leave most of the behavior of those animals, and particularly of human beings, unintelligible to us. In order to understand behavior we must resolve

<sup>1</sup>This statement seems less true today, as proof of this article passes through my hands, than it appeared to be when these lines were written. Evidence is accumulating that the experimental behaviorists have already broadened the field of their interest. Witness, for example, the investigations by Professor Watson of emotion in infants, published in this REVIEW, May, 1919.

it into a system of interrelated *functions*, just as in order to understand the physiological workings of the human body we must envisage the complex of chemical and mechanical processes as falling into such functional groups as digestion, circulation, etc., constitutive of the physiological economy. Now just as there is a physiological economy, so there is a larger vital economy, in closest union with, yet distinguishable from it. This is the system of behavior, by means of which the being, animal or human, maintains his relations with the environment and forms a factor in its transformation. The science of behavior has the task of tracing the lineaments of this larger economy. This economy is actually carried on, to be sure, by means of muscular contractions, or, if you please, by means of chemical reactions; but just as truly, and far more significantly, must it be said to be maintained by the performance of such functions as instinct and habituation, perception and emotion, or even memory and thought.

We should be rash, indeed, to assume that a scientific analysis of behavior would reveal the presence of all the classes of phenomena recognized by traditional psychology. What we have to expect is the discarding of some and the modification of many, as well as the discovery of a certain number of new psychological categories. But if behaviorism is to fulfill its promise, it must deal definitively in some manner with the classical phenomena of psychology.

It would be wrong to assert that nothing has been done in this direction. We are in possession of at least the outlines of a behaviorist theory of perception. But there has been little progress toward a larger systematic account of psychological phenomena. And yet there are many problems for which, it would seem, the materials for a solution, or at least for a profitable discussion, were at hand. How, for example, are the general functions of cognition and affection related to each other? More specifically, how is perception related to emotion? What distinctive place has each in the economy of life? On this problem it would seem that behaviorism is able to cast a flood of light.

It is evident, to begin with, that emotion is closely con-

nected with distinctive bodily activities in a way in which perception is not. One may readily tell from the behavior of a man or animal, especially from his attitude or expression, that he is angry or afraid; while the fact that he is intent upon the sight and smell of smouldering fire is not evident to one observing his demeanor. As the difference has sometimes been imperfectly characterized, one is active in emotion and passive in perception. Using the language of a more recent terminology, the generalization might be made that an emotional state is conditioned by specific types of response (including organic disturbance) and is relatively independent of the specific nature of the sensory stimulus, while perception is conditioned by the specific sensory stimulus and is relatively independent of the specific nature of the response. For example, fear may be aroused in a child by the sight of some strange animal, by loud and startling sounds, by the sudden seizure of its arms, etc., and on each occasion it may exhibit its terror in much the same way, *e.g.*, by crying and struggling to hide its face in its mother's skirt. On the other hand, the perception of the mother can be experienced only if the appropriate stimuli of light, etc., are acting on its sensory end-organs, while the response to such stimulation may vary indefinitely.

Such a generalization in regard to the relation of emotion to perception is obvious; but it does not carry us very far. It does serve, however, to set the problem a little more specifically. Since the rejection by psychology of the formerly alleged 'innervation feeling,' the general assumption has been that experienced content is determined by sensory stimulus in independence of motor discharge. Indirectly, indeed, the response has been supposed to play a part, and a very important part, in so far as the movements of the response arouse fresh sensory stimulation. But motor nervous activity itself has been regarded as wholly inoperative in determining content. Accordingly, the fact that emotion is connected with characteristic bodily reactions has led the psychologist to find the content of the emotional state in the sensory excitations arising from the emotional response



itself. Thus, to use James's familiar example, what the grieving mother experiences is the coldness and clamminess of her skin, the distortion of her face, her labored breathing, etc. Similarly, *what* we feel when we are angry or afraid is the mass of bodily and organic sensations aroused by tensed muscles and unusual organic activity. Our generalization that emotion differs from perception in being conditioned by response rather than stimulus, is displaced by the generalization that the content of emotion is determined by proprioceptive sensory stimuli while that of perception is commonly exteroceptive. This proposition, however, is evidently insufficient to distinguish emotion from perception. Emotion is not a mere perception of bodily activity. In addition to the sensory elements of emotion it has been customary to accept an ultimate feeling-tone, or affective element. The nature and number of affective elements has been a notorious bone of contention; and, moreover, there has been no agreement, and usually no clear-cut doctrine, as to the so-called neural correlate of affection. But it is significant that it has usually been held to depend on the general bodily or nervous condition, such as 'depression' or 'excitement,' rather than on a specific nervous discharge. The uncertainty and disagreement on this point is certainly a symptom of systematic weakness. If affective content is admitted to vary with general nervous condition or state of activity, the assumption that the motor discharge following a given sensory excitation plays no direct part in determining content becomes highly questionable.

There is a further distinction between emotion and perception which, while less obvious, is familiar to psychologists, and which is of the greatest significance. It is the characteristic difference between them in their relation to attention. The content of perception is marked by varying degrees of attentive clearness. This is not true of an emotional state. Indeed it might be said that emotion has no content in the sense in which it may be ascribed to perception. When the attempt is made to observe an emotion introspectively, the emotion vanishes, and one finds oneself noting the beating

of one's heart, or the strain in the back of one's neck, or some such item of bodily condition. It is essential to the maintenance of the characteristic emotional 'tone,' that these proprioceptive sensations should remain in the margin of attention. What occupies the focus in the emotional state is the object to which the emotion is directed, or, to use the traditional term, the externally excited sensations which form the content of perception. So soon as the organic and kinæsthetic elements shift to the focus of attention, they become thereby elements of a new perceptual content (the body). In order that the emotion should persist the attention must be fixed upon the content of perception.

This, then, is the crux of the matter. Since in emotion attention is directed to the perceived object arousing the emotion, we must ask how such an object is experienced as different from the object which is merely perceived and not emotionally responded to. It is in the object upon which attention is fixed and which is controlling behavior, that we must look for the content of the emotion, and not in the body. It is only the intellectualism of the theorizing psychologist that has found it in the latter place. That the object which excites rage, or fear, or love does *eo ipso* possess its specific quality, language abundantly testifies. The person with whom we are deeply angry is 'hateful,' and we call him so. The glance of his eye, the turn of his head, the tones of his voice are charged with this hatefulness. The object that inspires us with fear is a 'terrible,' or 'fearful,' object. The rumbling of the thunder is 'ominous'; and the coiled snake is 'loathesome.'

These qualities of things, which are discovered to us in states of emotional excitement, I shall call 'affective qualities' (to distinguish them from perceptual qualities and properties, such as shape, size, texture, color, etc.). It is these that form the content of emotion. It is these that we are aware of, or feel, in our emotional states, and not the organic activities and muscular tensions. It is true that momentarily there may flash up into consciousness the beating of one's heart, or the sinking in the pit of one's stomach; but it is not

these momentary flickerings of attention that are constitutive of the essential peculiarity of the emotional state. The field of consciousness is never steady in normal life, but is constantly streaked across with flashes and flickerings which mark the pulses and variations in nervous functioning. To feel emotion is to be aware of the affective quality of the object or event arousing the emotion. And, conversely, to become aware of such an affective quality in an object is to be emotionally aroused toward that object.

It will help to prepare the way for the account of emotion which I wish to present, if we pass in brief review some related and contrasted points in other theories. Of the classical school of analytical psychologists, who hold that all conscious phenomena are analyzable into complexes of processes, little need be said. According to this school, emotion and perception are alike complexes of sensation. Emotion is distinguished in that (1) there is a strong affective coloring of pleasantness or unpleasantness, (2) the sensations making up the complex are largely organic and kinæsthetic, (3) the emotion-complex is never in the focus but always in the margin of attention. In emotional states (at least of a primary sort) the focus is occupied by a perception-complex. It is for this reason that, when one tries to observe an emotion introspectively, the emotion disappears and one finds oneself attending to some item of bodily condition; *i.e.*, the marginal sensations constitutive of the emotion have become focal and *ipso facto* perceptual. It only remains to add that this school takes no account of the affective qualities which are immediately experienced as inhering in the object exciting the emotion.

The other classical school, which takes as its point of departure, not the idea of *process*, but the idea of *awareness*, holds all conscious phenomena to be resolvable into the act of awareness *plus* the content of which one is aware. There is a divergence of opinion among the members of this school, some holding that differences in conscious phenomena, notably in the case of desiring and willing, are due to differences both in the kind of act and in the content; while others

reduce all differences in the phenomena to differences in content. Among these latter, so far as their psychological affiliations are concerned, are the American neo-realists, who are of peculiar interest to us in that they identify the act of awareness with selective activity of the nervous organism in responding to those features of the environment and body which act as stimuli. Thus, according to them, to react to a stimulus is to be aware of that stimulus. The variety in conscious life, then, and even what we may call the structural complexity of experience, is put on the side of content. This method of treatment, which lumps together all classes of behavior as response to stimulus and equates this with a simple awareness, tends to obliterate the empirical differences between such phenomena as emotion and perception. Thus the dog, circling around his adversary for attack, is responding to the physical properties of his enemy, such as his shape and size and the sounds he emits; but he is also responding to a no less real property, the *inimicalness* of the enemy, or his *hatefulness*. So, when we shudder with fear at the coiled snake, the neo-realist would describe our experience as a response to, *i.e.*, an awareness of, the dreadfulness or loathesomeness of the reptile, just as our vision of the coils and the glisten is a response to, or an awareness of, those physical properties.

Now this neo-realist theory is so far similar to the behaviorist view which is to be presented here, that it recognizes the existence of affective qualities and treats emotion as the experience of these qualities. But intent upon his general philosophical position, the neo-realist has failed to take account of the very distinction which sets our problem. So far as I know, no attempt has been made by him to explain how such a property as the loathesomeness of the reptile is related to such sensible properties as its glisten and its shape. These perceptual properties may be correlated more or less indirectly with physical stimuli, but it would be impossible to equate the loathesomeness with a determinate type of physical stimulus. Furthermore, the neo-realist has not made clear what part, if any, he would give to the alleged

proprioceptive sense-data (I use his own terminology) in emotion. If present at all as content, they must, on the neo-realist view, be present in the same way and stand on the same footing as the perceived sensory qualities. What the neo-realist is interested in is the ontological status of such properties as *loathesomeness* and *hatefulness*. The goal of his endeavors is to exhibit them as objectively real, as properties belonging to the independent world, brought within our ken by the selective activity of the nervous system. Their distinctive character he ignores.

Different in background and method as pragmatism is from neo-realism, there are affinities between them in certain respects which have been the subject of considerable discussion. The particular point of interest to us is the pragmatist's realistic treatment of what I have called affective qualities. "Things are," says Professor Dewey, "what they are experienced *as*." The windowshade flapping in the night, which is invested with nameless terrors before I discover it to be nothing but the shade blown by the wind, is as really 'fearsome' as it is 'loud' or 'intermittent.' So the snake is really 'loathesome,' the castor oil 'nasty,' the fire in the grate 'cheerful.' What properties things have is, according to Professor Dewey, as truly determined by, as determinant of, the response they make. The process of determining the response, which constitutes the act of perception, is at the same time the process of determining the stimulus.

Thus the pragmatist, like the neo-realist, tends to treat emotion as the perception of a certain class of attributes, such as hatefulnes, loathesomeness, etc., which may be termed affective. What they both have failed to realize, or at least to take adequate account of, is the very real and important distinction between such affective attributes of things and what are commonly regarded as objective properties, such as shape, or size, or texture, the objects of perception proper. The coiledness of the snake has a determinateness, a sort of fixity and solidity, which its loathesomeness lacks. It lends itself to description, to the noting

of detail. The loathesomeness on the contrary, cannot be described; it presents no detail; it can only be felt as ultimate unanalyzable *this*. Moreover, it cannot be attended to in the same way. It either vanishes upon persistent scrutiny into mere shine and glisten and shape, or it overwhelms one with a shuddering. One catches it evanescently in the slimy glisten and slow movement, but it is not these nor anything qualitatively determinate. *Mutatis mutandis*, the same thing is true of that nameless overpowering charm which the beloved has for the lover. To the bystander or to the would-be sympathetic friend, the other's beloved may be fair and lovely, as many other women may be; but that peculiar charm which the lover celebrates can be experienced only by himself. To fall in love is precisely to become aware of this unique and inexplicable charm which sets her apart from all others. One could not experience this quality, indeed, without loving her. Like the loathesomeness of the slimy snake, this quality is featureless. One may catch it in the turn of the head, the tone of the voice, or in a hundred other perceived details; but it is none of these. One can only feel it.

It is evident that these affective qualities have distinctive characteristics, which a simple act-content theory is unable to take account of, and which pragmatism has neglected. Whatever their metaphysical status—and with that we are not here concerned—their empirical psychological status differs in important respects from that of the perceptual properties.

For these differences behaviorism provides an ample ground of explanation. Starting as it does with the assumption that experienced content is not determined directly or alone by the sensory stimulus, but varies also with the system of motor response brought into play, that is, with function rather than with process, it finds a natural way of accounting for the existence of affective qualities and a ground for distinguishing them from perceptual properties. The object which is experienced as fearful owes this affective quality of fearfulness to the fact that the sensory stimulus is arousing the specific response of fear. Now this response, while

it is specific, is not an invariable set of muscular contractions and organic reactions. Even though we recognize by the unmistakable demeanor and 'expression' that the child or the dog is afraid, we find the task of definite and detailed description of the criteria of fear extremely difficult. What the recent researches of Cannon and others have brought out is that in all strong emotion there are characteristic organic disturbances, notably the activity of the ductless glands, which have the effect of releasing stores of latent energy for the immediate utilization of the voluntary muscles, while the digestive processes are inhibited. That is, the function of emotional excitement is the sudden energization of the body. This is, of course, very important information; but it has in itself no bearing on our problem. So far research has failed to reveal any specific differentia of fear or rage. No activity, either organic or muscular, is universally present in all cases of fear or is peculiar to fear. The particular reaction brought out on any occasion of fear may be headlong flight, frantic struggling, or the trembling suspension of all overt activity. All these as well as other activities are currently recognized as fear-responses, and rightly, because they all belong to a single system of alternative responses, genetically and functionally bound together. They are functionally related in that they normally serve the common end of escape from danger. They have probably arisen as differentiations and modifications of some simpler reaction, or reactions; and they retain the mark of their common descent in the ease with which one form of reaction passes into another, trembling inactivity into stealthy crouching under cover, or this into headlong flight and calls of distress. Any stimulus that brings into play this system of alternative responses is experienced as fearful. The common and distinctive character of the emotion of fear is not to be found in any set of organic and kinæsthetic 'sensations' excited by the emotional reaction. If such sensations were present they would vary too much from occasion to occasion to account for the identity of the emotional content. The identity of the fear-experience is to be found, not in any

identity of separate sensory or motor elements of the varying nervous activity, but in the unity of the total functional system brought into play. The activity of this specific system of fear is, as it were, projected upon the exciting object and incorporated in it as the affective quality of fearfulness. Hence, in spite of the enormous differences in the sensory stimuli coming from feared objects, they are experienced as in so far alike.

Perception, in contrast with emotion, involves no specific response, in the sense just described, at all. Yet to say that perception has no function in the determination of response, or is not itself an element of behavior, would be false. No one would today claim that perception is a mere passive receptivity to sensory stimuli. On the contrary, it is just its part in the determination of response that is its essential characteristic. Professor Dewey has described it as the act of 'constituting the response.' In perception we take an attitude of readiness to respond to the perceived object in a way not yet fully determined. We perceive, in other words, what we are attending to and just in so far as we are attending to the thing in question. Now attention involves the postponement of response, and the possibility of the selection of alternative modes of response. There is, indeed, a sort of response which is going on during the period of attention, *i.e.*, the adjustment of sense organs to further and fuller excitation from the exciting source—the cocking of ear, the tension of head and neck muscles, the fixing of eye, etc., which characterize the attentive attitude. The prolonged excitation which comes pouring into the cortical centers from the perceived object thus has opportunity to bring into tentative activity the whole repertoire of muscular reactions and, by mutual inhibition and reinforcement, to constitute the final response. What Professor Dewey has pointed out is that this process is a reciprocal one, and that the constitution of the response is at the same time the constitution of the stimulus, considered as a psychological factor. Aside from the part the stimulus has in the total nervous activity, it is merely a physical event. It is *psychological* only in so far as it is a functional element.



If this is true, the perceptual qualities of things are not determined by the nature of the stimulus considered as excitation of end-organ or sensory center. We are in the habit of thinking of sameness of perceptual content as being sufficiently conditioned by sameness of sensory stimulus, but a little reflection shows that this assumption is far from true. We may see and recognize the same house, the same landscape, the same face on different occasions, although the actual visual stimuli acting on retinal end-organs vary widely from occasion to occasion. Moreover, the perceptual content, *i.e.*, *what* we perceive, may vary considerably while the sensory stimuli remain relatively constant. For example, the disk of the full moon looks different as we see in it now the face of the friendly 'man' or the profile of the lovely 'lady.' Similar and even more marked changes occur in those puzzle-pictures which at first view present merely a confused mass of lines and blotches, but which reveal to a more persistent scrutiny a deer, a face, or what not. Once the hidden object flashes into view, it is often wholly beyond one's power to see the picture as the mere confused mass it formerly appeared to be.

The principles by which perceptions are individuated and classified are not to be deduced from any mere likenesses and differences in stimuli received. It is not that the specific character of the stimulus does not count, but it counts only as a factor or element in a functional system.

To perceive something is not to respond to it in any particular way. It is rather to take an attitude of readiness to respond in some way not fully determined. It may be said that the capacity for perception, *i.e.*, for a cognitive awareness of the object, is the capacity for postponing response and thus for the selection of alternative possible responses to a relatively specific stimulus. The distinctive characteristic of the stimulus perceived, as contrasted with the stimulus emotionally felt, is the indirectness and complexity of its relation to response. We may generalize the relation of cognitive awareness to affection thus: In so far as a stimulus is calling into play a specific type of response, belonging to a

single genetic and functional system, it possesses the affective quality experienced in emotion; in so far as the stimulus calls into play an attentive postponement of response, it arouses cognitive awareness and possesses perceptual quality.

Now it is obvious, if this generalization holds, that cognition and affection are not antagonistic to each other, or mutually exclusive. They are, on the contrary, correlative moments in behavior, and as such clearly distinguishable. An emotion is never felt except in conjunction with some perception. One may be all charged, so to speak, with irritation or timorousness and just ready to be angry or to be afraid on the slightest provocation; but the anger or fear when it comes centers about some perceived object or event. In order to be angry one must be angry at something. To be angry at this thing is to assume an attitude of attack or menace, to be 'set' for destructive action. The particular form which this anger-attitude is to take, however, is variable. It remains to be determined, and progressively determined, by the exigencies of the occasion. The angry dog is charged with hostile intent; he is 'set' for growling and snarling, for leaping and biting; but whether he growls *or* leaps, just when he bites, and where—all this is determined by the acts and position of his adversary, *i.e.*, by the *perceived* features of the situation. It is just the relative indeterminateness and the variability of the anger-response that necessitates its dependence on the determining action of perception.

Each animal organism is endowed with inherited modes of response congenitally organized more or less completely. Thus, for example, a member of the cat family will exhibit very early the attitudes and movements characteristic of the pursuit and attack of prey. At first these are brought out by a variety of stimuli, often in a ridiculously inappropriate fashion. Even when the exciting stimulus is a natural object of prey, the various elements of the total response are badly coördinated with a view to successful capture. The stalking-period is too short, the grass is too obviously stirred, the spring is premature, and from the wrong position, etc. But gradually the function becomes perfected. If the prey is

not in a suitable position, the stalking is postponed, or a detour is made. A dozen circumstances serve to retard or to hasten, to modify and vary, the various stages of the pursuit. At any point it may be abandoned altogether, or taken up with a fresh orientation. Moreover the whole mode of procedure is varied according to the kind of prey hunted. Now this development and perfecting of the hunting-response is conditioned by a development of perception in the animal. At first, when the abortive reaction is set off almost regardless of circumstances, such perception as occurs is of the vaguest and simplest. Perceptual and affective qualities are scarcely differentiated. Even the prey itself is not distinctly perceived until the response called out is adapted both to the kind and to the special circumstances. Its discrimination as prey depends on the discrimination of other objects and items as definitely determinative factors in the total situation. For an object or item to be perceived it must have acquired the power to function as an element or factor in the total situation. It must be a conditional determinant of response, in a more or less definite and systematic way.

Perhaps the grasping child will serve to illustrate more fully this distinction. When the grasping period begins, the baby reaches out both arms in an aimless jerky fashion. He is 'set' for grasping, and almost any visual stimulus which arouses fixation suffices to initiate this response. At this stage the baby cannot safely be said to see the exciting 'ball' or 'rattle' or 'face' or 'light.' He cannot be said to see any 'thing' whatsoever, for the simple reason that he gives no evidence in the way of discriminatory behavior. All these stimuli are, however, undoubtedly exciting and attractive, although the words carry too definite a meaning properly to describe the formless content of the baby's experience. But discrimination rapidly develops. Not only is there preferential selection among the things grasped, but the movements of body, arm, and hand are adjusted to position and distance of the thing grasped for. As this coördination is consummated, and the hands reach the object of their search, they fumble it over and bring it to the mouth. So gradually

is brought about that complex adaptation of grasping and manipulating movements, not only to position and distance, but also to size, shape, and even texture. Not until these complicated systems of response are organized does perception of the thing as object having position and solidity, shape and size, come into being.

Let us note more closely the relation of the perceived property of distance to the behavior of the grasping child. What sets the child to grasping at all at the object is the vaguely yet specifically attractive look of it. But the nice adjustment of leaning just so far in such a direction, and stretching out the arm just so far, and moving the wrist and fingers just so far, is controlled by the pattern of the sensory excitation from retina and eye-musculature. There is no simple one-to-one correlation of sensory excitation with motor reaction, in which each variation in excitation calls out its own specific variation in response. On the contrary since a given distance and position may be represented by a number of different sensory complexes, these functions are equivalent. That is, each and every such complex serves as stimulus to that coördinated sequence of tensions and flexures which results in the fingers' touching the object so placed with reference to the child. But this sequence of movements may vary considerably. One may bend the body farther and stretch the arm commensurately less; or, if one bends first to one side, one must compensate by stretching the arm differently. As bodily skill develops, one learns to execute a multitude of combinations of movements, each of which results in bringing the fingers in contact with an object in the given position. The perception of position, then, is conditioned by the development of a complicated functional system, the characteristic feature of which is the correlation of a variety of equivalent patterns of sensory complexes with a variety of equivalent patterns of motor complexes.

The account just given is of course merely schematic. The interweaving of sensory and motor patterns is enormously more intricate than has been indicated. Perception of spatial position and distance of an object involves not merely

the capacity for reaching it with the hand, but for walking around it, avoiding it in movements directed toward other objects, and, not least important, for dealing with it indirectly by means of other objects in a thousand ways.

The perception of spatial properties is of course peculiarly rich in the wealth of definite and detailed correlations by which it is conditioned. But a similar type of schema underlies all perception. The concrete perception of any object is made up by a complex of sensory patterns of position, shape, size, texture, etc., each of which has its own subordinate correlations with motor patterns, and which possesses in consequence a certain degree of what may be called functional independence; that is, it may appear as a factor in other total complexes having the same potential value as a conditional determinant of response.

What is true of the perceptual properties which belong to the object is true of the perceived object as an individual totality. It too is represented by a great variety of sensory patterns, corresponding to the view of it in different positions and illuminations in different states. These are all united, in spite of their diversity, by their functional equivalence. Thus any view of a familiar plaything may serve as well as any other view of it to initiate the type of play-response appropriate to the particular object, although the detail of the movements of walking toward it or climbing for it will vary with its position on table or on high shelf. It is, indeed, the functional equivalence of the varying sensory complexes aroused by an object that forms the basis of the recognition of it as *the* object—ball, or knife, or mother—which it is.

The parts which emotion and perception play in behavior we have seen to be characteristically different. The distinctive peculiarities of the content belonging to each of these functions correspond to the part played by each. The affective qualities of things represent the immediate and direct functional value which the things possessing them bear to the animal, and they control specific type-reactions directed to the thing thus qualified. The perceptual qualities

of things represent indirect and conditional functional values. They control the detail of activities, whose general form is dictated by some type of affection. Their capacity for this control of detail is conditioned upon the development of complicated systems of nervous activity, in which groups of differing sensory patterns have like functional relations with corresponding groups of differing motor patterns. Perceptual qualities and properties are thus conditional factors, possessing, because of their conditionality and systematic interrelationship, functional independence. It is the functional independence of the perceptual quality that gives it its attentive clearness, its qualitative distinctness. It is the wealth and variety of its functional relationships which makes it a fit subject for description and comparison. The featurelessness of the affective quality, on the other hand, its indescribability, is due to the very simplicity of its relation to the organism. Because it is relatively unconditional in its control of behavior, and is not a factor having a determinate place in a vast system of nervous organization, it eludes attentive scrutiny. Because to experience it at all is to be already committed to a specific type of response, it can only be felt, and not cognized.

As emotion and perception are correlative to each other, so in a more general sense do affection and cognition play complementary parts in the economy of life. It is our feelings that furnish the springs of action; it is our intelligence that guides and directs that action. By virtue of their affective qualities, objects and fellow-beings, situations and events become ends of action; for affective qualities represent the direct functional relations which things bear to us, their power to work us weal or woe, to satisfy our needs or thwart our activities. They arouse us; they fill us with longing or with aversion; they beckon us on or warn us away. They form as it were the foci of the larger curves into which the course of living falls, the points of reference for the determination of general direction. The properties and relations of things, on the contrary, which our intelligence discovers, and which are constitutive of them as entities making up an

interrelated but independent world, have no direct and simple relation to our needs and capacities. The understood causal relation of events, like the perceived shape of objects, leaves us cold. It may have an interest for us, but it is rather as a means than as an end. It represents a sort of condensed expression for a multitude of definite conditionalities. It says, as it were: If you will gain (or avoid) one object, you must deal thus and so with another. If the affective qualities determine the larger curves and the greater changes of direction, it is the cognized properties and relations that account for the complicated twistings and turnings and interlacings which mark our course.

# DARK-ADAPTATION WITH ESPECIAL REFERENCE TO THE PROBLEMS OF NIGHT-FLYING

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## INTRODUCTORY

This work was undertaken with a view to designing a method whereby the visual fitness of the aviator for night-flying might be tested. His absolute sensitiveness for light is one datum sought. Another is his rate of recovery after the relative blinding following exposure to light for short periods.

Researches heretofore made on dark-adaptation have shown that in the course of an hour spent in the dark the sensitiveness of vision for light increases gradually, and may in the course of the hour increase several thousandfold over its initial value;<sup>1</sup> or less according to the duration and the intensity of the light to which the eyes have been previously exposed;<sup>2</sup> until a point is reached beyond which no further change is to be found.

The present work contemplated exposing the eyes of the observer to light of a standard intensity for certain measured periods of time, and subsequently investigating the course of his light sensitivity. Since from what has just been said it appears that exposure of the eyes to light prior to the time the observer comes under investigation may produce effects which do not completely wear off in less than an hour, it was necessary, before beginning the experiment proper, to make sure that the observer had reached a standard and reproduc-

<sup>1</sup> Piper, 'Ueber Dunkeladaptation,' *Zeitsch. f. Psychologie u. Physiologie d. Sinnesorgane*, 31. See also Lohmann, 'Disturbances of the Visual Functions,' Blakistons, Philadelphia, 1914, Chapter V.

<sup>2</sup> Nicolaï, *Zentralblatt f. Physiologie*, 21, vide Lohmann, 'Disturbances of the Visual Functions, 1914, p. 71; and Rabinowitsch, *Zeitsch. f. Augenheilk.*, 19.



ible condition. The obvious way to accomplish this was to measure his light sensitivity from time to time, as he remained in the dark, until a stage was reached beyond which no further increase was noted. It was then assumed that his vision had become completely dark-adapted, and was in a state which could, for the individual and for experimental purposes, be taken as a standard. The actual experiment began after this point had been reached.

#### APPARATUS AND METHODS

The test of light-sensitiveness is, in the first instance, the photometric brightness of a surface which is just visible with certainty. The instrument by which such a dimly-bright surface is maintained and its brightness known is the adaptometer. The particular one used here was designed on the fundamental principle proposed by Nagel,<sup>1</sup> with certain technical modifications. It consists of a box, made of  $\frac{1}{2}$ -inch white pine, 29 inches (73.6 cm.) long, 8 inches (20.3 cm.) wide and  $7\frac{1}{4}$  inches (18.4 cm.) high, inside measurements. A  $\frac{1}{2}$ -inch cross-partition divides its length into two compartments 11 inches and  $17\frac{1}{2}$  inches (27.9 and 44.4 cm.) long (Fig. 1, *A*). At the outer end of the longer one of these compartments a milk glass, *MG*, 5 inches (12.7 cm.) square is attached to cover a somewhat smaller opening. The test-pattern is projected upon the inside of this glass by an optical arrangement which is in effect a miniature stereopticon, consisting of a projecting lens, *P*, supported in an opening in the cross-partition; and within the smaller compartment a condensing lens, *C*, and an electric lamp, *L*, together with a 'slide,' *S*<sub>1</sub>. This last is a thin slip of brass in which openings are cut (Fig. 1, *B*), the image of the openings projected on the translucent screen, *MG*, being the test-pattern viewed by the subject.

The lamp is a 12 c.p. tungsten-filament auto headlight lamp, rated at 6 to 8 volts, 2 amperes, with a  $1\frac{1}{2}$ -inch spherical bulb. It was actually operated at 1.75 amperes to avoid too rapid deterioration. The current was supplied by a small

<sup>1</sup> *Zeitsch. f. Augenhk.*, 17, 3.

storage battery and controlled by rheostat and ammeter. The lenses are  $1\frac{1}{2}$ -inch ophthalmologists' trial lenses removed from their rims. *C* is of 18 diopters, and *P* of 13 diopters' power. The first focuses the image of the lamp filament upon a slit, *S*<sub>2</sub> (Fig. 1, *A*), the distances being such, that a reduction in size as 5:3 takes place. That is, *LC* is 14.8 cm. and *CS'* 8.9 cm. approximately. The slit is  $\frac{1}{2}$  mm. wide by 10 mm. high. The amount of light entering the slit is reduced

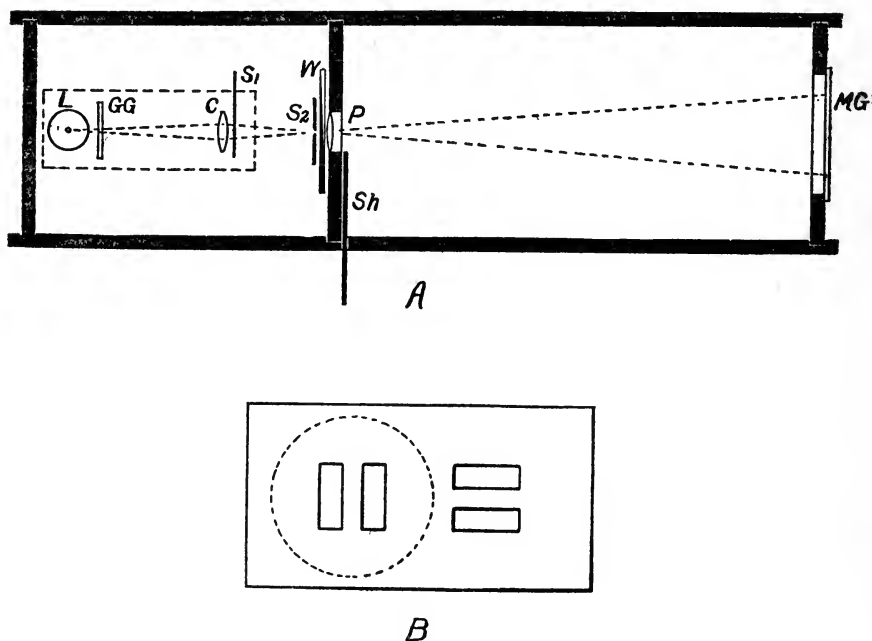


FIG. 1. The adaptometer. *A*, plan of the instrument in section. See text for explanation. *B*, the slide, showing the test-pattern in the two positions. The face of the projection lens is indicated, for one position of the slide, by the dotted circle.

by a ground glass, *GG*, and, by way of adjustment, may be further decreased by moving the glass farther from the lamp. The last adjustments as to the relative positions of these parts were made without the ground glass in place so that the image of the slide, *S*<sub>1</sub>, was accurately focused upon the milk glass, *MG*.<sup>1</sup> It was then determined just where the

<sup>1</sup> The distances determining the size of the test-pattern are *S*<sub>1</sub>-*P* 9.2 cm. and *P*-*MG* 46.2 cm. approximately. The magnification is here as 1:5.

image of the lamp-filament fell and the slit was permanently placed at just the same distance from the cross-partition. This was accomplished by mounting the lamp, condensing lens and slider ( $L$ ,  $C$ ,  $S_1$ ) in their proper relative positions permanently on a small table which could be shifted about slightly and then clamped.

The finer changes in the brightness of the test pattern, necessary during the experimental procedure, were brought about by an absorbing wedge ( $W$ ), of which the light transmission varies by indefinite gradations in the ratio of 29,100 : 1 between its extreme positions 100 mm. apart. This is mounted immediately behind the slit, between it and the projecting lens, and is provided with a millimeter scale and a mechanism by which it may be shifted from the outside of the box. It was found by photometric measurement that the brightness of the test-pattern was 0.0272 candles per square meter when the wedge was set at zero of the scale. In such a wedge the change in the amount of light transmitted

TABLE I

THE PHOTOMETRIC BRIGHTNESS OF THE TEST PATTERN, IN CANDLES PER SQUARE METER, FOR EACH CENTIMETER OF THE ADAPTOMETER SCALE. THE COLOGARITHM OF THE SAME WITH THE DIFFERENCE FOR EACH MILLIMETER DIVISION, FROM 0 TO 10.

Candles per Sq. Meter	Scale, Cm.	Colog. C. P. Sq. M.	Proportional Parts	
			Mm.	Diff.
$272 \times 10^{-4}$	0	1.566	0	0.000
$973 \times 10^{-5}$	1	2.012	1	0.045
348	2	2.458	2	0.089
102	3	2.905	3	0.134
$446 \times 10^{-6}$	4	3.351	4	0.179
159	5	3.798	5	0.223
$570 \times 10^{-7}$	6	4.244	6	0.268
204	7	4.690	7	0.312
$730 \times 10^{-8}$	8	5.137	8	0.357
261	9	5.583	9	0.402
$935 \times 10^{-9}$	10	6.029	10	0.446

is in a certain constant ratio for each equal linear shift of the wedge. In the wedge used each shift of 10 mm. from 0 toward 100 mm. was found to change the amount of light transmitted by the factor  $1/2.795$  or 0.358. The relation between scale reading and brightness is best worked out by logarithms and is as indicated in Table I. At 0 mm. the brightness is 0.0272

c. per sq. m., logarithm  $-1.5656$ . The logarithm diminishes by  $0.44638$  for each cm., or by  $4.4638$  for 10 cm. In the table the negative signs are dropped, hence the figures are cologarithms from which brightness may be directly found. As they stand in the table the logarithms may be taken as expressing the grade of sensitivity corresponding to the respective scale-readings.<sup>1</sup>

The test-pattern itself is in form a pair of bars of such dimensions that they represent the remainder of a square after its middle third is removed. Two such patterns were cut in the slide (Fig. 1, *B*) in such a way that the image might be shown in the horizontal or in the vertical position. Each opening in the slide is  $15 \times 5$  mm. and the strip left between the two, in each pair, is 5 mm. wide.

Directly above the test-pattern and on a level with the eyes of the observer is a fixation-point (Fig. 2), consisting of a 4- to 6-volt, tungsten filament, tail-light lamp operated on one dry cell. The light from the lamp, operated so much below its rated voltage, was dim and reddish and it was further dimmed by coloring the lamp with red dip and by interposing layers of paper between it and a diaphragm (with an opening of about 6 mm. diameter) which is directly in front of it. The fixation point was kept just bright enough to be located in the dark. Its distance from the eyes of the observer was 52 cm. The center of the test-pattern is 12 cm. below the fixation point, and therefore 13 degrees below the center of the visual field. The test-pattern subtended about 8 degrees both horizontally and vertically in the visual field. A sliding shutter (*Sh*, Fig. 1, *A*) was arranged to darken the test-pattern completely except when actual observations were being made.

During the progress of the experiments in the dark all light, except that from the adaptometer, that from the fixation point, and that from a small reading lamp, was excluded from the room. This last is a 4- to 6-volt tail-light lamp, operated on a single dry cell. It was used in recording the

<sup>1</sup> This mode of expressing the grade of dark-adaptation is quantitatively different from that proposed by Best (*Arch. f. Ophthal.*, 76, pp. 146-58) but the same in mathematical principle.

results and was always kept well shaded. Possible stray light from this and other sources was kept from the observer by a black felt curtain.

### PROCEDURE

The subject was seated in darkness before the adaptometer with his forehead just touching a fixed support, and fixated the red fixation-point (Fig. 2). The shutter was opened for approximately one second and he was asked to state whether he saw the test-pattern, and if so, in what position. By repeated trials the point on the scale was found at which he was just able to report the position of the test-pattern correctly. The time was immediately taken and entered in the record along with the scale reading. This was repeated at intervals of about five minutes, the subject resting in the mean time. Usually the subjects had been spending the last hour or two, before the room was darkened, in some part of the laboratory and as a consequence the time required to reach a constant threshold was less than an hour. In general, at the end of twenty or thirty minutes it was found that two or three readings had been taken which were in agreement to within the limits of precision of the method.

When this point was reached a white screen was placed before the eyes of the subject, leaning against the front of the adaptometer at a definite inclination; and the center light of the room was turned on. The subject was instructed to maintain his position and direct his eyes toward the center of the screen. In preparation for what was to follow, the test-stimulus, set at a certain point, was exposed by drawing the shutter. Five minutes from the time the light was turned on the screen was quickly removed and the light turned off after an interval of perhaps one or two seconds in which the observer could locate the fixation point. He remained at this fixation until the test stimulus, at first invisible owing to the five minutes blinding, became visible to him. The time required for this to take place was noted. Subsequently readings were taken as before, but somewhat more frequently, until the former absolute threshold was again reached.

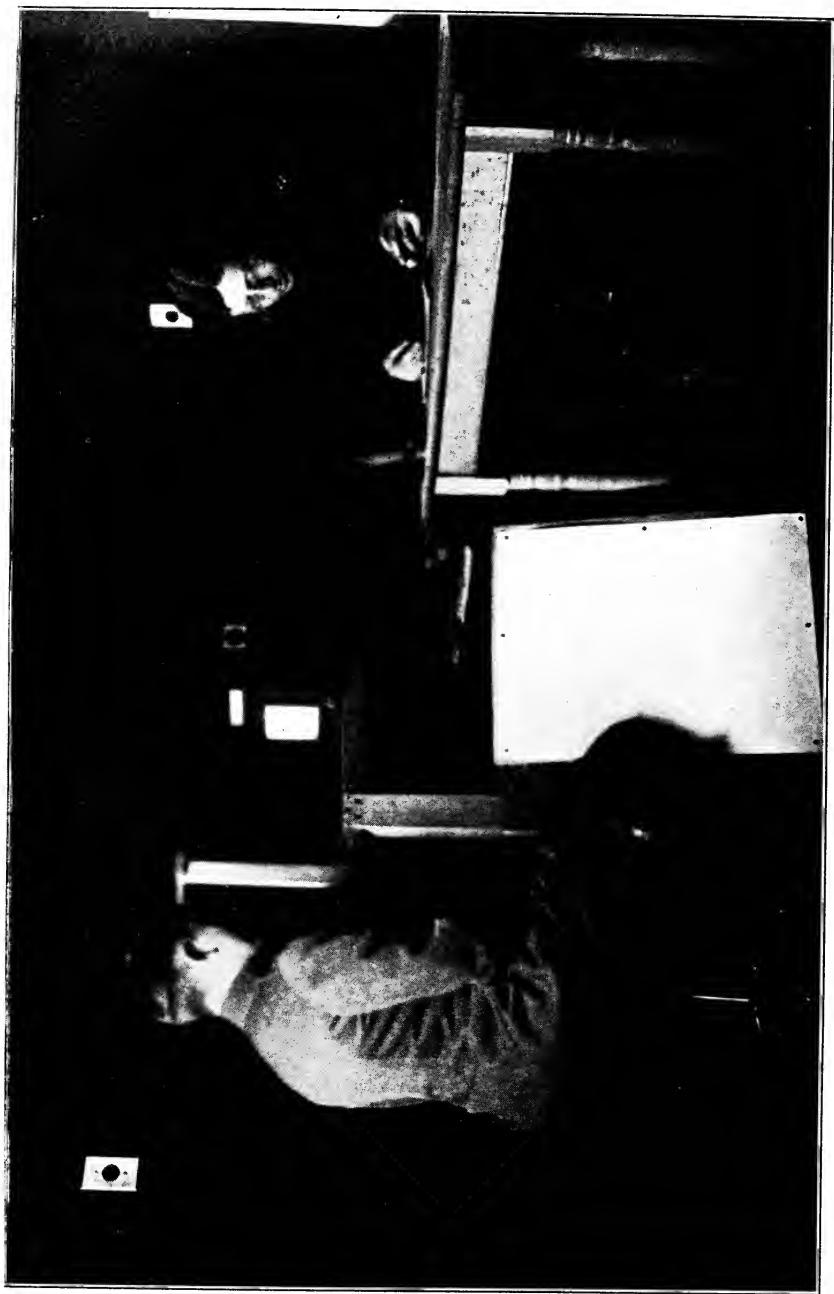


FIG. 2. General view of the adaptometer, the observer (to the left) and the experimenter. The test-pattern appears on the square white surface on the end of the instrument and is not visible except in the dark. Above this is the fixation point with the dry cell attached. The white screen used for the purpose of 'blinding' the subject is below in the foreground. The experimenter holds the reading light in his left hand.

Finally, the white screen was replaced and the procedure of the last paragraph repeated with fifteen seconds exposure to the white screen, in an exactly similar way. From the time the subject entered the room until the end of this third portion of the experiment the whole procedure occupied from an hour to an hour and a quarter, according to the speed of the subject in recovering his absolute threshold.

The screen used for 'blinding' the subject was a sheet of white blotting paper 24 by 19 inches, mounted on a light frame. Its angle of inclination and distance from the eyes were such that it subtended, at the eyes, an angle of 67 degrees vertically and 70 degrees horizontally in respective planes through its center. Its lower half was nearer the observer, consequently including a larger angle, easily including that part of the visual field occupied by the test-stimulus and much more in every direction. The photometric brightness of this screen averaged 13 candles per square meter by actual measurement.

### RESULTS

The various values for sensitiveness were plotted, each against the time at which it was obtained. Thus each experiment yielded three curves as in Fig. 3. The first of the three began with the state of adaptation in which the subject happened to be and was carried on until his absolute threshold had been reached. This was taken as one datum in his case.

The second curve represents his recovery after five minutes blinding<sup>1</sup> by a surface of standard extent and brightness. Here the question was rather his rate of return to dark adaptation than the ultimate point. The first datum here, in the chronological course of the experiment, was the time required to see an initial stimulus, already exposed when the room was darkened, as his dark adaptation progressed and the sensitivity of his vision grew sufficient to take account of it. At this stage the increase in sensitivity is

<sup>1</sup> The word 'blinding' is used here for want of a better and equally short expression, meaning only that effect, due to which the subject is relatively and temporarily blind to stimuli of the very low intensities considered.

rapid, for which reason the technic just described was made use of. The method was not satisfactory in that the whole result depended upon a single judgment of the observer. If for any reason he failed in this, as happened on certain occasions, there was no way of repeating without undue expenditure of time. The results so obtained are nevertheless given and are not, on the whole, without significance.

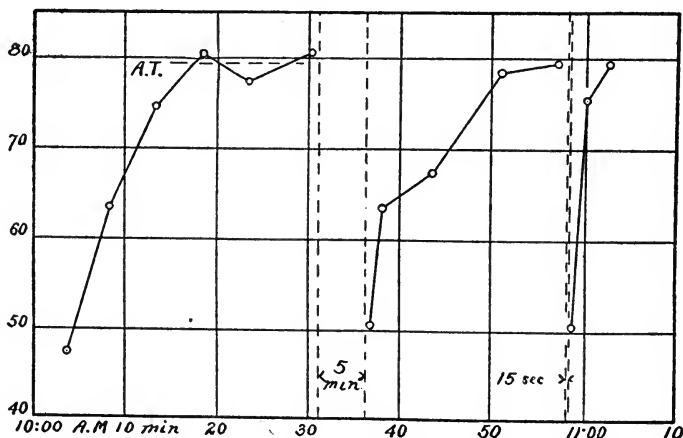


FIG. 3. Specimen plot of one experiment. The first curve was taken without preparation. In this case the mean of the last three points (79.5 mm.) was taken as the absolute threshold, and its location on the plot is indicated by the horizontal interrupted line. The two blinding periods are indicated by the vertical interrupted lines. The return of both curves to the exact value of the mean absolute threshold is simply coincidence; usually there is some discrepancy.

It could not, of course, be predicted when the subject would reach a given degree of sensitiveness. Every exposure to the test stimulus seemed to blind the subject slightly, especially in the later and more complete stages of dark adaptation, where the disturbance might amount to a number of millimeters on the scale. When, after a waiting interval, the right point was hit upon, rather by accident, the stimulus might be seen at a very low value; whereas after several trials by the trial and error method a new and larger value (lower on the scale) would be found; the subject in the mean time being unable to repeat the initial observation. It would seem then, that the first point, taken with the stimulus



set and exposed on increasing dark adaptation immediately after darkening the room, is not strictly comparable with the later observations, taken by a trial and error (perhaps better called a 'bracketing') method, with the stimulus darkened in the intervals. In general a rapid method had to be used, since the quantity measured was, for the most part, continuously changing; and a different and more rapid method had to be used to get an index of the initial rapid rise within the first minute or so.

A study of all the plotted curves led to the selection, for the purpose of discussion, of a comparatively few data. Some of these rest upon an arbitrary basis, but all the possibilities are involved.

Those so selected are:

1. The absolute threshold.

2. (a) The time required to reach an arbitrary, rather low degree of sensitiveness, obtained by noting the instant that the test stimulus appears to emerge from the darkness.

(b) The time required to reach an arbitrary level (70 mm. on the scale) obtained by linear interpolation from the curve of rise.

(c) The time required for the individual to reach a point on the scale 15 mm. short of his own absolute threshold.

The data indicated under (2) comprise two parallel sets, the one following five minutes blinding, the other following 15 seconds. It will be noted that while (a) and (b) under (2) are data relative to fixed stimuli those under (c) are relative to the subject's own standard and will indicate his individual capacity, rather his individual rate of recovery than his competitive performance under given external conditions. The data just described are given in Table II. The arrangement of the observers here in the upper block of the table is approximately in the order of magnitude of the absolute threshold, the third column giving the same in terms of the adaptometer scale, the fourth giving the corresponding logarithmic sensitivity (the cologarithm of the brightness in candles per square meter). The fifth gives the brightness itself in hundred-millionths. The first value in this column is thus 0.00000134 candles per square meter.

TABLE II

## THE RESULTS OF THE DARK ADAPTATION EXPERIMENTS

The absolute threshold is stated (1) in terms of the scale reading, (2) as the logarithm of the brightness of the test-pattern and (3) as that brightness itself in hundred millionths of one candle per square meter.

Under 'AT-15' is given the time of return of the individual to a threshold 15 mm. below his own absolute threshold on the scale (which is photometrically 4.7 times as bright).

Similarly, under '70 mm.' and '50 mm.', the times of return to these respective points on the scale, as described in the text. These two points are respectively 0.0000204 and 0.000159 candles per square meter brightness.

The lower block of the table gives the comparative results of repetition of the same experiment upon each of three observers, except in the case of W, in whose case the order was reversed and 15 seconds blinding given first.

The same was the fact in the case of observer M, in the upper block of the table.

In the last column the asterisk (\*) indicates that the course of adaptation was too rapid to permit of taking the time as the first point (50 mm.±) was reached.

Date	Obs.	Absolute Threshold			AT-15 Mm.		70 Mm.		50 Mm. ±	
		Scale Mm.	C. P. Sq. M. Colog.	C. P. Sq. M. $\times 10^8$	Sec. After Blinding		Sec. After Blinding		Sec. After Blinding	
					5 Min.	15 Sec.	5 Min.	15 Sec.	5 Min.	15 Sec.
Nov. 7, '18	R	96.5	5.873	134	300	106	151	76	30	12
Dec. 20, '18	Do	91.5	5.650	224	385	84	79	66	37	10
Jan. 6, '19	O	88.5	5.516	305	315	82	213	72	48	15
Jan. 7, '19	Sh	88.0	5.494	321	291	—	236	—	48	—
Jan. 17, '19	G	88.0	5.494	321	160	76	142	66	30	5
Oct. 31, '18	Ho	87.0	5.449	356	140	—	127	—	10	—
Nov. 4, '18	Si	89.0	5.539	289	86	43	62	36	20	10
Jan. 17, '19	Co	86.0	5.405	394	204	87	170	83	43	12
Jan. 18, '19	W	83.5	5.293	509	281	95	307	99	50	13
Jan. 8, '19	B	81.5	5.204	625	300	63	405	75	62	5
Jan. 3, '19	L	82.5	5.248	565	135	51	151	56	32	16
Jan. 17, '19	Du	81.0	5.182	658	129	70	291	87	25	8
Nov. 13, '18	Hn	79.5	5.114	769	192	70	542	87	30	25
Nov. 26, '18	M	77.0	5.002	995	212	44	390	60	20	*
Jan. 14, '19	Cr	77.0	5.002	995	86	56	230	82	25	20
Mean.....	.....	85.1	5.364	497	214	71	233	73	34	12.6
M. V.....	.....	4.5	.201	218	78	16	103	12	11	4.4
Nov. 7, '18	} R	96.5	5.873	134	300	106	151	76	30	12
Jan. 15, '19		92.0	5.672	213	265	98	148	74	20	10
Oct. 31, '18	} Ho	87.0	5.449	356	140	—	127	—	10	—
Nov. 1, '18		87.5	5.471	338	177	—	117	—	15	—
Nov. 9, '18	} W	80.5	5.159	693	259	44	662	52	55	*
Jan. 18, '19		83.5	5.293	509	281	95	307	99	50	13

It will be seen that the variations here are quite large, 134 and 995 hundred-millionths being the extremes, which

are as 1 to 7.4. On this account it seemed more rational to average these logarithmically, thus arriving at the geometric mean as has been done in columns three and four, than to average the numerical values of column five. The following figures show the results of the two methods comparatively:

	Geometric Mean		Arithmetic Mean
	Cologarithms	Numerics $\times 10^8$	
Mean.....	- 5.364	433	497
M. V.....	$\mp$ 0.201	—	$\pm$ 218
Mean + m. v.....	- 5.163	687	715
Mean - m. v.....	- 5.565	272	279

The arithmetic method gives a larger result with a range of variation only slightly broader.

The remainder of the table consists of the various data just enumerated.

### DISCUSSION

The meaning of the results will perhaps be more readily apprehended from Table III. Here the results in each

TABLE III

SIMILAR TO TABLE II (Q.V.) GIVING RATINGS INSTEAD OF EXPERIMENTAL VALUES.

The ratings are 1 to 5 in order of superiority, 3 indicating the average class.

Date	Obs.	AT	AT-15 5 Min.	AT-15 15 Sec.	70 Mm. 5 Min.	70 Mm. 15 Sec.	50 Mm. 5 Min.	50 Mm. 15 Sec.
Nov. 7, '18.....	R	1	5	5	2	3	2	3
Dec. 20, '18.....	Do	1	5	4	1	2	3	2
Jan. 6, '19.....	O	2	5	4	3	3	5	4
Jan. 7, '19.....	Sh	2	4	—	3	—	5	—
Jan. 17, '19.....	G	2	2	3	2	2	2	1
Oct. 31, '18.....	Ho	2	2	—	2	—	1	—
Nov. 4, '18.....	Si	2	1	1	1	1	1	2
Jan. 17, '19.....	Co	3	3	4	2	4	4	3
Jan. 18, '19.....	W	4	4	5	4	5	5	3
Jan. 8, '19.....	B	4	5	2	5	3	5	1
Jan. 3, '19.....	L	4	2	1	2	1	3	4
Jan. 17, '19.....	Du	4	1	3	4	5	2	2
Jan. 13, '19.....	Hn	5	3	3	5	5	2	5
Nov. 26, '18.....	M	5	3	1	5	2	1	*
Jan. 14, '19.....	Cr	5	1	2	3	4	2	5
Nov. 7, '18.....	} R Ho W {	1	5	5	2	3	2	3
Jan. 15, '19.....		1	4	5	2	3	1	2
Oct. 31, '18.....		2	2	—	2	—	1	—
Nov. 1, '18.....		2	2	—	1	—	1	—
Nov. 9, '18.....	} W {	4	4	1	5	1	5	*
Jan. 18, '19.....		3	4	5	4	5	5	3

column in the first section of Table II. have been 'rated' in five classes, according to the theory of probability, in such a way that out of an indefinitely large number of cases, of which these fifteen may be considered a sample, one fifth showing the most favorable values would rate 1, the next fifth 2, and so on, the lowest fifth rating 5. Thus 3 would be the rating of the intermediate fifth of the whole number, the group of one fifth falling nearest the average.

This was done independently for each column of results (excepting the actual brightness, column 5, Table II.) and the ratings are given in Table III. It was found that those rating higher as to absolute threshold were, on the whole, slower to return to within 15 mm. of that point ('AT-15 mm.'). Accordingly, the arrangement of the observers in the table was based in these two considerations and the same arrangement followed in constructing Table II.

This fact, upon a few moments reflection, is not surprising. The observer who has a very low absolute threshold is taken farther from it, so to speak, in adapting to the standard blinding brightness and might be expected, other considerations equalized, to be longer returning to it, or to a point remote from it by an equal interval, and is not necessarily slower in getting back to a given threshold. For example L, rating 4 as to absolute threshold, rates on the whole almost, if not quite, as well as R or Do in point of his return to 70 or to 50 mm.

It is fortunate that Table III exhibits in four individual cases four types of adaptation. These are: high and low grade adaptation, each with rapid and slow recovery as indicated:

		Absolute Sensitivity	
		High	Low
Quick Recovery.....	Si 2, (1,1)		Cr 5, (1,2)
Slow Recovery.....	R 1, (5,5)		W 4, (4,5)

The plotted curves for these are reproduced in Fig. 4. The curves for the other observers, drawn from the observations following the 5 minutes blinding, fall within the limits of these, except for one or two which intertwine with one of the extremes appearing in the figure, and except for

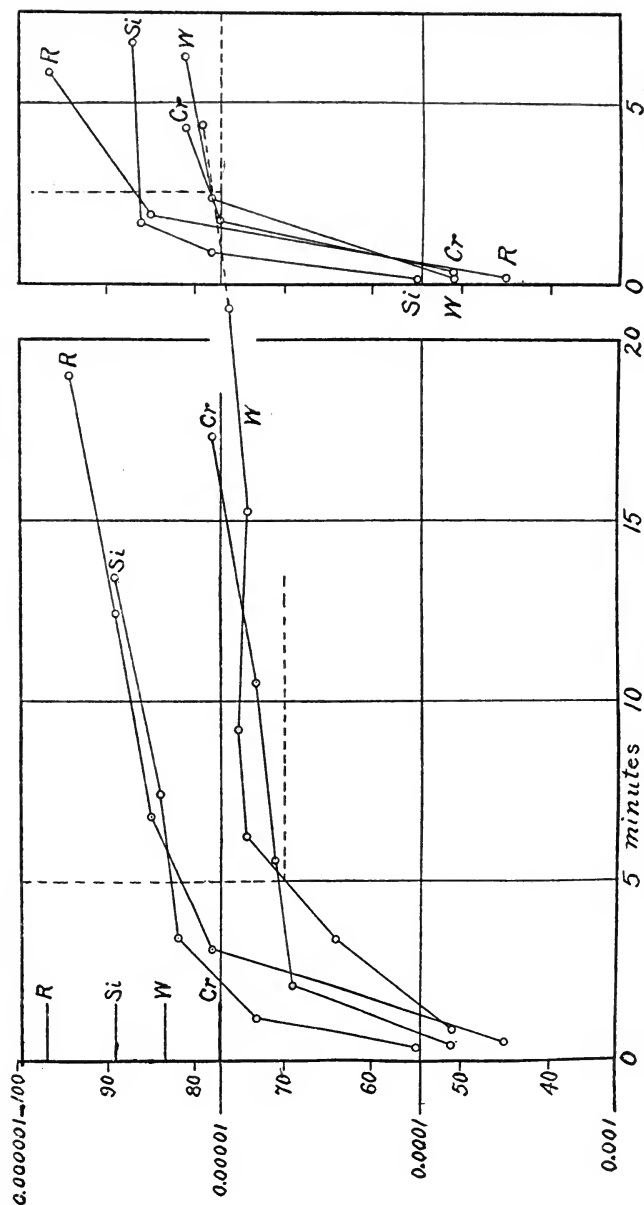


FIG. 4. Four cases showing typical courses of adaptation as to extent and rate:  $R$ , high sensitivity, slow recovery,  $S^2$ ; same, rapid recovery;  $C_T$ , low sensitivity, rapid recovery,  $W$ , same, slow recovery.

On the left, after 5 minutes blinding, on the right after 15 seconds blinding.

The absolute thresholds, previously taken, are indicated near the upper left angle of the figure.

two special cases (which will be discussed later) in which the curve distinctly drops below the lowest one of the four.

It will be noted by comparison of the curves of R and Cr showing respectively high absolute sensitivity with slow recovery and the reverse, that Cr is somewhat more quick than R in reaching a given stimulus, up to the neighborhood of 70 mm. The case is somewhat different if we compare the curves of R and Cr, resulting after 15 seconds blinding, as they appear in the right-hand portion of the plot. Here Cr appears to be the slower and this would, in all probability, be shown more definitely if the curves could have been drawn from more abundant data and more in conformity with the course of the absolute threshold instead of being, as they are, simply broken lines joining rather widely separated points.

Examination of all the curves points to a possibility of dividing the course of adaptation, for the technical purpose of studying and testing individuals, into two portions: the early abrupt rise out of the relatively blinded state, and the late, slow approach to the absolute threshold. The tentative limits of these are indicated in the plots (Fig. 4) by the dotted lines—in the case of 5 minutes blinding all observations after five minutes and above 70 mm. on the scale (within the dotted right angle) being classed in the latter portion of the curve; the corresponding limits being  $2\frac{1}{2}$  minutes and 76 mm. in the case of 15 seconds blinding. All this, of course, is tentative and is to be taken in connection with what is subsequently to be said in regard to the practical phase of the subject and the photometric conditions which the vision of the aviator has to meet in actual flying.

Examination of Table III., columns headed 70, shows that the first seven observers having the lowest absolute threshold return to the point 70 on the scale in better time, on the whole, than the average, and the last seven observers in slower time. The point 70 mm. therefore reflects on the whole rather the absolute threshold than the individual's speed of recovery.<sup>1</sup>

<sup>1</sup> The variations in the average times of recovery under the different experimental conditions, to be expected concomitantly with different degrees of absolute

Under the head 50 mm. the distribution of ratings is seen to be more even, regardless of the two individual factors. These latter results (time to reach 50 mm.) are however less complete and less trustworthy than the former. It can only be said that a selected point on the scale, taken as the sole basis of rating and reflecting on the average neither the absolute threshold nor the individual rate of recovery (that is, giving equal weight to both) would have to be chosen somewhat below 70 mm. on the present scale.

The degree of uniformity with which results may be reproduced in successive experiments is indicated by the figures in the lower blocks of Tables II. and III. In the cases of *Ho* and *R*, the ratings are either identical or, in some cases, one stage apart. The wide difference in the case of *W* is accounted for. In the experiment upon him of November 9 the usual order of experimentation was reversed in that 15 seconds blinding was carried out first, then 5 minutes. The outstanding feature of the result is that in the case of November 9, the subject rates high after the 15 seconds period and low after the 5 minutes, whereas on January 18 with the usual order of experimentation his ratings for the two cases are more consonant. The like is true, less markedly, in the case of *M* (upper portion of tables), upon whom also the experimental order was reversed. He shows, especially under the head 70 mm., a disparity of rating for the two

sensitivity, were worked out statistically from the data of Table II. In the following, *m* is the number of seconds difference in the average recovery-time to be expected with a difference of absolute sensitivity corresponding to 1 mm. scale, *i.e.*, as 1 : 1.108, while opposite *r* is given the respective coefficient of correlation followed by its probable error.

Recovery		After a blinding period of:	
to:		5 minutes	15 seconds
AT-15 mm. or	<i>m</i>	7.45 sec. per mm.	2.05 sec. per mm.
AT $\times$ 4.7	<i>r</i>	0.443 $\pm$ 0.140	0.611 $\pm$ 0.117
70 mm., or	<i>m</i>	- 16.4 sec. per mm.	- 0.68 sec. per mm.
204 $\times$ 10 <sup>-7</sup>	<i>r</i>	0.671 $\pm$ 0.099	0.244 $\pm$ 0.176
c.p.sq.m.			

It will be noticed that the time to recover to 70 mm. after 15 seconds blinding is least of all governed by the absolute sensitivity. The rate of variation is - 0.68 seconds per millimeter of scale and the coefficient of correlation is not much greater than its probable error.

different periods of blinding which is unusual, and in every case he rates higher after the 15 seconds blinding than after 5 minutes. It is also true that these two (W and M) show in the same experiments (after 5 minutes blinding where this was preceded by the 15 seconds period) curves which drop below and somewhat outside of the limits indicated by the four types of Fig. 4. This would indicate that in these two cases, at least, the return after the shorter blinding period is more rapid owing to its not having been more or less immediately preceded by an experimental blinding period, while, *per contra*, the rise after a longer blinding period is delayed by reason of a foregoing blinding. This with reference to the general results of the usual order of experimentation as a standard. A further conclusion is that the general result in the bulk of the cases is influenced in a similar way, and the general rate of recovery after the short blinding period is delayed, owing to a residual effect of the prior blinding. If this be true the rise after 15 seconds blinding should, under equal preceding conditions, ensue with greater relative speed than the average results would indicate.

It is possible that the sudden onset of the rather bright light, after complete dark adaptation, may, so to speak, surprise the visual apparatus into more profound changes than would be induced by the same exposure to light applied with gradual onset under similar conditions. Aside from the changes in the size of the pupil there are two anatomic factors undoubtedly concerned in dark and bright adaptation: the exhaustion and regeneration of the visual purple (or possibly other unknown photochemical substance); and the migration of the pigment of the hexagonal cells. This latter may be a protective device that acts fairly promptly, and has the effect of enclosing the retinal rods and by its own light-absorbing qualities reducing the amount of light absorbed by the individual rods. It is conceivable that a sudden flash of light might anticipate this action and produce a strong destruction of the photochemical material in a short time, before the pigment cells have had time to react, while with gradual onset of light the time is adequate



for the pigment cells effectively to assume this protective function.

Some of the curves strongly suggest two factors playing a part in dark adaptation. In Fig. 5 one of these is shown together with a typical smooth curve. Any attempt to smooth out *D*'s curve here would have to proceed on the assumption of large experimental errors at the second and

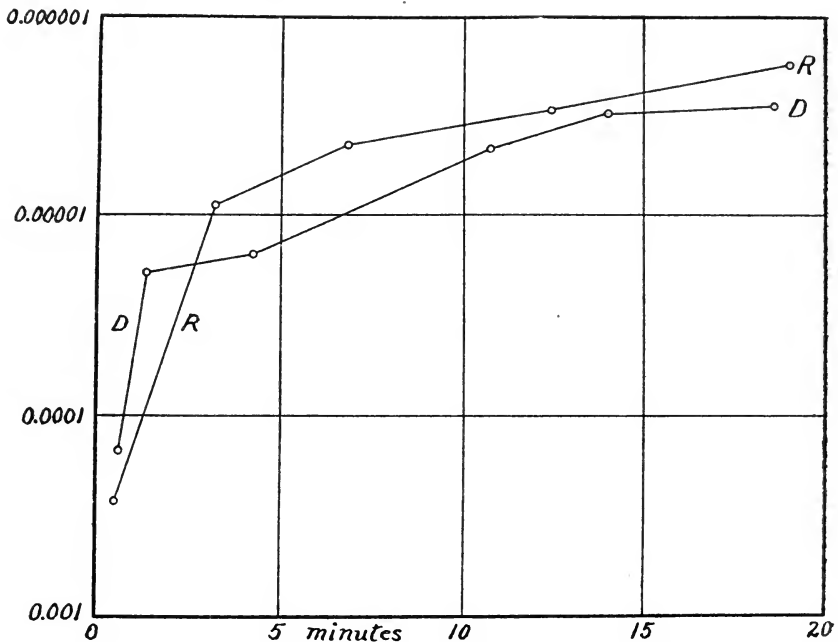


FIG. 5. Two types of adaptation curve. *R*, 'normal' or regular form. *D*, irregular form showing a halt within the first five minutes and subsequently a more rapid rise.

third stations; or else the results would have to be interpreted as arising from two more or less independent mechanisms one of which overtakes the other, in effect, at the end of about four minutes. Such terrace-like irregularities, which may be interpreted as a halt in the progress of dark-adaptation, are present in 14 out of a total of 52 adaptation curves taken, and are suggested in four others. The results obtained after five minutes blinding are plotted for all 15 observers in Fig. 6, to show the great variety of forms the curves may take.

It will be evident from the foregoing discussion of the experimental results that the time required for adaptation to progress to the point of visibility of any test-stimulus is a function of the individual's optimum and also of his rate of recovery from the effect of disturbing light; with one or the other of these of predominant effect according to the magnitude of the test-stimulus selected. No single rating derived from time and stimulus magnitude would necessarily be

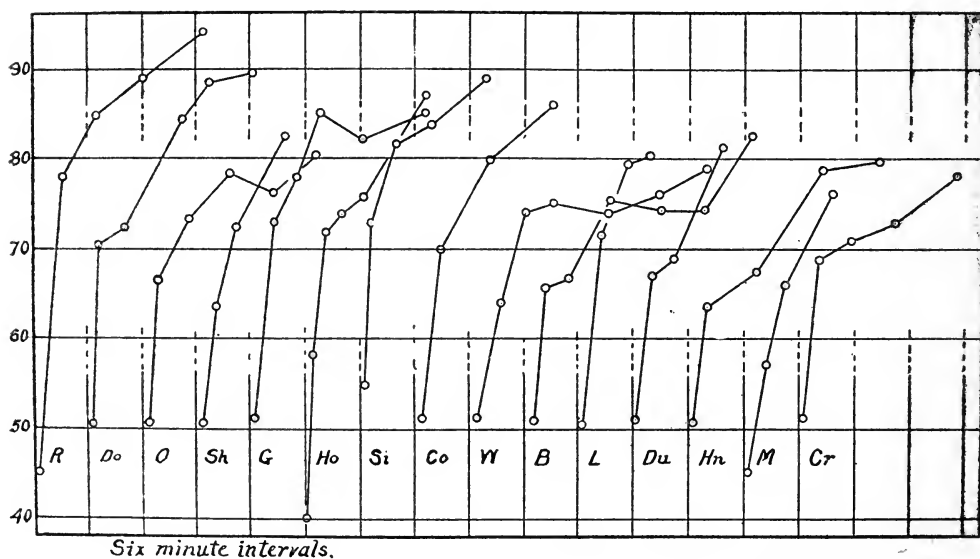


FIG. 6. The course of adaptation after 5 minutes blinding in 15 different subjects. The vertical line in each case is the zero ordinate for the corresponding curve, indicating the instant the blinding period came to an end and the dark-adaptation began. These ordinates are arbitrarily plotted six minutes apart.

valid as an index of the efficiency of the same individual with respect to stimuli of other magnitudes. Again, the ratings of two individuals, taken in this way, might, by the arbitrary selection of the test-stimulus, be so made as to place either one of the individuals above the other. Obviously, such a method of estimating the efficiency of night vision cannot be considered valid.

By way of outlining a method for testing night vision, there is little to be stated in detail until definite data are at

hand (1) as to exactly what, photometrically considered, the aviator must necessarily be able to see in night-flying, and (2) what transient light conditions he may meet which tend to make him blind relatively to such visual objects. The former might afford a basis for the rational selection of a test-stimulus, the latter a basis for standardizing the duration and intensity of the experimental blinding.

As far as is known to the writer, photometric measurements of the brightness of the sky, the earth's surface and the objects upon it have never been made under natural illumination at night. Such measurements would show whether the requirements of vision at night are more lenient than those demanded at the absolute threshold in the experimental dark room, or equally exacting; and whether the rate of recovery is more important, for practical purposes, than the value of the absolute threshold. In short, such data, taken in connection with data such as those of the present work, would show which portion of the adaptation curve is the more worthy of intensive study from the present viewpoint: the early, abrupt rise, out of the relatively blinded state; or the slow approach to the absolute threshold.

#### THE RELATION OF SHADE-PERCEPTION TO THE DARK-ADAPTATION RESULTS

In connection with the foregoing some experiments were undertaken in shade-perception to determine what, if any, relation exists between the perception of small brightness-differences at fairly high levels of illumination on the one hand and the facts of dark-adaptation on the other.

The test-object used for testing shade-perception is a chart composed of ten rows of eight white letters each, on gray back-grounds ranging from very dark to very light.<sup>1</sup> The various gray backgrounds, each bearing a row of letters, are 4 x 18.3 cm., with a white interval 1.8 cm. between. The row on the darkest gray is marked L 1/10, the next 2/10 and so on, the lightest being L 1. The letters are of the

<sup>1</sup> Plate V. of DeWecker et Masselon, 'Échelle Métrique pour Mesurer l'Acuité Visuelle.' O. Doin et fils, Paris, 1914.

gothic style and approximately 7.6 mm. high, designed to subtend 5 minutes angle at 5 meters and would thus represent 20/20 vision if printed in black and white. The chart was illuminated by a 40-watt vacuum tungsten filament lamp, with reflector, from approximately 80 cm. distance, the room otherwise being in darkness.

TABLE IV

THE RESULTS OF THE SHADE-PERCEPTION EXPERIMENTS, COMPARED WITH THE DARK-ADAPTATION RESULTS: (1) ABSOLUTE THRESHOLD AND (2) INDIVIDUAL SPEED OF RECOVERY.

In the latter the class-numbers are plotted, cf. Table III. The shade-perception chart was used at 5.1 meters except as specified.

Obs.	Vision		Dark Adaptation.		Shade Perception		Remarks
	Uncor.	At Expt.	AT, Mm.	Speed Class	Lt. Adpt.	Dark Adpt.	
R		20/15— 20/15—	94.2 92.0	5/5	1/10—4 1/10—3		
O		20/15 20/15	88.5	5/4	9/10—1	9/10+2	
Sh		20/15 20/20	88.0	4/—	7/10—2	6/10—2	
G	20/20— 20/20—	20/15+ 20/15+	88.0	2/3	5/10—2	6/10—1 7/10—4	
Ho	20/30 20/20—	20/10— 20/10—	87.2	2/—	9/10+4		
Co	20/200— 20/200—	20/15—4 20/15—4	86.0	3/4	5/10—1 6/10—2	6/10—4	
L	20/33 20/25	20/10— 20/10—	82.5	2/1		9/10—1+3	At 4.8 m.
W	20/200 20/200	20/15 20/15	83.5	4/5	7/10—1 8/10—1 9/10—3		
W			82.0		9/10+0		
Du	20/70 20/70	20/15 20/15	81.0	1/3	10/10—3	10/10—7	
Hn		20/30—2 20/30	79.5	3/3	9/10—1		At 3.4 m.
Cr		20/15 20/15	77.0	1/2	9/10+1	9/10—1	

The results are given in Table IV., where following the observers' designations are given (1) visual acuity without correction when glasses are worn; (2) vision as applying to the experiment, corrected in case glasses were worn at the time; (3) the absolute (dark-adapted) threshold (mm. scale); (4) the rating with respect to speed of recovery of the same (AT-15, Table III.); (5) the performance with the DeWecker et Masselon chart, light-adapted; (6) the same, dark-adapted.

Investigations with this chart are unsatisfactory, probably arising from the fact that the letters upon it are, with respect to size, too near the visual acuity threshold. It would have been desirable to have a chart in which the former was put out of consideration by having the letters so large that visual acuity, in the usual ophthalmologic sense, could be ruled out.

The chart was actually used at 5.1 meters distance. In the case of observer L, the distance was slightly less, 4.8 meters, due to a purely fortuitous circumstance. In the case of Hn it was 3.4 meters, owing to the fact that this observer could not read even the first line at a greater distance. No relationship is known by which these readings could be corrected so as to be strictly comparable with the others.

Another difficulty with this chart is the fact that the observer is frequently able to read several lines, excepting, in each line, an approximately equal number of letters. That is to say, there is more divergence in visibility between letters of the same line than between one line and the next (see observers G, Co, W, Table IV.).

The shade-perception readings under the head 'light-adapted' were taken either just before the dark-adaptation experiment, or in some cases at another time. Those in the next column, headed 'dark-adapted' were taken at the close of the dark-adaptation run, immediately upon the turning on of the light. The latter gives in most cases a somewhat poorer indication, usually by a few letters only, occasionally by as much as a line. In one or two cases the reverse is noticed (O, G) sometimes plainly the result of memory of

the chart and of practice (compare Co, W, under 'light-adapted,' with G). The chart was difficult and uncertain to work with, and the effect of dark-adaptation, to which a slight falling off might in some cases be attributed, was very transient and soon wore off or became masked as the observer studied the chart and became familiar with it. In what follows the effect of the state of adaptation upon shade-perception will be ignored, and for each observer his best reading will be used, for comparison with the extent and speed of his dark-adaptation.

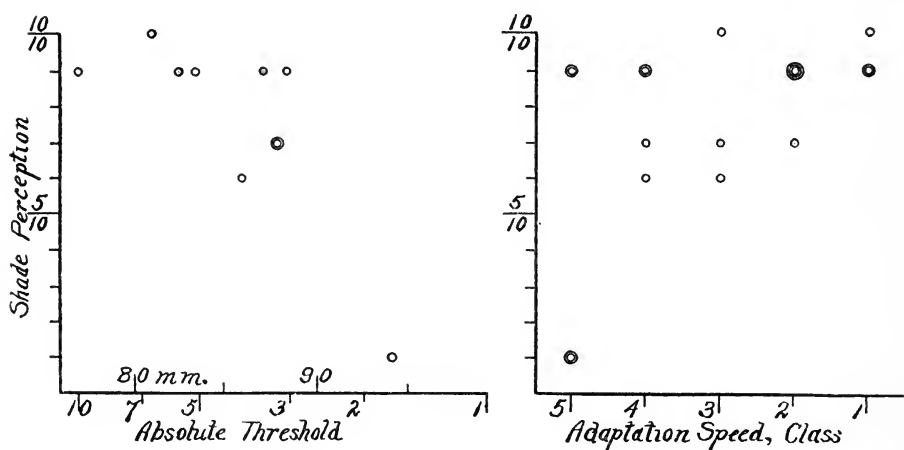


FIG. 7. Relation between dark-adaptation and shade-perception, the latter taken with the DeWecker and Masselon 'light sense' chart. In the figure on the left the abscissæ are absolute threshold, in millimeters of the scale above the line and in millionths of one candle per square meter below. This unit is equal to one hundred of those used in Column 5, Table II.

In Fig. 7 the shade-perception readings are plotted (a) against the absolute threshold and (b) against the speed-rating (AT-15, Table III.). The numerical designation of the last line that the subject attempted to read was plotted, disregarding the few letters missed, since there was no way of appraising the significance of the latter. In so far as any correlation is shown, the sensitiveness to shade difference relates itself in an inverse sense with the absolute threshold (a, Fig. 7) and in a direct sense with the speed of recovery (b, Fig. 7). These results are unsatisfactory. A more

appropriate and less ambiguous form of test chart (or other apparatus) for measuring the sensitiveness to small brightness-differences, and further investigation, are necessary for definite conclusions.

Plotting the shade-difference results against visual acuity, measured by means of the Snellen chart, either with or without the usual correction, showed even less correlation than when the former were plotted against the dark-adaptation results as in Fig. 7.

### SUMMARY

1. Upon investigation of fifteen subjects it appears that the limit of vision in dark adaptation (absolute threshold), measured by the least brightness at which gross form may be recognized, is variable within extreme limits expressed by the ratio of 7.4 to 1. These limits lie between 1 and 10 millionths of one candle per square meter.

2. The rate of recovery, after dark-adaptation is disturbed by a standard exposure to light, is also variable as between individuals and by no means always in a sense corresponding with the value of the absolute threshold. The extremes of variation between individuals as to time of recovery are as 4.5 to 1 and as 2.5 to 1 for a longer and shorter period of blinding respectively. Recovery, as here used, means the individual's return to a definite multiple (by the factor 4.7) of his own absolute threshold.

3. The time of recovery to the point of distinguishing a test-object of standard brightness is to be looked upon as a function of both of these, the one or the other preponderating in effect according to the test stimulus chosen. For a test stimulus 4.7 times the (geometric) mean absolute threshold of all observers, the extreme variations in time are expressed as 8.7 to 1 and 2.75 to 1 for a longer and a shorter period of blinding respectively.

4. The times of recovery are less diverse after a short blinding (15 seconds, 13 candles per square meter) than after a longer one (5 minutes, same), and in the former case recovery takes place, on the average, in about one third the

time, or probably less when all antecedent conditions are equalized.

5. The occurrence, in the course of dark-adaptation, of more or less definite arrests in the decrease of the threshold

TABLE V  
THE OBSERVERS: AGE, VISION AND REFRACTION

Obs.	Age	Refraction			During Experiment	
		Vision Uncorrected	Correction or Remarks	Vision Corrected	Glasses Worn	Vision
R	46	O.D. 20/15— O. S. 20/15—	+0.75 sph +1.00 sph		No	20/15— 20/15—
Do	37	O.D. 20/30— O. S. 20/30—	+1.00 sph +1.00 cyl. ax. 95° +1.00 sph +1.00 cyl. ax. 85°	20/15 20/15	Yes	20/15 20/15
O	24	O.D. 20/15 O. S. 20/15	Slight marginal blepharitis		No	20/15 20/15
Sh	45	O.D. 20/15 O. S. 20/20			No	20/15 20/20
G	36	O.D. 20/20— O. S. 20/20—	−0.25 sph +1.25 cyl. ax. 84° −0.75 sph +2.00 cyl. ax. 95°	20/15+ 20/15+	Yes	20/15+ 20/15+
Ho	39	O.D. 20/30 O. S. 20/20—	+1.37 sph +0.87 cyl. ax. 70° +1.37 sph +0.62 cyl. ax. 110°	20/10— 20/10—	Yes	20/10— 20/10—
Si	27	O.D. 20/20 O. S. 20/20	+0.62 sph +0.12 cyl. ax. 90° +0.75 cyl. ax. 90°		No	20/20 20/20
Co	47	O.D. 20/200— O. S. 20/200—	−2.75 sph −0.62 cyl. ax. 5° −1.75 sph −0.62 cyl. ax. 175°	20/15−4 20/15−4	Yes	20/15−4 20/15−4
W	39	O.D. 20/200 O. S. 20/200	−2.50 sph −0.50 cyl. ax. 100° −2.75 sph −0.75 cyl. ax. 100°	20/15 20/15	Yes	20/15 20/15
B	41	O.D. 20/20−1 O. S. 20/20−3	+1.00 sph −0.37 cyl. ax. 100° +1.00 sph −0.37 cyl. ax. 80°	20/15* 20/15*	Yes	20/15* 20/15*
L	55	O.D. 20/33 O. S. 20/25	−0.75 sph +1.50 cyl. ax. 48° −0.75 sph +1.62 cyl. ax. 92°	20/10— 20/10—	Yes	20/10— 20/10—
Du	25	O.D. 20/70 O. S. 20/70	−2.00 sph −0.25 cyl. ax. 120° −1.75 sph	20/15 20/15	Yes	20/15 20/15
Hn	49		−0.50 cyl. ax. 180° −0.50 cyl. ax. 180°	20/30−2 20/30	Yes	20/30−2 20/30
M	36	O.D. 20/20−1 O. S. 20/20+4			No	20/20−1 20/20+4
Cr	23	O.D. 20/15 O. S. 20/15	Possible slight snow-blindness from flying over snow fields some months previously.		No	20/15 20/15

\*Glasses worn by B contained correction for slight presbyopia. Vision was thereby somewhat fogged at 20 feet, but presumably equivalent to 20/15 at the distance used, 52 cm.

suggests the independent working of two physiologic mechanisms. These halts occur, more or less definitely, in 18 out of 52 courses of dark-adaptation investigated, and appear as more or less definite 'terraces' on the rising slope of the curve.

6. A definite photometric knowledge of the minimum



light conditions under which the flier may be called upon to use his vision is necessary before a test method can be intelligently proposed, or further investigation advantageously carried on.

7. The relation between the limit and speed of dark-adaptation on the one hand, and sensitivity to shade-difference under relatively high illumination on the other, is not clear. It would appear that shade difference sensitivity is related to absolute (dark-adapted) sensitivity in an inverse sense and to the speed of recovery of dark-adaptation in a doubtful, possibly direct sense; but the results are such as to leave these conclusions in doubt.

# A DIRECT DEDUCTION OF THE CONSTANT PROCESS USED IN THE METHOD OF RIGHT AND WRONG CASES

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The object of this article is to show how the Constant or Fechner-Müller-Urban Process of calculating thresholds can be directly deduced from first principles. The historical development of the process is first traced, and then contrasted with the direct deduction. The latter throws into clearer relief the nature of the assumptions and approximations made, and justifies Urban's, as against Müller's, Table of Weights.

## I. INTRODUCTION

The Constant Process is employed to fit the integral of a 'normal' curve to data accumulated by the Method of Right and Wrong Cases. The process has been given various names. Often the name 'Method of Right and Wrong Cases' has been used so as to include not only the method of experimenting but also the above process of dealing with the data after collection. But since many other mathematical processes can be applied to the same data it is better to use a second name. Professor G. E. Müller calls it the 'Konstanzmethode.' Professor Urban speaks of the ' $\Phi(\gamma)$  Hypothesis'<sup>1</sup> and the 'Konstanzmethode,'<sup>2</sup> and I have elsewhere called it the 'Constant *Process*,'<sup>3</sup> using the word process to indicate a process of calculation, and leaving the word method to mean a method of experimenting. The process can be

<sup>1</sup> F. M. Urban, 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen,' *Archiv f. d. ges. Psychol.*, 16, 1909, p. 366.

<sup>2</sup> F. M. Urban, 'Die Praxis der Konstanzmethode,' Leipzig, 1912.

<sup>3</sup> G. H. Thomson, 'A Comparison of Psychophysical Methods,' *Brit. Journ. Psychol.*, 5, 1912, p. 210.

applied to data collected by other methods, *e.g.*, The Method of Non-Consecutive Groups.<sup>1</sup>

In the descriptions of the Constant Process given by Professor Müller<sup>2</sup> there is an error of omission which remained undetected until the appearance of Professor Urban's article already quoted, in 1909. Historically the Constant Process arose as an application, to a particular problem, of the Method of Least Squares, of which latter I next give a short account.

## II. THE METHOD OF LEAST SQUARES

When observations have given a certain number of observation equations for a smaller number of unknowns, it is impossible in general to find exact solutions for the whole set of equations, for they are, owing to observational errors (and, in psychophysics, also owing to subjective variations) more or less inconsistent with one another. In such a case what is called the Method of Least Squares can be applied to find what are, with certain assumptions, the most probable values of the unknowns. Let there be  $k$  equations for  $g$  unknowns ( $k > g$ ), and suppose that these equations are linear, as follows:

$$\begin{array}{rcl} a_1x + b_1y + c_1z + \cdots + f_1u + g_1w + \alpha_1 & = & 0, \\ a_2x + b_2y + c_2z + \cdots + f_2u + g_2w + \alpha_2 & = & 0, \\ \cdot & & \cdot \\ \cdot & & \cdot \\ a_kx + b_ky + c_kz + \cdots + f_ku + g_kw + \alpha_k & = & 0. \end{array}$$

Moreover, suppose that these equations are not equally trustworthy, but are based upon various numbers of experiments  $n_1, n_2, \dots, n_k$ . Then the rules of the Method of Least Squares run as follows:

To find the 'normal equation' for  $x$ , multiply each equation (1) by its weight  $n$  and (2) by the coefficient of  $x$  in that

<sup>1</sup> G. H. Thomson, *loc. cit.*, p. 204.

<sup>2</sup> G. E. Müller, 'Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik,' Wiesbaden, 1904, par. 11: 'Ueber die Massbestimmungen des Ortssinnes der Haut mittelst der Methode der richtigen und falschen Fälle,' *Pflüger's Archiv*, Bd. 19, 1879, pp. 191 ff., and elsewhere.

equation, and then add all the equations together. Similarly, 'normal equations' can be found for  $y, z, \dots, u$  and  $w$ . There will thus be exactly as many normal equations as unknowns, and the values of the latter found therefrom are the 'most probable' values, provided we assume that the errors or variations arising in our experiment were all 'normally distributed.'

### III. A DESCRIPTION OF THE FECHNER-MÜLLER-URBAN PROCESS AS IT AROSE HISTORICALLY

This process arose from an application of the above rule to a particular problem in psychophysics. Certain stimuli

$$r_1, r_2, r_3, \dots, r_k$$

were presented each  $n$  times,<sup>1</sup> in a certain way, to a subject, who gave at each stimulus value a different proportion of the looked for reaction, these proportions being

$$p_1, p_2, p_3, \dots, p_k.$$

The assumption is now made that

$$p' - 1/2 - \frac{1}{\sqrt{\pi}} \int_0^{h(r-S)} e^{-x^2} dx = 0, \quad (1)$$

$p'$  being a function of which the above  $p$ 's are experimental values, while  $S$  is the required 'threshold' and  $h$  is its 'precision,'  $\sigma = 1/(h\sqrt{2})$  being the 'standard deviation' of the distribution. By inserting in equation (1) the  $k$  experimental pairs of values of  $p$  and  $r$  we obtain  $k$  observation equations for the two unknowns  $S$  and  $h$ . The equations will never in practice be exactly consistent with each other, and no pair of values  $S$  and  $h$  will exactly satisfy them all, but will leave small residuals  $v_1, v_2, v_3, \dots, v_k$  instead of giving zero in each case.

The Fechner-Müller argument then goes on (erroneously, as Professor Urban showed later) to postulate that the best

<sup>1</sup> To avoid unnecessarily complicating the argument I assume each was presented  $n$  times. If the different  $r$ 's are presented different numbers of times a 'weight' in the ordinary sense of the word has to be attached to each observation equation in what follows, proportional to the number of presentations of that stimulus.

pair of values  $S$  and  $h$  is that which makes the sum of the squares of these residuals,  $\Sigma v^2$ , a minimum. As the rules for forming normal equations are only intended to apply to linear observation equations, however, a plan had to be devised to replace the above equations, which at present include integrals and are anything but linear, by linear equations. This was ingeniously managed by Professor Müller by means of the 'method of weights,' as follows:

In tables of the probability integral we find values of  $p$  and corresponding values  $\gamma$  of the upper limit of the integral of equation (1). Fechner's Fundamental Table, as it is called, is merely such a table with rather wide intervals specially published for psychologists at that time. We can therefore write down those  $k$  values of  $\gamma$  which correspond exactly with our  $k$  experimental values of  $p$ , and equate them to the  $k$  values obtained by inserting  $r_1, r_2, \dots, r_k$  in the upper limit  $h(r - S)$ . We thus have  $k$  equations of the form

$$\gamma = h(r - S), \quad (2)$$

where the  $r$ 's are the experimental stimulus values, and the  $\gamma$ 's are found (with the aid of a table) from the experimental  $p$ 's, and  $S$  and  $h$  are required. If we write  $hS = c$  these become linear equations in  $h$  and  $c$ , viz.,

$$\gamma - hr + c = 0. \quad (3)$$

Any pair of values  $h$  and  $c$  inserted into the  $k$  equations (3) will leave residuals  $u_1, u_2, u_3, \dots, u_k$ . If we apply the rules of the Method of Least Squares to *these* equations as they stand, the values of  $h$  and  $c$  obtained from the normal equations will be those which make  $\Sigma u^2$  a minimum. This however is not what we want, we want  $\Sigma v^2$  to be a minimum, the sum of the squares of the residuals of the equations (1). We could obtain this if we were to find a set of 'weights'  $\Gamma_1, \Gamma_2, \Gamma_3, \dots, \Gamma_k$  to use on the equations (3) such that each ( $\Gamma u^2$ ) will equal the corresponding  $v^2$ , for the normal equations of the equations (3) thus weighted will then make  $\Sigma(\Gamma u^2)$ , that is  $\Sigma v^2$ , a minimum. The problem of finding these weights is in effect the problem of finding what change in  $p$

corresponds to a change  $\delta\gamma$  in  $\gamma$ . We have

$$p - \frac{1}{2} = \frac{1}{\sqrt{\pi}} \int_0^\gamma e^{-x^2} dx, \quad (4)$$

and therefore

$$\delta p = \frac{1}{\sqrt{\pi}} \int_\gamma^{\gamma+\delta\gamma} e^{-x^2} dx. \quad (5)$$

When  $\delta\gamma$  is sufficiently small this integral, which is the area of a narrow element of a curve, may be replaced by the rectangle found by  $\delta\gamma$  (width) and  $e^{-\gamma^2}$  (height), *i.e.*,

$$\delta p = \frac{e^{-\gamma^2}}{\sqrt{\pi}} \delta\gamma. \quad (6)$$

$\delta\gamma$  corresponds to a residual  $u$ , and  $\delta p$  corresponds to a residual  $v$ , so that, since  $\Gamma u^2$  has to equal  $v^2$ , the sought for weights  $\Gamma$  are given by

$$\Gamma = \frac{e^{-2\gamma^2}}{\pi}. \quad (7)$$

It is convenient that the weights should run from 0 to 1, and the  $\pi$  is therefore omitted in the tabulated values. As weights are merely relative quantities this has no effect on the final result of the calculations. These Müller weights are to be found in several textbooks of experimental psychology. *They are however incorrect, or rather incomplete, and should be entirely dropped for Urban's Weights.*

Urban's weights differ from Müller's in that they contain further the factor  $1/[4p(1-p)]$ . The need for this factor was overlooked by Professor Müller and was first pointed out by Professor Urban (*op. cit.*, 1909). It does not arise from the transition from the equations (1) to the equations (3), but exists previously to this, because the standard deviation of experimental determinations of a constant proportion  $p$  is equal to  $\sqrt{np(1-p)}$ , where  $n$  is the number of trials in each experiment, the distribution being binomial in form. If we write

$$P = \frac{e^{-2\gamma^2}}{4p(1-p)}, \quad (8)$$

the  $P$ 's are Urban's weights which should replace Müller's



Throughout our argument we shall use dashed letters for theoretical values, undashed letters for observed values.

Now let  $n$  experiments be made with each stimulus, and let the proportions of reactions at the respective stimuli be

$$p_1, p_2, p_3, \dots, p_k.$$

What is the probability  $W$  of obtaining this particular set of experimental values for the probabilities? The probability of obtaining any  $p$  as an experimental value of the corresponding  $p'$  is, from first principles, equal to

$$\omega = \frac{n!}{(pn)!(qn)!} p'^{pn} q'^{qn}, \quad (11)$$

where

$$q = 1 - p,$$

$$q' = 1 - p',$$

$$n! = n(n-1)(n-2) \dots 3, 2, 1$$

and since the  $n$  experiments made to find one  $p$  are quite independent of the  $n$  experiments made to find any other  $p$ , the probability of the simultaneous occurrence of  $p_1, p_2, p_3, \dots, p_k$  is the product

$$W = \omega_1 \omega_2 \omega_3 \dots \omega_k. \quad (11a)$$

Now each  $p'$  must, quite generally, be a function of the stimulus  $r$  and of a number of parameters  $\alpha, \beta, \gamma, \dots$ . Those values of  $\alpha, \beta, \gamma, \dots$ , will be the most probable *which make  $W$  a maximum*, and a necessary condition for this is that

$$\frac{dW}{d\alpha} = \frac{dW}{d\beta} = \frac{dW}{d\gamma} = \dots = 0. \quad (12)$$

These equations are those required to find  $\alpha, \beta, \gamma, \dots$ , when the form of the function  $p'$  has been decided upon. When the value of  $W$  is inserted from (11a) each equation of the system (12) assumes the form

$$\sum_1^k \frac{1}{\omega} \frac{d\omega}{d\alpha} = 0$$

and on further substituting the values of  $\omega$  from (11) the sys-



tem (12) becomes

$$\sum_1^k \frac{p - p'}{p'q'} \frac{dp'}{d\alpha} = \sum_1^k \frac{p - p'}{p'q'} \frac{dp'}{d\beta} = \dots = 0. \quad (13)$$

It is at this point, therefore, that the distinguishing factor of Urban's weights, viz.,  $1/p'q'$ , makes its appearance. It arises from equation (11) and the assumption which precedes that equation. We have not yet made any assumption as to the relation between  $p'$  and  $r$ .

*Second Assumption.*—Assume now that  $p'$  is given by

$$p' - 1/2 - \frac{1}{\sqrt{\pi}} \int_0^{h(r-S)} e^{-x^2} dx = 0. \quad (14)$$

Thus the parameters are two in number,  $S$  and  $h$ , and we have on differentiating

$$\begin{aligned} \frac{dp'}{dS} &= \frac{-h}{\sqrt{\pi}} e^{-h^2(r-S)^2} = \frac{-h}{\sqrt{\pi}} e^{-\gamma'^2}, \\ \frac{dp'}{dh} &= \frac{r-S}{\sqrt{\pi}} e^{-h^2(r-S)^2} = \frac{r-S}{\sqrt{\pi}} e^{-\gamma'^2}, \end{aligned}$$

where we have written

$$\gamma' = h(r - S) \quad (14a)$$

for abbreviation. From eqn. (13) we therefore have, by substituting for  $dp'/dS$  and  $dp'/dh$ ,

$$\sum_1^k \frac{p - p'}{p'q'} h e^{-\gamma'^2} = 0, \quad (15)$$

$$\sum_1^k \frac{p - p'}{p'q'} (r - S) e^{-\gamma'^2} = 0. \quad (16)$$

In (15) the  $h$  can be brought outside the  $\Sigma$  and then dropped: while (16) can be written

$$\sum_1^k \frac{p - p'}{p'q'} r e^{-\gamma'^2} - S \sum_1^k \frac{p - p'}{p'q'} e^{-\gamma'^2} = 0,$$

of which the second term is zero by (15). We therefore have as simplified forms of (15) and (16) the equations

$$\sum_1^k \frac{p - p'}{p'q'} e^{-\gamma'^2} = 0, \quad (15a)$$

$$\sum_1^k \frac{p - p'}{p'q'} r e^{-\gamma'^2} = 0. \quad (16a)$$

Herein the  $p$ 's and  $r$ 's are known, and the  $p$ ''s,  $q$ ''s,  $\gamma$ ''s are functions of  $S$  and  $h$  given by (14) and (14a), ( $q' = 1 - p'$ ). These two eqns. (15a) and (16a) are the strict eqns. for finding  $S$  and  $h$ , and *up to this point there are no approximations*. A final test of any Gaussian values of  $S$  and  $h$ , in whatever manner found, must be that they give values of  $p'$ ,  $q'$  and  $\gamma'$  which satisfy (15a) and (16a).

Except by trial or graphical methods however these equations are not soluble, and approximations are necessary to enable them to be handled. These approximations we now proceed to discuss.

Consider the quantity  $p - p'$ , the difference between the experimental and theoretical values of the probability at the stimulus  $r$ . From  $p$  we can find in tables of the probability integral the value of  $\gamma$ , the upper limit of the integral, which corresponds to  $p$ . This  $\gamma$  is then an experimental value of the  $\gamma'$  of eqn. (14a). From  $\gamma$  we can in imagination find, if we suppose the  $S$  and  $h$  known for the moment, a value  $r'$  from

$$\gamma = h(r' - S), \quad (17)$$

$r'$  will in general differ from the real value  $r$ . If we draw a normal curve as in the accompanying diagram, and if  $R$  is the position of the stimulus  $r$ , then the theoretical value  $p'$  is the area of the curve up to the ordinate at  $R$ . Let the real value  $p$  be (say) greater than this, then  $p$  can be repre-



sented as the area to a point a little further on,  $R'$ , corresponding to the value  $r'$ . The quantity  $p - p'$  is then represented by the area  $RPP'R'$ . If this is narrow it may be replaced without great error by the rectangle  $RQP'R'$ , *i.e.*, we write as our *first approximation*

$$p - p' = (r' - r)e^{-\gamma^2}, \quad (18)$$

$e^{-\gamma^2}$  being the ordinate  $R'P'$ . In doing this we have made our *third assumption*, viz., that the experimental values  $p$  can be sufficiently well fitted to make each  $p - p'$  small enough to replace by a rectangle. *This will for example not be the case if there are gross irregularities, such as 'reversals of the first order,' in the sequence of  $p$ 's.* The Constant Process should therefore not be applied to such data; if it is, the values obtained from the approximate normal equations (10) will not satisfy the exact equations (15a) and (16a) with sufficient accuracy.

On inserting the value of  $p - p'$  from eqn. (18) in the eqns. (15a) and (16a) we obtain

$$\sum_1^k \frac{r - r'}{p'q'} e^{-2\gamma^2} = 0, \quad (15b)$$

$$\sum_1^k \frac{r - r'}{p'q'} r e^{-2\gamma^2} = 0. \quad (16b)$$

*Second Approximation.*—To further simplify these, we next replace all the theoretical values except  $r'$  by their observed values, i.e., we write

$$\sum_1^k \frac{r - r'}{pq} e^{-2\gamma^2} = 0, \quad (15c)$$

$$\sum_1^k \frac{r - r'}{pq} r e^{-2\gamma^2} = 0. \quad (16c)$$

This approximation also rests on the third assumption above. The equations are now the same as the normal equations (10) as can be seen as follows.

Write as before

$$P = \frac{e^{-2\gamma^2}}{4pq} \quad (8)$$

and use

$$\gamma = h(r' - S) \quad (17)$$

to eliminate  $r'$ . We thus obtain

$$\sum_1^k P \frac{hr - \gamma - Sh}{h} = 0, \quad (15d)$$

$$\sum_1^k Pr \frac{hr - \gamma - Sh}{h} = 0. \quad (16d)$$

Bring the  $1/h$  outside the summation, drop it, and use the Gauss notation  $[x]$  for  $\sum_1^k x$ , and we obtain

$$\begin{aligned} [Pr] h - [P\gamma] - [P] Sh &= 0, \\ [Pr^2] h - [Pr\gamma] - [Pr] Sh &= 0, \end{aligned} \tag{10}$$

which are identical with our former equations (10), since  $Sh = c$ .

# TIME RELATIONSHIPS IN THE FORMATION OF ASSOCIATIONS

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This paper presents experimental data from the field of animal psychology concerning two related problems. (1) The first question concerns the relative merit of simultaneous and successive presentation in relation to the speed of learning. The animals were required to associate two factors which we may term *A* and *B*. With one group the two terms were presented simultaneously and in the other they were presented successively with an interval of approximately 1 second between them. The comparative number of trials necessary to establish a given strength of functional connection constitutes the measure of the relative efficacy of the two methods of presentation. (2) The second problem concerns the readiness with which a given temporal association will function in a backward as compared to the forward direction. One group of rats learn the association in the order *A—B*, while in a second group the terms are presented in the order *B—A*. In the test experiment the terms are required to function in the order *A—B* for both groups. Comparative records were taken of the number of trials necessary to attain a functional connection of a given strength.

A diagram of the apparatus is given in Figure 1. The dimensions of the box are 24 x 36 inches. The runways are 4 x 6 inches. From the food box emerges the entrance alley which divides into two paths (*R* and *L*) both of which return to the food box. The three openings into the food box can be closed as desired by means of sliding doors (1, 2 and 3). The return paths were broken up by projections from alternate sides of the alley. This device prevented the animals from sensing from a distance whether the door leading to the food was open or closed. Two electric buzzers (*A*) were

located in each return alley. With the exception of the food box and the ends of the return paths the apparatus was covered with a glass top.

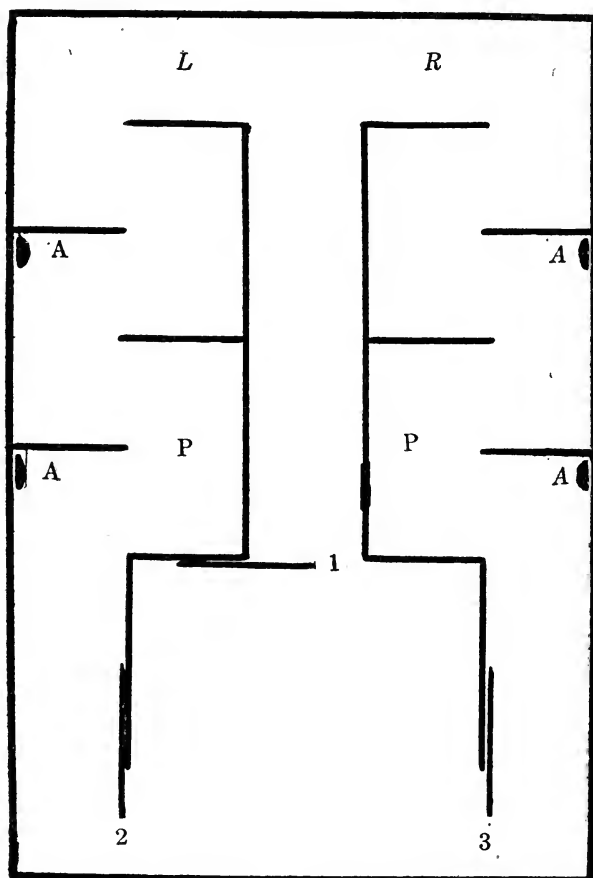


FIGURE 1. Diagram of Apparatus.

All animals were given preliminary, training and test series of trials. In the preliminary series one of the return paths was invariably blocked for each trial. The right and left paths were opened alternately. The rat was inserted into the entrance alley from the food box and allowed to choose between the return paths at random. Naturally the open path will be chosen in approximately 50 per cent. of the trials. The number of consecutive runs per day was

gradually increased from four to a maximum of twenty. The solution of this phase of the problem involved the choice of one of the return paths, the habit of traversing it to the end, entrance into the food box if the path is open, and if the path is blocked a return and choice of the opposite path. The objects of this series were to train the animals to run twenty consecutive trials per day, to eliminate loitering, hesitations, indecisions, and unnecessary activities, and to teach the animal the trick of turning around immediately in response to a closed door and selecting the opposite pathway to the food.

The object of the training series was to establish an effective association between an auditory stimulus (*A*) produced by the electric buzzers and the act (*B*) of turning around and retracing the path when a closed door was encountered. This response had already been thoroughly inculcated by the preliminary series of trials. The procedure for all animals involved the following steps: Both return doors were closed. The rat was introduced into the entrance alley from the food box. The rat traversed the entrance alley and entered one of the return paths. In 20 per cent. of the trials, the door of the pathway chosen was opened after it was entered; in the remaining trials the door of the path not chosen was opened. In the first case the rats secured food by means of the selected path; in the second case the rats invariably found the selected pathway to be blocked and that food could be secured only by means of the opposite path. When the door of the selected path was opened, no auditory stimulus was given; when the pathway was blocked, the auditory stimulus was invariably given in a certain temporal relation to the act of turning around in response to the closed door. In all cases the duration of the stimulus approximated an average of .8 seconds. From this point the procedure differed according to the group. With group 1 consisting of eight rats the stimulus was given while the animal was turning around. The two factors to be associated were thus presented simultaneously. With group 2 consisting of eight animals the buzzers were sounded just

as the rats passed the point *P* (indicated in the diagram, Figure 1) before they reached the closed door. The average time involved in traversing from point *P* to the closed door approximated 1 second. Consequently the two factors were presented in the order *A—B* with a second's interval between them. The third group contained nine rats and with these the stimulus was given at the point *P* after the closed door had been encountered and while the rats were retracing the path. The two factors were presented in the temporal order of *B—A* with an interval of approximately one second.

A test series was necessary to determine the degree to which an effective association had been formed. The procedure was the same as that employed in the training series with the exception of the time when the stimulus was given. The stimulus was presented for all groups at some point in the pathway before the rat encountered the closed door. In order to avoid the formation of a position habit, this point was varied in an irregular manner. As in the training series, the door of the selected path was opened and no stimulus given in 20 per cent. of the trials. This procedure prevented the formation of a habit of invariably turning around in the first path chosen. The formation of an effective association between the two factors will be evidenced by an immediate turn whenever the auditory stimulus is given before the closed door is encountered. Since we desired to measure the gradual growth of the association, it was necessary to interpolate the test trials at regular intervals during the training series. Moreover the number and frequency of the interpolated trials must increase as the problem is mastered. It is unnecessary to give this schema of progression. Suffice it to say that the increase relative to the degree of mastery was the same for all animals.

In groups 1 and 3, progress was measured by the increasing percentages of correct responses made during the test series. A correct response consists of an immediate turn when the auditory stimulus was presented, and traversing the whole length of the path in the 20 per cent. of the trials in which no stimulus was given. Since the latter response



had become inculcated during the preliminary series, mistakes in these trials were rare. As a consequence the percentage records of these groups will begin at 20 and increase to 100 as the response to the auditory stimulus is acquired. With group 2 as the problem is mastered there is nothing to prevent the animals from responding to the stimulus during the training series. The purpose of a test series with this group is mainly to prevent the formation of a position habit. Consequently the progress of this group was measured by the increasing percentages of correct responses in both the training and test series. Since no stimulus was given in 20 per cent. of the trials and these responses were invariably correct, the initial records approximate 20 per cent. and the values will increase to 100 per cent. as the association between auditory stimulus and the act of turning is mastered.

The learning records of the three groups are represented by the graphs of Figure 2. Graph 1 represents the values for the group for which the two factors were presented simultaneously. The records for the group in which the stimulus was given after the act of turning are plotted in graph 3. With both groups the values represent the percentage of correct responses made in the test trials given during each consecutive 100 of total trials. Graph 2 gives the records of the group in which the stimulus was presented before the act with which it was to be associated, and these values are the percentages of correct responses made during each consecutive 100 of training and test trials. These percentage values are plotted in relation to the total number of training and test trials. This procedure is justified on the ground that the test trials probably offered as much opportunity for learning as did the training series. Limitation of time and an accident to a number of the animals prevented a continuance of the experiment beyond a total of 1,400 trials for each animal.

The following conclusions are justified by an inspection of the data:

All curves start at approximately the 20 per cent. level. This result is due to the fact that in 20 per cent. of the trials

the door of the path selected was opened and no stimulus was given. The habit of exploring a pathway to its end was inculcated by the preliminary series of trials, and this mode of response was continued during the training and test trials. Such responses were counted as correct. Progress in associating the sound with the act of turning is thus indicated by a rise of the curve above the 20 per cent. level.

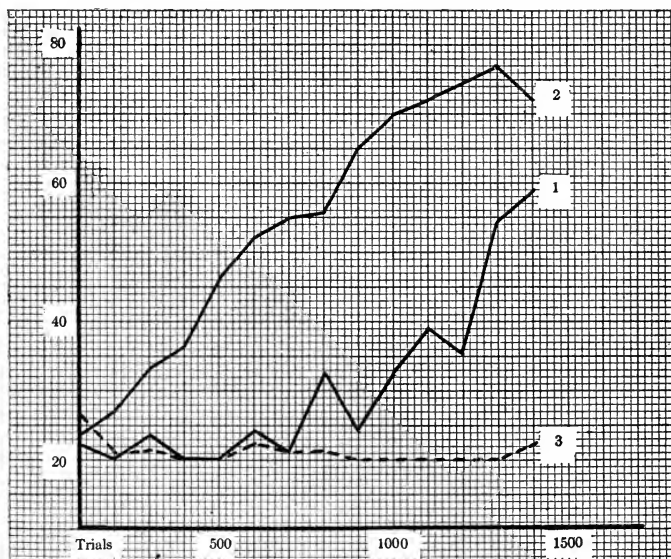


FIGURE 2. The graphs represent the percentages of correct responses per 100 trials. Graph 1 gives the group record for simultaneous presentation; Graph 2 represents the values for successive presentation; Graph 3 records the progress of the group for which the association was tested in the reverse order from that in which the terms were presented.

With our conditions successive presentation of the two terms constitutes a much more favorable method for their association than does simultaneous presentation. This fact is apparent from a comparison of graphs 1 and 2. With successive presentation four of the eight rats attained a proficiency of over 90 per cent. of correct responses, while 80 per cent. was the best record attained by any animal given simultaneous presentations. It is of course possible

that the final records of the two groups would have been approximately identical if the experimentation had been continued until complete mastery of the problem had been effected.

An effective association can be readily formed over an interval of one second provided this interval is filled with some given definite activity.

Associated terms function much more readily in a forward than in a backward direction. Within the time limits of our experiment there is no indication that the third group was making any progress in establishing an association that would function in a backward direction. This general statement holds for each individual as well as for the group as a whole. It is possible that some progress would have been manifested if the experiment had been continued. Such progress might be due, however, to the training effect of the test trials alone.

The curve of learning for simultaneous presentation is one of positive acceleration. That for successive presentation approximates a straight line ascent with some indication of a slight negative acceleration. This difference indicates that the type of curve is a function of the temporal mode of presentation. The result is interesting in comparison with Wylie's results.<sup>1</sup> Our apparatus and procedure were the same as those of Wylie in establishing a negative response to an auditory stimulus with the exception of the mode of presentation of the two terms. Wylie gave the auditory stimulus irregularly but more or less continuously while the animal was traversing and retracing the blocked pathway. He invariably obtained a positive acceleration curve in this type of problem. His animals mastered the problem much more readily than did ours, and this fact is probably due to his method of giving the stimulus. A continuous but irregularly broken sound is likely to be more effective than one of such short duration as that employed in our experiment.

So far as we are aware there are no data in the field of

<sup>1</sup> Wylie, H. H., 'An Experimental Study of Transfer of Response in the White Rat,' *Beh. Monog.*, Vol. 3, No. 5.

animal behavior bearing upon our first problem of the relative merits of simultaneous and successive presentation. The problem has been investigated with human subjects by Bigham,<sup>1</sup> Chamberlain,<sup>2</sup> Wohlgemuth,<sup>3</sup> and Froeberg.<sup>4</sup> In general the data indicate the superiority of the simultaneous mode of presentation. Especially was this result attained in the better controlled experiments. Our data support the superiority of successive presentation. In the human experiments memorizing activities were employed exclusively. Evidently the relative value of the two methods is a function of the kind of subject or the type of activity employed.

Watson's experiment contributes the only data in the comparative field bearing upon our second problem. He tested the ability of rats to run a maze backward after learning it in the forward direction. Considerable ability was manifested. The experiment is not conclusive owing to the fact that animals do considerable retracing while mastering the problem. The subject has been investigated in the human field with memory material by Ebbinghaus, Müller and Pilzecker, and Wohlgemuth. Ebbinghaus employed the relearning method and found a rather strong tendency in the backward direction, but its strength was never equal to that of the forward tendency. With the method of recall, Müller and Pilzecker demonstrated the existence of a backward tendency whose strength was less than that of the forward direction. Wohlgemuth<sup>5</sup> experimented rather extensively varying both method and material. In five cases the forward direction was the stronger, and in two cases the tendencies were approximately equal in strength. He concludes that the forward direction is the stronger when articulation is used in memorizing, but that the two tendencies

<sup>1</sup> Bigham, John, 'Memory,' *PSYCHOL. REV.*, 1894, 1, p. 435.

<sup>2</sup> Chamberlain, A. H., 'A Memory Test with School Children,' *PSYCHOL. REV.* 1915, 22, p. 71.

<sup>3</sup> Wohlgemuth, A., 'Simultaneous and Successive Association,' *Brit. J. Psychol.*, 1914, 7, p. 434.

<sup>4</sup> Froeberg, Sven, 'Simultaneous versus Successive Association,' *PSYCHOL. REV.*, 1918, 25, p. 156.

<sup>5</sup> Wohlgemuth, A., 'On Memory and the Direction of Associations,' *Brit. J. Psychol.*, 1912, 5, p. 447.

do not differ in strength when articulation is prevented. Our experiment proves that the formation of an association between a stimulus and a motor response by animals is exceedingly difficult and perhaps impossible when the stimulus is presented after the act has occurred.

## RETROACTIVE HYPERMNESIA AND OTHER EMOTIONAL EFFECTS ON MEMORY

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A student of mine not very long ago questioned me regarding a puzzling aspect of his recollection of the earthquake of 1906 in San Francisco. And from this I was led to gather reports, partly oral and partly written, from over two hundred students concerning their memory of this and other unexpected crises. Some of the features which appear seem to me to warrant a written account, placing what is perhaps novel into connection with the better-known effects of excitement on our power of recollection.

The effects of excitement due primarily either to bodily or to mental causes may be divided into two kinds, those which are transitory, experienced only during the excitement, and those which are relatively permanent, still noticeable long after the excitement has subsided. Belonging to the first class, there is upon occasion an apparently *general hypermnesia*. One who at the time was in a condition of hypomania told me his surprise at the marked freshening of all his memories; it seemed to him that he could clearly recall the substance of every book he had ever read, and I know him to have been a very wide reader. Beers observed something similar in himself as he passed from depression to elation.<sup>1</sup> *Hypermnesia* has been observed at the moment of danger from a railway train as well as during asphyxiation: the past as a whole, or some great stretch of it, lies clear before the mind;<sup>2</sup> there has been a wide awakening of dormant memories.

In at least superficial contrast, while yet belonging to the same class of effects, there is a more *selective hypermnesia*,

<sup>1</sup> 'A Mind that Found Itself,' p. 80.

<sup>2</sup> Féré, 'The Pathology of Emotions,' Eng. tr., pp. 156, 295.

where certain rather narrow lines of association are followed: a person during the excitement leaps in memory to particular past events connected with the present by similarity or other motive for selective recall. When one of my students was in the excitement which followed the killing of his first buck, there arose in his mind with unusual vividness the detailed scenes, "as good as forgotten," of an earlier experience in that same country. Or, with another, the sudden news of a friend's death enlivened to an uncommon degree, for a few moments, the recollection of particular and hitherto disregarded experiences years before.

"I have found," writes still another, "that if an event later turns out to be very important I can remember exact details, concerning this event, that I was not aware, at the time, of even perceiving."

And as a further illustration: "About a year and a half ago I met casually at the dedication of a service flag in Oakland a man. We spoke of various trivial things and I did not once think of him as one whom I should ever see again. He was almost forgotten by me and indeed when I met him six months later a re-introduction was necessary. This time, however, we became very good friends, and with the development of our friendship there came back to me suddenly one day the picture of this meeting, the people present, the ceremony, the trivial conversation in all its detail, almost with all the little by-play of motion, etc."

The effect belonging to the second class, the effect still present long after the excitement is past, is more varied—at least the observation of the effect is, perhaps merely because the opportunity for observation is more favorable. First there is *hypermnesia for events experienced during the excitement*. The person recalls in almost photographic detail the total situation at the moment of shock, the expression of face, the words uttered, the position, garments, pattern of carpet, recalls them years after as though they were the experience of yesterday. The following will serve as examples.

In 1906 the father of one of my students was almost fatally injured in a boiler explosion; the moment described is that of receiving word of the accident. "I was standing in the hall a few

steps behind mother. She had opened the door and an intimate friend of my father's was standing there. His blanched face told that he had bad news to tell. Mother read the expression immediately and started forward with these words, 'Oh Mr. —, something has happened!' The expression on her face was ghastly and this part is all very vivid to me. I recall the words but do not hear the voices. This friend straightened up and said, 'Now don't let me frighten you too much, but there has been an accident (here mother put her hand to her head) at the plant this afternoon.' Mother cried, 'Oh Will! Will!' and started out the door. 'Is he dead?' The beginning of his reply was 'No, no—' but I don't remember the rest. Up until the time I heard the word 'dead' I had stood motionless but now I ran to my mother and put my arms around her, screaming these very inane words, 'What shall I do? What shall I do?' Mr.— then took me very firmly by the arm and pushed me back, saying in a calm voice, 'What you can do is to be very quiet for your mother's sake and don't cry.' I was somewhat stunned and stood there with my eyes fixed on the hall rug. They were saying something about bringing him home in a few minutes. They had gone into the next room and I heard mother say, 'I'm afraid you're not telling me all. How can they bring him from the hospital if he is so badly hurt?' All this time I was looking at the rug which I can still see plainly. The background was a soft brown and it had a blue and white design over it. Three large diamond-shaped figures were most prominent. They were white and marked off into smaller diamonds by blue lines. For some reason my fancy made them into immense cut glass dishes and I always think of that rug in those terms. There was a blue band which went around the rug and outlined each of the figures. I do not remember leaving the hall, nor do I remember anything more *about mother* until the shock of the earthquake came early the next morning."

The following is from a student's recollection of an automobile accident in 1913. I have directly questioned the writer and am confident that the account has not been 'touched up' for effect. When the collision seemed inevitable, "I gripped the side of the motor to brace myself for the shock and sat there just frozen with terror. I wondered too if I would get blood on my new coat and if my bones would make a noise if they broke. This sounds ridiculous, I know, but I am telling you frankly just what I remember, and what I have been rather ashamed to admit even to myself; it sounds so cold-blooded.



"The man was intoxicated who was driving the other car, and when we turned out on this other road [a detailed diagram illustrates the various positions], instead of going around us, he deliberately left 40 feet of clear road and ran us into the ditch after having struck us broadside. I remember hearing the crash and seeing his front fender strike our mud-guard. I was seated right where it struck, but something saved my feet from being cut off. I felt myself hurled through the air and a hot little knife-stab pain running through the back of my head and neck as I landed right on the back of my head. Then all was oblivion."

But this is by no means the only way in which such shock may work upon our power of recall; there may instead be an unusual reduction of distinctness and fullness of recollection or there may even be a total oblivion of all the immediate circumstances under which the mortal blow fell. *Hypomnesia* may be illustrated by the following taken from my reports.

Some children, one of whom writes the account for me (about four years later), are driving home from school, suddenly to discover that their house is afire. Other and less important events before and disconnected with the fire, and later events, are recalled in vivid and trivial detail, but "after the first discovery I remember in a sort of dazed way things that happened during the hour that I spent comforting the children and helping my mother to save a few things."

Less than a year ago there came to another of my students an intense surprise. Details of the experience preceding and following have been given me, and these arise in memory spontaneously and with remarkable vividness, but the recollections of what occurred *during the exciting events* "can be pictured," says my reporter, "when I call them up, but they do not come of themselves."

Total *amnesia* of the concurrent events, in the cases before me, occurs only where there has been a physical and stunning blow; mental shock alone, in the persons reporting to me, does not have the effect noticed in pathological cases.

These effects, whether of reduction or of heightening, are of course not confined to the instant of perhaps maximal disturbance but may continue for the entire period of the excitement.

Thus far the effects remaining long after the emotion have been effects upon *concomitant* experiences; the emotion has caught up the current perceptions and the inner observations and has either preserved them vividly and readily accessible or has suppressed them, dissociating them in part or in whole.

But there are also lasting effects upon experiences *that have gone before*, effects that are of opposite quality.

And first there is the well-known *retroactive amnesia*, where the person has no recollection of events preceding the crisis.<sup>1</sup> The shock may make it impossible to recall any experiences during a preceding stretch of days, of weeks, or even of the entire past. Among my data, I find what are perhaps examples, but not clearly so, of this effect, in as much as, in many of those who experienced the earthquake in San Francisco, nothing is remembered of the hours or days immediately preceding the event.

But more interesting, perhaps only because it seems to have received far less attention, is the very opposite effect, namely a *retroactive hypermnesia*, of which I find many instances.

The following tells of scenes during boating which began about half an hour before an exciting occurrence; much of the original narrative is omitted. "My first vivid memory comes when we had reached the boat and were just starting from the shore. There was a small wooden platform about five boards wide from which we stepped into the boat. There were noticeable cracks between the boards of the platform and the wood was wet and frayed along the edges of the cracks. Throughout the boat-ride I do not remember what the other persons in the boat said or did or how they looked. The water was absolutely smooth when we started, but there were no reflections in it because it was already dusk and the high banks of the stream made it seem even darker. Perhaps that is why I remember no colors. On the other side of the stream the bank was flat and was covered with tall rushes. The rushes extended out into the water so the edge of the bank could not be distinctly seen. A short distance farther on there was a bend in the stream. On the inside of the turn there was a limb of a broken

<sup>1</sup> Cf. Féré, 'Pathology of Emotions,' Eng. tr., p. 295; Janet, 'Major Symptoms of Hysteria,' pp. 37 ff.

tree projecting out into the water. It had some rather stubby looking twigs sticking up from it and was itself in the midst of some driftwood and leaves which had accumulated there. At one place we passed under a log bridge and I remember noticing that the bark had been pulled off in places and that what remained was wormeaten. On our way back down the stream we passed close to a bank of large brake ferns and I can distinctly see how each one, as I looked up the bank, seemingly overlapped the one above it, and they seemed to grow smaller and their outlines less clear in a definite degree as they neared the top. I do not remember anything about the end of the ride, or my going up to our cottage except that I know it took place."

The following is from the recollection of the events before an automobile accident. "The 22d [of August] dawned clear in the San Joaquin valley. I arose early and remember wearing a pink gingham dress which was difficult to fasten and of having called upon my cousin to fasten it for me. I remember being rather tired that day and being vexed when asked to run a few errands for my grandmother. We were sitting at the luncheon table when she asked me to go on these errands and her eyes never seemed to me to be so vividly blue as then. There was a canary in the window, too, and I remember his having sung until it was necessary for us to remove his cage. My afternoon was spent in a siesta since it was one of those hot, sultry days of August. About seven o'clock in the evening my aunt, uncle, their two-year-old baby, my two young lady cousins and myself went to Stockton to the theater. It was a motion picture and we started home at half-past nine because the baby was fretting. My aunt, uncle and the baby were seated in the front of the motor while my two cousins and I were in the back. I was quiet and rather serious during the first two miles and did not feel very well. There is a crossroad about three miles from Stockton where we turned off to go to our home. I shall draw a diagram to show the exact position of the cars, and where the accident occurred."

Over two years ago, in the middle of a Sunday afternoon, unexpected word came by telephone of the death of the person's father, a sudden death by accident. "What happened after that call I do not remember nearly so distinctly as the things which had happened during that Sunday before the message came. I remember distinctly running for the car in the morning when going to church, the sermon, people I had never seen before that day who

were in church. I remember the ride home in a machine. The thing which stands out clearest is the discussion a girl and I had when drying the dishes, although in no way was this subject under discussion connected with the events which followed."

The particulars of this last account have been written out for me—the description of the strangers met, the character of the voice of one of them, the outline of the sermon, the name of one of the hymns sung, subject of the substance of the discussion while busied with the dishes. It will be unnecessary to quote these details.

Finally, there is a curious combination of both these latter effects; for the same person the stretch of time preceding the critical event may show an irregular alternation of heights and depressions, vivid recollections followed by a period of utter blank.

"In the spring of 1912 I was living in Beirut, Syria. The Turko-Italian war was in progress. One Saturday morning two Italian battleships appeared in the bay in an engagement with two small Turkish gunboats, which they finally sank. All the events of the early morning are exceptionally distinct, even to slight details. The engagement took place some hours later, and its details are also vivid.

"The time limits which I shall designate below are only approximately correct. My recollection of the first fifteen minutes before the crisis [which came "shortly after nine o'clock" and "lasted about half an hour"] is not very vivid. I know that I was studying—writing a composition. I have a general image of the room in which I sat, and of a few of its details. The half hour previous to that is blank. The hour before that is very clear in my mind. I remember little conversation, but I have distinct memories of all that occurred. The rest of the events of the early morning I can remember, but not picture to myself. I cannot recall what happened the preceding day. The crisis I shall consider as the period from the time the first shell was fired until the firing ceased. The first five to ten minutes are confused. I have a few vivid but disconnected images. The remainder of the period covered by the crisis is very vivid and full of detail. My recollections are both visual and auditory.

"I cannot distinguish the ten minutes following the crisis from the crisis itself in regard to vividness or fullness of detail.

The succeeding hour marks a gradual decline in the vividness of recollections. Always a diminution in the clarity of my visual images is accompanied by a smaller and smaller amount of detail in the pictures. The next hour is relatively vague. I remember, but cannot picture events. During the next hour and a half my recollections again become very distinct; then gradually fade out. The succeeding three hours are almost blank. Then for 15-20 minutes I again have vivid images. The remainder of that day and the next morning until 7:45 are blank. For an hour and a half thereafter I again have recollections as vivid and as detailed as those of the crisis. The morning of the succeeding day I can recall rather vividly, but with very little detail. Then my memory becomes normal. Throughout, my recollections consist of a series of disconnected pictures with varying amounts of detail."

In another case, preceding the sudden word of an explosion at a mountain mill, there is a period of about an hour of photographically literal recollection; and after that hour and before the fatal news, there is about half an hour during which nothing whatever is recalled.

This from the recollection of events before an earthquake when the student writing was about six years old: "The evening before the earthquake I remember very well. We had all spent the evening with some friends; and as they lived very near our house, we walked home instead of riding. I remember a great deal of the conversation; many remarks were passed as to the weather, and that it was 'real earthquake weather.' I well remember who were at the party and the way in which we passed the evening, also in what the refreshments consisted.

"I only remember that which happened the evening before, not the next morning at all. (I believe the earthquake was about one o'clock in the afternoon.)"

## II

Of the permanent *retroactive hypermnesia* in the cases which have come to my notice some further word will perhaps not be amiss. In selecting the material it is obvious that one must keep to cases where the crisis comes wholly without warning. Otherwise there would almost of necessity have been an anticipatory emotion concurrent with the events preceding the crisis and well capable of vivifying them, and there would be no occasion to ascribe their vivid recall to

some back-stroke of the emotion coming only at the time of the crisis.

# I. TEMPORAL FEATURES OF THE RETROACTION

The effect in the cases before me rarely goes beyond the day preceding the critical event. More frequently, and in particular when the crisis came early the next morning, it touches merely the latter part of the preceding day. Still more frequently, when the crisis occurred late in the day, the effect is found only in that day. Furthermore, it is noteworthy that occasionally but not always the effect seems to have sharp borders: it does not come on gradually, but suddenly; there is a clear demarcation between the beginning of the vivified and the end of the unaffected territory; and likewise of the close of the hypermnestic period. Not infrequently, as has already been stated, the antecedent hypernesia is followed, even before the critical event, by hypomesia or amnesia. But in this respect, as in so many others, there are decided personal differences. Indeed, all that is stated here should be understood not as occurring wherever there is an emotional crisis, but only where certain special and often obscure conditions are fulfilled.

The following diagrams illustrate some of these temporal relations and their individual variety. The number of hours or minutes given are of course subject to considerable error.

## STUDENT IN EARTHQUAKE OF 1906

Day Before		Morning of Earthquake	Five or Six Days Following
Early part	Evening	Most vivid and full.	Disconnected memories.
Nothing.	Fairly vivid.		

## STUDENT IN EARTHQUAKE OF 1911

Preceding Time	The Crisis		After-time	
About one or two hours.		About 1 hr.	About 3 hrs.	5"-10" later
Very clear.	Very clear.	Greatest vividness.	Recalled.	Vividly recalled.

## ACCIDENTAL SHOOTING OF THE STUDENT'S SISTER, 4½ YEARS BEFORE

Preceding Part of Day	Shortly Before	Time of Accident (2:30 P.M.)	Shortly After	Latter Part of Day	Next Day
Vivid.	Most vivid.	Most vivid.	Most vivid.	Less vivid.	Normal.

## INTENSE SURPRISE, ABOUT 8 MONTHS BEFORE

Preceding Time		The Crisis	After-time	
Half hr.	Quarter hr.	Hr. and a half	One hr.	Later
Exceptionally vivid.	Vague.	Least distinct.	Vivid.	Nothing is distinct.

## 2. ITS QUALITY

Many of the persons whose experiences I have been permitted to use have testified to the unusual vividness and fullness of these antecedent effects. In some cases the memories preceding the crisis are not only far more vivid than those which accompany and follow it, but they are more vivid than any other of the person's memories, save the most recent.

"I remember numerous little 'nothings' that I can remember of no other particular day," writes one of my reporters, of a time beginning hours before an automobile collision six years ago. "It is as if I were seeing this one day and night through a pair of powerful glasses while I view the other days with just my own eyes, and see their events with almost a gray fog over them."

Another writes: "Events previous to the time of my being struck by the train stand in bold relief. I should say that the events of the five minutes previous are as vivid now (after 3½ years) as are any events two days past. I believe I can account for every move of these five minutes. The time is completely filled in my memory."

The vividness according to these reports is not confined to visual images: sounds are often clearly represented, the precise wording of a conversation, the thoughts which entered into the conversation or which silently passed through the person's own mind, the mood, the general state of mind. Far more frequently, however, there is a vivifying of the pictorial aspect of the experience. But between or before such vivid stretches there are occasionally events recalled "in general," that is, they are clearly known to have occurred, yet they are represented in no living fashion. Personal differences also appear in the ratio of the vividness preceding to that which accompanies and follows the crisis. In some cases instead of the relation mentioned where the preceding was more vivid than the succeeding, there is found an opposite relation: the later out-tops what goes before.

Among the preceding examples, there are sufficient illustrations of a greater vividness before the crisis. The following will further illustrate equal or greater vividness of what accompanies and follows the crisis.

*Automobile Accident, Summer of 1915:* "I can remember the details vividly for about ten minutes before the accident. Before that I have only a general recollection that we were riding along and that I was trying to read. But I can remember the details vividly for a much longer time after the accident. The vividness extends for about two hours or more."

*Earthquake when Seven Years Old:* "I can remember vividly back as far as about noon of the day preceding the earthquake. I remember just as vividly the day of the earthquake and for a week after. But for about a month after, I recall many things; and although the events are vivid, still there are some empty spaces."

*Earthquake of July, 1911:* "The minutes of greatest vividness for me are those immediately following the earthquake for about one hour; then another period of vividness about four hours later; that later period of vividness only covers a period of a few minutes.

"Memory of events for about two hours before crisis is very clear."

The most striking fact is the odd selection emotion makes: it sharply picks out certain stretches of experience for one effect and a neighboring and apparently equally inviting stretch for the very opposite effect. The sharp delimitations and reversals of its effect in one and the same mind are extraordinary.

### 3. FREQUENCY

My data indicate that the retroactive effect is by no means of rare occurrence. Among some 225 persons from whom reports have been received, as many as twenty-five have given me detailed accounts in which this appears. Nor does it seem to be confined to a single limited period of life. There are cases where the effect has come at some crisis in very early childhood. In other cases it has occurred at a later date. I am not clear whether or not the effect is of more frequent occurrence among women than among men.



There is some indication, but not wholly reliable, that the women experience it the more frequently.

#### 4. CAUSES AND CONDITIONS

The peculiar quality of the emotional excitement seems of far less importance in producing the result here described than the intensity of the shock. In some cases fear is the exciting emotion. In other cases it is shock without fear, having more the nature of fearless surprise. In other cases it is sudden and deep sorrow. In still others it is markedly pleasurable surprise. If it is found by later evidence that this phenomenon of retroactive hypermnesia is of more frequent occurrence among women, one would be tempted to ascribe it to the greater mobility of their emotions. Nor would it be surprising to find that this greater emotional mobility, this lower threshold of emotion in their case, explains also the general mnemonic superiority of women, indeed that this and the emotional hypermnesia, concurrent as well as retroactive here discussed, are but different effects of a common cause.

The retroactive effect does not in most cases seem to me to have been caused by a frequent review of the experience, as though the interest in the emotion-giving event had caused something like a deliberate and repeated attempt to connect it with the events which led up to it. The reason for rejecting this explanation is the qualitative difference between these antecedent memories and the memories of all or almost all other events, and which are frequently counted as of far greater importance. The resemblance which the antecedent vivifying bears to that which is concurrent with the emotion indicates that both come from the one source. But it is doubtless true that in some cases there was this later review, and the items are recalled not because of some immediate effect of the emotion, but by an indirect effect, by giving a motive for an interested and one might almost say a deliberate attempt to recover them.

One can as yet only surmise the cause of opposite effects in different persons. It may either be that they have a decided

difference of emotional mobility, or else that their psychophysical organization is such that even with the same quality and intensity of emotion the sudden rush of intense currents does not have the same fixating effect upon the traces of former experience. But still more obscure is the opposition of effects in one and the same person,—I mean not only where the antecedent events are now intensified and now suppressed, and this in irregular stretches, but also where the events preceding are intensified and those which accompany and follow the crisis are reduced in liveliness. It would appear that there are certain limits of intensity within which an emotion vivifies backward or forward. If this intensity is exceeded the experiences connected with it are in some degree suppressed; while if it is not reached the experiences are lost, not so much by an active suppression as by a mere failure to gather up the events into the mesh of interest. The effect of emotion would seem to be analogous to that of certain drugs, where a certain dose excites, while a still greater dose depresses.

Since it is undoubtedly true that emotion facilitates the recall of whatever is noticed during the excitement, and since it is probable that memory images are not different in kind from images of perception, there would seem to be no reason why these memory images should not be treated by the emotional onrush in precisely the way that the perceptive images are treated. An obstacle in the way of such an explanation of the retroactive hypermnnesia would be that preceding experiences, especially those of hours past or of the preceding day, probably do not at the time of the emotion exist in the form of actual images. Their existence is rather in the form of psychophysical dispositions or traces, prearrangements by which the image may under suitable conditions become actual.

The emotion, then, would seem to have the power to go behind mere imagery into these dispositions or traces, and to strengthen them and the connections by which they may be called into life. And not only the traces of the experience which aroused the emotion, but also of trivial and neutral events antecedent to the emotion itself.





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