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Correction: May, 1915, p. 216, line 2, delete "in." The sentence when corrected should read: "One mental process is the meaning of another mental process if it is that other's context."

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THE PSYCHOLOGICAL REVIEW

A REVISION OF IMAGELESS THOUGHT[1]

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Several years ago I was led by some experiments on voluntary movement to conclude that an act might be thought of without any representative or symbolic image, and further study led me to extend this conclusion to other thoughts. My attention was soon called, in a review of this work by Angell, to previous discussions of the same question, connected with Stout's assertion that there was nothing psychologically absurd in the conception of imageless thought. Looking into the contemporary experimental literature, I then made the acquaintance of Binet and of Watt, Bühler and others of the Külpe school, and my own work soon fell into insignificance beside these extensive and many-sided contributions. Even the merit of independent confirmation was not specially important in this case, since such confirmation was forthcoming even from those who, like Wundt, were not at all in sympathy with the conclusions of the imageless thought party. It appeared that imageless thought, the mere gross fact of observation, had come to stay, and that the only question was what to do with it. Some psychologists have assigned great importance to this fact as a demonstration of non-sensory content, while others have avoided so revolutionary a conclusion by explaining the fact away through one interpretation or another; others again have accepted the fact but minimized its importance, treating it as a mere limiting case; and some, while

[1] Address of the President before the American Psychological Association, at Philadelphia, December, 1914.

accepting the *gross* fact, have doubted that it would stand the test of more refined introspection. Meanwhile, my own views have been maturing as the result of continued thought and experiment, and the time is perhaps favorable for resuming the offensive, and endeavoring to uncover the weaknesses of the negative interpretations, and for offering a conception of the matter which may possibly appear superior to those hitherto presented, or at least worthy of some consideration.

Of the interpretations of imageless thought which explain the fact away without allowing it to modify existing systems of psychology, the most important is that of Wundt. It will be recalled that the method employed by the Külpe school in studying the thought processes was drastically criticized by Wundt, who objected to their experiments as being experiments in appearance only, and held that real thinking could not be done to order in the laboratory. He himself preferred to rely on incidental introspections during spontaneous thought, and in fact reports such observations of his own.[1] "In such self-observations," he writes, "it became perfectly clear to me that the thought was not formed during the process of its verbal expression, but was present as a whole in consciousness before the first word was reached. At first none of the verbal or other images, which subsequently appeared in running through the thought and giving it expression, was present in the focus of consciousness, but these parts of the thought appeared successively as the thought was allowed to develop." With only this fact in mind, he admits, one might easily be led to regard the thought as a unit with a distinctive elementary character. But quite a different conclusion is reached when other facts are also taken into account, that of the narrowness of the field of attention, that of the existence of dim content in the background of consciousness, and that of the "total feeling," itself a unit, though generated by a complex of images. A thought, in Wundt's view, is essentially a complex of images, but these parts of the thought are too numerous to be present together in the field of attention. They are present at first only in

[1] *Psychologische Studien*, 1907, 3, 349.

the background and are not introspectively visible; but as the thought is dwelt upon and expressed, its constituent images come successively into view. What then was the apparently unitary thought with which the process started? This, explains Wundt, was a "total feeling," generated by the complex of images in the background, and itself occupying for an instant the center of the stage.

It is obvious that such a position is almost inexpugnably entrenched. The extremely hypothetical nature of the ground renders a direct attack hopeless. So much as this may be ventured, that, if the words expressing a thought are really its constituent parts, it is curious that the same thought can be thought in different words, and even in different languages, and still more curious that the words to fit the thought are not always at hand. Apparently, the same complex may be composed of different elements, and may exist with some of its elements lacking. Further, it is curious to reflect that these verbal images in the background must somehow be present simultaneously and yet in proper sequence, since otherwise they might compose quite a different thought or no thought at all.

But the principal doubt to be raised concerns the "total feeling." This unitary feeling, present without observable images, and "adequate to the thought," would almost meet the demands of the opposing party, except for Wundt's insistence on its being a feeling, to the neglect of its noetic character. Certainly it is not a feeling, in any strict sense, that straightway finds expression in a statement of fact. Wundt's analysis leaves out of account the core of the whole experience, namely, the fact or supposition which was subsequently expressed in a sentence, but which was definitely and clearly present in mind in advance of the words.

Several writers have called attention to the presence of vague or apparently irrelevant imagery in moments that would otherwise appear imageless. The presence of kinesthetic sensations, habitually unattended to, has also been shown in many cases, and thus we have become wary of asserting that a given moment is really devoid of sensory content. Of course, no one has ever supposed that bodily sensation could be absent from the background of any conscious state, but it has been thought possible to distinguish between irrelevant content and content related to the topic of thought. We must, however, recognize the probability that apparently irrelevant sensations and images sometimes enter into the web of thinking. Especially has the attempt been made with some success to extend the James-Lange theory of emotions to cover the so-called "conscious attitudes"; and some would even extend it to cover the imageless awareness of definite facts, contending that every thought has its own peculiar motor expression, and that the sensations generated by the movement furnish the conscious content of the thought; but no one, as far as I know, has found empirical support for this extreme view.

It is worth remarking that the presence of images and sensations in many or most moments of thinking does not disconcert the supporter of imageless thought. He is perfectly willing to admit that such content is often or even usually present; and the only real importance of a few well-attested instances of thought without such content is that they furnish him his most direct evidence of the existence of other content. His main contention is that other content exists, and that it is the most essential and characteristic of all.

But some psychologists, while admitting the occasional occurrence of imageless thought, deny its evidential importance. It is merely the limiting case, they say, in a continuous gradation from thought in clear images, down through thought in medium and dim images, to thought in images at or near the zero mark. The most attractive form of this interpretation is that which sees in the graded series the progressive automatization of a thought through practise. When the thought is novel, it comes with abundant sensory content, but as it grows familiar and habitual it becomes less sensuous, that is to say, less conscious, until, just as it is about to become automatic and unconscious, it still shows a feeble spark of conscious life; and this feeble spark is pounced upon by the imageless thoughter and rashly heralded forth as proof of some unrecognized species of conscious experience. In reality, imageless thought is imageless because it is all but unconscious. This genetic interpretation has been presented with most force by Titchener[1] and by Book.[2]

The undoubted attractiveness of this conception comes from its following so neatly from the law of practise, and its deficiencies arise from its taking account of only one side of the practise effect. There is much in practise besides the tendency toward automatism. Seldom does the course of training consist of repeating time after time the same performance, only with increasing smoothness and speed. Usually the process begins with varied and tentative reactions, and advances by selection and elimination. Moreover new forms of reaction, made possible by the progress in facility, make their appearance in the course of training. Thus the perfected act omits elements present at the start and contains elements not present at the start, and may be an entirely different means of reaching the same result. If therefore the first thinking on a given topic is fraught with imagery, while the practised thought on the same topic is bare of images, it does not in the least follow that the imageless thought is a condensation of the imaginal. It may be a more economical substitute. The imagery present at the start may have been due to a diffusion of excess energy such as is common in unpractised acts, or it may have furnished a round-about way of dealing with the problem and have given place with practise to the more direct attack represented by the imageless thought.

Practise experiments give little ground for believing that a series of part acts, by simply becoming very easy and swift, blend together into a total act in which the parts are lost to sight. Rather has it been found true that the more inclusive acts, such as dealing with words and phrases as units, in typewriting and telegraphy, arise suddenly as new forms of action, in the progress of training, and themselves make possible a great increase in the speed of the partial or lower-

[1] "Experimental Psychology of the Thought Processes," 1909, pp. 173, 183, 187.

[2] Psychological Review, 1910, 17, 381.

order acts. The partial acts do not blend to produce the inclusive act, but the latter is hit upon and causes the former to blend. Attention deserts the parts, which thus become automatic; but attention still remains keenly alive, being directed to the more inclusive acts. These higher acts are real units, and not mere blends; they are clearly conscious and yet not in imaginal form; indeed, they seem the very type of an imageless thought.

Observations of new ideas, at their first appearance in an individual, would be of interest in relation to the interpretation of imageless thought as exclusively old and well-drilled thought. In the hope of gathering such observations, I have sought to catch myself at moments when some new idea germinated in my mind. Unfortunately, opportunities have not presented themselves with the frequency that could be desired; but, in the few instances that I have collected the experience could be described as the dawning of some new meaning in things, sometimes with scrappy verbal and visual images, sometimes with none that were observable. When they occurred, the images were promptly forgotten, though the thought was firmly impressed on memory. So far from accepting the view that imageless thought is automatized thought, I should be inclined to believe that a new thought is characteristically imageless, and that it attaches itself secondarily to a word or other convenient symbol, and is more apt to occur with an image when it is somewhat familiar than when it is new.

Still another interpretation of imageless thought, or of the observations that purport to reveal it, presents a serious obstacle to our progress. Frequently such statements as these are contained in the subject's retrospective report: "I thought of such and such an object," or, "I thought that such and such was the case," this being the extent of the subject's description of his experience, except for the purely negative statement that no images were present. The objection has been raised by Dürr,[1] von Aster,[2] and Titchener,[3] that in

[1] *Zeitschrift f. Psychol.*, 1908, **49**, 313–340.

[2] *Ibid.*, 56–107.

[3] *Op. cit.*, p. 147.

such reports the subject is not playing the game. He has fallen from psychological description into the commonsense habit of telling what he has been thinking about. He has committed the Kundgabe or expression error: instead of describing his thoughts, he is expressing them. He has committed the stimulus or object error, and, instead of describing consciousness, is mentioning the objects with which consciousness was concerned. Confronted with this objection, the subject is apt to reply that he has done his best, that what was present in his mind was precisely the fact or object mentioned, and that if he is forbidden to refer to the object, all he can do is to hold his peace. Though this reply fails to satisfy the critic, there is something to say in the subject's behalf. Suppose, for the sake of argument, that the specific thought content exists: how would you propose to describe it? You offer the subject his choice of sensory terms, but these he rejects as not fitting the case. If then you exclude reference to objects, you have nothing further to offer him beyond a few vague and negative terms, such as "imageless," "peculiar, unanalyzable state," etc. In fine, the objection has force only on the assumption that the state should be described in sensory terms, and that non-sensory content is non-existent. It prejudges the case.

It is curious that the presence of the stimulus error in reports of images is not treated with a similar seriousness. Seldom in the literature will you find an image really described. Instead of an analysis of the visual picture as composed of colors and shadings in a certain spatial arrangement, instead of an analysis of the auditory image as consisting of a sequence of elementary sounds, you read of "a visual image of a Massachusetts town," or of "an auditory image of the experimenter saying 'subordinate concept.'" If it is committing the stimulus error to report a "thought of" such and such an object, it is equally committing it to report an "image of" the object. A strictly descriptive regimen would require the subject, one would think, to exclude all reference to the object in the one case as in the other.

Yet consider the situation of an observer who is forbidden to refer to the object in describing his images. He would have to confine his report to such statements as "a bright, somewhat variegated spot against a dark ground," omitting to state that this was an image of his friend's face. Yet, if the image, whether faint or vivid, schematic or detailed, was for him, at the moment, an image of his friend's face, can he properly describe the consciousness of that moment without reference to his friend? No question of the logic of meaning is here involved, but a mere question of fact: Was or was not a reference to the object present in the momentary consciousness; and, if so, can the state be described without reference to the object?

The same question arises when we have a presented object instead of an image. I hear a noise from the street and say, "There is a horse galloping past." This is a commonsense reaction which makes no pretense of describing consciousness. But suppose I do attempt to describe consciousness. It is then, perhaps, in order for me to tell exactly what auditory sensations I had. If I do this as well as possible, and find nothing further, such as an image, to report—have I then, with my inventory of auditory sensations, fully accomplished my task of describing consciousness? It would seem not, if I actually was conscious of a galloping horse, while my report makes no mention of this object. It is all very well to warn me of the stimulus error if I show a tendency to go beyond my momentary experience and tell something about the horse which may be objectively a fact but was not present in my mind at the moment; but if I stick closely to the momentary experience, reference to the object is quite in order and in fact indispensable; for, as a matter of fact, reference to the object was probably the most prominent part of the experience. This is equally true in the case of an image, and I must conclude that an observer is perfectly justified in reporting an "image of his friend's face," and that he could not omit this reference to the object without badly mutilating the experience. If so, the observer who reports the "thought of such and such an object" is equally within his rights. He may have omitted something which a complete description should include, but he has, in all probability, reported the most prominent datum of his momentary consciousness.

One further important objection to the doctrine of imageless thought is contained in the teaching of such men as James, Ebbinghaus and Dewey. In speaking of non-sensory content, we have neglected to define sensation, or, worse yet, we have, according to these authors, fallen into the error of excluding relations, forms, patterns, meanings from our concept of sensation, and then being badly put to it to explain how they get into perception and thought. It is impossible, we are told, to draw a line in sense perception between what is sensation and what is perception; and there is therefore no excuse for speaking of non-sensory content in sense perception, nor for speaking of such content as present in thinking, unless we are ready to make the improbable assertion that positive content is vouchsafed us when withdrawn from the world of sense that can never be experienced in the presence of physical objects.

Instead of attempting to meet this objection directly, I propose to go on with a positive interpretation of imageless thought, in the hope that it may avoid the difficulty, and ultimately find a legitimate ground for the distinction between sensory and non-sensory.

To reach a positive interpretation that shall have any real significance, it is essential to turn away from the isolated fact thus far considered, and seek other facts which may be brought into relation to it. A hint as to the most profitable direction in which to seek for related facts is afforded by the following consideration. Thought deals largely with data derived from past experience. New ideas may certainly be generated in the process of thinking, but in very large measure the content of thought is provided by memory; and it is usually this memory content which appears in the imageless form. It may then be profitable to bring our rather extensive knowledge of memory into relation with the phenomenon of imageless thought; and it is in that direction that I propose to search.

On examining the way in which recalled facts present themselves, we are at once struck by something that broadens the outlook considerably. It is not only in thinking, properly so called, that facts come to mind without images, but in the most commonplace acts of memory. I recall, without visual, verbal or other observable images, what I have in my pockets, where I left my umbrella, whether my neighbor is at home today. This imageless recall is with some individuals quite the rule. The facts are clearly enough present in mind, but if there be any image it is so excessively dim as to elude detection. Such imageless recall is indicated though perhaps not fully demonstrated by some of Galton's results; and Miss Martin has recently[1] given a clear demonstration of the existence of memory content that is "unanschaulich."

In imageless thought, then, the imagelessness has nothing particular to do with the thinking process; and we are permitted to drop, with some relief, the elevated tone that has sometimes seemed appropriate to the topic. Thought is imageless because its data are recalled in an imageless form, and not because it does not thrive in a sensory atmosphere. Much effective thinking occurs in the physical presence of its object. The use of the word "thoughts" to denote non-sensory content is unfortunate, for the words "thought" and "thinking" customarily denote a certain mental function or group of functions, and cannot easily be restricted to any particular sort of content. The best word would be one that suggested recall rather than thinking; but I am not at present prepared to suggest a suitable nomenclature.[2]

[1] *Zeitschrift f. Psychol.*, 1912, **65**, 417–490.

[2] Unless the following suggestion can be seriously entertained. It has long appeared to me that we psychologists were on the wrong track in our selection of technical terms. Our custom is to choose some term of common usage that may convey to the uninitiated a suggestion of the technical meaning newly attached to it. The trouble is that the untechnical usage continues alongside of the technical and tends to cause confusion; until finally psychologists are driven to exclude the untechnical use from their discourse, and thus lose a very convenient tool of expression. It is nothing less than a scandal, for example, that the word "feeling" should have been so refined in usage that the psychologist can no longer speak of a "feeling of hesitation," and scarcely of a "feeling of familiarity," without an apology and the dread of being misunderstood by his colleagues. The older sciences, with their greater need for an extensive technical vocabulary, have gone to work in quite a different way. They either take unfamiliar Greek and Latin words and derivatives, or they set apart

What, then, is it, in general, that is recalled? An old standard answer is that we recall our past experiences. Objection has several times been raised to this answer within the last two decades; but the following line of criticism is perhaps new. In experiments on testimony, or on "incidental memory," the subject is found to be incapable of recalling much that has been before his eyes, and even within the general scope of his attention. If he could call back his original experience, it would seem that he could give the testimony required of him. A specially instructive experiment, for our present purpose, is that of Thorndike,[1] who asked his subjects to call up an image of a certain scene, as of the front of a familiar building, and then, after they had estimated the vividness of their images, asked them specific questions, as to the number of pillars in the facade and similar details. He found a marked inability to answer the specific questions, even on the part of individuals with very lifelike images; and, in fact, there was little or no correspondence between vividness of image and correctness of report on details. I have frequently repeated this experiment with the same results. I have never found an individual able to read off the number of pillars from his image. Only those could tell the number who had at some time counted them; and other subjects protested that it was not fair to expect them to find the number of pillars in the image, when they had never counted them in the original. All this seemed highly suggestive. It suggested that only that was recalled which had been noted in the original experience; and that even vivid

some proper name to serve the special purpose. Thus they have their watts and volts and ohms and amperes, terms regarding the meaning of which no one need ever be in doubt. Such terms are much better than "thoughts," or than "Bewusstseinslagen," with its doubtful translation of "conscious attitudes." I would propose, accordingly, to follow the lead of physics and chemistry; and since Bewusstseinslagen were first reported and defined in the work of Marbe and his associates, I would suggest calling them "marbs," the term to be defined for all time by reference to the original description by Marbe. Similarly, since the "thoughts" were gradually brought to light by the school of which Külpe was the guiding spirit, I would suggest calling them "kulps," defining this term similarly by reference to the original works. These terms are certainly beautifully compact and euphonious, and those who can bring themselves to use them will find them very convenient.

[1] *J. of Philos.*, 1907, 4, 324.

images, described as being fully equal to the actual experience, were in fact something quite different.

I was thus prompted to undertake an examination of images and other content of recall, in order to see how far they could be described as revivals of past experiences, and how far they consisted of facts noted in the past. I set myself to recall events from my past life, and in other cases to recall persons, buildings, towns, and such specific facts as the exact colors of postage stamps, the quality of a friend's voice, the shapes, tastes, odors, etc., of a great variety of objects. What I got was sometimes to be called an image and sometimes not; but in all cases, with a few doubtful exceptions, it consisted of facts previously noted. When I say "facts," I do not mean verbal statements of facts, but a direct consciousness of some thing, quality, relation, action—of something which I had observed in the original experience. I did not get back experiences as concrete totals, but only facts which I had discriminated out of those totals. In the original experiences, those facts had had a concrete setting or background; but this setting was not recalled. The facts were recalled in isolation.

Often, indeed, a rudimentary setting was present, consisting of either a personal reference, or a spatial reference, or both. By "personal reference" is meant that the fact was recalled as my own experience, or that the relation of the fact to me, or my attitude to it, was recalled along with the fact. By "spatial reference" is meant that an object was recalled as being to the right or left, or in a certain town, or in a certain direction from my position at the time of recall. Spatial reference was more frequently present than personal. Neither was universally present; and, aside from them, no setting was recalled. It frequently happened that several facts derived from the same experience, or from different experiences, were recalled almost or quite simultaneously, so that the recall was richer than would be suggested by the expression, "isolated fact." Nevertheless all of these facts had been previously noted, and they did not bring their concrete setting back with them.

As an example of my results, I will cite the recall of a colleague speaking in faculty meeting. What I got was a certain quality of voice and precise manner of enunciating, rather different from the conversational tone of this individual. There were no words nor particular vowel or consonantal sounds present in recall, but simply the quality of the voice and enunciation. I got also the fact that the speaker was speaking as chairman of a committee, and something of the rather critical attitude of the faculty towards him, these facts being recalled in the "imageless" way. Besides, I got a spatial reference, in that the speaker was located in a certain position with respect to my position in the meeting; and a vague personal reference amounting to an attitude of support or well-wishing. Beyond this, nothing. No visual background of faces or furniture, no auditory background of words spoken, no somesthetic background of myself sitting.

Among the facts thus recalled in relative isolation and without concrete setting were the following:

Of persons: shape of head or of nose, breadth of face, color of eye, curliness of hair, blotchiness of complexion, facial expression, tone of voice, trick of gesture, "smoothness" of manner, social position, ability, industry, relation to myself, as being friendly or unfriendly, a superior or dependent, agreeable, a bore, etc., or as having been seen recently or long ago.

Of buildings: location, size, color, material, architectural style.

Of towns: location, general topography, old or new style, abundance of shade, holiday atmosphere, quietness, association with certain events.

These facts run the gamut from simple to complex, and from sensory to abstrusely relational. They are so varied as to indicate that any observed fact can be recalled in isolation. Among the striking instances of isolation were recall of the color of an object without its shape, of its shape without its color, of its gloss or shading without either color or shape.

The following interpretation seems scarcely more than a restatement of these results. An actual situation presents an almost unlimited variety of facts or features, of which an observer notes a few, the rest remaining undiscriminated in the background and giving the concrete setting of the features noted. Later, he may "remember" the situation, but this is not to reinstate it in its original multiplicity and continuity. He recalls the features which he observed, or some of them, but not the great mass of material which remained in the background. Lacking this setting or background, he is not in a position to make any fresh observations in recall, and thus arises the weakness of incidental memory.

If generalized to cover all cases in all individuals, this statement does indeed go beyond the evidence at hand. But if the possibility of an occasional recall of the concrete setting is left open, and the assertion simply made that an observed fact is often recalled without its original setting, this conclusion, though modest, is sufficient to furnish a positive interpretation of imageless recall.

Were it true that a recalled fact always brought with it its original setting, then, indeed, all recall would involve sensory imagery. But if a fact is recalled in isolation, it depends on the nature of the fact whether the recall would be called imaginal or imageless. If the fact lay as it were on the sensory surface of things, such as color or tone, its recall would usually be spoken of as an image. If the fact lay below the sensory surface, as the fact that a speaker was exaggerating, or speaking as chairman of a committee, an isolated recall of this fact would be unhesitatingly pronounced imageless, unless, to be sure, it were accompanied by a verbal or symbolic image derived perhaps from another source than the original setting of the fact. The definitely imaginal and the definitely imageless are the extremes of a series, between which lie many intermediate facts difficult to place in either class. The expression of a face, the composition of a painting, the style of a building or piece of music, recalled in an isolated way, are difficult to classify.

If you set yourself to discover what are the objects of your attention in a sensory experience, you will usually find that the actual sensations are less prominent than the things signified by them. You are more conscious of the horse galloping past than of the actual noises that you hear. When, therefore, you later recall hearing a horse gallop past, it is not surprising that the thing signified should be recalled more distinctly than the noises; and you are left in doubt whether to class the recall as an image or not. This is a type of numerous cases. An observed feature of a situation often lies partly "on the sensory surface" and partly below, and the observer does not take separate note of the sign and of the thing signified, but perceives them together as a single fact. His recall of the fact may then partake both of the sign and of the thing signified, though the sensory flavor is usually weakened in recall. The distinction between imaginal and imageless, between sensory and non-sensory, is not perfectly sharp, and appears, from our present point of view, to be of minor significance, the main principle being the isolated recall of observed facts.

I ought really to rest content with the conservative statements that precede, and leave imageless recall as an incident to the occasional, or frequent, recall in isolation of previously noted facts. But in the interests of a more clean-cut theory, I am tempted to more radical and general statements. I propose to strike out boldly and formulate a theory, hoping that, whether acceptable or not, it may prove a stimulus to thought and perhaps to experiment.

The first step towards this theory is to generalize the conclusion derived from observations already cited, and to offer the hypothesis that all recall is of facts previously noted, freed from the concrete setting in which they occurred when noted. This generalization I hold to be correct for my own case, and, though the testimony of many individuals regarding their imagery is on its face in flat contradiction with mine, the objective test of incidental memory seems to show that there is something radically wrong with their testimony. My generalization has the advantage of squaring with the facts of recall as objectively tested, and the only difficulty is to explain away the introspective reports of images "fully equivalent to actual experience," and of "living over the past as if it were present."

Without pretending to do full justice to this testimony, I must for the present content myself with a few remarks. Undoubtedly a person may become deeply absorbed in a remembered experience, because of its great interest for him. Now his present interest is probably the same as that which dominated him in the original experience and led him to observe and react to certain features. If, his interest reviving, he gets back these features and reactions, he has the essentials of the original experience from his own point of view, and satisfactorily lives it over again, even without the concrete background, the absence of which, in his absorption, he would not notice, any more than he noted its presence in the original experience.

As to the vivid image, said to be "in all respects equivalent to the actual scene," we undoubtedly have, in such a case, a revival of personal attitude and emotional value, which alone are enough to create a strong atmosphere of reality. We must also recognize that what an artist might call the general effect of a scene is as much a fact to be observed as any other. The features which can be analyzed out of a situation are not exclusively details, but include broad effects and syntheses and anything that can be the object of attention. If now you recall the emotional value and general effect of a scene, along with some of the colors and other previously noted details, you perhaps have enough to make you testify, rashly, that your image is in all respects equivalent to the actual scene. A test of incidental memory would soon convince you that the "equivalence" is an illusion.

It is also true that a person may observe a scene in such detail as to recall a great number of its features; and he might express the wealth of his recollection by asserting that he revived the entire experience; but, so long as what he recalls is what he previously observed, he offers no exception to the rule that has been formulated.

We have not yet by any means exhausted the relevant information to be derived from studies of memory. Evidently we should be much helped in any study of recall by having at hand a report of the process by which what is now recalled was originally learned. We should be helped in our present inquiry by knowing whether "impressing a thing on the memory" consists in simply standing before the thing and letting it "soak in," or whether it consists in reacting to the thing by observing its characteristic features. It may be said at once that studies of memorizing give little sign of a purely receptive attitude on the part of the learner, and much evidence of a reactive and analytical attitude. Meumann emphasized the importance of the "will to learn." A subject might attentively examine a list of nonsense syllables, and yet make little progress in memorizing it unless his will to learn were excited. Now the "will" can scarcely be conceived as acting without means or tools; and its tools consist of various specific reactions to the matter set for memorizing, the reactions varying with the material and with the test of memory that is to be met. Some of these reactions may properly be called motor; here would be classed the rhythm, accents, pauses and vocal inflections that are read into the list by the learner. But in large measure the reactions are of the perceptual sort, and consist in observing positions, relations, patterns, meanings, in the matter to be learned. The recent studies of Müller throw all these factors into clear relief. Memorizing is very largely a process of observation, of noting those features of the material that will serve to hold it together in the desired way. Some of these features, such as patterns and relations and the nearer-lying meanings, are, as it were, found in the material itself; while other features, the more far-fetched meanings and associative aids, are imported from without; but this distinction is only one of degree.

The reactions made in learning, it should once more be said, are specific, and adapted not only to the material learned but also to the kind of memory test that is anticipated. If the subject expects to recite a list of words or syllables throughout, he observes positions, sequences, patterns and relations that will serve to bind the whole list together. If he expects simply to respond to each of the odd-numbered words in the list by giving the following word, as in the method of paired associates, he takes each pair as a unit, and observes characteristics of the pair that bind it together, but neglects the sequence of pairs. If he expects to be called upon to recognize the individual words of the list, he fixes his attention on them singly, observing in each, as far as possible, some character that may serve to impress it. There is no one uniform process of learning, and the will to learn cannot be conceived as a general force or agency. What we find in memorizing is a host of specific reactions, largely of the perceptual sort.

I may be permitted to cite the results of a little experiment designed to test this matter. I read a list of twenty pairs of unrelated words to a group of 16 adult subjects, instructing them beforehand to learn the pairs so as to be able to respond with the second of each pair when the first should be given as stimulus. But, after reading the list three times, I told them that they should, if possible, give also the first word of the following pair on getting the second word of the preceding pair as stimulus. I then read the first word of the list, waited 5 seconds for the subject to recall and write the second word; then read this second word, and waited the same time for them to recall and write the the third word, namely, the first word of the second pair; and so on through the list. The results were most definite: the second members of the pairs were correctly recalled in 74% of all the cases, but the first members were recalled in only 7% of the cases. The subjects reported that this great difference was apparently due to the fact that they had examined each pair with the object of finding some character or meaning in it; whereas they had neglected the sequence of pairs as being of no moment.

This result is instructive in several ways. It indicates, first, that the will to learn operates not by favoring a general receptive or memorizing attitude, but by leading to specific reactions of the observational type. It serves, next, to fortify the results of other experiments on "incidental memory." Here the objection cannot be raised that the incidental matter that is not recalled was never attended to; for the first words of the pairs were attended to as well as the second. The experiment also shows the unsatisfactory character of Ward's conception of the process of learning. He has said that associations are formed by the movement of attention from one to the other of the terms associated. But here attention moved from the first to the second member of a pair, and thence to the first member of the next pair; yet the first movement seems to have established a strong association, and the second, comparatively speaking, none. Evidently something much more specific than a mere movement of attention has been in play. The members of a pair are associated by the sequence, connection or meaning that is found in the pair. Finally, this experiment serves to strengthen doubts that have often been raised, especially by the work on incidental memory, regarding the adequacy of contiguity in experience as an associating force. Here the contiguity between the members of a pair was scarcely greater, in matter of time, than that between successive pairs; yet the association within pairs was strong, and that between successive pairs almost negligible. Since the associations within pairs gave 10 times as good a score as those between pairs, we may perhaps say that mere contiguity does not contribute more than one tenth of the whole associating force, the remaining nine tenths being contributed by the noting of suitable features in the material. Even the small fraction thus left to contiguity does not necessarily belong to it; for it is not improbable that the sequence and relation of successive pairs were sometimes observed. In fact, of the few correct recalls of first members, practically all occurred at the beginning or end of the list of twenty pairs; and it is quite likely that, in these favored positions, attention was occasionally directed to such incidental matters as the sequence of pairs or their positions in the list. Except at the ends of the list, the score for first members was only 1/85 as good as that for second members of the pairs; and this fraction, rather than 1/10, probably represents the proportion of the total associative force that should be assigned to mere contiguity; though even this is a doubtful concession.

It may be considered superfluous bravery in me to challenge the doctrine of association by contiguity, in addition to all the other enemies already on my hands; but, in reality, I have this doctrine on my hands at any rate. For if contiguity in a momentary experience is a strong and sufficient associative force, then any item that is later recalled will in turn recall its contiguous items and redintegrate the whole experience or a large part of it, and my hypothesis that what is recalled is observed facts without their setting would become untenable.

Now association by contiguity has played a worthy and important part in the development of psychology, and its attempt to absorb into itself all other laws of association has, in my opinion, been a success. Things become associated only when they are contiguous in experience. That is to say that contiguity is a necessary condition of association. But is it a sufficient condition? There is little in the experimental work on memory to indicate that it is sufficient, and much to indicate that it is not usually depended on to accomplish results. The things to be connected must be together, in order to arouse the reaction connecting them; but, unless they arouse some such reaction, they do not become connected, except it be very weakly. The reaction may be described in a general way as a reaction to the two things together; it is perhaps sometimes a purely motor reaction, but most often, I believe, is rather to be called a perceptual reaction, consisting in the observation of some relation between the two things, or some character of the whole composed of the two taken together. In any case, the reaction is specific; and it is this specific reaction, rather than any general factor like contiguity, or the movement of attention, or the will to learn, that does the work of association. To judge from the memory experiments, then, what is recalled is what has been noted—not past experiences in their totality, but definite reactions which occurred in those experiences.

This conclusion is perhaps even more clearly indicated by experiments in the learning of nonsense drawings than in the more usual work with linguistic materials. An instructive experiment is that of Judd and Cowling,[1] who exposed a rather simple drawing for successive periods of 10 seconds,

[1] "Studies in Perceptual Development," Psychol. Rev. Monograph 34, 1907, 349–369.

requiring the subject to reproduce it as well as possible after each exposure. The results, both objective and introspective, showed that the subject usually got first the general character and shape of the figure, and, continuing his analysis, noted one fact after another, until a sufficient number of facts was known to make a satisfactory reproduction possible. There was no evidence of an inner reproduction of the entire sensory experience, from which the subject might read off such information as he required. In a somewhat similar experiment, T. V. Moore[1] called for the learning of a series of simple drawings. He supposed at the outset that a group of figures would be memorized by visual imagery, but experience taught him that there was another factor that was a powerful aid to memory. This was "a more or less complete analysis of the figures, an analysis which it is utterly unneccessary for the subject to put into words." It consisted in noting the parts and composition of the figures and their resemblances to familiar objects. He then undertook to compare the efficiency of memorizing by visualization with analysis excluded, and by analysis without visualization; and found a uniform superiority of the analytic method over the visualizing. But he also found that it was impossible to exclude analysis altogether. "Associations crop up spontaneously," he writes, "and one simply cannot exclude all analysis of the figure. . . . It is much easier to memorize by analysis to the exclusion of imagery than *vice versa*." He believed, however, that learning by visualization, i. e., by forming an image which should be a "more or less perfect replica" of the visual sensation, was a real process. Under the circumstances, it was evidently impossible for him to prove this; for if analysis occurred spontaneously—and one has only to look at a drawing to realize how inevitable it is to note either details or broader characteristics—and if also analysis was a more powerful memorizing agency than visualization, it remains possible that all the learning was accomplished by analysis. The reality of the strictly visualizing or photographic process of learning is, I believe, still open to doubt. It is certainly

[1] "The Process of Abstraction," *Univ. of California Publications in Psychology*, 1910, 1, 139–153.

impossible to avoid perceptual reactions, and to assume the purely receptive attitude of a photographic plate.

Miss Fernald's data on the memorization of pictures[1] show that even good visualizers depend largely, at least, on specific observations of the features which were later remembered; and her results on the recitation of letter-squares in changed orders[2] showed that even the best visualizers among her subjects were unable to do what it had been supposed was the prerogative of a visualizer to do, namely, "see the whole set of letters at once and simply read them off" in the changed order. She does not doubt the existence of persons able to accomplish this feat, but believes that they must be rare. This matter of visualization evidently requires further study, but the possibility is still open that even the best visualizer does not carry away a photograph of the scene, or replica of his visual sensation, but an image which amounts to a synthesis of specific observations, including observations of broad effects and observations of parts and their relations.

But it is time that I brought my theory out of hiding and placed it squarely before you. I call it, for lack of a better name, the mental reaction theory, or perhaps the perceptual reaction theory. Its basic idea is that a percept is an inner reaction to sensation. I call it a mental reaction to distinguish it from the motor reaction which several psychologists have put forward as being important in attention, perception, association and the like; for it appears to me that these suggestions, while on the right track in insisting that *reaction* is dynamically important, have mistaken the locus of the reaction, and so are unable to account for the conscious content that appears in these mental activities. This mental reaction is not, however, of the nature of an associated sensation, appearing as an image, as if the visual sensation of an orange, to give the percept orange, must reproduce the sensations of handling or tasting the orange. Nor, on the other hand, is the perceptual reaction an emphasis or pattern or meaning residing in the given sensations. It is something new, not present in the sensations, but, theoretically, as

[1] PSYCHOL. REV. MONOGRAPH 58, 1912, 81ff.
[2] *Ibid.*, p. 71.

distinct from them as the motor reaction is. It adds new content which cannot be analyzed into elementary sensations; so that the sensory elements, which are often held to supply, along with the feelings, all the substance of consciousness, in reality furnish but a fraction of it, and probably a small fraction. Each perceptual reaction is specific, and contributes specific content. In recall, it is these perceptual reactions that are revived, and not sensation; and therefore the content of recall is never, in the strictest sense, sensory. Nevertheless, as was said before, some percepts lie, as it were, nearer to sensation than others, so that the distinction between an image and an imageless recall, while not perfectly sharp, is still legitimate.

It is possible that this theory may appear not so radical after all, and not worth the expenditure of so much breath; for all will perhaps admit that a percept is, in some sense, a reaction. It is therefore my duty to show that the theory is worse than it seems, and this I shall attempt to do in the case of patterns or Gestaltqualitäten. It has long been known that the same pattern (for example, a melody) can sometimes be found in different sensory complexes, and it is also true that different patterns can be found in the same sensory complex, as in the case of the dot figure. A rather difficult problem is thus raised, for one would think that the compound would be determined by the elements. But the real crux of the difficulty is to get some conception of a pattern or of a compound, to show what is meant by the togetherness or grouping of the elements. There are three theories that attempt to solve this puzzle, that of synthesis, that of systasis or mere togetherness, and that of synergy, which is none other than the mental reaction theory. The synthesis theory brings in the subject or ego to put the elements together; the systasis theory rejects this deus ex machina, and says that the elements merely are together, or get together and so constitute the compound or pattern; the synergy theory holds that the elements act together, as stimuli, to arouse a further reaction which is the pattern. The synthetic theory occupies a weak position, since, unless the systatic theory succeeds in showing what is meant by the elements being together, there is no advantage in saying that something puts them so.

Now it is difficult to understand what can be meant by the elements being together or getting together so as to produce the group and pattern. If the group included the whole momentary content of consciousness, we could say that being together meant simply being simultaneously present, and speak of the pattern as a character of the whole conscious moment. But the group does not include the whole of consciousness, but—as in the case of three dots among a larger number, seen for an instant as a triangle—may occupy but a small part of the conscious field. The pattern is not the pattern of consciousness, but a pattern within consciousness. Nor will it help matters much to substitute for consciousness the field of attention; for the extent of a group may be either greater or smaller than that of this field; and, besides, a familiar pattern, such as a melody or arrangement of lines or dots, may come to consciousness quite outside the field of attention. Apperception, then, in the Wundtian sense, does not explain groups and patterns nor give them any intelligible meaning. But if we lay aside apperception and try to describe groups and patterns in terms of their constituent elements, we are in no better case. What is it that changes when the pattern changes, the elements remaining constant in quality, intensity and spatial position? This question is as serious for the synthetic theory as for the systatic. The synergy theory cuts the Gordian knot by admitting at once that there is no change in the elements. In fact, there is no real grouping or pattern of the elements; they neither get together nor are put together by some higher agency; but some of them simply act together, as a complex of stimuli, to arouse a perceptual reaction which constitutes the grouping and pattern. The pattern is numerically distinct from the elements, as a motor reaction is distinct from the complex of stimuli that arouses it. What pattern shall be aroused at any moment depends on the readiness of different perceptual reactions to be aroused, and thus on such factors as frequency and recency of past exercise, fatigue and present interest and control. In short, the synergy theory proposes to extend to patterns, and to all percepts, the same explanation that is accepted for such admittedly mental reactions as the sequence of one idea after another. No one doubts that one idea may represent a stimulus for the arousal of another idea, nor denies that the aroused idea is numerically distinct from the stimulus idea and adds new content to it. It is the same with sensation and perception, except that the reaction is usually very prompt and the perceptual content intimately fused with the sensational. The fusion is so complete that the pattern seems to lie right in or among the dots, as the galloping horse of an earlier illustration seemed to be actually heard in the series of noises.

But now, finally, I suspect that the party, which allowed me to proceed some time ago without coming to terms with their demand for a definition of sensation, will no longer be restrained. They will insist on taking the floor and addressing you as follows: "The speaker is certainly right in calling a percept a reaction; that is too obvious a fact to need discussion. But we ask, A reaction to what? And our answer is, To the physical stimulus. This 'sensation' that the speaker has interpolated between the physical stimulus and the percept is pure gratuitous assumption. There is no warrant for it in introspection, for he himself admits that the sensation and the percept content are intimately fused. We regret that he has fallen into this obsolescent way of speaking, and would suggest that, in reviewing his remarks, you use the blue pencil of the censor wherever the word 'sensation' occurs."

This objection is almost too serious to be dealt with in brief. I should freely admit that sensation and percept cannot be distinguished by direct introspection. Yet there are introspective facts that make the distinction appear legitimate. When we hear the galloping horse, we are not only aware of the horse, but we are able to state that we hear him. It is not quite correct to say that we get only the meaning, for we know also the sense by which we get the meaning. So, again, when we have changing percepts of the same stimulus, as in the case of the dot figure, the change of pattern does not amount to a complete change of the figure, but there is a constant substratum underlying the changes; and it seems appropriate to speak of this as sensation. In recall, even the best images lack something when compared with actual sensory experience. They lack body and incisiveness; and it appears probable that this lack is nothing more nor less than a lack of sensation, or, in other words, that the real sensory process is not resuscitated in the image.

But the concept of sensation might never have arisen in a purely introspective psychology. At bottom it is a physiological or psychophysical concept. Sensation is that conscious content which is in closest relation to the physical stimulus. It is the primary response to the stimulus, and may be followed by secondary responses. Neurology gives good ground for such a distinction, in tracing the sensory nerves to certain limited areas of the cortex, and finding the rest of the cortex to be only indirectly connected with the sense organs. Destruction of the cortical receiving station for any sense abolishes all conscious use of that sense, while destruction of neighboring areas, without making a person blind, for example, abolishes his power of reading, or his power of recognizing seen objects, or his power of orienting himself in visual space. Such perceptions are apparently secondary reactions, while the primary reaction, corresponding to the activity of the receiving station, is precisely that which distinguishes a person who is word-blind and object blind, from one who is totally blind. Here is a person who sees without perceiving, and here is one who does not see at all. The difference I would like to call sensation. Sensation, accordingly, would be the consciousness attending the activity of the sensory receiving stations of the brain, while percept-content would be the consciousness attending the activity of neighboring areas. Besides these secondary reactions, there are undoubtedly tertiary and further reactions, less and less directly connected with the incoming sensory impulses. They need not have a sharply limited localization in the cortex, yet they must be neurologically distinct, and it may well be that every distinct cerebral reaction is attended by its peculiar conscious content. I know of no reason in neurology or psychology for supposing that the elements of conscious content are contributed solely by the sensory receiving centers.

According to this theory, the sensation aroused by a physical stimulus must precede the secondary or perceptual reaction; but the interval need not be supposed to exceed a hundredth of a second, and could not be introspectively detected. The fusion of the primary and secondary reactions in consciousness is a fact which I cannot attempt to explain, since fusion is one of the fundamental peculiarities of consciousness as contrasted with its cerebral correlates. But I may perhaps make the whole conception a little more tangible by reverting to the similitude of photography.

A certain photographer found himself without sensitive plates, though with his camera, in the presence of a scene which he much desired to preserve. He therefore focused on the ground glass at the back of his instrument, and, stretching transparent paper over the glass, traced some of the outlines of the optical image. He thus created patterns, which lay really in his drawing and not in the optical image, but which were blended with the image as long as the image remained. He preserved his tracing, and found it to differ from a photograph in containing only the facts to which he had definitely reacted.

In this parable, the optical image is sensation, which is gone forever when the physical stimulus ceases. The tracing is perception, which may be preserved, though subject to decay. But the fusion of the two, depending in the case of the camera on the presence of the photographer's eye, is in the case of sensation and perception more deep-seated and inexplicable. Finally, the photographer was more restricted than is the process of perception, since he could only trace outlines and shadings and perhaps colors, and could not commit to his drawing the more remote relations and meanings which can be perceived, and, being later recalled, furnish the content of "imageless thought."

A NEW MEASURE OF VISUAL DISCRIMINATION[1]

BY KNIGHT DUNLAP

The tests on which this report is based were carried out in the Nela Research Laboratory in August and September of 1914, as an adjunct to other experimental work which will be reported later. The instrument used was constructed at The Johns Hopkins University several years ago, and as a result of work with it since that time has been modified into the present form, which seems good in principle although the mechanical operation may still be improved.

The instrument which, for convenience, may be called a *duoscope*, consists essentially of a polished crystal of Iceland spar mounted in a telescopic brass tube, which has an eye-aperture at one end, and a rotatable ring at the other. The ring is rotated by a worm-screw with a knurled head, and is provided with a vernier scale, so that the angle of rotation may be read to one fifth of a degree. The ring is arranged to hold a disc fitting within it, so that various forms of objects may be viewed at a fixed distance from the eye.

The first objects tried were as near linear as possible: a diamond-scratch on a clear glass disc; a fine glass filament crossing a circular aperture in a metal disc. These were tried against various backgrounds, and were not satisfactory because of the difficulty of securing a line of sufficient uniformity and without sheen. A narrow slit was equally unsatisfactory. Finally a rectangular aperture of appreciable width was found workable when used against a bright background.

The Iceland spar crystal gives a double image of the line (or rectangle) used as an object, and the relation of these two images may be altered by rotating the ring which carries the line with it. If the line stands exactly in the refracting

[1] From the Nela Research Laboratory, National Lamp Works of the General Electric Company.

plane of the crystal, the two images are superposed over their greater length, their displacement being longitudinal only. If the line be rotated 90° from this position, the two images are displaced laterally to the maximal distance possible from the prism (1.089 mm. with the crystal used in the present work), and if this distance be greater than the width of the line (or rectangle) the two images are separated.

In this way, with a proper linear object, it would be easily possible to measure (in terms of the visual angle) the displacement of the images giving just perceptible doubleness; *i. e.*, the minimum visibile. The advantage of the device lies in the exactness of measurement and ease of manipulation, with the possibility of accommodation for relatively short or 'reading distance.' It is possible to obtain a suitable linear object, but before I had found one I discovered that observation on a rectangle of appreciable width (*i. e.*, a relatively wide slit) is much easier, and have therefore adopted such an object for the present. The fineness of measurement possible with this instrument is indicated by the fact that the reading unit in the vernier scale (one fifth of a degree) corresponds, in the middle of the scale used in the present work, to 1.6″ of visual angle, or .0029 mm. lateral displacement of the image.

The discrimination of doubleness in lines is of course a matter primarily of difference-sensibility for brightness. If (in the case of two bright lines, or two images of a single objective bright line), there is a perceptible dark stripe down the middle of the combined lines, they are seen as two; if there is no dark stripe, as one. The three factors involved in 'visual acuity' as tested by the linear method[1] are therefore: (1) The physical distribution of the light-flux on the retina, determined by the 'resolving power' of the eye; (2) The distribution of energy or activity in the physiological image of the retina, determined by the distribution, in the physical image, and by irradiation, etc.; (3) The difference-sensibility for brightness differences. Anything which changes any of these factors, as a change in the lens system, or in the irradi-

[1] In the case of determinations by means of two points instead of two lines, the situation is different, and the histological texture of the retina may be a factor.

ation, will therefore change the 'acuity,' although the difference-sensibility remains the same. Thus the practical usefulness of the acuity-determination depends on thorough (and perhaps unattainable) control of the conditions of observation. These matters are so obvious that no further discussion is needed here.

The method of testing 'acuity' by the double images of a bright rectangle is now apparent. If the two images (physical) overlap sufficiently, there is a brighter line in the middle of the combined images; if they are sufficiently separated there is a darker line in the middle. It is a relatively simple matter to determine the points at which the dark line and the bright line are *just* perceptible.

The crystal used has a maximal image-separation of 1.089 mm., and the slit was fixed at a distance of approximately 36 cm. from the eye. The slit was 3 cm. long and 0.77 mm. wide.

The most difficult part of the adjustment of the instrument is the determining of the position in which there is no lateral, but only longitudinal, displacement of the images. This determination was made by long series of observations of the bright line obtained by moving in each direction alternately, determining the middle point from these. The zero point thus obtained is sufficiently exact for practical purposes; besides, no more exact method is available.

The background against which the slit was viewed was a plane disc of plaster surfaced with magnesia, at a distance of 97 cms. from the rectangle.[1] This was used first in a darkened room and illuminated with a beam of light from a nitrogen tungsten lamp. The lamp being enclosed, the only illumination of the room was the light diffused from the disc. From the direct radiation of this light the observer was protected by a black screen, through which the instrument protruded. The observer was therefore not in complete darkness, but the illumination in his direction was low (1/27 to 2 c.p.). Ten minutes or more was allowed for adaptation, so that the subject was really in a fair state of darkness adaptation.

[1] Tests were made at other distances, but as was expected, the distance proved not to be a factor in the results.

Subsequently the instrument and plaster disc were moved into a room well lighted with daylight, so that measurements were obtained with daylight adaptation.

In the darkened room five illuminations of the plaster background were used, giving brightnesses of 3, 10, 36, 82 and 168 candles per square meter.

The observers were: laboratory helper Mr. Eric Martienssen, a high school graduate; Dr. P. W. Cobb; Dr. H. M. Johnson; and myself. The readings on me were taken by Martienssen; the readings on the other observers were taken by me. Usually twenty-five determinations were made in one sitting. Thus, in the work with five intensities, five determinations were made on each intensity at a sitting, with only one sitting a day. Each subject had preliminary practice in observing. These intensities were taken in a different order on different days. No practice effect is noticeable in the measurements of any of the subjects.

The observer started with the bright line plainly visible and rotated the slit until the dark line was just visible: then he rotated the slit in the other direction until the bright line was just visible: or *vice versa*. The observers found it easier to make the changes rather quickly. Long looking caused the difference in brightness to disappear.

It is unfortunate that the instrument was made with the crystal fixed, and the object rotating. It was so made because this form allows easier construction, and has advantages in adjustment of the object for the zero point which was desirable while the instrument was in the provisional stage. The next instrument will be made with fixed object-holder, so that the axis of the slit will not change during observations.

The rotation of the axis in these experiments was small, however, and does not vitiate the results. In the table below, where the axis is not specified, it was 90°, that is the slit was vertical when in the medial position, *i. e.*, in the position in which the images were not displaced laterally.

The readings given in the table are the displacements from the zero position in visual angle computed from the averages of the designated number of readings on the scale of the instrument. Theoretically, the visual angle should have been computed for each instrument-reading, and then the averages of the computed values taken; practically, the computation for the average of the instrument-readings is sufficiently accurate. The mean variations are not given, because I have so far not been able to discover what the true mean variations are. It is evident that the variations cannot be referred to the averages, because these vary with the width of the slit employed, regardless of the uniformity of observations; nor to the average of the range from dark line to bright line, because there may be variations in the readings which do not affect this.

TABLE I

THE INFLUENCE OF BRIGHTNESS AND OF ADAPTATION

	1. *Martienssen.* *Left Eye* *A.* Dark Adaptation. Av. of 25			2. *Johnson.* *Right Eye* *A.* Dark Adaptation. Av. of 25		
Brightness	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
3	7′ 10″	6′ 44″	26″	6′ 59″	6′ 46″	13″
10	7′ 10″	6′ 49″	21″	7′ 1″	6′ 49″	12″
36	7′ 10″	6′ 50″	20″	6′ 59″	6′ 49″	10″
82	7′ 10″	6′ 53″	17″	7′ 00″	6′ 49″	11″
168	7′ 0″	6′ 54″	13″	7′ 2″	6′ 50″	11″
	B. Daylight Adap. Av. of 20			*B.* Daylight Adap. Av. of 20		
	7′ 2″	6′ 51″	11″	6′ 59″	6′ 47″	12′
	3. *Cobb.* *Right Eye* *A.* Dark Adap. Av. of 10			4. *Dunlap.* *Left Eye* *A.* Dark Adap. Av. of 25		
3	6′ 50″	6′ 27″	23″	7′ 10″	6′ 39″	31″
10	6′ 53″	6′ 31″	22″	7′ 11″	6′ 48″	23″
36	6′ 49″	6′ 26″	23″	7′ 11″	6′ 49″	22″
82	6′ 51″	6′ 34″	17″	7′ 11″	6′ 50″	21″
168	6′ 52″	6′ 33″	19″	7′ 12″	6′ 50″	22″
				B. Daylight Adap. Av. of 75		
				7′ 4″	6′ 49″	15″

In the *A* parts of Table I are given the general results of the tests with different brightness of slit under darkness adaptation, and in the *B* parts, the corresponding results with daylight adaptation. Two points are clear. First, that in general the daylight adaptation gives greater acuity; and

Table II

Influence of Angle of Axis of Rectangle

1. *Dunlap. Right Eye*

A. Daylight Adap. With lens correction. Av.of 20

Axis	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
80	7′ 2″	6′ 46″	16″			
90	7′ 2″	6′ 45″	17″			
100	7′ 2″	6′ 42″	20″			
125	7′ 2″	6′ 42″	20″			
170	7′ 6″	6′ 50″	16″			

B. Daylight Adap. Without lens Av. of 10 — *C.* Dark Adap. Without lens Av. of 40

Axis	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
80	7′ 4″	6′ 43″	21″	7′ 4″	6′ 40″	24″
125	7′ 10″	6′ 32″	38″	7′ 17″	6′ 22″	55″
170	7′ 9″	6′ 55″	14″	7′ 13″	6′ 46″	27″

2. *Martienssen. Daylight Adap. Av. of 20*

A. Right Eye — *B.* Left Eye

Axis	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
90	7′ 9″	6′ 53″	16″	7′ 2″	6′ 51″	11″
135	7′ 00″	6′ 48″	12″			
180	Unable to	see lines.		7′ 8″	6′ 54″	14″
67.5	Unable to	see lines.		7′ 43″	7′ 22″	21″

3. *Johnson. Daylight Adap. Av. of 10*

A. Right Eye — *B.* Left Eye

Axis	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
90	(6′ 59″)	(6′ 47″)	(12″)	6′ 59″	6′ 45″	14″
135	6′ 55″	6′ 46″	9″	6′ 59″	6′ 47″	12″
180	7′ 00″	6′ 48″	12″	6′ 58″	6′ 45″	13″
67.5	7′ 00″	6′ 48″	12″	6′ 59″	6′ 46″	14″

second, that there is no uniform influence of brightness within the limits of the conditions obtaining.

The results with the lowest brightness differ appreciably from the results with the higher brightness, but this is due, in part at least, to the difficulty in judging with this illumination when near the line-threshold. This is a condition which must be distinguished carefully from the raising of the threshold as such, and was clearly a factor in my own case. Leaving out the dimmest light, the influence of the brightness is negligible for Cobb, Johnson, and Dunlap.

The change to daylight adaptation is, however, influential except in the case of Johnson, whose acuity seems to be exceptional. Tests with the Cobb acuity-object also have shown Johnson to have unusual acuity with darkness adaptation. It is quite probable that the slight effect of the increasing illumination in the darkened room was due to the lessening of adaptation.

The most striking result is the uniform lowness of the threshold. The average range from dark line to bright line lies for the most part near 20″, and is lower in some cases. In the ordinary test object, using object lines, the measurement from fusion to dark line is from 30″ to 60″. The corresponding measurement in the present case is less than 20″; how much less cannot be determined, as it cannot be assumed that either the points of geometrical contiguity, or of physical uniformity of the images, lie half way between the points at which the dark line and the bright line respectively appear. Schuster states that the intensity at the edge of the *geometrical* image of a uniformly bright surface must be "half the intensity observed at some distance inside the edge," because when two surfaces are placed with edges in contact a uniformly illuminated surface is obtained.[1] Assuming this to be true of the physical image, it is not necessarily true of the psychological image, as irradiation and contrast (physiological) effects occur at the margins of the images. The fact that both light line and dark line thresholds tend to shift with darkness adaptation indicates influences of this sort, and we should accordingly expect the light line threshold in general to be nearer the point of geometric image contiguity than is the dark line threshold: an expectation that is justified by the facts.

Table II gives the results of tests to find the effects of lenticular aberrations. My right eye is corrected with a lens of 0.50 C., axis 80°, prism ¾° B.D. Tests were accordingly made on the eye with and without the correcting lens, in the astigmatic axis and at 45° on either side. The results (II, 1, *A*, *B*, and *C*) show that even a low degree of astigmatism is detectible by this means and also that my eye is slightly under-corrected by the lens.

[1] Schuster, 'Theory of Optics,' page 151.

Tests were carried out on both of Martienssen's eyes (II, 2). He preferred to use his left eye in any sort of monocular observation. On being questioned about this he said he had always used that eye because it seemed more natural. The tests seem to indicate a slight degree of astigmatism in the right eye with less in the left. Martienssen's eyes had never been refracted.[1] The degree of astigmatism is not great, for with uncorrected eyes requiring from one to two diopters of cylindrical correction, the instrument cannot be used at all.

Whether the instruments and methods for acuity test above described will be practically useful remains to be seen. In the matter of precision and convenience the apparatus seems superior to devices hitherto in use. The fact that the results differ from those obtained by means of the several other devices is immaterial. Measurements of this kind give comparable results only when the same instruments and methods are used. Since the duoscope method seems sensitive to adaptation changes, it may be possible to use it as an adaptometer; since it seems not sensitive to brightness changes over a considerable range, it may be a useful instrument for practical testing of eyes. It seems especially suitable for detecting slight degrees of astigmatism, and for detecting the accuracy with which lens corrections for astigmatism are made, in experimental work where accurate control of the observer's eye is required.

[1] Since the above was written, Dr. Cobb has refracted Martienssen's eyes, with the following results (sine midriatic):

O.D. —. 37 cyl. axis 175°

O.S. —. 25 sph. —. 37 cyl. axis 155°.

It is probable that the fact that the duoscope readings for the Right Eye are not harmonious (*e. g.*, the inefficiency at 67.5), is due to over accomodation.

AN ELECTRO-MECHANICAL CHRONOSCOPE

BY JOHN W. TODD

University of North Dakota

Provided that certain changes are made in its electro-magnets and that it is skilfully handled the most reliable chronoscope known is the Hipp chronoscope. But for want of skilful handling it is not unusual to see a dust-coated copy of the instrument stored away in a museum for apparatus that looks nice but is rarely used. Nevertheless the Hipp deserves more respect. After having invested in the costly piece with its control-hammer additional, the experimenter should fit it for service by rewinding its electro-magnets with coarser wire, by insuring a steady current with a good gravity battery of 12 cells, by discarding the control-hammer and employing some type of gravity chronometer for control tests.[1] It is the fineness with which the instrument is designed to record times that makes it unsatisfactory in inexperienced hands but that insures reliability when correctly operated.

Even after the corrections indicated are made the instrument must be constantly watched and tested, as a fluctuation of the current or a slight change in the adjustment of the delicate parts of the apparatus may produce a chronoscope variation that will entirely obscure the variations in reaction time. The instrument responds to all irregularities and is never popular when operated in a hit and miss manner. Many attempts have been made to devise a simpler chronoscope than the Hipp. The special aim has been to construct one that will eliminate the constant care of control and minimize

[1] The manner of making these changes and the reasons for them may be found in the *National Academy of Sciences*, 7, 397 ff., 1893 (Cattell and Dolley). After correcting the instrument as indicated these writers found average variable errors for seven series of ten single tests of the chronoscope as follows: 0.96, 0.8, 0.42, 0.4, 0.64, 0.64, and 0.56σ. Using the same instrument corrected by Cattell the present writer in making several thousand reaction tests found an average variation of the chronoscope in control tests of about 1σ ("Reaction to Multiple Stimuli," *Archives of Psychology*, No. 25, 8, 1912).

the possibility of getting out of adjustment. The simplest chronoscope is one so designed as to harness the force of gravity for marking off units of space that may be given time values. This arrangement eliminates delicate clockwork propelling devices and reduces the number of adjustable parts three fourths. Even after the chronoscope is reduced to its simplest terms three difficult problems remain, First, to devise a reliable chronoscope release; Second, to construct a sufficiently accurate reaction recorder, and, Third, the greatest problem of all, *to put down a chronometric scale that is trustworthy.*

The chronoscope described in this article consists of a

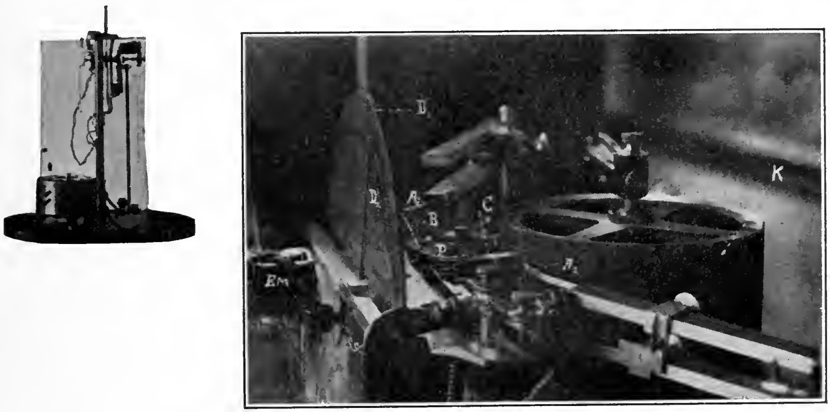


Fig. 1.

disc compounded of two adjustable parts (D_1 and D_2, Fig. 1), which are two circular planes of 1/16 in. brass, 11.5 in. in diameter, with a concentric semicircle of each having a radius of 4.75 in. cut away, leaving in each case a marginal area 1 in. in width. One of the discs is constant with respect to a pendulum attached to their common axis while the other is adjustable to allow the various apertures of a tachistoscopic attachment described later in this article. These discs are held rigidly together by a set-screw (*S. sc.*, Fig. 2) and rock with the vibrating pendulum. In making a chronometric reading with the electrical arrangement the initial position of the pendulum, *i. e.*, horizontal, is maintained by the force of an electro-magnet (EM, Fig. 2) upon the armature (A_2) carried near the base of the pendulum whose socket ($P.S.$) is shown. To minimize friction the shaft carrying the discs and the pendulum has cone bearings (Fig. 2). The pendulum weights are two cylinders of lead set in brass. The gross relations of the various parts of the apparatus are shown in the upper left corner of Fig. 1, a lateral photograph of the apparatus.

Fig. 2 is a diagram showing the details of the chronoscope

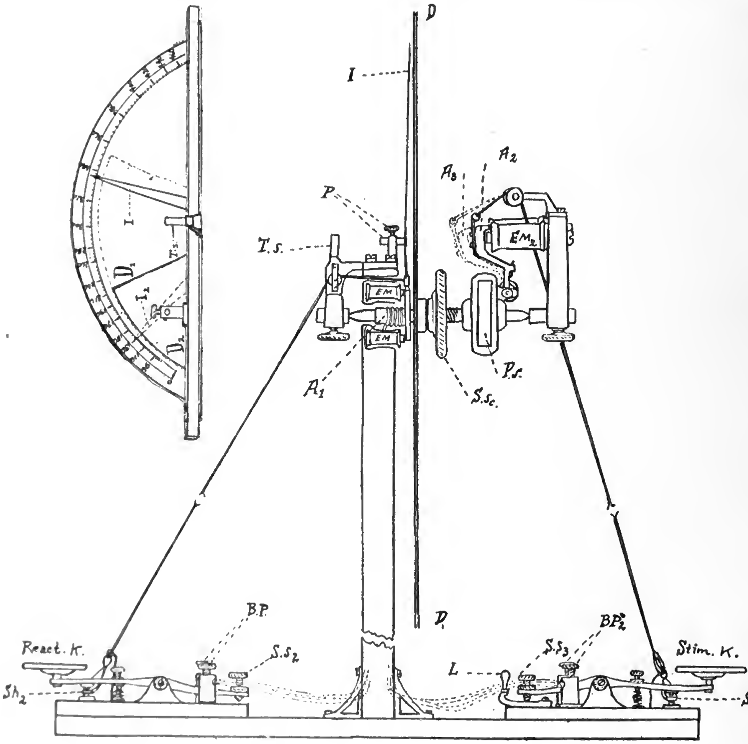


FIG. 2.

arranged for both *mechanical and electrical* stimulus key and reaction key. The figure shows the apparatus set for *mechanical* operation. Sh_1 is a copper shoe held firmly between the contact points of a stimulus key by means of the insertion of the inclined surface of the lever, L, and the resiliency of the key shaft, supporting by means of a cord over a pulley the armature, A_2, which holds the pendulum in the initial horizontal position. Sh_2 is likewise a copper shoe between the contact points of the reaction key held firmly by the reagent's finger upon the button, and holding the armature (A_1) of the reaction index to its initial position against the arm of the post, *P*. The armature, A_1, to which the reaction index (*I*) is attached moves freely upon the spindle bearing the chronoscope disc, and by means of a coiled spring flies against the disc when release is made and is carried with it allowing the index pointer to escape the arm of the post, *P*, by which it is held during the reaction interval.[1] When the pendulum is in the initial position the index point rests upon the zero point of the chronometric scale. The index may be thrown back to the zero point from any position on the scale by pulling upon the cord to which Sh_2 is fastened.

When the stimulus lever (*L*) is suddenly pulled the stimulus hammer ($S.S_3$) by virtue of the resilient shaft which carries it and the rebound of the strong coiled spring near its fulcrum strikes the solid metallic base a blow emitting a sound which serves as a stimulus, and whose intensity is variable by means of set screws. With the drop of the stimulus hammer upon the metallic base and the emission of the sound the shoe actuated by the strong spring of the release armature flies from between the points, while the armature flies back to position A_3. This releases the pendulum and carries the chronometric scale in the negative direction counting against the reagent until the index flies upon the disc marking the close of the stimulus-reaction period.

The difficulty in attempting to devise a mechanical chronoscope is to give it versatility. It is not hard to provide a release that at the same time serves as a sound stimulus, and that offers no resources for the presentation of touch and light stimuli. With the mechanical device described above, however, it is possible to give all three stimuli. This is shown by Fig. 3, a diagram of the three-stimulus key. The method of giving the sound stimulus is described above.

[1] This type of armature although employed in a somewhat different manner was first used by Bergström in a pendulum chronoscope figured and described in the Psychological Review, VII., 1900, 438 ff.

When it is desired to present a light stimulus with the mechanical chronoscope a light wooden arm long enough to extend beyond the base of the chronoscope is attached to the shaft of the reaction key. This wooden arm carries a small black square (A) operating as a shutter to a 1 cm. aperture (Ap) in a black screen large enough to conceal the movements

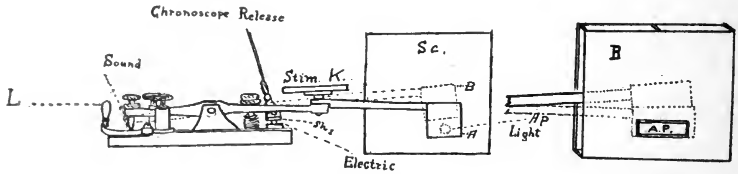


Fig. 3.

of the experimenter. When it is desired to use tactual stimuli the stimulus key, by means of the binding posts, is inserted into the primary circuit of an induction coil preferably with condenser. The reaction key is introduced in the secondary circuit in such manner that the cathode is in the button of the key and the anode in a shallow vessel of salt water. If the vibrating armature of the current interrupter of the induction coil is tied back securely against its adjustable contact and the reacting finger rests upon the cathode and the fingers of the other hand rest in the vessel, a shock is felt in the reacting finger when the primary circuit is broken at Sh_1. As the key is figured above it is set for all three stimuli which would be administered simultaneously with the sudden pull toward the experimenter of lever L.

By removing the arrangement for light stimulus the pair of stimuli, sound and shock, may be given together; or by getting out of series with the induction coil, the paired stimuli, sound and light, may be given. Likewise it is possible to eliminate any two stimuli and administer a third singly. When light is presented singly a small rubber plate is attached to the posterior side of the stimulus lever (L) to eliminate the sound. In all these cases it is seen that the chronoscope release is mechanical, by means of Sh_1. Fig. 2 by means of dotted lines from the binding posts suggests the wiring arrangement for electrical operation of the chronoscope. When the current is used the two cords are serviceable to set the pendulum armature and to throw the reaction index back to zero.

It is seen that the mechanical device can be operated only when the experimenter and reagent are at close range. When, however, it is desired to give stimuli from a distance or to have the reaction from another room the electrical arrangement mentioned above must be employed. The reaction movement called for by the apparatus is in all cases of the break-circuit variety. The chronoscope may be put to the same tasks that are attempted by any type of electrical chronoscope, at the same time affording a mechanical arrangement that would seem to meet the objections of those opposed to the electric chronoscope.

Method of Laying the Chronometric Scale

A chronoscope is a device for visualizing intervals of time by freely initiating or terminating the regular movement of either a point along a graduated scale or a graduated scale past a point, each division of the scale being the space traversed in a given unit of time. Many a chronoscope has been devised with perfect balance and bearings but failed because its scale was too largely a matter of speculation. *The chronometric scale is the real chronoscope*—propelling the scale or moving an index uniformly along it are comparatively easy accessories.

The possibility of laying a definite chronometric scale was one of the factors that prompted the present device. Fig. 1 shows the method of deriving the scale and of placing it upon the chronoscope disc. One of a pair of synchronized differential tuning forks of 256 v.d. frequency is loaded with a small aluminum feather (A_1) by means of a stiff wax. It is then sounded with its companion, the number of beats per second counted and its vibration-rate calculated. Then by means of wax another aluminum feather (A_2) is attached to the second fork, and small increments are made to the wax until the beats disappear. The forks are again synchronical and their vibration-rate is that formerly calculated. They are mounted as shown in Fig. 1 in such manner that feather A_2 touches the smoked drum of a kymograph and feather A_1 rests upon the smoked edge of the chronoscope disc. Into this arrangement a third member is brought, a Morse key rearranged for break-circuit contact, and bearing two aluminum feathers at the end of its shaft beneath the button (P). One of these feathers is adjusted to lie within the same radius of the disc as the tuning fork feather, A_1, and the other is squared with the point of the feather upon the kymograph.

After the disc is brought to its initial position and the circuit to the electro-magnet (EM) is closed, it is seen that a pressure upon the button (P), which short-circuits the current to EM, will release the disc. At the same time a mark will be recorded upon the kymograph and another upon the smoked edge of the chronoscope disc. The entire procedure is as follows: Start the forks, and with the hand suddenly revolve the drum of the kymograph, having disconnected it from the clock-work mechanism, and with the other hand press upon the button (P) at least twice in quick succession.

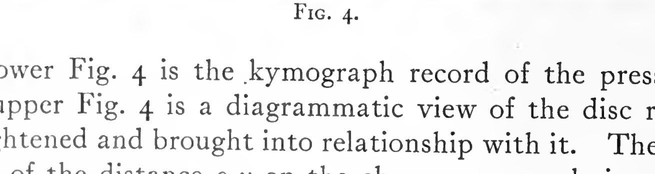


Fig. 4.

Lower Fig. 4 is the kymograph record of the pressures, and upper Fig. 4 is a diagrammatic view of the disc record straightened and brought into relationship with it. The time value of the distance o-y on the chronoscope scale is readily counted off from the distance on the kymograph scale, A-B. In laying the chronoscope scale it is necessary only to read toward o from the point y to establish the first distinguishable chronometric value from o. There will always be the space o-x whose time value *in toto* is known but whose individual waves are too close together to be distinguished.

After setting the chronographic records with shellac the scale of time values is engraved upon the chronoscope disc at grade points located by producing a radius of the disc through the crest of each tuning-fork wave. The time value of each wave-length was 4^{σ} which save in the case of waves near x is graded in four equal parts, or to the 1^{σ}. This chronographic method of laying the scale is superior to the method of employing sparks produced by an induction coil with a tuning-fork interrupter because the sparks deviate considerably in making the aerial gap, and fail to indicate the true location of the scale.

In order to test the reliability of the chronoscope a control

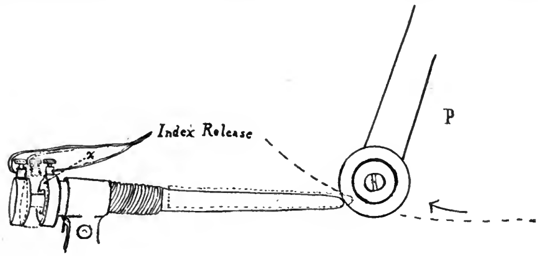


Fig. 5.

apparatus (Fig. 5) is used consisting of a shaft with a polished metal point so poised that its axis if produced is a secant of the pendular arc. This shaft is carried by a comparatively facile spring in a rigid base. When the pendulum strikes the shaft it is pushed back breaking the circuit at x, thus releasing the chronoscope index and recording upon the scale the interval between the release of the pendulum and its striking the shaft. The values given below are the averages of 12 groups of control tests of 10 trials each, or 120 tests. The mechanical release was used in the tests.

Average Interval	Average Variation
519.4^{σ}	0.84^{σ}
520.2	1.08
520.4	0.64
520.1	0.18
520.4	0.64
521.4	0.80
519.5	1.00
519.0	1.00
520.3	0.42
519.8	0.48
519.9	0.72
522.0^{σ} Gross Av. 520.2^{σ}	0.42^{σ} Gross Av. 0.68^{σ}

The present device by means of its adjustable discs (D_1 and D_2, Fig. 1) affords a tachistoscope that is fairly serviceable.[1] The point-exposure time may be read off from the chronometric scale, and affords a maximum point exposure of 420^σ. The stimuli are held by a clip behind the discs and are seen through the sector of the compound disc. *B*, in Fig. 3, is a diagram showing a tachistoscopic attachment for the stimulus key (*Stim. K*) making it possible to expose words, colors, etc., in a rectangular aperture for reaction experiments in discrimination, cognition, choice and association. The screen is large enough to conceal the operations of the experimenter from the reagent. By using the rubber plate under the sound hammer the exposures are made almost noiselessly.

Summary of the Special Features of the Chronoscope

1. It allows either mechanical or electrical release of the time scale, involving in each case the same parts, and making it possible to work where a steady electric current is not available.

2. The reaction mechanism may be operated either mechanically or electrically.

3. The device makes it possible to lay a chronometric scale whose units can be exactly placed to within a short distance of the zero point, and whose total value is exactly known.

4. By means of an attachment time exposures may be made that are measured off on the chronometric scale, and the variety of compound-reaction stimuli can be given.

[1] In his "Mental and Physical Tests—Simpler Processes," 1914, pp. 263 ff., Whipple figures and describes a disc tachistoscope of his own construction and one now commonly in use. In his instrument the point-exposure times are calculated from the relative positions of weights upon the two counterbalancing arms that rotate the disc. In the present arrangement the times are shown on the chronometric scale.

XVIII. PRACTICE IN ASSOCIATING COLOR-NAMES WITH COLORS[1]

BY WARNER BROWN

It has long been known that the process of recognizing and naming a color takes more time than the process of recognizing and naming an isolated printed word, such as the word, for example, which designates the same color.[2] The following experiments represent an attempt to gain a clearer understanding of this phenomenon.

The first hypothesis which presented itself was that words can be recognized and named more rapidly because we have had more practice in doing this than in naming colors.[3] Accordingly a practice experiment was contrived on the basis of Cattell's familiar color-naming test.[4] Experi-

[1] From the Psychological Laboratory of the University of California.

[2] James, W., 'Principles of Psychology,' 1890, Vol. I., p. 559. Cattell, J. McK. 'Ueber die Zeit der Erkennung und Benennung von Schriftzeichen Bildern und Farben, *Philos. Stud.*, Vol. 2, 1885, pp. 635–650.

[3] This explanation of the phenomenon is clearly stated by Cattell in the account of its discovery which he gives under the title, 'The Time it Takes to See and Name Objects' (*Mind*, Vol. 11, 1886, p. 65). He says, "The time was found to be about the same (over ½ sec.) for colors as for pictures, and about twice as long as for words or letters. Other experiments I have made show that we can recognize a single color or picture in a slightly shorter time than a word or letter, but take longer to name it. This is because in the case of words and letters the association has taken place so often that the process has become automatic, whereas in the case of colors and pictures we must by a voluntary effort choose the name."

The same interpretation is given by J. O. Quantz in his monograph, 'Problems in the Psychology of Reading,' Psychol. Rev. Monog., No. 5, 1897, p. 10. "The association is of the same sort in words as in forms or colors, for the connection between the written symbols and the spoken sound of any given word is just as arbitrary as is that between a particular geometrical form and its name as uttered. But the association between forms or colors and their names, being less necessary than between written and printed (spoken?) words has been less frequently formed and the former has remained a voluntary process while the latter has become automatic through repetition."

[4] Cattell and Farrand, 'Physical and Mental Measurements of Students of Columbia University,' Psychol. Rev., Vol. 3, 1896, p. 642. Wissler, C., 'The Correlation of Mental and Phsyical Tests,' Psychol. Rev. Monog., No. 16, 1901, p. 8. Hollingworth, H. L., 'The Influence of Caffein on Efficiency,' *Arch. of Psychol.*, No. 22, 1912, p. 16.

ence had shown that the Columbia test was weak in the following points: Not all the color names were equally familiar; they were not all equally hard to say (for example *red*, *yellow; blue*, *violet);* there were strong brightness contrasts between some of the colors; the chance arrangement of the colors resulted in some bad sequences; the one-centimeter squares were too small, making it difficult to 'keep the place' with the eye. The test was accordingly modified in these respects: The color squares were increased in size to one inch; the sequence was so arranged that no color square was placed next to another of the same color and a color was not permitted to occur less than twice nor more than three times in any row; only four different colors were used in any one set and these were all either 'light' (white, pink, brown, gray) or 'dark' (black, red, blue, green);[1] the colors all had one-syllable names; all of these names were highly familiar.[2]

It was expected on the hypothesis of Cattell and Quantz that sufficient practice would make it possible to read off the color names as rapidly from the colors themselves as from a printed list. If the difference in speed depends upon previous practice it should, by further practice, be possible to reduce the time consumed in reading colors but not possible to reduce to any considerable extent the time required to read a list of words. In order to test the truth of this hypothesis it was necessary to show not only that the speed of color naming can be increased by practice but also that the speed of reading words can *not* be increased so much by an equal amount of practice. For the practice in reading words, lists were typewritten with the one hundred color-names arranged in the same order as the colors themselves. The words in

[1] The colors used were the papers supplied by the Milton Bradley Company, of Springfield, Mass., under the following designations: Black, White, Neutral Gray No. 2, Engine Colored Paper No. 2B (brown) and No. 1B (pink), Red, Green, and Blue.

[2] The modified form of the Columbia test recommended by Woodworth and Wells, 'Association Tests,' Psychol. Rev. Monog., No. 57, 1911, p. 49, meets most of the difficulties mentioned above, but unfortunately it was not published until after the present experiments were partly completed. It may be noted that in the Woodworth and Wells test the colors appear on a white background whereas in the form here used the squares were larger and juxtaposed without background.

each line were separated by a comma and one space; the lines were separated by a triple space. For each set of colors there were, of course, four distinct lists of words, corresponding to the four arrangements of colors which were encountered on beginning in the four different corners of the color-set. For every practice trial in associating the colors with their names there was a practice in reading the words from the corresponding list.

A record-blank, including complete directions, was given to each worker at each practice sitting; it read as follows:

Directions for the Experiment on Naming Colors

There are two boards of colors. Each board contains 25 squares of each of 4 colors, and there is a different color in each corner of the board. There are 4 typewritten lists of colors for each of the boards, and each list begins with the name of the color in one corner of the board, and gives the names of the colors in the order of their appearance on the board.

The purpose of the experiment is to measure the maximum rate of speaking when reading the lists of words or naming the colors, and to see how much this rate can be increased by practice.

First day's work. Take the time with a stop-watch for reading aloud, as fast as you possibly can, the words on the typewritten list beginning with Black. Enter the time, in seconds and fifths of a second, opposite "List black" in the table below. Then take the time for calling out the names of the colors, as fast as you possibly can, from the board, beginning with Black in the upper left-hand corner and reading by rows from left to right. Enter the time opposite 'Board black' in the table. Then enter the time for each of the remaining items in the table, being careful to take them in the order indicated by the numbers.

1. List black...........	3. List white...........
2. Board black.........	4. Board white.........
5. List blue............	7. List brown..........
6. Board blue..........	8. Board brown.........
9. List green...........	11. List pink............
10. Board green.........	12. Board pink..........
13. List red............	15. List gray............
14. Board red...........	16. Board gray..........

Second and succeeding days. Use only one board of colors and the lists which belong with it. Do not look at the other board or its lists, nor allow any one to read them in your hearing. Record the times for the right (left)[1] hand half of the table in the order given, and do nothing with the other half of the table.

Twelfth day. Exactly the same as the first day.

Forty-five students took part in the experiment. All

[1] If the subject was to practice the 'dark' colors the word *right* was expunged; if he was to practice the 'light' colors the word *left* was expunged.

practiced for twelve practice-periods. Most of them worked twice a week, but a few practiced daily. Twenty-five of the forty-five were women. Twenty, of whom ten were women, practiced on the 'dark' colors. Twenty-five, of whom fifteen were women, practiced on the 'light' colors. As no essential difference appears between the light and dark colors the data have been combined for the entire forty-five workers.[1]

The condensed data are presented in Table I. The table

Table I

Gain by Practice in Naming Colors and Reading Words

Average of 45 Subjects

The time is the average of the 4 trials made each day.

Day.	Colors: Av. Time Required to Name Them, Secs.	Colors: Av. Gain in Speed Over 1st Day, Secs.	Colors: Av. Gain in Speed Over 1st Day, Per Cent.	Words: Av. Time Required to Read Them, Secs.	Words: Av. Gain in Speed Over 1st Day, Secs.	Words: Av. Gain in Speed Over 1st Day, Per Cent.	Ratio: Time for Colors Divided by Time for Words
1	55.8			35.2			1.59
2	50.9	4.9	8.8	33.0	2.2	6.3	1.54
3	46.4	9.4	16.8	31.6	3.6	10.2	1.47
4	45.2	10.6	19.0	30.8	4.4	12.5	1.46
5	43.7	12.1	21.7	30.2	5.0	14.2	1.44
6	42.8	13.0	23.2	30.4	4.8	13.6	1.41
7	42.4	13.4	24.0	29.9	5.3	15.1	1.42
8	41.4	14.4	25.8	29.5	5.7	16.2	1.40
9	41.4	14.4	25.8	29.4	5.8	16.5	1.41
10	41.1	14.7	26.4	29.0	6.2	17.6	1.42
11	40.7	15.1	27.1	29.4	5.8	16.5	1.38
12	41.4	14.4	25.8	29.3	5.9	16.8	1.41

shows the average time required by the 45 subjects for naming the 100 colors and for reading aloud the 100 words. The time is the average for the four trials which were made each day.[2]

[1] On the first day of work, when records were made for all of the subjects with both light and dark sets (*i. e.*, the first practice record with one set and the first check record with the other set) the times were as follows:

Time required to name 100 dark colors 56.0 sec.; 100 'dark' words 36.0 sec.

Time required to name 100 light colors 55.8 sec.; 'light' words 35.2 sec.

This insignificant advantage of the light sets remains unehangcd through the course of practice. Most persons prefer to work with the light colors on esthetic grounds. Some subjects complain of getting the tongue twisted around the words, *blue* and *black* in the dark sets because of the identity of their initial sounds.

[2] These four trials did not differ greatly from one another. As a rule the first trial was better than the others except that on the first day, and to some extent on the

The practice gains are shown both in seconds and in per cent. In both cases the amount of gain is computed on the basis of the speed on the first day of work. The table further shows the ratio between the time required for colors and the time required for words.

In Table I*a* the records are shown for the tests which

Table I*a*

Tests on Unpracticed Sets, for Which Records were Made on the 1st and 12th Days of Practice

Column headings as above.

1	55.9			35.8			1.56
12	47.4	8.5	15.2	32.0	3,8	10.6	1.48

Table I*b*

Separate Statement for Men and for Women for the 1st and 12th Days of the Regular Practice Work

Figures for Women in Italics

Headings as above.

1	58.9			35.6			1.66
1	*53.3*			*35.1*			*1.52*
12	42.0	16.9	28.7	29.9	5.7	16.0	1.40
12	*39.9*	*13.4*	*25.1*	*29.0*	*6.1*	*1.74*	*1.38*

were made on the first and last days with different sets of colors and words.

In Table I*b* the data of the first and last days are arranged to display the fact that women excel men in speed in naming colors, but that men improve more with practice.[1]

From the data of Table I. and from an inspection of the curves of Fig. 1 it can be seen that the hypothesis on which this experiment was based is probably not true. At the end

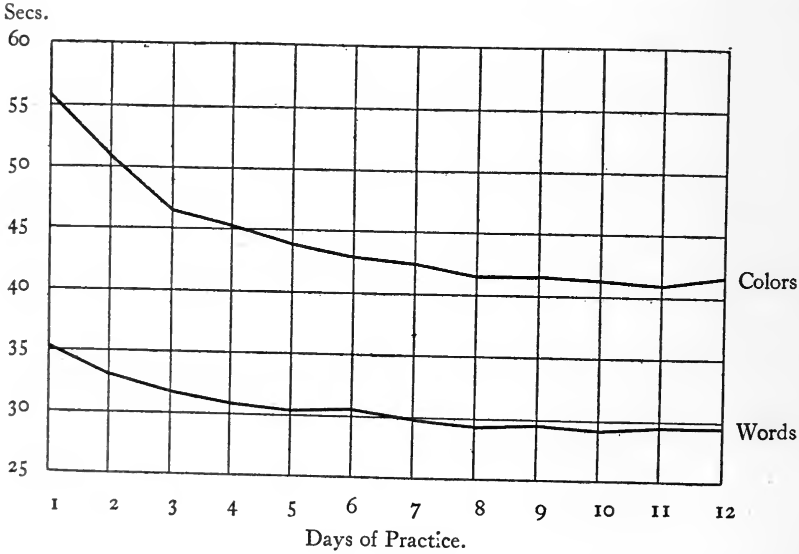
second day, there was improvement from trial to trial. The following figures were obtained by averaging the records for the last ten days of practice:

	1st trial	2d trial	3d trial	4th trial
Time for 100 colors..........	42.3	42.3	43.0	42.8
Time for 100 words..........	29.0	30.4	29.9	30.5

Evidently the practice gains during this period occur in the intervals between sittings, 'overnight,' and not during the course of a sitting.

[1] The superiority of women in naming colors has been observed by Woodworth and Wells, Psychol. Rev. Monog., No. 57, 1911, p. 51, and by Wissler, Psychol. Rev. Monog., No. 16, 1901, p. 17.

of twelve periods of practice it is evident that only a very slight further increase of speed in naming colors can be anticipated, no matter how much more practice is taken; yet the absolute rate in naming colors remains much slower than the rate of reading the same words from the list and is even slower than the word rate was before the beginning of practice. Furthermore the life-long practice which we have had in reading words has not brought that function to a maximum speed; on the contrary it shows an amount of practice-improvement almost proportional to the improvement shown in naming the colors. For every second gained in naming colors at any stage of practice approximately half a second has been gained in reading words. The ratio between speed



in color naming and speed in word reading (the last column of Table I.) shows no indication of approaching unity.[1]

From these data it seems safe to conclude that the dif-

[1] The statements of this paragraph are true not only for the average results given in the table, but for each individual subject who took part in the experiment. No statement of the variability or probable error of the measurements has been made because such a statement could have no direct bearing upon the interpretation of the figures in the present connection. The individual differences in absolute speed were very large, but they do not in any way affect the results.

ference in speed between color-naming and word-reading does not depend upon practice.

Further confirmation of this conclusion is found in the fact that the effects of training in reading words are specific for the particular words read and do not extend to other words. It will be recalled that each person was trained upon either the 'light' or the 'dark' set, but that a test was made, at the first and last sitting, of his speed with the other set (the one he did not practice). The results of these tests are indicated in Table I*a*. The speed on the unpracticed sets at the end of twelve days of practice is better than on the practiced sets on the second day of practice, but not so good as on the third day. In other words, three days of direct practice are better than two days of direct practice plus ten intervening days of indirect practice. This, too, in a case where the conditions regarding eye-movement and general adaptation to work might lead us to anticipate a considerable amount of transference of practice or 'formal' training. In the present connection the significant fact is that the amount of transfered practice is but little greater in the case of reading words than in the case of naming colors. Apparently we must have practice in reading specific words before we can attain great proficiency in reading them. It can not, therefore, be safely asserted that we read color names faster than we name colors simply because of the large amount of practice which we have had in reading words in general.

The Second Experiment

It now seemed clear that the effects of previous practice do not afford a sufficient explanation of the difference in speed between color-naming and word-reading. Accordingly the problem was attacked from another quarter. The introspections of practically all of the students who had taken part in the first experiment agreed upon one point: It is easier to speak a printed word than to name a color because when you want to name a color you have first to think of the name (the word) and then speak it, whereas the printed word can be uttered without your having to think of anything. The observations of our foreign-born students were particularly clear on this point.[1]

On the basis of these introspections the hypothesis was formed that the process of color-naming would be facilitated by suggesting the word at the moment the color was presented. For actual experiment color-sets were prepared which had entire words or parts of words printed on the face of the colors themselves.

Nineteen students finally completed all the stages of this experiment. They were first given fifteen periods, twice a week, of practice in reading the lists and naming the colors from sets upon which nothing was printed, just as in the previous experiment. The color-set was named over three times at each sitting and the list of words was read once.

Table II

Day	Time Required to Name 100 Colors, Secs.	Time Required to Read 100 Words, Secs.	Ratio: Time for Colors Divided by Time for Words
1	53.8	35.5	1.52
2	48.2	32.7	1.47
3	46.2	31.7	1.45
4	45.3	31.1	1.46
5	42.9	29.8	1.44
6	42.2	30.2	1.39
7	41.3	29.4	1.41
8	40.7	28.8	1.41
9	39.3	28.5	1.38
10	39.5	29.0	1.36
11	40.4	28.8	1.40
12	38.8	27.6	1.41
13	38.2	27.7	1.38
14	37.9	27.6	1.37
15	37.1	27.4	1.35

Only the 'light' set was used. The words of the list, instead of being printed in a regular list with the ten words of a line separated by commas, were now typewritten on separate squares of paper, one inch square, which were mounted on a board just as the color-squares were mounted, so that the eye-movements involved were as nearly as possible the same as for the colors. The data for these fifteen preliminary

[1] Three Japanese and one Armenian took part in the experiment, but their records are not included in the tabulations.

practice sittings are given in Table II. The figures agree substantially with those presented in Table I.[1]

After the preliminary practice, which was only intended to bring the students to such a point that their speed for simple colors and lists of words would be nearly uniform from day to day, experiments were begun with sets of colors arranged just like the others except for words or letters typewritten upon them. The following transcript of the directions gives a sufficient outline of the course of this experiment.

Directions

Sixteenth day. Read the list of words beginning with *brown:* then read the simple color-set beginning with *gray*. Then read color-set 2 with *b* on brown; then set 3 with *w* on white; then set 6, *b* on brown, *w* on white, *p* on pink, and *g* on gray.

Seventeenth day. Read the list of words beginning with *gray:* then the simple color-set beginning with *pink*. Then read color-set 7, *gr* on gray; then set 4, *p* on pink; then set 10, *br* on brown, *wh* on white, *gr* on gray, and *p* on pink.

Eighteenth day. Read the list of words beginning with *brown:* then the simple color-set beginning with *gray*. Then read color-set 11, *own* on brown; then set 12, *ink* on pink; then set 15, *own* on brown, *ink* on pink, *ite* on white, and *ay* on gray.

Nineteenth day. Read the list of words beginning with *gray:* then the simple color-set beginning with *pink*. Then read color-set 16, with full words on all colors. Then read the simple color-set again beginning with *brown*. Then read color-set 16, full words, again.

Twentieth day. Read the list of words beginning with *pink:* then the simple color-set beginning with *gray*. Then read color-set 16, with full words on all colors, two times. Then read the simple color-set beginning with *brown*.

Twenty-first day. Read the list of words beginning with *brown:* then the simple color-set beginning with *gray*. Then read color-set 13, with *ite* on white; then set 14, with *ay* on gray; then set 15, *own* on brown, *ite* on white, *ink* on pink, and *ay* on gray.

Twenty-second day. Read the list of words beginning with *gray:* then the simple color-set beginning with *pink*. Then read color-set 8, with *br* on brown; then set 9, with *wh* on white; then set 10, *br* on brown, *gr* on gray, *wh* on white, and *p* on pink.

Twenty-third day. Read the list of words beginning with *brown:* then the simple color-set beginning with *gray*. Then read color-set 4, with *p* on pink; then set 5 with *g* on gray; then set 6, with *b* on brown, *p* on pink, *w* on white, and *g* on gray.

The data for the last eight days of this experiment are presented in Table III. They are combined in the table so that wherever two records of the same kind were obtained on

[1] It may be noted that the rate of improvement is here almost the same as in the earlier experiment in spite of the fact that the colors were practiced only three times and the words only once instead of four times as in the earlier experiment. In view of the fact already mentioned that the first trial of a sitting is usually the best there is reason for believing that nearly the same results could be obtained in this work by one trial per day as by four trials per day.

Table III

Time Required to Name 100 Colors, to Read 100 Words, and to Name 100 Colors with the Help of Printed Cues

Day	Simple Colors	Words	Colors on Which the Following Letters Were Printed as Cues to Help in Naming the Colors	
16	38.1	28.1	40.8	An initial consonant on one color.
			39.4	An initial consonant on each color.
17	36.6	27.6	38.8	Initial pair of consonants on one color.
			36.4	Initial pair of consonants on each color.
18	36.6	27.6	38.0	Vowel and final consonant on one color.
			38.4	Vowel and final consonant on each color.
19	36.1	27.6	28.6	Entire word on each color.
20	35.7	27.2	28.3	Entire word on each color.
21	36.1	27.2	37.3	Vowel and final consonant on one color.
			36.4	Vowel and final consonant on each color.
22	35.0	27.9	36.9	Initial pair of consonants on one color.
			32.3	Initial pair of consonants on each color.
23	35.3	27.9	35.2	Initial consonant on one color.
			34.6	Initial consonant on each color.

the same day only their average appears. When the entire words are printed on the colors it is possible to read the words without attending to the colors, but even in that case the average speed is not so great as when the words are read alone without the colored background, as may be seen in the records of days nineteen and twenty. After having practiced with the full words on the colored backgrounds some of the students found it possible to read the color-names directly upon seeing the initial letters without considering the background. This accounts for the fact that the records for the twenty-second and twenty-third days, with initials, are considerably better than the records for the sixteenth and seventeenth days under the same conditions.

From the results of this part of the experiment it may be concluded that the association process in naming simple objects like colors is radically different from the association process in reading printed words. The presence of a visual symbol of the sound does not greatly, if at all, facilitate the process of association between color and color-name. Phonetic symbols which might suggest the name of the color do not help us in naming it unless they are so clear that they enable us to read the name itself directly without going through the process of naming the color. The one association process does not reinforce the other. The introspections of all the subjects confirm the figures in declaring that the letters printed on the colors do not serve as helpful cues or prompts, but on the contrary actually interfere with the process of association.[1]

Conclusion

The conclusions of these experiments seem to be entirely negative. No facts have been adduced to explain why more time is required to associate speech movements with a color than with the corresponding printed word. But the evidence does throw some light on the problem in so far as it eliminates very definitely two lines of explanation which have been thought possible. First, the phenomenon does not spring from a difference in the amount of practice which the two functions have had in the past. Second the process of reading words is not involved in the process of naming colors as a subsidiary function. The two functions do not overlap, and in all probability they depend upon distinct physiological processes.

[1] A very similar problem has been attacked with the chronoscope by Bourdon. "Sur le temps nécessair pour nommer les nombres," *Rev. Philos.*, Vol. 65, 1908, p. 426. He finds that the time required to perceive and name a number of points of light (not exceeding four) is only slightly greater than the time required to read arabic numerals. Accordingly he infers that the process of perceiving a few points as a number is as simple as perceiving the symbol of the number. Apparent conflicts between this observation and the results in the case of naming colors are now under investigation in this laboratory.

XIX. THE APPARENT RATE OF LIGHT SUCCESSION AS COMPARED WITH SOUND SUCCESSION[1]

BY BERTHA VON DER NIENBURG

It has often been observed that we perceive a duration marked off by lights as shorter than an identical duration marked off by sounds, a result readily explained by the presence of after-images in the case of the light sensations. Preliminary experiments with series of lights and of sounds indicated, however, that not infrequently the light rate seemed *slower* than the sound rate.[2] This study was undertaken to look into the subject more thoroughly, first from a descriptive view point and later from a causal point of view.

The experiments were conducted during the period from September, 1910, to May, 1911, in the psychological laboratory of the University of California. The subjects were taken from the class in general psychology; their number varied for the several parts of the work.

I. In the first group of experiments the light succession and the sound succession were of equal rapidity. The apparatus in the main consisted of a metronome and a minature electric light, a telegraph sounder, and the necessary switches and connections. The current flowing through the metronome, which was placed in a distant room so that its ticking

[1] From the Psychological Laboratory of the University of California.

[2] Experiments were tried upon a class in psychology at the University of California, a sound-series and a light series of equal rate (240 a minute) being given to all the students together, who thereupon reported their independent judgments in writing. In a first experiment the class was left in ignorance as to the relative rates of the two series, the questions being in the form: "Are the two series of equal rate? If not, which is faster?" The judgments were;

That the light-series was faster (L.F.)	24
That the two series were equal (E.)	55
That the light-series was slower (L.S.)	50

A fortnight later the same class was told that the two series were of equal rate, and the students were asked to tell how the series *seemed* in this respect—whether they seemed equal, or, if not, which seemed faster. The judgments were,

L.F. 41 E. 10. L.S. 70.

might not be heard by the subject, could be sent either into the telegraph sounder, thus marking off the intervals by sound, or into the incandescent electric bulb, when the duration was marked off by light. The entire apparatus was enclosed by screens, so that no light could reach the eye save indirectly through a small aperture (¼ inch in diameter) which flashed the light upon a screen.

The subject, who was seated before the screen upon which the flashes appeared, was told that he would be given a series of taps to be followed by a series of flashes, and that he was to compare the rate with which the taps were coming and the rate with which the flashes appeared. He was allowed a trial in the beginning to insure a perfect understanding of the experiment.

Eighteen subjects, ten women and eight men, were experimented upon. Each subject was allowed to form his judgments in whatever manner seemed most natural to him.

Three rates were used, namely 61, 154, and 183 impressions per minute, or each interval was approximatly 1″, .43″, and .32″ in length. The series were arranged according to the following plan. First, 30 taps at .43″ following 30 flashes at the same rate were given, and the judgment was recorded. This was repeated until 10 judgments had been obtained. The order was then reversed, 30 flashes being given first, with judgment. Next came 10 taps followed by 10 flashes, and a reversal; and 20 taps following 20 flashes, and then the order reversed. This procedure was then followed for the other two rates.

The percentages of respective judgments for the eighteen people, combining, at first, all rates, lengths, and orders, are as follows:

Fourteen subjects were used for one hour only, and while 60 judgments from each was the aim, yet because of irregularities in the apparatus and in the subjects themselves the judgments differed in number from 30 to 100. Four other subjects gave four hours and passed from 180 to 230 judgments. I have grouped these subjects into three groups: those passing from 30 to 50 judgments, those passing from 60 to 100 judgments

'Light faster'	'Light equal to sound'	'Light slower'
37.5%	54.9%	7.6%

NUMBER AND PERCENTAGE OF RESPECTIVE JUDGMENTS PER INDIVIDUAL

Subjects	Total Number of Judgments Passed	'Light Faster' Judgments		'Light and Sound Equal' Judgments		'Light Slower' Judgments	
		Number	Per Cent.	Number	Per Cent.	Number	Per Cent.
Men							
A.	200	151	75.5	49	24.5	—	—
B.	30	18	60.0	9	30.0	3	10
E.	100	48	48.0	50	50.0	2	—
L.	230	46	20.0	184	80.0	—	11.7
N.	60	25	41.7	28	46.7	7	—
P.	40	4	10.0	36	90.0	—	10
S.	40	19	47.5	17	42.5	4	—
T.	210	115	54.8	79	37.6	16	7.6
Sub-totals and average per cents	910	426	44.7	452	50.2	32	5.2
Percentage of sum of each class of judgments, of total judgments.			46.8		49.7		3.5

NUMBER AND PERCENTAGE OF RESPECTIVE JUDGMENTS PER INDIVIDUAL

Subjects	Total Number of Judgments Passed	'Light Faster' Judgments		'Light and Sound Equal' Judgments		'Light Slower' Judgments	
		Number	Per Cent.	Number	Per Cent.	Number	Per Cent.
Women							
C.	40	14	35.0	26	65.0	—	—
D.	60	32	53.3	27	45.0	1	1.7
F.	180	2	1.1	178	98.9	—	—
G.	60	25	41.7	27	45.0	8	13.3
H.	40	10	25.0	23	57.5	7	17.5
I.	80	12	15.0	67	83.8	1	1.3
K.	40	14	35.0	15	37.5	11	27.5
M.	60	38	63.3	1	1.7	21	35.0
O.	80	6	7.5	74	92.5	—	—
R.	50	23	46.0	27	54.0	—	—
Sub-totals and average per cents	690	176	32.3	465	58.1	49	9.6
Percentage of each class of judgments, of total judgments.			25.5		67.4		7.1
Grand totals and average per cents for all 18 observers . .	1,600	602	38.5	917	54.1	81	7.4
Percentage of sum of each class of judgments, of total judgments.			37.6		57.3		5.1

and those passing from 180 to 230 judgments and giving equal weight to each individual's results we have the following table:

Judgments	No. of Observers	L. F.	E.	L. S.
30 to 50	7	36.9%	53.8%	9.3%
60 to 100	7	38.64%	52.08%	9.28%
180 to 230	4	36.46%	61.6%	1.94%

It will be noticed that there is very little difference in the results of the first two groups. The difference in the results in the third groups I judge to be due to the personnel of the group. I think that from these figures I may conclude that the number of judgments does not noticeably affect the final results and that it is therefore not distorting the facts to throw these individuals together.

With two exceptions each individual varied to a large extent in his successive estimations. With no person was the sound declared faster for a majority of the judgments, while in the case of seven persons it was never considered faster at all. In the case of twelve of the observers, the greater number (the 'plurality') of their judgments were of equality, and with the remaining six observers, the light rate of succession was deemed faster than the sound rate.

II. To obtain a numerical evaluation of the differences in the apparent ratings of the succession of these series, the apparatus was now arranged so that the rate of the flashes could be altered at will. While the sounder was still operated through the metronome, the electric bulb was put on another circuit. The physical light was now continuous; but by revolving on a kymograph before it a wheel from whose circumference eighteen acute angled notches were cut, the effect of flashes was given to the subject who saw the light through a small aperture in a black screen. A piece of ground glass, placed directly at the back of the wheel, served to make the light more distinct. By means of the kymograph, the rate of succession could be varied at will. In this part of the work the duration of each flash of light was the same as that of the dark interval which followed it. The sound rate remained constant, that, is 154 beats per minute. The light rate was varied, by steps of eight beats per minute, between the limits of the variable judgments, which proved to be between 110 and 166 beats per minute. Twenty clicks followed by twenty flashes were given throughout this group. The light succession first compared was at a rate of 110 a minute, then 126, and so to 142, 158, 118, 134, 150, and 166. Ten judgments for each series were obtained. Five subjects designated alphabetically in the following table, were used, the first four persons having been subjects in the previous work.

The approximate points at which the succession of lights were judged to be of equal rapidity with the 154-beat-per-minute sound-rate were:

Person	
A	134
B	138
C	138
D	126
E	146

In each of these cases, therefore, the light rate was estimated to be (relatively to the sounds) faster than it really was. In the former part of the experiment, where equal rates of succession were judged, the results for such of these subjects as there acted as observers read for the 154-impressions-a-second rate:

	L. F.[1]	E.[1]	L. S.[1]
A	95%	5%	—
B	2.5%	97.5%	—
C	5%	95%	—
D	17.5%	62.5%	20%

Where the two series to be compared were kept equal, the judgments do not show the same uniformity as in those series where the rate of the light succession was varied.

In the entire group of experiments it was evident that there were variations in the judgments of the same individual from day to day, and from individual to individual.

III. *A*. Attention was now given to the various factors that might cause some of the variations in the judgments passed upon the equal rate of succession. The influence of

[1] These abbreviations stand here and elsewhere for judgments 'Light Faster than Sound,' 'Light Equal to Sound,' and 'Light Slower than Sound.'

the rate itself was first observed. Three rates were used, 183, 154, and 61 beats per minute; *i. e.*, each double phase was approximately 0.32″, 0.43″, and 1″ long. From 20 to 120 judgments were given for each rate of six subjects. The percentage of the respective judgments for all are given in the accompanying table:

Number and Per Cent. of Judgments for Varying Rates

Subject	Rates: 61						154						183					
	Judgments of																	
	L. F.		E.		L. S.		L. F.		E.		L. S.		L. F.		E.		L. S.	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
A ..	57	71.3	23	28.8	–	—	38	95	2	5	–	—	56	70	24	30	–	—
E ..	7	35	11	55	2	10	32	53.3	28	46.7	–	—	9	45	11	55	–	—
F...	—	—	20	100	–	—	1	2.5	39	97.5	–	—	1	0.8	119	99.2	–	—
I...	—	—	19	55	1	5	9	22.5	31	77.5	–	—	3	15	17	85	–	—
L...	—	—	40	100	–	—	3	5	57	95	–	—	43	35.8	77	64.2	–	—
T ..	26	37.1	41	58.6	3	4.3	7	17.5	25	62.5	8	20	82	82	13	13	5	5
	90	23.9	154	72.9	6	3.2	90	32.6	182	64.0	8	3.3	194	41.5	261	57.7	5	.8

These results would indicate that the light appears relatively faster as the rate of succession is increased. Individual differences, however, exist.

B. The influence of different lengths of the series was now tested. Three series, of 30 beats, and 20 beats, and 10 beats, respectively, were used. The percentages are made up of from 20 to 120 judgments by eight individuals for each series.

Individual Tabulation of Percentage of Respective Judgments for Series of Varying Lengths.

Subject	Length of Series																	
	10						20						30					
	Judgments of																	
	L. F.		E.		L. S.		L. F.		E.		L. S.		L. F.		E.		L. S.	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
A ..	93	77.5	27	22.5	–	—	46	76.7	14	23.3	—	—	12	60	8	40	–	—
F...	—	—	40	100	–	—	1	1	99	99	—	—	1	2.5	39	97.5	–	—
G ..	5	25	11	50	4	20	11	55	9	45	—	—	13	65	7	35	–	—
I...	3	75	36	90	1	2.5	6	30	14	70	—	—	3	15	17	85	–	—
H ..	7	8.75	73	91.2	–	—	24	24	76	76	—	—	15	37.5	25	62.5	–	—
N ..	6	30	12	60	2	10	13	65	7	35	—	—	12	60	7	35	1	5
O ..	1	2.5	39	97.5	–	—	3	30	7	70	—	—	35	87.5	5	12.5	–	—
T ..	40	44.4	48	53.3	2	2.2	58	58	28	28	14	14	17	85	3	15	–	—
		24.5		71.2		4.3		42.4		55.8		1.8		51.6		47.8		.6

There is here a tendency for the apparent rate of light succession to increase as the series is lengthened. This is not only manifested in the percentages of the eight subjects together, but in those for each individual save one. All subjects agreed that the thirty-beat series was unnecessarily long. Those persons who formed their judgments immediately felt that the ten-beat series allowed sufficient time to make the judgments, while those who formed no estimation until the final beat preferred the twenty-beat series.

C. Since it was considered by many of the observers to be much easier to form judgments when the sound succession was given first, the effect of the order of the light and sound successions was next considered. The same eighteen subjects were experimented upon with the following results:

Per Cent. Distribution of Judgments

Sound-Light			Light-Sound		
L. F.	E.	L. S.	L. F.	E.	L. S.
38	51	11	42	52	6

These combined results indicate a slight increase in the 'light faster' judgments when the order is reversed from sound-light to light-sound. But taking into account the variation in individual cases, we may conclude that while the order may increase the ease with which the judgments can be made, its effect upon the quality of the judgment is slight.

D. All the observers felt that it was easier to form an idea of the sound intervals than of the light intervals, and gave as their reason, that in the latter the flashes disappeared gradually, instead of sharply as the sound did. By raising and lowering the light before the notched wheel described on page 59 the ratio of light to darkness could be changed. The effect of this change upon the rate at which the light succession was judged to be of the same rapidity as the sound succession was studied in experiments upon four persons.

First of all, with the duration of the flash equal to that of the dark phase, and with the sound-series preceding at a rate of 154 beats per minute—there was found for each individual the rate at which the light-series seemed equal to that of the sound series. While no point was considered the 'equality' consistently by any person, yet one rate was judged to be so more often than others. This rate I shall designate as the 'point of apparent equality.' The ratio of the light to the total cycle of light-phase plus dark-phase was then increased to 75 per cent., and ten judgments made; and then decreased to 25 per cent., and ten judgments recorded; and then back to 50 per cent. The order of these shifts from one ratio to another was inconstant, so that sometimes we would go from 50 per cent. to 25 per cent. and then to 75 per cent., or again from 25 per cent. through 50 per cent. to 75 per cent., and so on. For the various persons the results were as follows:

Subject	Apparent Equality Point of Light Rate When Sound Rate Equals 154[1]	Ratio of Light to Light-plus-Dark								
		25 Per Cent.			50 Per Cent.			75 Per Cent.		
		L. F.	E.	L. S.	L. F.	E.	L. S.	L. F.	E.	L. S.
A ..	128	76	18	6(i)	8.0	78	14	20	58	22(d)[2]
B ..	148	24	24	72(d)	16	78	6	—	—	100(d)
C ..	134	4	24	72(d)	2	76	22	60	20	20(i)
D ..	150	76	24	—(i)	16	78	6	22	38	40(d)

A shows a slight tendency to increase his 'light faster' and 'light slower' judgments at the expense of the equality judgments in the 75 per cent. ratio, while in the 25 per cent. ratio there is a marked increase in the 'light faster' judgments. *B* shows a great increase of judgments 'light slower' at the 75 per cent. ratio, and nearly as great a one for the 25 per cent. *C* shows a like tendency for the 25 per cent. ratio, but exhibits just the opposite tendency for the 75 per cent. ratio. *D* shows an increase of 'light slower' judgments for 75 per cent. and of 'light faster' judgments at 25 per cent. In three cases, therefore, and contrary to expectation (since the interval between flashes is now decreased) the 75 per cent. ratio of light to darkness *decreases* the apparent rapidity of the light, and the 25 per cent. ratio *increases* it.

E. After a month's time, the same observers together with a new one, *E*, were used as subjects to see whether the ten-

[1] Here and in the following table, 'd' indicates decrease in the apparent rate, 'i' increase and 'N' no change.

[2] Determined afresh for this portion of the experiment.

dencies displayed above would exist when the gradation method was used. The work was conducted in the manner described above;[1] that is, with the sound rate at 154, the light rate was varied by steps of eight beats per minute from 110 beats to 166 beats. This was done for each of the three ratios of light to dark, ten estimations being recorded for each rate compared. The approximate points where the light rate was judged equal to the sound rate were:

Rate of Lights per Minute Which Seemed Equal to 154 Sounds per Minute

Subjects	Ratio of Light to L. + D. 25 Per Cent.	Ratio of Light to L. + D. 50 Per Cent.	Ratio of Light to L. + D. 75 Per Cent.
A.........	134 (N) (i)[2]	134	132 (I?) (d?)
B.........	130 (I) (d)	138	118 (I) (d)
C.........	136 (I?) (d)	138	126 (I) (i)
D.........	130 (D) (i)	126	137 (D) (d)
E.........	142 (I)	146	136 (I)

A's point where the light rate seems to equal the 154 sound rate is somewhat higher than it was in the earlier group of experiments; he no longer exhibits the same tendency as before, that is, to regard the light rate as faster for the 25 per cent. ratio than for the other ratios. *B*'s equality point is the same as before, but now the light succession appears to come much faster for both the 75 per cent. and 25 per cent. ratios, which is just the opposite of the earlier results. *C*'s equality point now is somewhat lighter. His judgments are consistent for the 75 per cent. ratio with those of a month earlier, but not for the 25 per cent. ratio. *D*'s point of equality has fallen fourteen beats per minute, and instead of an increased apparent light rate for 25 per cent. as at first, it is decreased. To the new subject, the apparent light rate is increased by the greater ratio of light, and to a less degree is increased by the smaller ratio.

In most cases, then, the 75 per cent. ratio has apparently quickened the light rate, and the 25 per cent. ratio has also quickened it. In the previous group of experiments, the apparent rate was increased or decreased for the 25 per cent.

[1] See page 60.

[2] The lower-case letters indicate, 'increase' and 'decrease' respectively, in the earlier groups (see p. (63)).

and the 75 per cent. ratio about indifferently. The conclusion seems warranted that the judgment of the rate is not based greatly upon the interval *between* impressions. For were the apparently greater rapidity of the light-succession due to its blur and after-image filling in the interval between impressions, and so making them appear to come closer together, then the 75 per cent. ratio of light to darkness should greatly increase the rate of the light succession, and the 25 per cent. ratio should decrease its apparent rate in somewhat the same degree. This, however, did not happen in the above experiments.

F. Throughout the previous work, a record had been kept of the natural and spontaneous manner in which the subjects formed their judgments. Four different ways predominated. Some said that they got an idea of the succession of the beats in the first series given, and noticed the likeness or difference in the rate of the second series to it. This mode will be designated by the term "general impression." Others seem to have judged by various synchronous muscular rhythms, of which I shall count as the second group those in whom this rhythm was less voluntary and conventional, occurring, *e. g.*, in the forehead, or in the back of the head. In a third group are placed those who counted; and in a fourth group, those who tapped.

Dividing twelve of the people used in the first part of the work according to their mode of making a judgment, three fall in Group I., three in Group II., four in group III., and two in Group IV. The percentage of respective judgments for the varying groups is as follows, when light rate and sound rate were physically equal (154 per sec.).

Group	L. F.	E.	L. S.
I.	73.6	25.6	.8
II.	20.0	70.8	9.2
III.	21.9	70.0	8.1
IV.	38.75	51.25	10.0

These figures indicate that those who simply get an 'impression' give nearly three fourths of their judgments as 'light faster,' while those judging by muscular rhythm and those who counted give as such less than a quarter of their judgments. In the latter two groups the number of 'equality' judgments is increased to more than one half the entire number, while the 'light slower' judgments are greater than in the first and the fourth group.

G. Ten subjects who had never been experimented upon previously were taken to make a further study of the direct effect upon the judgments themselves of deliberately following these different ways of forming their judgments. They were first given the sound-light series with the eight light-rates and the 154-beat-per-minute sound-rate. One judgment was passed upon each series. Then ten judgments were made in the series in which the sound and light successions were both 154 per minute; the first procedure of one judgment on each series with a varying light rate was again gone through. Nothing was said as to ways or means of judging, so that each subject employed the method most natural to him. The above process was then repeated twice with other definite methods of forming the judgments now imposed. If the original method used had been by 'general impression,' the person was asked to count for one series, and to beat for the next. Where the 'general impression' method had to be imposed, to be certain that there would be no muscular rhythm, the subjects were asked to say and repeat, 'There is a black cat,' aloud as fast as possible. Great care was taken that this was in no way made to fit in with the rhythm of either series. This procedure after it had been tried one or two times by the subject in no way hindered the person from giving attention to the series and did stop any involuntary vocal synchronisms, so that the person would be judging by his impression alone.

When left to do as they would, six of the subjects beat, two counted, and two judged by their impression of succession. Throughout this part of the work it was noticed that while each individual may attempt to judge in the same manner, many variations still remain. For instance, in beating, a person may move his whole arm, his hand entire, or use his finger only slightly. He may synchronize the sound and keep beating that rate throughout the light series, forming his judgments of the latter by the way its flashes fall in with his beating, or he may change his tapping as the flashes change, or he may cease to tap entirely for the light. He may tap a decided rhythm, he may synchronize well for the sound and not for the light, or he may not be able to do so for either. In cases where the tapping changed with the oncoming of the flashes, it was noticed that in some cases this change in the tapping would be reflected in the answer, while in other cases the difference in tapping was no indication of what the judgment would be. Similar differences were noticed in the counting. If a person tapped with marked irregularity, his counting was of the same sort. With all this variety in these two aids to the formation of time judgments it could hardly be expected that harmonious results for different individuals who are beating and then counting could be obtained. The average percentages of judgments by the three modes employed are, however, as follows:

Light Series and Sound Series at Equal Rate (154 per Minute)

General Impression			Counting			Tapping		
L. F.	E.	L. S.	L. F.	E.	L. S.	L. F.	E.	L. S.
52.5%	32%	15.5%	59%	29%	12%	55.5%	28%	16.5%

The later tables that give the judgments in more detail show the great difference in the different individuals. The number of 'light slower' judgments seems marked when we remember that in the first group of persons examined 7.6 was the average per cent. of such judgments given. The apparent equality points for these observers were, as found by the gradation method:

General Impression	Counting	Tapping
141.8	142	146.6

As elsewhere, the judgments by this method do not coincide with those made on the equal rates. In the six cases where the natural method was to tap, the average percentages indicate a falling off of 'light faster' judgments for the imposed methods with an approximate increase of 50 per cent. in the 'light slower' judgments when judging by the General Impression method. Results:

Effect of Departing from Natural Method

Subjects	Natural Method			Imposed Methods					
	Tapping			General Impression			Counting		
	Judgments of								
	L. F.	E.	L. S.	L. F.	E.	L. S.	L. F.	E.	L. S.
1	100%	—	—	80%	10	10	100%	—	—
2	60	35	5	50	40	10	60	40	—
3	20	65	15	—	60	40	20	80	—
4	25	60	15	50	40	10	50	50	—
5	90	10	—	60	40	—	60	40	—
6	80	20	—	80	20	—	70	20	10
Average Per Cent.	62.5	31.67	5.83	53.33	35	11.67	60	38.33	1.67
	Counting			General Impression			Tapping		
7	80%	10	10	30	30%	40	50%	20	30
8	30	30	40	80	20	—	50	30	20
Average Per Cent.	55.0	20.0	25.0	55.0	25.0	20.0	50.0	25.0	25.0%
	General Impression			Tapping			Counting		
9	50%	40	10	60%	40	—	80%	20	—
10	65	—	35	20	—	80	40	—	60
	57.5	20.0	22.5	40.0	20.0	40.0	60.0	10.0	30

Those who naturally counted show individually a considerable change in result under the other methods, though when these results are combined there is but a slight tendency to quicken the light during the general impression and to quicken the sound while tapping.

When counting and tapping are the imposed methods the 'light slower' judgments are markedly increased with one of the subjects.

In only four cases does the natural method tend to give a greater number of equality judgments.

Two individual records in this group deserve special attention. Instead of judging of the succession of beats as did the other subjects, one of these judged of the succession of the intervals, so that in tapping and counting, instead of matching his beat to that of the metronome, he counted and tapped after its beat. His results are as follows:

Judgments for 20 S-L, with both sound and light at the equal rate of 154 per minute,

General Impression	Counting	Tapping
65% L.F. 35% L.S.	40% L.F. 60% L.S.	20% L.F. 80% L.S.

Equality point when sound was at 154, and the light rate varied (gradation method):

General Impression	Counting	Tapping
154	138	154

A contradiction in the results obtained by the two methods thus is evident. While this subject's original method was that of the general impression, he said after he had tried the others, that tapping was by far the surest.

The other person had a noticeably high 'equality point'; in fact he was the only person of the twenty-nine subjects experimented upon who ever gave as an apparently identical rate, a higher rate of light succession than of sound succession. His results read:

	General Impression	Counting	Tapping
Equality point by gradation method (16 judgments each)	160	150	154
	F. E. S.	F. E. S.	F. E. S.
Distribution of judgments per cent. when light-rate = sound-rate (154). (10 judgments each)	— 60 40	20 80 —	20 65 15

From these various modifications of the experiment we may perhaps be justified in drawing the following conclusions.

1. The commonly accepted statement that of equal times marked off by light and sounds the light-limited durations seem shorter than the sound-limited is by no means universally true, when applied to the apparent *rate of succession* of series of light and of sound impressions.

The experience that the light succession is *less rapid* than the sound succession comes not infrequently, and with some observers comes indeed more frequently than does the opposite experience, that the light succession is more rapid.

2. With persons who are practiced the impression that of two equal rates the light rate is the slower does not appear to be influenced directly by the amount of such practice. But when there is no practice at all, as in the class experiments, a greater number of persons take the light series to be slower than take it to be faster.

3. The apparent difference of rate between light succession

and sound succession does not seem to be directly connected with the persistence of the light effect upon the retina, which makes the blank lapse of time between the impressions less in the case of light than in that of sound. For in the first place, this could not explain those not infrequent cases where the light rate seems slower; and, secondly, the effect of artificially varying this blank lapse without changing the rate itself affects in no simple and direct way the apparent rate of the impressions. The change of the blank interval to an interval less than has become familiar seems more frequently to have an effect similar to a change in the opposite direction (*i. e.*, an increase of the blank interval); although different persons respond differently to this experiment, and differently at different times.

4. The *higher the rate* of the two kinds of succession here compared, the more pronounced becomes, with most observers, the illusion that the light series is the more rapid.

5. The *longer* the series of impressions to be compared, the more pronounced is the illusion that the light series is the more rapid.

6. The order in which the two series is given affects the result: the impression of greater rapidity in the light series comes, with most observers, more frequently when the light series *precedes* the sound series.

7. The method of forming the judgment of the relative speed of these successions differs greatly with different observers. Those who naturally incline to assist their judgments by counting or by noticing some hidden organic rhythm, in general have less frequently the sense of greater speed in the light series than have those who depend upon their 'general impression' (whatever that may mean) or upon an overt tapping by hand or finger. Yet the illusion of greater speed in the light does not seem to depend upon the presence or absence of mental aid from any noticeable organic rhythm, whether voluntary or involuntary.

XX. A MEMORY TEST WITH SCHOOL CHILDREN[1]

BY ARTHUR H. CHAMBERLAIN

The Problem Stated

With a view to determining the power of recall in school children, a series of tests were made. It was sought to ascertain the effect upon the power to recall when:

1. A number of objects are displayed (*a*) singly; (*b*) three together.

2. The objects chosen interest, we might suppose, particularly (*a*) the boy's mind; (*b*) the girl's mind.

3. The subjects tested are of different school grades.

4. The subjects tested are of different sex.

Objects Chosen

Fifteen objects were selected as follows: pocket knife, roll of string, marble, watch, key, flat-iron, threaded needle, thimble, scissors, doll, pencil, notebook, two-cent stamp, five-cent nickel, and match.

These objects fall into three groups: First, those that interest particularly the boys and are handled by them in their daily routine. These are the first five objects listed. Second, those that might be expected to interest the girls. These are the second five objects named. Third, those that are of equal interest to both sexes. These consitute the final five objects. All of the objects are found constantly in the child's environment. Choice was made of objects that were not too greatly different in size.

The Subjects

The subjects were chosen from the third, fifth and eighth grades, sixty from each grade, one hundred eighty pupils, all told. They were equally divided, thirty boys and thirty girls from each grade. They represented several different

[1] From the Psychological Laboratory of the University of California.

schools, in various sections of the city where the tests were made.

Apparatus Used

The apparatus for the tests was simple: A circular disc of wood, one-half inch thick and eighteen inches in diameter served as a stand upon which, near its outer edge, the objects were arranged at equal distances apart. A second disc of like thickness and twenty-four inches in diameter, had an opening whose size could be varied as desired. The larger disc was placed four inches above the smaller, which rested flat upon a desk or table. The upper disc was held in its horizontal position and made to revolve over the lower disc as a wheel revolves on an axle. This was accomplished by means of a rectangular block having either end cut to a step-cylinder and housed or shouldered into the inner faces of the two discs at their centers.

Arrangement of Objects

Those five objects supposed to be more familiar to the girls were placed in sequence—the threaded needle, flat-iron, thimble, doll and scissors. In the same way those objects pertaining chiefly to the boys were arranged in sequence—marble, knife, watch, key and roll of string.

Modes of Display

During the tests there were two modes of display. First, the objects were shown in such a way that, at all times, three were visible, while as each new object entered this group, one of the older members of it dropped out. Second, they were displayed singly. Any one subject was tested with only one mode of display.

Method of Procedure

Each subject was allowed one minute for observation of the fifteen objects, whatever was the mode of display. In the first test, the subject was placed directly in front of the cut-out sector and, as the upper disc revolved, the objects came into view, three at a time. The subject moved with the disc, thus keeping directly in front of the opening. In the second test, the same order of arrangement was maintained, but the opening in the disc revealed only one object at a time. By allowing three seconds for each exposure, with one second interval, a total of one minute was given as in the other test.

In every instance note was made of the object at which the observation was begun. The immediate recall was tested at the close of the experiment by having the pupil name all the objects he could remember. A list of these objects was recorded by the operator in the order in which the pupil named them. The subject was also given a sheet of paper having a circle described upon it and corresponding to the disc. Upon this circle he was asked to locate the objects in the order in which they were placed on the disc. Any additions to the list of objects originally recalled were placed to the observer's credit. Record was then made of the total number of objects recalled by each pupil, out of a possible fifteen; of the order of recall, that is, the sequence in which the objects were named; and of the order preserved in placing the objects on the circle. An object is said to be in a '*correct*' position when it is located upon the circle in a position exactly corresponding to its original position on the disc. Two objects were said to be in a '*relatively*' correct position when they simply changed places in location,—were transposed. Or, if two objects in adjacent positions on the disc were given a corresponding location upon the circle, but not arranged properly as regards other objects in the group, the placing was said to be relatively correct. As only objects not included in the list of those correctly placed are included in the 'relatively correct' column, the average for the former is usually greatly in excess of the latter. Whenever the average for correct placing is relatively high, the average for relative placing is considerably lessened. There are twenty judgments when the objects are displayed singly and twenty when they are displayed three together.

The Results

Table I shows the results of the test in the three grades with the two methods of exposure.

These results are then analyzed in several tables that bring out: (1) The effect of the different methods of display without regard to grade, sex or arrangement of objects; (2) The effect of the particular grade of pupil upon total recall, correct and relative placing and the like; (3) The effect of sex upon total recall, the recall of boys' and girls' groups,[1] etc.

Table I

	Sex	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recalled
Grade 3:							
One at a time.....	Boys	7.4	5.7	.7	2.9	2.7	1.17
	Girls	8.1	5.4	.9	2.6	3	1.5
	Both	7.75	5.55	.8	2.75	2.85	1.61
Three at a time....	Boys	9.1	6.4	.9	2.9	3.7	1.92
	Girls	8	5.5	.7	3.4	3	1.4
	Both	8.55	5.95	.8	3.15	3.35	1.66
Grade 5:							
One at a time.....	Boys	10.5	3	2.5	3.6	3.3	1.7
	Girls	9.2	2.3	3.4	2.8	3.3	1.04
	Both	9.85	2.65	2.95	3.2	3.3	1.37
Three at a time....	Boys	9.5	3.7	2.9	2.9	3.2	1.4
	Girls	9.5	2.4	3.7	3.3	3	1.1
	Both	9.5	3.05	3.3	3.1	3.1	1.25
Grade 8:							
One at a time.....	Boys	9.2	5.7	1.9	2.7	3.4	1.04
	Girls	9.6	7.1	.4	3.8	3.2	1.24
	Both	9.4	6.4	1.15	3.25	3.3	1.14
Three at a time....	Boys	10	6.4	2.3	3.1	3.4	1.4
	Girls	10.1	6.6	2.1	3.1	3.7	1.5
	Both	10.05	6.5	2.2	3.1	3.55	1.46

Table II. shows the collective results with the two methods of display.

Table II

Method of Display	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
Singly............	9	4.87	1.633	3.07	3.15	1.37
Three at a time....	9.37	5.166	2.1	3.12	3.33	1.46

[1] The letters G. and B. will be used throughout as referring to the objects spoken of as the girls' and boys' group respectively.

The display of the objects three at a time gives an advantage in the average number of objects recalled, as well as in the average number correctly placed. When shown three together the relative placing also is better than when the objects are displayed singly. The tendency to recall the G. or B. group receives very slight advantage in any one method of display over another. The M.V. of total recall varies only slightly in the two methods.

Table III. shows comparative results in grades three, five and eight.

TABLE III

Grade	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
3	8.15	5.75	.8	2.95	3.1	1.63
5	9.67	2.85	3.12	3.15	3.2	1.31
8	9.72	6.45	1.67	3.17	3.4	1.30

The pupils of the fifth grade show a marked superiority over those of the third grade in the average number recalled. The eighth grade is not greatly in advance of the fifth in this particular. In correct placing, the third grade is almost abreast of the eighth, while the fifth grade drops back to one half the showing made by the third. In relative placing, the fifth grade students far excel those of the third and are much superior to the eighth. The fifth and eighth grades show about equal power in recall of the *B* group, while the third grade makes nearly as good a showing. In every grade the average for recall of the *G* group is slightly better than that of the *B* group.

Table IV. shows the effect of sex in all grades.

TABLE IV

Sex	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
Boys .	9.28	5.15	1.86	3.02	3.28	1.43
Girls .	9.08	4.88	1.86	3.16	3.2	1.30

Table V shows the relation of boys to girls in the different grades.

TABLE V

Grade	Sex	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
3	Boys	8.25	6.05	.8	2.9	3.2	1.55
	Girls	8.05	5.45	.8	3	3	1.45
5	Boys	10	3.35	2.7	3.25	3.25	1.55
	Girls	9.35	2.35	3.55	3.05	3.15	1.07
8	Boys	9.6	6.05	2.1	2.9	3.4	1.22
	Girls	9.85	6.85	1.25	3.45	3.45	1.38

In the third and fifth grades, the boys have a slight advantage over the girls in the total number recalled and in the number correctly placed. The objects composing the *G* group are recalled somewhat better by each sex. The ratio between the recall by girls and boys of the *B* group and the *G* group is only slightly different from the ratio between girls and boys in the total recall.

CONCLUSIONS FROM THE STUDY

The results of the various experiments would seem to justify the following conclusions:

1. Recall is stronger when the objects are seen three at a time than when shown singly.

2. The average for total recall shows a considerable increase from the third to the fifth, with an almost negligible increase from the fifth to the eighth grades. This difference is emphasized when we consider that the age-difference between the fifth and eighth grade was approximately twice as great as that between the third and fifth. In other words, ability to memorize or to recall does not increase regularly with advance in age or experience.

3. The total average of recall for all grades and with all methods of exposure of objects shows the girls not to be superior to the boys. This is not in accord with the usual outcome of experiments in memory. No clear difference is discernible between the boys and the girls in the attraction exerted by the so-called boys' group and girls' group of objects; for both sexes the girls' group was slightly more attractive.

XXI. PRACTICE IN ASSOCIATING NUMBER-NAMES WITH NUMBER-SYMBOLS[1]

BY WARNER BROWN

In a recent study[2] the writer employed the difference between the time required to perceive and name a series of colors and the time required to read the same color-names when they are printed out in type as a typical instance of the general rule that it takes longer to perceive and name a simple object than to perceive and name a word. At that time reference was made to an experiment by Bourdon in which the time for calling out the number of points of light in a small group was said to be no greater than the time for naming the corresponding arabic numeral.[3] On the basis of this experiment Bourdon argues that the process of perceiving a small number of points as a number is no more complicated than the process of perceiving the symbol of the number. If this is correct it means that it is possible in this case to associate the name of an object with the object itself as quickly as with the symbol of the name and this would make it seem probable that the association processes in color-naming involve difficulties peculiar to themselves and are not typical of the general situation in which simple objects are perceived and named.

What follows is an attempt to discover whether number-naming is really a process which is free from the time-consuming difficulties of color-naming. A practice experiment was devised in which number-naming was subjected to the same analysis that was applied in the previous case to color-naming. The material for the experiment was all prepared with the typewriter. It consisted of four sheets, each con-

[1] Studies from the Psychological Laboratory of the University of California.

[2] This REVIEW, p. 45.

[3] *Rev. philos.*, Vol. 65, 1908, p. 426.

taining ten lines of ten items. The first sheet contained the type-written words *one*, *two*, *three*, and *four*. There were 25 words of each sort, arranged in irregular order with not more than 3 nor less than 2 of a kind to a line. Four different sets of sequences of words were used to prevent memorization of any particular sequence. The lines were separated by triple space and the words were separated from each other by the space of three letters. The second sheet contained the arabic numerals corresponding to the words of the first sheet so arranged that each symbol occupied a position, which was relatively the same as the center of the corresponding word in the sheet of words. In this way the eye-movement factor was kept as nearly constant as possible. The third sheet (known as 'dots') was made up of dots arranged to represent the numbers as follows: *one* was represented by a *period*; *two* by a *colon*; *three* by a colon with a period after it; *four* by two colons. The fourth sheet represented the numbers by an appropriate number of oblique strokes ('scores') made with the key used in printing fractions on the typewriter.

The time was measured which was required to read aloud the one hundred items on each of these sheets. The sheets were read one after the other in the order given above, and then all four were read again, so that the time stated for each sheet on each day of practice is the average of two records, the first and fifth, second and sixth, etc.[1]

The accompanying table gives the results of eleven days' practice with this material on the part of twenty-four students. None of these students knew the real purpose of the experiment. They were all encouraged in the supposition, which came naturally to all of them, that they would be able with practice to read the "dots" or "scores" as fast as the words or symbols.

[1] The experiment with colors referred to above, made it clear that there is no considerable difference in time between successive trials on the same day. The average time for naming 100 colors was found to be 42.3, 42.3, 43.0, and 42.8 seconds for 4 successive trials; the time for 4 successive trials of reading 100 words was 29.0, 30.4, 29.9, and 30.5 seconds. There can be no serious objection, therefore, to comparing the rate of reading one sheet with the rate of reading another sheet when their sequence is the same each time. It is understood, of course, that the order of the items on the sheet is different on successive sheets.

TABLE FOR ELEVEN DAYS OF PRACTICE ON THE PART OF 24 PERSONS, SHOWING THE AVERAGE TIME REQUIRED TO NAME 100 ITEMS PRESENTED AS WORDS, AS ARABIC NUMERALS, AS GROUPS OF DOTS, OR AS GROUPS OF SCORES; AND SHOWING FURTHER THE RATIO OBTAINED BY DIVIDING THE TIME FOR THE ARABIC NUMERALS INTO THE TIME FOR EACH OF THE OTHER PERFORMANCES.

Day	Word	Arabic	Dot	Score	Ratio of Arabic to:		
					Word	Dot	Score
1	30.1	27.8	39.6	36.0	1.08	1.42	1.29
2	28.9	26.5	35.9	34.2	1.09	1.35	1.29
3	27.6	25.8	34.4	33.2	1.07	1.33	1.29
4	27.1	25.0	34.7	32.5	1.08	1.39	1.30
5	26.6	25.0	34.0	32.4	1.06	1.36	1.29
6	26.5	24.7	33.0	31.7	1.07	1.34	1.28
7	26.3	24.3	32.3	30.5	1.08	1.33	1.25
8	25.7	24.2	31.8	30.9	1.06	1.32	1.27
9	25.5	23.8	31.9	31.0	1.07	1.34	1.30
10	25.1	23.6	31.2	30.4	1.07	1.32	1.29
11	25.0	23.4	30.9	29.9	1.07	1.32	1.28

The result shows that the time required to perceive and name the number of a small group of marks is longer than the time required to perceive and name the corresponding word or the arabic symbol of the number. In this respect the results agree perfectly with the experiment in naming colors, and support the general dictum that the naming of objects is slower than the naming of words.[1]

The same peculiar inhibitions appear in the reading of the "dots" or "scores" which are encountered in color-naming. Some persons are more troubled by the "dots" and others find more difficulty in the case of the "scores" but no one notes any considerable disturbance of this kind in the case of the words or arabic symbols.

This experiment agrees with color-naming in the essential point that the ratio between the time required to name an object and the time required to name its symbol resists the

[1] As a matter of fact Bourdon's statement that the dots are named as fast as the symbols does not seem to be fully supported even by his own figures. His interpretation of his data raised a doubt which the results of the present experiment tend to clear away. There is no striking conflict between the original data of the two experiments. The method of serial reactions which has been used in the present case undoubtedly tends to magnify the loss of time in the association process and might reasonably be expected to show a greater difference in time between the two processes than would be shown by Bourdon's method of single reactions. But it is not probable that the serial reactions would show a difference unless the single reactions also gave some difference and as a matter of fact Bourdon's reactions do give some difference.

action of practice. This seems to argue in both cases that previous practice is not the basis of the relative rapidity of the latter process. So, too, the fact that the process of naming the symbol is itself capable of a very material improvement through practice precludes our speaking of it as automatic in comparison with another process (the slower one of naming the object) which improves no faster. It does not appear probable that differences in previous practice have much to do with the relative speed of the two processes.

The present experiment confirms the inference drawn from one part of the color experiment that phonetic symbols such as letters do not seem to be responsible for the advantage in speed of one association process over the other. It might be thought that the sight of the different letters would guide the complex movements of the vocal organs in uttering the word, and so facilitate the reaction to a word, but it appears that the words with their phonetic symbols can not be read quite as fast as the arbitrary arabic symbols which contain no phonetic elements. This is true for nearly every individual person and in spite of the fact that the spacing of the words and figures on the page gave nearly normal conditions of eye-movement for the words and rather unusual conditions for the arabic symbols.

In conclusion it may be said that the causes of the delay of the association processes in naming a simple object remain as obscure as ever. But on the negative side it seems clear that the greater speed with which words are named does not depend upon an advantage in practice and does not depend upon the suggestiveness of the letters in the words.

XXII. INCIDENTAL MEMORY IN A GROUP OF PERSONS[1]

BY WARNER BROWN

The study which is reported below leads to the conclusion that the factors which make it easy or difficult for an individual to recall certain of the incidental observations of his past experience also tend to affect in the same way the collective memories of a large group of persons.

The material for the investigation was obtained by having the members of a large college class write down, in a limited time, the names of all of the advertisements which they could remember having seen recently in the street-cars. They also, after making out the list, wrote down answers to certain questions about the advertisements, but that has nothing to do with the present report. The experiment was performed twice. The first time 175 persons wrote lists. The lists contained in all 896 items and these items were found to include mentions of 215 different advertisements. Thus the average person recalled 5.1 advertisements, and the average advertisement was mentioned 4.2 times. Table I is arranged

TABLE I

The Number of Items in the List	The Number of Persons Giving a List of this Length	The Total Number of Items in the Lists of this Length	The Relative Frequency of Occurrence of Such Items
0	21	—	—
1	8	8	23.1
2	17	34	24.8
3	15	45	16.2
4	22	88	22.6
5	16	80	20.8
6	20	120	18.0
7	13	91	21.9
8	12	96	19.7
9	9	81	15.7
10	7	70	14.4
11	6	66	14.4
12	3	36	11.4
13	3	39	9.5
14	3	42	12.5

[1] From the Psychological Laboratory of the University of California.

according to the length of the lists and shows the number of persons who gave a list of each length from 0 to 14 items. Table II. shows, roughly, the number of mentions accorded to

TABLE II

The Number of Items Mentioned	The Number of Mentions Received by Each of These Items	The Average Position of These Items in the Lists	The Number of Times One of These Items is Found at the Head of a List, Per Cent.	The Number of Times One of These Items is Found in the 8th or a Lower Position, Per Cent.
{ 53	{ 1	5.4	5.7	22.6
{ 52	{ 1	5.3	11.5	19.2
34	2	5.0	11.8	19.1
8	3	4.6	11.1	14.8
14	4 or 5	3.9	14.8	14.7
8	6 or 7	5.2	7.8	19.6
8	7 or 8	4.5	11.9	17.0
7	8 or 9	5.0	10.2	18.6
6	9, 10, 11	4.4	17.5	10.5
4	12–15	3.6	16.6	7.4
3	15–18	3.8	28.0	10.0
3	18–20	4.0	28.1	12.3
2	23, 30	.8	24.5	15.1
1	55	3.5	20.0	5.5
1	55	2.9	30.9	5.5
1	58	3.0	24.1	5.2

different advertisements. Thus 105, or nearly half of them, are mentioned by but one person, while three of them are mentioned more than 50 times each. These three 'best' advertisements (Arrow collars, Spearmint gum, and a local confectioner), receive almost one fifth of all the mentions.

In what has just been said we have before us the essential points upon which the investigation is to be based. Some of the advertisements are much better remembered than others; and at the same time some persons remember a larger number of advertisements than other persons do. Is it true then, that the advertisements which are remembered by the greatest number of persons are the ones which permit of the easiest recall on the part of those persons who can remember several? The answer is obtained by finding whether the advertisements which are forgotten by most persons are written down later than those which can be recalled by more people. The result shows (Table II.), that in the average, one of the 105 which are mentioned only once is not mentioned until five others have been mentioned. The most popular advertisement has an average position of third. In other words the average person will write down an item which many other persons can remember, sooner than he will write down one which only a few can remember. Less than 9 per cent. of the 105 straggling items are found at the head of a list, while about 24 per cent. of all the mentions of the most popular one are found at the top of a list.

Table II. has been arranged to show the result when the whole number of mentions is broken up into 16 approximately equal groups of from 50 to 60 cases each, on the basis of the frequency with which the items were mentioned. The table shows the average position of the item in the list; the items most frequently mentioned stand ahead of the rarer items in the lists. It also shows for each advertisement or group of advertisements the proportion of its mentions which stand at the head of a list; those most frequently mentioned are more apt to be mentioned at the very start. The correlation of rank

$$\left(r = 1 - \frac{6\Sigma D^2}{n(n^2 - 1)}\right)$$

in order of mention, in the above table, with average position in the list is .85, and with proportion of leading mentions it is .76.

While conducting this investigation the writer labored under the impression that those persons who mentioned only a few items would be apt to have peculiar reasons for remembering the few advertisements which they could recall, and that they would mention many wild and eccentric examples. The analysis of the data shows that such can not be the case. The sporadic item very seldom occurs early in any list; it can only rarely occur in a short list. The last column of Table I. shows the relative frequency with which the items in lists of different lengths are mentioned. This is found by listing all of the items in all of the lists of a certain length and entering opposite that item the total number of times that it is mentioned in all of the lists, *i. e.*, the frequency of the item. The total amount of all of these frequencies is then divided by the total number of items concerned. The results of this computation make it evident that the items which occur in a very short list are items which are more often remembered than the items which occur in longer lists. As the length of the list increases it includes more and more sporadic or infrequent items. The original data show that of the 42 items contained in the one-item or two-item lists three-quarters were mentioned by more than ten different people, while of the 42 items contained in the fourteen-item lists three quarters were advertisements receiving *less* than ten mentions and one quarter were rarities, mentioned by less than four persons. Turning to Table II., the last column, it is evident that the less frequently mentioned items are more apt than the others to find mention toward the end of a long list, in the eighth or a still lower position. The correlation between rarity of occurrence and low position in the list is .78.

The facts warrant the conclusion that the items in the short lists are not determined by individual or special conditions, but are simply the items which are most easily remembered. There seems to be good reason to believe that the same factors, whatever they are, which cause an advertisement, or other similar incidental impression, to be recalled early in the memory of one individual cause it to be recalled early in the memory of another, regardless of the number of items which may, or may not, follow after it. The difference between individuals in this respect seems to be a difference in the *number* of the items recalled, and not in the kind or identity of the items. Items, which, for any reason, are difficult to recall appear late in long lists and do not appear at all in short lists.

This conclusion is significant for experimental work in memory as it puts a new value upon the relative position of the items in the recalled series. Apparently the first items to be recalled are generally those which make a universal appeal; the special personal appeals are reported later, or not at all. Moreover there seems reason to believe that "poor" incidental memory involves, at least with this material, no other abnormality than poverty or "weakness."

So far as the actual advertisements which were used in the investigation are concerned, it is important to note that some make a much more lasting impression than others. The difference can not be expressed by saying that one advertisement appeals to more persons than another; it must be stated as a difference in the strength of the appeal. The good advertisement makes an appeal so strong that it can not be forgotten; the poor advertisement is forgotten by all except those persons who can remember very weak impressions.

The results of this investigation are fully confirmed by a repetition of the experiment some five months later with another college class, among the members of which there were only a few who had taken part in the first experiment. As a result, perhaps, of the interest aroused by the first experiment, the average number of items per student rose from 5.1 to 9.9. The number failing to report any advertisements fell from 21 to 3 although the class was only a third smaller. There were no lists of one or two items presented. In spite of the larger number of items per student, the variety of the items increased to such an extent that the average number of mentions per advertisement only increased from 4.2 to 4.6. Under these changed conditions all of the conclusions of the first experiment were confirmed. The correlation between frequency of mention and high position in the list was found to be .76. Of the straggling single items only 7 per cent. were found at the head of a list, while 24 per cent. of the mentions of the most popular advertisement headed lists. The correlation between popularity and primacy was found to be .61. On the other hand the correlation between infrequency and a position somewhere lower than ninth on the list was found to be .76.

In conclusion it may be said that the items which appeal to the largest number of persons make the strongest appeal to most of those persons, and that those items which appeal to only a few make a weak appeal even to them.

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THE PSYCHOLOGICAL REVIEW

A PROPOSED CLASSIFICATION OF MENTAL FUNCTIONS

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Whenever anything is declared to be a function of mind we should be able to discover both the general sense in which the term 'function' is used, and also the setting of the particular function in question within a functional whole. This is as much as to say that classification of mental functions should have a place in functional psychology that will correspond to the position now occupied in structural psychology by lists of mental elements and modes of combination. Up to the present time such a systematic background has been lacking. As a consequence the undefined fringe of meaning in discussions of functions leaves still too much room for misunderstanding one another, or even oneself. Further, the lack of classification implies that we are not yet ready to begin describing functions in terms of functional laws. Such is the unsatisfactory situation out of which the present article attempts to take a single step. The results are necessarily preliminary and tentative; the most that I can hope for is that other investigators will be sufficiently interested to make good my deficiencies.

The approaches thus far made toward a classification of mental functions fall into the following classes:

(*a*) Affirmations of the purposive character of mind, without any list of specific functions.[1]

[1] *E. g.*, J. E. Creighton, 'The Standpoint and Method of Psychology,' *Phil. Rev.*, March, 1914; H. Münsterberg, 'Psychology, General and Applied,' 1914, and R. M. Ogden, 'Introduction to General Psychology,' 1914.

(*b*) The oft-made assertion that the fundamental functions of all life, mind included, are nutrition and reproduction. At a later point I shall ask what, as a matter of fact, mind does with these two vital processes. At once, however, I would point out that some of the so-called 'irradiations' from primitive hunger and love—for example, science—have characters of their own which it requires some violence to call either nutritive or reproductive.

(*c*) To each item in a structural classification of mind Angell has added the question, What is its function? There results what might be called an engineer's drawing of mind as an adjusting mechanism. It goes far toward supplying the functional classification that I am seeking, and as a consequence I shall borrow rather freely from it. That it needs supplementing, however, should be clear from these two considerations: *First*, Angell's list of functions is not based upon similarities and differences among the functions themselves; he merely finds and describes a function for each element of structure. *Second*, his genetic method keeps his eyes fixed upon the earliest mental reaction, the *terminus a quo*, whereas our problem—the direction of mental movement—requires us to consider also the most developed reaction as a *terminus ad quem*. I find no fault with Angell for not answering questions that he does not raise, but functional psychology must surely incorporate into itself a fuller description of the interests of developed mind. After we have named early utilities, and even after we have made such generalizations as that mind extends the control and organization of movements, something in the nature of function still remains over. To illustrate: If you should ask what are the functions of a dividing engine, I might answer by showing how each wheel and lever contributes to the accurate control of movement, and I might generalize by saying that this instrument as a whole has the function of so adjusting our motions as to enable us to make extremely minute divisions of a surface. This would be a functional description, no doubt, yet beyond it lies the destination of the whole, namely, certain sciences in the interest of which the dividing engine exists at all. Just so, the proposition that mind increases the extent and the fineness of our adjustments needs to be supplemented by inquiry into the terminal meaning of the whole.

(*d*) A fourth approach to a functional classification proceeds as follows: Mental functions are correlative with interests; interests have their roots in instinctive satisfactions; therefore an inventory of instincts would be *ipso facto* a list of the functions of mind. Let us, then, look to our original nature, that is to our unlearned tendencies to react in specific ways, to give attention to specific sorts of object, to take satisfaction in predetermined kinds of mental occupation. The program is attractive, and we shall see that it yields results that have an important bearing upon our problem, though not quite the results that are commonly expected. For, *first*, the 'original' nature of man means the part of his nature that is disclosed antecedently to all culture, that is, before the mind has performed some of its most characteristic acts.[1] *Second*, the broad mental areas traditionally called instincts are disappearing from the psychologic map, and in their stead there is appearing a vast, indefinite number of narrow adjustment acts. For example, Thorndike says that "reaching is not a single instinct, but includes at least three somewhat different responses to three very different situations."[2] Thus, the farther back we go in our mental history the greater the difficulty of functional classification, unless we constantly look forward as well as backward. On the other hand, the very minuteness and rigor of Thorndike's analysis reveal certain general, forward-looking tendencies. Thus, there is a tendency to be or to become conscious;[3] there is an original 'love of sensory life for its own sake';[4] there is spontaneous preference for experiences in which there is mental control;[5] finally, there is a native capacity for learning.[6] In short,

[1] E. L. Thorndike, 'The Original Nature of Man,' 1913, 198 f.; also 'Education,' 1912, Ch. v.

[2] 'Original Nature,' 50.

[3] *Ibid.*, 170 f.

[4] 141.

[5] 141 f.

[6] 171.

there are 'original tendencies of the original tendencies . . . original tendencies not *to* this or that particular sensitivity, bond or power of response, but *of* sensitivities, connections and responses, in general.'[1] Here, I take it, is where interests, in the proper sense of the term, come in. If we are to define our mental functions by our interests, we must consider not merely tendencies to this or that sensitivity, but also and particularly our tendencies to organize or do something with our sensitivities. Some results of Thorndike's analysis of such tendencies I shall take over into my own classification.

(*e*) Some of the conditions for a general classification of mental functions are fulfilled in recent discussions of value.[2] Here function is treated as function; it is not confused with elements of structure, nor is a given function identified with its earliest, crudest form. Sense of direction *from* something *to* something is here. Urban's list of values, in particular, conveys a sense of the general direction of the movement of mind. What is still needed is something like a combination of Angell, Thorndike, and Urban. The reason why lists of values need supplementing is twofold: *First*, they do not comprehend mind as a whole, for example, its biological aspects. *Second*, several types of value, as will presently appear, are not simple functions, but functional complexes.

These converging lines in recent psychology may be summarily described as follows: (1) All mental process whatsoever is purposive, and it should be analyzed from this as well as from the structural standpoint—that is, mental functions must be determined. (2) The human mind is functionally as well as structurally continuous with the animal mind, so that a classification of functions must include the biological point of view. (3) The *termini* of mind, by which functions are defined, include conscious interests, or self-defining ends. (4) Several specific functions of both the biological type and the conscious-interest type, have been defined here and there in scattered places.

What remains to be done is to systematize these results;

[1] 170.

[2] The chief classifications of value are summarized by J. S. Moore, 'The System of Values,' *Jour. Phil.*, VII (1910), 282–291.

to discover, and if possible, fill remaining gaps; and to show the relation of the resulting functional concepts to older, more current psychological categories. The whole must, of course, be description, not evaluation. The work of functional psychology is not to tell us what we ought to prefer, but to determine, as a matter of observable fact, what mind does actually go toward and 'for.' Two main divisions, each with several subdivisions, are implied in what has already been said.

A. Biological Functions.—To occupy the biological standpoint—which is simply a point of view used temporarily for certain purposes, and not necessarily more true or fundamental than other points of view—is to think of living beings without reference to any approvals or preferences, any 'better and worse.' The biological functions of mind consist in quantitatively determinable increases in range of response to environment. Our subdivisions of biological functions, accordingly, are as follows:

1. *Increase in the spatial range of objects responded to.*

2. *Increase in the temporal range of objects responded to.*

3. *Increase in the range of magnitudes to which response is made.*

4. *Increase in the range of qualities responded to.*

5. *Increase in the range of environmental coördinations to which coördinated responses are made.*

This list will remain the same whether we approach the facts from the behaviorist standpoint or from that of traditional psychology. I call these functions mental for two reasons: Because they characterize mind in its most conscious as well as its less conscious stages, and because these 'directions of movement' though they are established before we reflect upon them, become, after reflection, conscious purposes.

The relation of this analysis to the popular categories, nutrition and reproduction, requires a word of explanation. To begin with nutrition, what has mind, as a matter of fact, to do with it? (*a*) Mind connotes changes in the feeding reaction that fall under one or more of the above-listed functions. But the law here is a general one; it applies likewise to protection from weather, from accidents, and from enemies, and it applies also to social organization, science, and art. As far as range of response is concerned, then, we need no special nutrition category. (*b*) Mind connotes success in a competitive struggle over a limited supply of food. Increase of mind makes a difference here, but in what? Can the difference be expressed in terms of nutrition? No; for nutritive functions would go on at least as well if no competition occurred, or if the mentally inferior animal had happened to get the food instead of the mentally superior one. The difference made by mind is that some new object or quality is responded to, and that the more differentiated response tends to be perpetuated by inheritance. Here the function appears to be not nutrition but the production of a more specialized individual. (*c*) It is at least as correct to say that mind moves away from as toward nutrition. For, correlative with the growth of mind goes restriction of feeding to specialized kinds of food, and consequent increase in the mechanical cost of getting it. The ocean brings food to an oyster; a cat must hunt for its living. Everywhere the discriminative appetite is the expensive one. (*d*) If we scrutinize cases in which feeding appears to be the end of conscious effort, we find, almost if not quite invariably, that the very act of consciously seeking food gives to nutrition the place of means to some experience beyond itself. The labor movement illustrates this principle on a large scale. Even if the central stimulus of this movement could be identified as hunger (which is doubtful), the conscious end of the struggle is home life, leisure, culture, the education of children, free participation in the determination of one's destiny. (*e*) But it may be said that the social integration of men has as one of its most obviously important consequences the stabilizing of the food supply and a more even distribution of it. Civilization will soon reach a point at which famines can no longer occur. What, it may be asked, is the meaning of the present movement for agricultural instruction, and indeed for vocational training in its whole extent, if not just this, that men want enough to eat? Here, indeed, is excellent material for answering the question what mind is about when it seeks food. The crucial question for us is whether the direction of the mind's movement here can be defined as from hunger to repletion. Of course food is an object of conscious desire. So is getting to Albany on time an object of desire on the part of one who is travelling from New York to Buffalo by way of the New York Central. The road to our social ends certainly takes the food-supply route. But, as in the case of the labor movement, social food seeking that begins instinctively awakens, sooner or later, a consciousness of the social values broadly called cultural, and these it is that define the specifically mental destination or function.

Turning now to the question whether reproduction should be accounted a mental function, we find the course of evolution not at all ambiguous. Reproduction is most prolific in the lowest ranges of life. Mental development is clearly correlated with decrease in the birth rate. How many factors are involved in this decrease I will not attempt to say, but certainly mind is one of them. Herbert Spencer realized this fact,[1] though he did not bring out the full significance of it. John Fiske's two essays on human infancy[2] carry us much farther. Mind individualizes the various living beings that are involved, first the offspring and then the parents. The obvious mental function is not reproduction of existing types, but the production of certain new, more specialized types. Mind does not stimulate reproduction any more than it stimulates hunger; it does not increase fertility any more than it increases assimilation. But, just as mind specializes foods and increases the cost of feeding, so it individualizes living beings and increases the cost of each individual. The whole may be viewed as on the one hand an increase of inhibitions, and on the other hand a focalizing of dispersed attention. In short, the biological functions of mind can be altogether expressed as increase in the range of objects and

[1] 'Principles of Biology,' Part VI., especially Chs. XII. and XIII.

[2] Reprinted under the title, 'The Meaning of Infancy,' in the Riverside Educational Monograph series, Boston, 1909.

qualities responded to, and in range of coördination of responses.

B. Preferential Functions.—Our discussion of nutrition and reproduction has already brought us face to face with conscious preferences, that is, mind defining its own direction. We may take for granted, I suppose, that satisfactions are, in general, a sign of unimpeded mental action, and that we can tell one another about our satisfactions. One may, indeed, be mistaken as to what one likes, that is, as to what it is in a complex that makes it likable, but such mistakes can be discovered and corrected, chiefly by further communication. The functions of our second main division, then, are always qualitative (implying a 'better and worse'), and they are scientifically known through communication by means of language. Thus it is that many preferences have already been successfully studied, such as color preferences, the likes and dislikes of children with respect to pictures and with respect to future occupations, merit in handwriting, merit in English composition, merit as a psychologist, the comic, persuasiveness, even moral excellence.[1] Such experimental studies have the effect not merely of discovering preferences, but also of adding precision to preferences already recorded in the world's literature. Would that a Hollingworth might have been present throughout human evolution to record the growth of human preferences. As the case stands, we must combine experiment upon present preferences with the less precise study of life as reflected in literature, art, and institutions.

Where shall we look for a basis for the systematic subdivision of preferential functions? Suppose we compare early types of reaction with late ones, say, Thorndike's picture of original nature with value-analyses, which represent developed interests. Let us begin with the fact that there is satisfaction in merely being conscious. To be conscious, then, we may count as the first preferential function. Note, next, that satisfaction attaches to mere movement of atten-

[1] H. L. Hollingworth gives a list of 'order of merit' researches in 'Experimental Studies in Judgment,' *Archives of Psychology*, New York, 1913, 118 f.

tion from one object to another, as in 'love of sensory life for its own sake.' May we not say that a second preferential function of mind is to multiply its objects? A third appears in the preference for experiences that include control of objects. A fourth is closely related thereto, namely, the arrangement of objects in systems—it is a function of mind to unify its objects. This is seen all up and down the scale from the spontaneous perception of spatial figures in the starry sky to the ordering of an argument.

These four preferential functions appear to be fundamental, that is, not further analyzable. If we turn, in the next place, to the usual value categories to see whether we may not find further unanalyzable functions, we come upon the interesting, not to say strange, fact that ethical, noëtic, religious and even economic values presuppose a function that they do not name. Each of these types of value depends upon the existence of a society of inter-communicating individuals, yet it seems not to have occurred to anyone to include a social category—simply and specifically social—in discussions of either functions or values. Should not the fifth preferential function in our list, then, be the function of being social, of having something in common with another mind, in short, of communicating? The justification, not to say necessity, for recognizing a simply social function of mind, exists not alone in the social presupposition of several recognized values, but also in a long series of genetic studies which, from one angle after another, have revealed the fundamentally social nature of consciousness.[1]

There remains for consideration our esthetic experience. Doubtless it involves functions already named, particularly

[1] It is true that these are commonly studies of content rather than of function, and that 'I' and 'thou' appear therein as 'idea of I' and 'idea of thou.' For the purposes of merely structural analysis this is doubtless sufficient. That is, structural analysis as such has no place at all for the experience of communication. On the other hand, communication will loom large in any adequate general analysis of mental functions. In an article 'On Having Friends: A Study of Social Values' (*Jour. Phil.*, XII. (1915), 155–161), I essay a functional treatment of one easily accessible social experience. Satisfaction in having a friend I show to be satisfaction in *a second experiencing*. Social experience like this is distorted whenever attempts are made to construe it without the *-ing*.

the functions of unification and communication. But it seems to contain also an attitude somewhat different from those already named, the attitude of contemplation—the taking of satisfaction in objects merely *as there*, without regard to anything further that may happen to or with them. Hence I add contemplation to the list.

The preferential functions, then, are these:

1. *To be conscious.*
2. *To multiply objects of consciousness.*
3. *To control objects, oneself included.*
4. *To unify objects, oneself included.*
5. *To communicate, that is, have in common.*
6. *To contemplate.*

Some omissions from this list require explanation. *Play* is omitted because it involves a complex of 1 and 2, generally 3 also, sometimes all six, and because it is fully exhausted therein. *Truth* is omitted because, as far as it is not an abstraction from actual intellectual functioning, I hold it to be analyzable without remainder into functions already named, especially 4 and 5.[1] No *ethical* function appears because the three objectives that it includes—control, unification, socialization—already have appropriate recognition in the list.[2] As to *economic* value, it seems to be exhausted in the notion of control within a social medium. Finally, *religion* is without a place in the list because it offers no particular value of its own. Religion is not coördinate with other interests, but is rather a movement of reinforcement, unification, and revaluation of values as a whole, particularly in social terms.[3]

It will be asked, no doubt, whether the functions of mind can be named without any direct reference to instinctive desires. In addition to what has already been said concerning nutrition and reproduction—that they are, so to say, constants that find a supply at every level of mentality—it may now be added, as a general truth, that mental activity upon the objects of instinctive desire does not satisfy the desire in

[1] Cf. A. W. Moore, 'Truth Value,' *Jour. Philos.*, V (1908), 429–436.
[2] Cf. J. H. Tufts, 'Ethical Value,' *Jour. Philos.*, V (1908), 517–522.
[3] G. A. Coe, 'Religious Value,' *Jour. Philos.*, V (1908), 253–256.

its initial form, but modifies the desire itself. For example, what has at first only a derived interest as means to something else may acquire an interest of its own, become an end. This is surely the way that science has come into being, and very likely art also. The evolution of parental and of conjugal relations offers abundant examples of the truth that the distinctive work of mind with our desires is to differentiate and recreate them. Our list of mental functions, accordingly, need not specify particular desires, but only the primary ways in which mind works among them.

A question may arise, also, whether higher desires or ideal values ought not to appear in the list. Is not the most distinctive achievement of mind in the realm of desires, it may be said, the mastery of certain ones in the interest of others? I agree that 'the desires of the self-conscious' must be recognized as having a character of their own,[1] and that a list of mental functions must do justice to them. "The valuation of persons as persons constitutes a relatively independent type, one which presupposes a differentiation of object and attitude."[2] The list as it stands, however, will be found to do justice to this differentiation. Here are self-control, self-unification, self-socialization, with the implication that all this applies to any and every self, both actualized selves and ideal selves.

Finally, inasmuch as no pleasurable sense-quality of objects is mentioned in the list, but only 'objects, oneself included,' doubt may arise whether the functions here named are not merely formal and contentless. Functions would indeed be merely formal if they were so defined as to imply indifference to the specific qualities of things. 'Pure intellect' is certainly a mere abstraction, never a function. In a list of 'preferential functions,' however, satisfactions are everywhere presupposed, not ignored. Granted both painful and pleasurable objects as data, our question is what mind does with such data. Psychology is of course free from the old hedonistic fallacy that the only thing we can do with

[1] A. O. Lovejoy, 'The Desires of the Self-Conscious,' *Jour. Philos.*, IV (1907), 29–39.

[2] W. M. Urban, 'Valuation, its Nature and Laws,' London, 1909, 282; see also 269.

pleasures is to seek for them, and that the only thing we can do with pains is to avoid them. What we try to do in the presence of such data is to control and organize them, sifting out an item here, deliberately enlarging an item there, all in the interest of being, so to say, at home with oneself and with one's fellows. In short, the preferential functions here named represent minds as mutually attaining freedom in the world as it is. Such minds are as concrete as anything can conceivably be.

COLOR THEORY AND REALISM[1]

BY KNIGHT DUNLAP

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The suggestion made by McGilvary concerning a possible color theory in harmony with a doctrine of sensational realism[2] seems to me very important. No color theory, so far as I am aware, has attempted to do more than satisfy the psychological requirements (some have been contented with much less); but it is possible that we may find a theory which will satisfy both the psychological requirements and the requirements of realism. If such a theory be found, it will be a matter of interest and importance.

I should like to point out, in the first place, that none of the prominent theories can be given a realistic turn. The Franklin theory, and the Hering theory with its variants, fall short on the psychological side in so many places (as, for instance, in the inability to account for the perception of green without red) that they may be dropped out of consideration. The Hering theory, it is true, would perhaps be satisfactory to the realist, if it were psychologically defensible; but the Franklin theory being a physiological interpretation of the current form of the Young-Helmholtz theory, is not acceptable to the realist, for reasons which I will mention below.

The Young-Helmholtz theory, as at present held, is satisfactory to the psychologist, largely because it is, as I have elsewhere explained,[3] merely a psychological schematization, elastic enough to take in all of the accepted data of color vision, but not concerned with definite hypotheses in the physiological unknown.

The Young-Helmholtz theory assumes red, green, and

[1] Written in 1912, but withheld from publication on account of the crowded condition of the Review.

[2] 1912, *Philosophical Review*, XXI. (2), 171. If I am misrepresenting Dr. McGilvary's realism (which is more than possible), I offer my apologies.

[3] 'System of Psychology,' p. 73, footnote.

bluish-violet (indigo) as the three primary colors; not because they are the only ones on which the theory can rest; but because it happened to start out with them for simplicity's sake, and has as yet seen no sufficient reason for changing.

The statement of Helmholtz on this point is significant: "Der Wahl der drei Grundfarben hat zunächst etwas Willkürliches. Es könnten beliebig jede drei Farben gewählt werden, aus denen Weiss zusammengesetzt werden kann. Young ist wohl durch die Rücksicht geleitet worden, dass die Endfarben des Spectrum eine ausgezeichnete Stellung zu beanspruchen scheinen. Werden wir diese nicht wählen, so müsste eine der Grundfarben ein purpurner Farbenton sein, und die ihr entsprechende Curve in *Fig. 119* zwei Maxima haben, eines im Roth, eines im Violett. Es wäre dies eine complicirtere, aber nicht unmögliche Voraussetzung. So viel Ich siehe, giebt es bisher keine anderes Mittel, eine der Grundfarben zu bestimmen, als die Untersuchung der Farbenblinden."[1]

So far, no exception can be taken to Helmholtz. But in the consideration of color blindness, he made a mistake which was quite excusable at the time, and assumed that in the ordinary cases of dichromopsia the patient sees green and violet, since the natural explanation of dichromopsia seemed to be the absence of one of the three processes. Hence, he found no reason for abandoning Young's primaries. We now know (or at least it is generally believed) that the two colors seen in typical dichromopsia are yellow and blue or some color near blue. This gives us the alternative of abandoning Young's set of primary colors, or of stating the parachromopsias in terms other than those of absence of one of the three processes. The latter alternative has been chosen by adherents to the Young-Helmholtz theory,[2] and for psychological purposes is quite satisfactory so far. It might be thought, however, from the statement quoted above, that

[1] 'Physiologische Optik,' Zweite Abschnitt, § 20 (pp. 292–293 in the first edition).

[2] Opponents of the three-color theory make no mention of this, but refute the theory in its older form, which is much more satisfactory for their purposes. So far as I can find, the only modern statements of the theory in English text-books are in Greenwood's 'Physiology of the Senses' and my 'System of Psychology.'

Helmholtz would rather have considered the certification of yellow-blue dichromopsia as an indication that the three primaries should be changed.

The acceptance of green and red as primaries prevents a realistic interpretation of the phenomena of color vision, since a really *red* object is (probably) seen as *yellow* by the dichromopsic individual, whereas red is not compounded from yellow, but yellow from red, according to the theory. There may be some way of avoiding this difficulty, but it seems to me that the acceptance of the Young-Helmholtz theory in the present form, or of the Franklin physiological theory based on it, entails the assumption that colors are purely private content. So far as we can assume psychologically, some person or animal might see as blue, or as any other color, or even as the note b♭, the object I see as red.

It seems to be possible to modify the Helmholtz theory in such a way as to make it compatible with sensory realism; and possibly the modification would be just as satisfactory psychologically. We might eventually have to assume four colors instead of three, but for the present we may deal with three, with the addition of white (gray). This addition is necessary because of the fact that all sorts of parachromopsias seem to agree in the perception of white in quite a normal fashion (or at least it is now generally believed that this is the case).

If the three colors are yellow, the spectral color usually called blue-green, and the purple complementary to green, the theory seems to work out very well. As a matter of fact, if the total range of colors be studied *without prejudice*, this triad is seen to be at least as satisfactory (so far as mere qualitative comparison goes), as any other. Let us call the three colors *yellow*, *peacock*, and *mauve* (the hues commonly indicated by the last two of these terms are near enough to the colors in question to serve the purpose), and the white or gray *neutral*. We may then use the letters *Y*, *P*, *M* and *N* without confusion. Red is a mixture of *Y* and *M;* green a mixture of *Y* and *P;* blue and violet are mixtures of *P* and *M*. Practically all colors contain some *N*. Hence the fact that in complete achromopsia only N is seen, in certain parachromopsias only the Y of the red ($Y + M$) combination, and in others (possibly) only the P of the blue and violet ($P + M$) combinations, is perceived, is quite intelligible. It is quite conceivable, in other words, that the M curve of the 'slow' end of the spectrum might alone be lacking, or the M curve of the 'rapid' end, or even both together. Whether the common cases of dichromopsia with shortened spectrum are due to the absence of the M curves alone, or to the absence of the slow M curve and the P curve, we can not decide from the evidence so far. In the cases with unshortened spectrum, the P curve alone may be missing. If there are transitional cases between these two forms of dichromopsias (which some investigators deny), they can also be accommodated in this omnibus theory; for it may be pointed out that there are endless possibilities of difference in sensitiveness of the processes present.

The occurrence of pure white in normal vision is to be explained by the facts (which are established regardless of theory) that the stimulation of any color process renders it progressively less sensitive. If the eye is exposed to the influence of yellow light, it sees progressively (expressed loosely) less and less yellow, and more and more white. The apparent intensity of any component, in other words, is relative not only to the 'natural capacity' of the eye, but also to its condition at any moment; and this condition depends largely on preceding stimulations. We have to assume further that any mixture of light rays to which the eye has become well adapted, will therefore not stimulate the color processes, although it will stimulate the N process. It is a significant fact that the eye becomes as well adapted to yellow gaslight or to the bluish light of the mercury vapor arc, as to daylight, if it is subjected to one of these stimuli alone.

There is however a further necessary statement, which is not a statement of fact unless our hypothetical theory be true; and that is, that the three processes can not in any case be simultaneously stimulated. Any two of them may be set in action, but an additional stimulus, adequate, when occurring alone, to excite the third process, simply has an inhibitory effect on the first two (although acting as an increased stimulus to the *N* process), unless this third stimulus becomes relatively more intense than the first two, in which case it alone is effective, the other two serving to lessen its specific effect. This statement rather complicates the theory, but, for that matter, the complication indicated occurs in some form in every theory, even in the current Young-Helmholtz theory. While this realistic color theory seems to me highly interesting, and is probably just as workable as the accepted form of the Young-Helmholtz theory, I should not be inclined to substitute it for the latter unless the former theory should be shown to be the psychologically superior one. A sufficient factor of community in the world of objects may be given in the relational elements alone, and I think we might accept this point on a basis of realism rather than of idealism. It is however, well to bear in mind the realistic theory of color, as it apparently offers welcome possibilities of psychological research.

POINT SCALE RATINGS OF DELINQUENT BOYS AND GIRLS[1]

BY THOMAS H. HAINES

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The Yerkes-Bridges[2] point scale for measuring intelligence appeals to the worker in the province of the standardization of the development of human intelligence most strongly as a very useful rationalization of the Binet-Simon method.

1. It puts the whole process on such a basis that is is constantly self-perfective,—the norms approach the reality in direct proportion to the increase in number of records of normal children summarized.

2. Another advantage is that it becomes a simple matter to plot curves for the growth of intelligence in different races, in different sexes, and in different classes of society in the same locality. All resulting curves and tabulations, being made by the same examinational methods, are directly comparable.

3. It is not to be denied, also, that the point scale method of rating intelligence overcomes some arbitrariness in method, in that it allows the individual to make his credits anywhere along the line of some twenty tests, whereas the Binet method makes the passing of a given year depend upon a fixed standard, passing four out of five given tests.

4. Partial credit given for partial results also commend this method, as for example in 'words given in three minutes,' 'three words used in one sentence,' 'arrangement of weights,' and 'counting backward from 20 to 1.'

It was a very fortunate feature of the plan of the originators of the point scale, that they pursued the natural

[1] Read before the American Psychological Association, at a meeting at Philadelphia, December 29, 1914.

[2] See 'The Point Scale. A New Method for Measuring Mental Capacity,' by Robert M. Yerkes and J. W. Bridges, *The Boston Medical and Surgical Journal*, Vol. CLXXI., Number 23, December 3, 1914, pp. 857–866.

method of development, and relied almost entirely upon the Binet material and methods. Nineteen of the twenty point scale tests are Binet tests. Analogies are extra.

This makes it a very simple matter to conduct the two examinations on the same person at the same time, or rather to secure both a Binet and point scale rating of a given person from one examination. One has simply to follow the order of the point scale sheet. When this is complete, it is only a matter of half a dozen short tests to complete the Binet rating. This practice has been pursued by the author in examining two hundred delinquents in industrial schools in Ohio. The idea in mind, in the beginning, was to try out the point scale in comparison with the Binet-Simon method, to see what it was worth, as it was frankly recognized that the data summarized in the point scale, so far, from normal children, are rather limited, as compared with those from the use of the Binet scale.

It was soon apparent that results from the two methods showed a very close parallel, the point scale results tending a little higher, as would be expected with abnormal adolescent minors, because of the wider opportunities offered for securing credits, for any given year of intelligence age. But the paralleling is so close in the low grade morons, and the agreement is so close between the two methods in excluding intelligence defect in the higher grade delinquents, that disparity between the two ratings seemed at once to afford reasonable ground for doubt as to the value of the Binet findings.

We formed, therefore, a class of *doubtful cases*,—cases in which Binet rating made the child less than twelve years old mentally, and point scale credits were beyond twelve years. The close correlation of results by the two scales is the first result of interest in this work. The second is the value of the point scale as a check upon the Binet scale in determining the line between intelligence defect and normal intelligence. At the best, it is a delicate matter to decide between a high grade moron and one who has no intelligence defect, and each examiner must develop his own standards. But where a boy makes 11.4 years Binet and 80 points by point scale, or 14 years, it is certainly safe to consider his Binet record as in some measure accidental, and that he is more likely to make good, than the boy whose Binet is 11.4 years, and whose point scale is 71 points, or 11.6 years. The latter seems much more likely to prove himself a high grade moron, to have an irremediable defect in equipment on the side of intelligence. Of course, even here there is doubt. The point scale is a method contributing to the finer shading of our doubts, and the more precise statement of intelligence defects.

In scoring by the Binet method, we adopted the common procedure of requiring four credits to pass a given year, making the highest so passed a basal year, and allowing one year additional for each five credits. Half credits were often given, as *e. g.*, in two definitions superior to use in the nine-year tests, copying one design in the ten-year tests, or giving solution to one problem in twelve-year tests. Mentality was reckoned to tenths of years. In estimating mental age by the point scale, the spirit of the method was violated to this extent, that we assumed the lines of the curve between each two consecutive years to be straight, and we set down the equivalent to a point rating, in years, reckoned to the nearest tenth of a year.

The classification adopted in the first two hundred cases examined, one hundred at the Girls' Industrial School, and one hundred at the Boys' Industrial School, resulted in the following grouping. The average mentalities are given for each group.

GIRLS.

1. 34 low and medium morons, ranging from 8 to 10.5 years mentally.

   Binet average, 9.4 years. Point scale average = 54.5 points, or 9.1 years.

2. 24 high-grade morons, ranging from 10.5 years up.

   Binet average, 10.9 years. Point Scale average = 70 points, or 11.5 years.

3. 13 doubtful cases.

   Binet average, 11.5 years. Point scale average = 80.1 points, or 14 years.

4. 29 no intelligence defect.
Binet, 22 = 12 yrs. + Point scale average = 84
5 = 12 " points.
2 = 12 " −

Boys.

1. 2 high-grade imbeciles, each making 32 points = 6.6 years, point scale.
2. 40 medium and low-grade morons.
Binet average, 9.4 years. Point scale average = 56.2 points, or 9.5 yrs.
3. 25 high-grade morons.
Binet average, 10.9 years. Point scale average = 81.2 points, or 14.5 years.
4. 20 doubtful cases.
Binet average, 11.4 years. Point scale average = 81.2 points, or 14.5 years.
5. 13 no intelligence defect.
Binet 6 = 12 yrs. + Point scale average of the seven
1 = 12 yrs. 84.6 = 15 years + 2½ points.
Six of the thirteen were less than two years retarded.

The mean or average variations from these averages so far as calculable, both for years, and for points, are embodied in the following table:

	Av. Binet	M.V.	Av. P.S.	M.V.	Equivalent Years	M.V.
GIRLS						
Low and medium morons.	9.4 yrs.	.39 yrs.	54.5	5.5 =	9.1 yrs.	.58 yrs.
High morons...........	10.9 yrs.	.43 yrs.	70	3.5 =	11 yrs.	.32 yrs.
Doubtful intell. def......	11.5 yrs.	.24 yrs.	80.1	3.5 =	14 yrs.	——
No intell. def...........	22 are 12 + yrs. 5 are 12 yrs. 2 are 12 − yrs.		84	3.5 =	15 yrs.+ 2 pts.	
BOYS						
Low and medium morons.	9.4 yrs.	.50 yrs.	56.2	5.3 =	9.5 yrs.	.64 yrs.
High-grade morons......	10.0 yrs.	.42 yrs.	67	3.5 =	11.4 yrs.	.24 yrs.
Doubtful...............	11.4 yrs.	.20 yrs.	81.2	2.9 =	14.5 yrs.	——
No defect.............	6 are 12 yrs.+ 1 is 12 yrs.		84.6	2.7 =	15 yrs.+ 2 pts.	

These figures demonstrate the point scale, at the present state of development, to be quite as accurate a means of measuring the intelligence of high grade defectives as is the

Binet scale. In the groups directly comparable, by mean variations, we find the low and medium morons, both boys and girls, yielding considerably larger mean variations for point scale equivalents in years, and the high grade morons, both sexes, yielding considerably smaller mean variations for point scale equivalents in years. One could, of course, throw these comparisons either way by manipulating the data. The effort in grouping was to give equal weight to each set of figures. That this was done with fair success is indicated by the close approximation to each other, of the average mental ages, obtained by the two methods, in each of these two groups for both sexes.

The mean variations from the point scale averages, for the four groups, for each sex, indicate that we have made the three highest groups of about the same ranges of mentality, whereas the lowest has nearly twice the range. The low and medium grade morons could readily be set apart into two groups each, having a range equivalent to that of the high grades morons. The M. V. of each group would lower accordingly. It is a great satisfaction to feel that we have even a tentative means of assessing the intelligence development, between ten and fifteen years, in the normal child. Small and insignificant as are the *point* differences in these years, the point scale begins to let in the light upon these differentiations, which the Binet scale has left in the dark. When more data are collected, *qualitative* or *analytic* studies of the differences in the point scale findings with children rating within these years, both normal and abnormal, may be expected to yield significant results,—significant for diagnosis of the mentality of the exceptional child.

In view of the disparity of the results obtained by the two methods, in our group of 'doubtful intelligence defect,' two things at once occur to one. (1) It will be interesting to note the future history in the particular cases, to see whether such a *doubtful* child proves himself to be 11.5 years mentally as per Binet or 14 years as per point scale. There must remain a blot upon his intelligence, until he disproves it, for he is likewise retarded by both ratings as compared with the group above. It is rare for members of this group to achieve the 82 points of 15 years, while the higher group averages 84 points.

(2) The other matter in regard to this disparity holds in regard to the 'no defect' group also. In both groups and for both sexes the points attained by point scale rating correspond to higher years on the point scale curves, than are attained by the Binet rating. One cannot avoid the suspicion that the numbers of normal cases so far rated in these higher years by the point scale, may be so few and so exceptional that we have averages which are too low for given years, and that more work with normals will bring these figures higher. The close correspondence between the two ratings for the lower groups suggests this.[1] A thousand normal children, ten to fifteen years old, rated and grouped by point scale, would, in any event, bring needed light in this region. The same results studied analytically would be of great service in furthering insight into mental organization and development in late childhood and adolescence.

[1] Professor Yerkes states that point scale and Binet ratings, resulting from his own examinations of more than fifty normal children, show that the Binet ratings for children below the mental age of eight are too high, and for children above the mental age of eight they are too low. Professor Thorndike comes to a similar view by taking Dr. Goddard's results of the examinations of two thousand school children in Vineland, New Jersey, and working out the averages and median values. For Thorndike's results see the *Psychological Clinic*, December 15, 1914.

A PRELIMINARY STUDY OF THE DEFICIENCIES OF THE METHOD OF FLICKER FOR THE PHOTOMETRY OF LIGHTS OF DIFFERENT COLOR[1]

BY C. E. FERREE AND GERTRUDE RAND

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Synopsis

A satisfactory method of photometry should combine the following features. (1) It should enable one to detect small differences in luminosity and to reproduce results for a given observer with a small mean variation and for a number of observers with a comparatively small mean variation. That is, the method should possess an adequate degree of sensitivity. (2) It should be known either to possess of itself logical sureness of principle or its results must agree in the average with those of some method which can be shown to have this sureness of principle. The method of flicker probably satisfies the first of these requirements better than the equality of brightness method. It does not, however, possess of itself the needed sureness of principle, nor have its results been shown to agree in the average with any method which is accorded sureness of principle. Points are enumerated in the paper appended which raise doubt with regard to the correctness of the photometric balance obtained by the method of flicker. Only one of these is discussed, namely, the influence of the time element in the exposure of the eye to the lights to be compared. With regard to this point, it is shown from experimental data (1) that the sensations aroused by lights differing in color value rise to their maximum brightness at different rates; and (2) that the single exposures used in the method of flicker are much shorter than is required for these sensations to rise to their full value. The eye, therefore, is very much underexposed to its stimulus by the method of flicker. That is, the rate of succession used in the method of flicker is too fast for the single impressions to arouse their maximum effect in sensation and too slow for the successive impressions to add or summate as much as they would need to do to rise to their full value or perhaps even to a higher value than would be given by the individual exposures. Only one other possibility for a correct balance remains,—equality is attained at some value lower than the full value. This can not be assumed, however, without violating well-known laws relating to the factors which influence persistence of vision.

The principal point of discussion, then, is to what degree it should be held that the difference in lag between the sensations aroused by the single exposures used in the method of flicker is obliterated in a succession of exposures. Broadly considered, three positions are possible with regard to the point for the rates of succession that are employed in the method of flicker. (1) The difference is not obliterated at all. In this case the photometric balance should deviate from the true balance in direct pro-

[1] Paper read by C. E. Ferree at the Philadelphia section of the Illuminating Engineering Society, January 16, 1914.

portion to the difference in lag for the single exposures. (2) The difference is in part obliterated, but it is still present to a degree which renders the method untenable for precise work. And (3) the difference is entirely obliterated or so nearly so as to be of no practical consequence to the validity of the method. The second is approximately the position taken in this paper. The following evidence is offered in support of this position. (*a*) At high intensities of light the writers get by the method of flicker a deviation from the true photometric balance, as determined by the equality of brightness method, in a direction which corresponds to the difference in lag between these colors at high intensities as determined both in their own laboratory[1] and by Broca and Sulzer. (*b*) At low intensities they get a difference in lag for the colors which is in the same direction as the deviation obtained by Ives and Luckiesh at low intensities (the reverse Purkinje effect). (*c*) A change in the relative lengths of exposure to the two lights in the method of flicker produces a deviation from the equality of brightness balance which again corresponds in direction to the changes that are produced in the sensations aroused by the single exposures when similar changes are made in the relative lengths of exposure. And (*d*) determinations made at several intensities of light by the method of flicker show a deviation from the equality of brightness balance which is many times the smallest difference in brightness that can be detected by the method. Moreover, in their own results the writers find that these deviations in every case correspond to the difference in lag given by lights of the same order of magnitude of intensity, so far as can be judged from the determinations of lag that have been made up to this time. When, however, determinations have been made on a larger number of observers, individual differences may be found in the amount and distribution of lag just as they have been found in the amount and direction of the deviation of the flicker from the equality of brightness balance. Later in the interests of a fairer comparison the writers hope to make in every case compared the determination of lag and the photometric determinations on the same observer.

The writers have preferred to call the work of which this paper is a brief report a preliminary study for the following reasons. (1) Only one of the points directly pertaining to the method of flicker that should be investigated has been taken account of in the work. And (2) to complete the chain of evidence needed for this point, a more especially directed and perhaps more careful determination should be made than has yet been done of the time required for visual sensations colored and colorless to rise to their maximum of intensity. Such a study with especial reference to the needs of photometry is now in progress in our laboratory, but is as yet unfinished.[2]

[1] See this paper, footnote 1, pp. 125–130.

[2] In the work now in progress in our laboratory, attention will be paid to the following points. In case of colors, care will be taken to use lights of a small range of wave-length. The intensities of the lights used will be specified photometrically and radiometrically. The white light will in addition be specified either spectro-photometrically or spectro-radiometrically. For the sake of the comparisons needed in

A satisfactory method of photometry should combine the following features. (1) It should enable us to detect small differences in luminosity and to reproduce our results for a given observer with a small mean variation and for a number of observers with a comparatively small mean variation. That is, the method should possess an adequate degree of sensitivity. (2) It should be known either to possess of itself logical sureness of principle, or its results must in the average agree with those of some method which can be shown to possess this sureness of principle. Methods having these features have been developed for the photometry of colorless light. The problem of the photometry of colored light, however, has presented great difficulty.

Methods of Photometering Colored Light.

The methods for photometering colored light may be grouped under two headings: the direct methods and the indirect methods. In the former class we have the method of direct comparison or, as it is sometimes called, the equality of brightness method. Of the latter class the method of flicker has received the greatest amount of attention and has been the most favored. It will be the purpose of this paper (1) briefly to compare the relative advantages and disadvantages of the method of flicker and the equality of brightness method with regard to sensitivity; (2) to show that the method of flicker, so far as it has been developed up to the present time, does not seem to possess of itself the sureness of principle needed to meet the requirements of a satisfactory method; and (3) to show that as yet its results have not been found satisfactorily to agree in the average with those of any method which can be shown to have this sureness of principle. In a

the photometric work, all determinations for lights differing in composition will be made at the different intensities employed with stimuli equalized photometrically. Comparative results will be obtained for the same observers for the best of the methods already in use, and three new methods will be introduced. In part, results will be obtained for observers who have also been employed in the work on the method of flicker. The work will be done for different intensities of light, and both under dark and light room conditions. In a survey of the work done up to the present time, one can not help but note that too little care has been taken to observe even some of the most essential of the above conditions.

later paper a new method of photometry will be described which possesses approximately as high a degree of sensitivity for color work as the method of flicker and gives results which agree much more closely in the average with those obtained by the equality of brightness method. The second of the above points will be shown as follows. It will be pointed out that at the rate of speed at which the impressions are given in the method of flicker, the eye is very much underexposed to its stimulus. It can reasonably be assumed that this underexposure causes a reduction of the intensity of the sensation, and should lead, therefore, to a false estimation of the brightness of the colors. In fact, at the rate of rotation of the exposure apparatus required for lights of the order of intensity employed in practical work, this reduction produces for the observers we have used an effect similar to the Purkinje phenomenon.[1] At least a deviation from the equality of brightness values is found in our results for such intensities which accords well with the Purkinje phenomenon. That is, reds and yellows are underestimated in brightness, and blues and greens are overestimated. And (*b*) it will be shown that flicker itself, the phenomenon on which the equalization at the photometric screen is based, is subject to variations depending upon a number of factors the effect of which has not in all cases been adequately studied and in some cases not even recognized. An investigation of one of these alone, the effect of varying

[1] We do not mean to draw too close an analogy here between the effect on the brightness of sensation produced by keeping the intensity of light constant and reducing the time of exposure of the eye to the light, and the effect produced by keeping the time of exposure of the eye constant and reducing the intensity of the light employed (the Purkinje phenomenon). In attempting to interpret the effect produced by the short exposures used in the method of flicker, our data should be taken primarily from the results showing the relative rise of sensation to its maximum for white light and lights of the different colors. (See discussion of the development time of sensation, pp. 118–130). It is quite possible and in fact quite probable from Broca and Sulzer's results, for example, that for a part of the upward course blue and green rise faster than red, and conversely for a part of the course red rises faster than blue and green. (Yellow was not used by Broca and Sulzer.) The results of Broca and Sulzer are cited on this point, not by any means because their method of making the determination is the freest from criticism of any that have yet been used, but because they alone have attempted to plot the comparative curves for the different colors and white light at different points from the threshold to the maximum.

the ratio of the time of exposure of the eye to the lights to be compared, is enough to lead one seriously to question whether the method of flicker can be safely used in the work of heterochromatic photometry, at least not without calibration, and perhaps not without an amount of calibration which is in itself prohibitive of the use of the method in practical work. The third point will be covered in the following way. (1) It will be pointed out that the only method that has thus far been used as a standard with which to compare the method of flicker has been the equality of brightness method. The selection of this method as a standard has been recommended among others by Whitman, Wilde, and Schenck, and a comparison of the results of the two methods, more or less complete, has been made by a number of experimenters. And (2) it will be shown both from our own work and from a very great preponderance of the work done by others who have made the comparison, that the results by the method of flicker do not agree in the average with those obtained by the equality of brightness method; and, therefore, that justification for the adoption of the method of flicker can not yet, at least, be fairly claimed through its agreement in result with the equality of brightness method.

The Equality of Brightness Method.—With regard to sensitivity in the photometry of lights of different color, the equality of brightness method has the following disadvantages. (1) Small differences in luminosity can not be detected because the actual difference present is masked by the difference in color quality. (2) Results for a given observer can not be reproduced within a small limit of variation, because the ability to do this in turn presupposes the ability to detect small differences which, as has just been stated, can not be done. (3) Results can not be reproduced from observer to observer within a small limit of variation because (*a*) the sensitivity to color varies more among observers than does, for example, the sensitivity to brightness, hence there is a variable amount of the disturbing factor of color present for different observers; and (*b*) because the standard or pattern for the judgment of equality differs more from individual to individual when the factor of color is present than when it is not. That is, in any photometric judgment the observer must decide for himself what he will call equality and make all his judgments conform to this pattern or standard. When color is present to interfere with the judgment of equality, the selection of this standard varies more for different observers than it does when no color is present. With regard to all the points on which sensitivity depends, therefore, the equality of brightness method may be said to possess a low degree of sensitivity.

The Method of Flicker.—The method of flicker possesses greater sensitivity than the equality of brightness method. That is, smaller differences in the luminosity of the photometric surfaces can be detected, and the judgment of equality is surer and more reproducible.[1] This is because the disturbing factor of color difference in the impressions to be compared is eliminated from the judgment. That is, instead of being given simultaneously, the stimuli are given in succession and at such a rate that all color differences between them disappear, and the brightness impressions are permitted to develop in sensation unobscured by differences in color quality.

The use of the phenomenon of flicker to detect a difference in brightness between two illuminated surfaces can best be understood possibly by considering the phenomena that take place when successive impressions of colored and colorless light are made upon the retina at different rates of speed. When the retina is exposed successively to colorless lights differing in brightness, the following phenomena take place. When the rate of succession is low, the impressions remain

[1] This higher degree of reproducibility can be claimed perhaps only for the judgments given by a single observer. It does not seem to obtain to any considerable extent, so far as results are available for comparison, when results are compared from observer to observer. For example, in a group of eighteen observers Ives gets differences as great as 159 per cent. for .481 μ, 114 per cent. for .498 μ, 26 per cent. for .518 μ. 18 per cent. for .537 μ; 13 per cent. for .556 μ; 10 per cent. for .576 μ; 28 per cent. for .595 μ, 65 per cent. for .615 μ; 86 per cent. for .635 μ; and 122 per cent. for .655 μ. The percentage of average variation from the mean for these observers is 17 per cent. for .481 μ; 13.4 per cent. for .498 μ; 6 per cent. for .518 μ; 3 per cent. for .537 μ; 2.75 per cent. for .556 μ; 2.2 per cent. for .576 μ; 5.4 per cent. for .595 μ; 9.5 per cent. for .615 μ; 13.2 per cent. for .635 μ; and 19.3 per cent. for .655 μ. (*Philos. Mag.*, 1912, *24*, Ser. 6, pp. 853–863.)

more or less separate and distinct. At rates higher than this, we have in order Fechner's colors,[1] flicker, and the fusion of the two impressions into a uniform gray. When the eye is exposed successively to colored and colorless light, the following phenomena take place. At low rates, we have again the more or less separate successions of the two impressions. At rates slightly higher than this, we have first a phenomenon that may be called by analogy color flicker, and then an intermingling of color and brightness flicker. At still higher rates we have color fusion, brightness flicker, and complete color and brightness fusion. Thus, both in case of colored and colorless light, brightness flicker seems to be a phenomenon due solely to the succession at certain rates of speed of impressions differing in luminosity or brightness. Moreover, the phenomenon is very sensitive to changes in the luminosity of the successive impressions. That is, a very slight change in one of the impressions will produce flicker when there is no flicker, or will cause a noticeable change in its amount when there is flicker. Flicker thus becomes a very sensitive means of detecting brightness difference. This sensitivity, however, is not so great in case of colored as it is in case of colorless light. It would in fact in all probability be very low were it not for the fortunate fact that color fusion takes place at a very much lower rate of succession than brightness fusion.

Concerning the ease and sureness of making the judgment, then, the case with regard to the method of flicker may be summed up as follows: By giving the impressions to be compared to the retina successively at a certain rate of speed, the disturbing element of color difference, which so interferes with the detection of brightness difference when the impressions are given simultaneously, is eliminated, and the phenomenon of brightness flicker stands out clearly in a field uniform as to color quality. That is, by using a method of successive impressions we have succeeded in eliminating the

[1] Fechner's colors are best observed when the successions are made by rotating discs made up of white and black sectors, or by discs specially constructed for the purpose. This phenomenon occurs at a rate of speed near the upper limit required to give separate impressions, and consists of impressions of color mingled with the more or less separate impressions given by the white and black sectors.

feature that renders the comparison of the brightness of the simultaneous impressions so difficult to make, namely, the difference in color quality between the impressions to be compared. The judgment, then, is easy, and the principle on which the equalization is based seems to be clear. The method has come to have many supporters, but other things besides the sureness of judgment must be taken into account. This brings us to a consideration of our second point, namely, the method of flicker when applied to the photometry of lights of different color does not seem to possess the sureness of principle needed to meet the requirements of a satisfactory method. We have two reasons for making this assertion. In the first place, as we have already stated, at the rate of speed at which the impressions are given in the method of flicker, the eye is very much underexposed to its stimulus. And in the second place, flicker, the phenomenon on which the equalization is based at the photometric surface, is subject to many variations depending upon a number of factors the bearing of which on the application of the phenomenon to photometry, has not in all cases been adequately studied, and in some cases not even recognized. A few of these may be suggested in passing. (1) The intensity of illumination and the influence it exerts on the speed of alternation that has to be used in order to give the method maximum sensitivity. (2) The different rates of speed required for the fusion of the different colors, and the varying lower limit this difference puts upon the rates of speed that can be used. (3) The effect of the saturation of the colors used on the fusion rate. (4) The effect of field size. (5) The effect of the ratio of the time of exposure of the eye to the lights to be compared; etc. A better knowledge than we now have of the effect of these factors is, the writers believe, of fundamental importance in the employment of the phenomenon of flicker in the photometry of lights of different colors. At a later date they hope to report the results of a systematic study of these factors. In the present paper, the effect of only one of them will be considered, namely, the ratio of the time of exposure of the eye to the lights to be compared. An investigation of this point alone is enough to lead one seriously to question whether the method of flicker can safely be used in the work of heterochromatic photometry, at least not without calibration, and perhaps not without an amount of calibration which is in itself prohibitive of the use of the method in practical work.

The Action of Light on the Eye under the Conditions Imposed by the Method of Flicker.

Both of the above points will probably be more easily understood if a brief consideration is given to the way the eye responds to colored and colorless lights when the impressions are given to it in the manner they are given in the method of flicker. The eye is not an ideal sense organ, that is, it does not respond at once with its full intensity of sensation at the beginning of stimulation, nor does the sensation cease with the cessation of stimulation. It takes, for example, an interval of time for the sensation proper to a given stimulus to rise to its maximum; and also an interval to die away after the stimulation has ceased, depending for its length upon several factors.[1]

The interval of time required for a sensation to rise to its maximum will be called in this paper the development time of sensation. Plateau in 1834[2] first expressed the belief that

[1] There are two phases to this after-effect, positive and negative. The positive alone concerns us here. In this phase, which is often called the persistence of vision, the original sensation tends to persist in its original color and brightness. More accurately described, however, it rapidly loses in color and rapidly darkens. In the negative phase there is a brightness reversal, that is, what is light in the original sensation becomes dark, and the color changes to the complementary color. The negative phase is much longer than the positive. The length of the positive depends upon many factors: the intensity of the stimulus, the time of exposure of the eye to the stimulus, the state of adaptation of the eye, the general illumination of the field of vision, the brightness of the local preëxposure and post-exposure, etc. Unless the eye is put under very especial conditions of stimulation, the duration of the positive phase is very short indeed, in fact, momentary. For a further discussion of this point with reference to the method of flicker, see appendix.

[2] As early as the time of Bacon it was noted that there is a period of inertia in vision. ("At in visu (cujus actio est pernicissima) liquet etiam requiri ad eum actuandum momenta certa temporis: idque probatur ex iis, quae propter motus velocitatem non cernunter; ut ex latione pilae ex sclopeto. Velocior enim est praetervolatio pilae, quam impressio speciei ejus quae deferri poterat ad visum."—'Novum Organum,' lib. II., Aph. XLVI.). Later Beudant ('Essai d'un Cours Elementaire

color sensation does not come at once to its maximum. He, and later Fick[1] in 1863, showed that when a sector of white paper passes very rapidly only once before the eye it looks to be a dark gray. With the experiments of Exner in 1868, the work of determining the development time of visual sensation was definitely begun. Different methods of making the determination have been used by different investigators, and different results have been obtained. There is, however, among the different results a certain amount of agreement. At least the order of magnitude of the development time can be fixed within certain limits. The chief points of interest in these investigations have been (1) to compare the development time of the different sensations of color with each other and with that of colorless sensation; and (2) to determine whether the intensity of the stimulus has any effect upon the development time. All who have made the comparison have found that each of the color sensations has a development time different from the colorless sensations; all with the exception of Dürr and Berliner, that each of the colors has a different development time; and all with the exception of Dürr, that an increase of intensity shortens to some extent the development time of all sensations.

A table (Table I.) has been prepared showing the development time obtained by each of these men for the different

et General des Sciences Physique: Partie Physique,' p. 489, 3me edition) also stated that an object which moves with extreme rapidity before the eye is not perceived because impressions are not made on the eye instantly. Plateau ('Nouveaux Memoirs de l'Academie Royale des Sciences et Belles Lettres de Bruxelles,' 1834, *8*, p. 53) made the observation that when a bit of white paper passes very rapidly before the eye, it appears not white but gray. He was the first to express the belief that color sensation also does not come at once to its maximum of intensity. Swan (*Trans. Roy. Soc. Edinb.*, 1849, *16*, pp. 581–603) observed that the "light of the sky seen immediately over a ball in its descent through the air, seemed less bright than at those parts of the retina where the action of light had not been interrupted by the passage of the dark body"; and conducted some experiments to determine the intensity of light sensation with short exposures. Exposing the eye to lights of different intensities for intervals ranging from 1/100 to 1/16 of a second, colorless 'lights of different intensity produce like portions of their total effect on the eye in equal times.' While he does not directly determine the interval required for the light sensation to come to its maximum, he estimates it from the results of his experiments with short exposures to be about 1/10 of a second.

[1] Fick, A. *Archiv für Anatomie und Physiologie*, 1863, p. 739.

Table I

Showing a Comparison of the Development Time of Visual Sensation with the Average Time of Exposure of the Eye to Its Stimulus Used in our Experiments with the Method of Flicker.

		Sensation	Development Time	Average Time of Exposure of Color by Method of Flicker
Exner[1]	1868	White	5 intensities: .118–2.87 sec.	.0178 sec., when the value of the colored sector was 180°.
Kunkel[2]	1874	Different colors	.057–.133	
Charpentier[3]	1887	White,	5 intensities .014–.049	
Lough[4]	1896	White,	5 intensities .090–.148	
Dürr[5]	1902	White,	2 intensities: .266	
		Different colors	.541	
Martius[6]	1902	White	6 intensities: .013–.093	.0213 sec., when the value of the colored sector ranged from 45°–315°.
		Different colors	.020–.090	
Broca and Sulzer[7]	1903	White	8 intensities: .031–.125	
		Different colors	.07 –.125	
McDougall.[8]	1904	White	12 intensities: .049–.2	
		Different colors	.100–.108	
Büchner[9]	1906	White	3 intensities: .033–.230	
Berliner[10]	1907	Different colors	.130	

[1] Exner, S., 'Ueber die zu einer Gesichtswahrnehmung nöthige Zeit.,' *Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften*, Math.-Phys. Classe, 1868, *58*, pp. 601–632.

[2] Kunkel, A., 'Ueber die Abhängigkeit der Farbenempfindung von der Zeit,' *Pflüger's Archiv*, 1874, *9*, p. 197.

[3] Charpentier, 'Sur la periode d'addition des impressions illuminismes,' *Comptes Rendus Societé de Biologie*, 1887, *4*, pp. 192–194.

[4] Lough, 'The Relations of Intensity to Duration of Stimulation in Our Sensations of Light,' Psychological Review, 1896, *3*, pp. 484–492.

[5] Dürr, E., 'Ueber das Ansteigen der Netzhauterregung,' *Philosophische Studien*, 1901–1903, *18*, pp. 215–273.

[6] Martius, G., 'Ueber die Dauer der Lichtempfindungen,' *Beiträge zur Psychologie und Philosophie*, Leipzig, 1902, 1, Heft 3.

[7] Broca, A., and Sulzer, D., *Comptes Rendus der Séances de l'Académie des Sciences*, 1902, *134*, pp. 831–834; 1903, *137*, pp. 944–946; 977–979; and 1046–1049.

[8] McDougall, W., 'The Variation of the Intensity of Visual Sensation with the Duration of the Stimulus,' *British Journal of Psychology*, 1904–1905, 1, pp. 151–189.

[9] Büchner, M., 'Ueber das Ansteigen der Helligkeitserregung,' *Psychologische Studien*, 1906–1907, *2*, pp. 1–29.

colored and colorless sensations; and, for comparative purposes, the average exposure time that was used in our experiments for all the colors in the determination of their brightness by the method of flicker. In choosing this time of exposure for the method of flicker, in order to secure for the method the greatest possible sensitivity, we used the slowest rate of succession of colored and colorless sectors that could be employed.

An inspection of this table will show that while the results for the development time of sensation differ quite a little among themselves, they agree in one very important particular, namely, they are all much greater than are the intervals that are used in the longest exposures that are permissible by the method of flicker. That is, by the method of flicker, the eye is very much underexposed to its stimulus. The effect of this under exposure is obviously to cause a reduction in the intensity of sensation. That is, the rate of succession of impressions used in the method of flicker is too fast for the single impressions to arouse their maximum effect in sensation and too slow for the successive impressions to add or summate as much as they would need to do to cause the intensity of the sensation aroused by each light to rise to its full value, or perhaps even to rise to a higher value than would be given by the individual exposures. In fact as will be shown in an appendix to this paper the sensation can not be expected to rise to its full value through summation if the Talbot-Plateau law be true, however rapid is the rate of succession of the individual impressions (see appendix). Even when a rate is reached at which complete fusion takes place, both for the color and brightness components in sensation, there is according to the Talbot-

[10] Berliner, 'Der Ansteig der reinen Farbenerregung im Sehorgan,' *Psychologische Studien*, 1907, *3*, pp. 91–155.

W. Swan in an article entitled 'On the Gradual Production of Luminous Impressions on the Eye; Part II., being a description of an instrument for producing isolated luminous impressions on the eye of extremely short duration, and for measuring their intensity,' *Trans. Roy. Soc. Edinb.*, 1861, *2*, pp. 33–40, has described a very ingenious but complicated apparatus for getting short periods of stimulation of the retina, but apparently neither he nor any one else has ever used the apparatus described.

Plateau law, a reduction in the intensity of each sensation which is the same as would be gotten were the intensity of each light to be reduced in proportion to the time of exposure of that light to the total time of exposure of both lights, and no further increase in the rate of succession produces any change in the effect.[1] The possibility then of the sensations which, as is shown by the work on development times, are unequal for the single exposures used in the method of flicker, reaching equality by rising to their full value seems to be ruled out. In terms of the Talbot-Plateau law they could not reach their full intensity through an effect of summation, however fast the rate of succession be made,

[1] Ewald ("Versuche zur Analyse der Licht- und Farbenreaktionen eines Wirbellosen" (*Daphnia pulex*), *Ztschr. f. Psychol. u. Physiol. d. Sinnes.*, 1914, 48, pp. 285–325; and "The Applicability of the Photochemical Energy Law to Light Reactions in Animals," *Science*, 1913, 38, pp. 236–238) has made an interesting contribution with regard to the effect of the intermittent action of light on the eye which it may not be out of place to mention here. The facetted eye of the daphnia was used in his experiments. When exposed to light this eye responds by turning towards the light, and when lights of different intensities are used it turns towards the stronger light. After having determined the sensitivity of this response to difference in intensity of light by exposing the eye to a number of lights of different intensities acting continuously on the eye, he undertook to make a comparison of the effect of light acting continuously and intermittently. The intermittence was gotten by rotating a sectored disc in front of one of the lights. The lights were so chosen that the same amount of energy acted upon the eye in a given unit of time from both the continuous and intermittent sources. That is if a ratio of total open to closed sector of the value 1/10 was used, the light in front of which these sectors were rotated was made ten times as intense as the light acting continuously. The sectored disc was then rotated at different speeds. When a speed of 30 revolutions per second was attained the eye remained stationary. That is at this speed of rotation the two lights produced equal effects on the eye,—which is, of course, no more than a demonstration of the Talbot-Plateau law for the primitive eye. But when the speed was made slower than this, the eye invariably turned towards the light which was acting continuously. That is when the rate at which the impressions were given to the eye was made slower the result was to weaken the effect on the eye even though the same amount of light was received by the eye in a unit of time in both cases. Ewald's results show then that, so far as the primitive eye is concerned, when light impressions are given to the eye at certain high rates of succession (analogous to the fusion rates for the human eye) there is a reduction in the amount of response aroused which is the same as would be produced were the intensity of the light reduced by an amount proportional to the ratio of the time of exposure to the light to the total time of the observation; and when they are given to the eye at rates slower than these the effect on the eye is the same as if the light acting on it had been still further reduced in intensity.

let alone attain it at the rates which are employed in the method of flicker.[1]

[1] There seems to be only one other possibility that the method of flicker should give the true photometric balance between lights of different color values, namely, that the sensations aroused should reach equality at some value lower than the full value. That this is extremely improbable is shown by the following consideration. The weaker sensation or the sensation which has the slower rate of development for a single exposure would have to rise in value because of summation effect resulting from the succession of exposures until it became equal to the stronger sensation. To produce any effect of summation each individual impression would have to last over in sensation until the next impression of its kind is received which, since the impressions alternate, would be the next impression but one. And to produce the particular effect required here, not only would each excitation have to last over until the next one is aroused, but the weaker one would have to last over more strongly than the stronger one, else the effect of the summation would not be to produce the gain of the weaker on the stronger which is required to bring the two to the true photometric balance. That is, the advocate of this point of view would say that even though for the single exposure one color is weighted more than the other, the effect of this is obliterated in a succession of impressions and the two rise to equal value, because the weaker sensation would carry over more strongly hence would gain more relatively in the process of summating than would the stronger sensation. This is not at all in accord with the experimental evidence available at this time on the relation of the positive after-effect or persistence of sensation to the original sensation. Goldschmidt ("Quantitive Untersuchungen über positive Nachbilder," *Psych. Studien*, 1910, *6*, pp. 159–252) and others show, for example, that the stronger the original sensation, the more strongly does it tend to carry over after the light is cut off. Goldschmidt also concludes from his experiments, which is a very important point for this discussion, that the tendency of the sensation to carry over is, so far as its brightness is concerned, independent of the color. That is, suppose that a photometric balance was obtained for green and red lights of comparatively high intensities by the method of flicker. Then according to Broca and Sulzer's curves, also the results obtained in this laboratory, green would attain to a higher brightness value for the single exposure than would be attained by red. Hence if green is not to be overestimated by the method of flicker, red must carry over more strongly as the impressions succeed each other than does green, and thus make up by a summation effect the deficiency shown in the single exposure. But according to Goldschmidt's results this greater tendency to carry over could not be assumed for red, either because of its color value or because of its weaker intensity, and there is no other aspect of the sensation which could have any bearing on the question in hand. Moreover, this hypothesis is rendered still more untenable by the experimental fact that the situation at low intensities is reversed. That is, at low intensities red, as shown by the curves for difference in lag (see Fig. 2, p. 127), attains to a higher value than green for the single exposure. Then if red is not to be overestimated by the method of flicker and in direct proportion to the values given to the two sensations in the single exposure, green must be carried over more strongly in the succession of impressions than is red. The explanation of both of these points would require not only that the color value of the stimulus exerts an influence on the carrying over of the brightness aspect of the sensation, but that this influence reverses in passing from high to low intensities. For a discussion of how highly improbable it is for the rates of succession used in the method of flicker that one impression could last over until the next impression but one is received in any amount that could be of considerable consequence to the method, see appendix.

It seems fair to conclude, then, that instead of getting by the method of flicker the sensations that should be aroused by the lights with which we are working, we get sensations of lower intensity. But it may be asked what if there is a reduction of the intensity of the impressions received? Equalization is all we are working for and the intensity of both impressions is reduced. Is it not possible, therefore, to find a ratio of time of exposure to each light such that the amount of reduction in the intensity of both impressions will be equal? This would be comparatively simple if the rate of development for all the colored were the same as for all the colorless sensations. The intervals of exposure could be made equal as is ordinarily done when sectored discs are used and as apparently must be done when the exposures are given by means of a rotating prism. But the development time for color sensation is not the same as for colorless sensation, and, moreover, the consensus of evidence is that the rate of development is not the same for any two of the color sensations. Thus from the standpoint of the unequal reduction in intensities produced by the method of flicker, the task of selecting a proper ratio of exposure time of colored to colorless light, in case of the different colors, is one that requires a great deal of accurate knowledge if the method is to have the sureness of principle needed,—more, the writers think, than we now possess.[1]

[1] One scarcely needs point out in this regard that there is apparently no point in the intensity scale for which a given reduction in intensity for colored light gives the same change in luminosity in sensation that it does for white light. Beginning with the spectrum of fully saturated colors and comparing the effect of reduction by equal amounts of colored and white lights equal in photometric value, the blues and greens are found not to decrease in luminosity so fast as the white light, and the reds and yellows are found to decrease faster. Or as the phenomenon is ordinarily expressed, there is a relative lightening of the blues and greens and a relative darkening of the reds and yellows. Nor is the phenomenon of unequal change confined to the lower intensities. It is more striking for these intensities, but it occurs also for the higher intensities. This conclusion is drawn from the statement made by several writers that beginning with the spectrum of fully saturated colors and increasing the intensity of light, all the colors are found to tend towards white, and in so doing to change their luminosities at different rates. (For example, see Helmholtz, H., 'Ueber Hrn. D. Brewster's neue Analyse des Sonnenlichts,' *Pogg. Ann.*, 1852, *86*, p. 520; also *Handbuch der physiologischen Optik*, zw. Aufl., 1896, pp. 465–466; Chodln, A., 'Ueber die Abhängigkeit der Farrbenempfindungen von der Lichtstärke,' Sammlung physiologischer Abhandlungen von Preyer, 1877, *1*, p. 33 ff.; Brücke, E., 'Ueber einige

We have discussed here, moreover, the effect of underexposure at only one rate of rotation of the exposure apparatus. The situation becomes still more complicated when this rate is changed. If it were changed, as it must be to preserve the sensitivity of the method in passing from high to low illumination, the whole scale of magnitude of the underexposure would change, and a shift in the relative evaluation of the luminosities of the different colors might very well be expected from the shape of the sensation curves as they rise to their maximum. In fact this shift is found in the work of previous investigators[1] who have made the comparison at

Empfindungen im Gebiete der Sehnerven,' Sitzungsber. der Wiener Akademie, Math.-Natur. Klasse, 1878, 77, Abth. 3, p. 63.) As we have already stated, however (p. 113), we do not mean to draw too close an analogy here between the effect on the brightness of sensation produced by keeping the intensity of light constant and reducing the time of exposure of the eye to the light, and the effect of keeping the time of exposure of the eye to the light constant and reducing the intensity. The degree to which the analogy holds can scarcely be considered as fixed until more work is done showing the way in which the luminosity curves for the different colors rise to their maximum as the time of exposure of the eye to the different colored lights is increased.

[1] See, for example, the phenomenon called by Ives the "reverse Purkinje" effect (*Philos. Mag.*, 1912, *24*, Ser. 6, pp. 170–173); later demonstrated and discussed by Luckiesch (*Electrical World*, March 22, 1913, p. 620). These writers have found that the red end of the spectrum shows a relatively higher luminosity value as compared with the green end by the method of flicker at low than at high illuminations. From the shape of Broca and Sulzer's curves for the rise of visual sensation, to its maximum, for example, this result might very well be due to the difference in the relative luminosity value of the colors caused by the difference in the length of exposure given to the eye in the method of flicker at the faster rates of speed required for the higher illuminations and at the slower rates for lower illuminations. That is, the longer exposures given by the slower speeds of rotation allow the colors to attain a higher intensity. For example, the speeds used by Ives for what he calls 10 Illumination Units range, for the different colors for five observers, from 7 to 10 cycles per second, and for 250 Illumination Units from 10 to 22 cycles per second.

Broca and Sulzer's curves (Fig. 1) are appended here for one order of intensity of stimulus (*Comptes Rendus*, 1903, *137*, p. 978. For other intensities see p. 945). The curves given were selected because they alone show a comparison between the results for colored and white light. It will be seen from these curves that for exposures less than .07 sec. (approximate value), blue and green rise to a higher value than red; for exposures ranging from .07 to .11 sec., blue rises to a higher value than red, and red higher than green; and for exposures ranging from .11 sec. to about .25 sec., red rises to a higher value than blue or green.

There is also a very strong probability that the relative lag in sensation for the different wave-lengths is not the same for lights of low intensity as for lights of higher intensities. In fact the results that have been obtained so far in this laboratory in determining the development time of the sensations aroused by red, yellow, green, and intensities low enough to necessitate a decided reduction in speed of rotation. And that the shift is different from the normal effect on the brightness of the colors produced by a decrease of illumination, is shown by the fact that according to the results of these investigators it is in a different direction from that given by the equality of brightness method. Moreover, in the later paper it will be shown that a change in this evaluation amounting to several times the smallest difference in luminosity that can be detected by the method, is also produced by working the reverse variation, that is, by keeping the rate of rotation constant and changing the ratio of value of colored to colorless sector. It is difficult, therefore, to avoid the conclusion that the type of exposure used in the method of flicker is an important factor in the cause of its disagreement with the results obtained by other methods.

blue lights of spectrum purity show that at low intensities red and yellow rise more rapidly in photometric value than green and blue. This result is quite marked in those parts of the curves representing an exposure time of the same order of magnitude as is used in the method of flicker. In order to show this point we have appended here

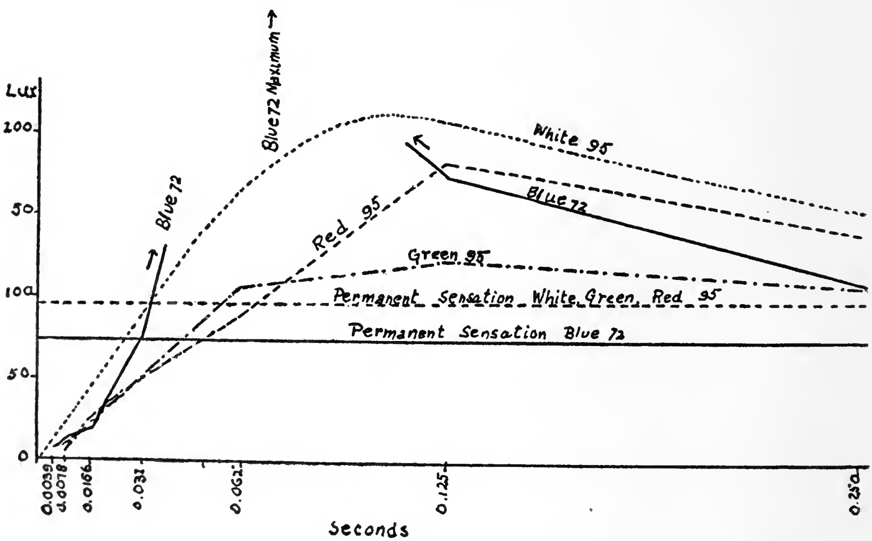

FIG. 1

three curves representing the relative rates of development of red, yellow, green, and blue at intensities which we will designate for the present as low and intermediate; and of red, yellow, and green for a higher intensity. These determinations were made by

Reëxamining the case, then, with regard to the underexposure of the eye by the method of flicker, we find that the short exposure times necessary to the method cause a re-

M. A. Bills of this laboratory. Later, results will be given for red, green, blue, and yellow at a number of intensities, and specifications will be made of the intensities employed in both photometric and radiometric units. The colored lights used in determining the curves given below were obtained from a spectrum of good definition and were in each case equal in photometric value, as they should be if results are to be used in interpreting the action of light on the eye under the conditions imposed by the

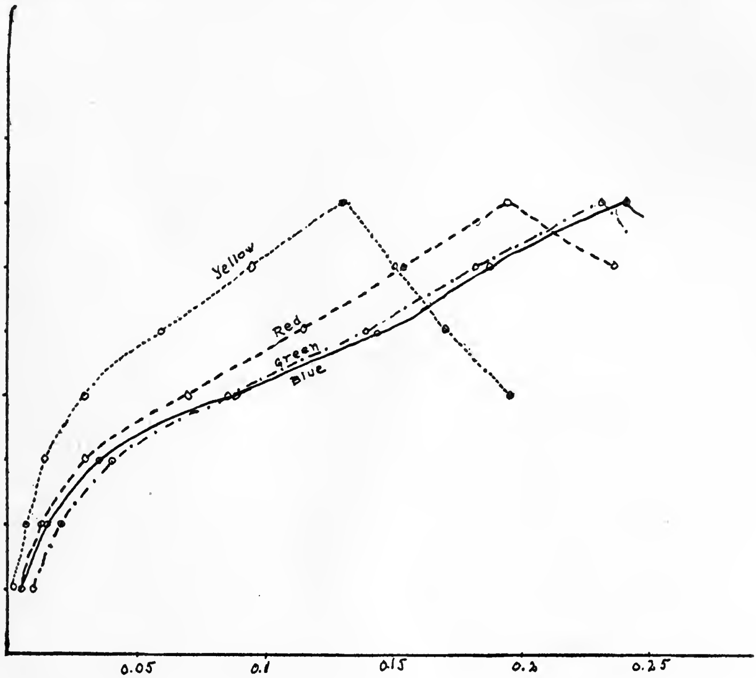

FIG. 2

method of flicker when the photometric balance is attained. In constructing these curves time of exposure is plotted along the abscissa and brightness of color along the ordinate. The curves in Fig. 2 are for the low intensity; in Fig. 3 for the intermediate intensity; and in Fig. 4 for the higher intensity.

It is very probable that there is considerable individual difference in the amount and distribution of lag. A rigid test of the correspondence of difference in lag to the direction of deviation of results gotten by the method of flicker from those obtained by the equality of brightness method would require that the photometric determinations and the determination of lag should be made for a given quality and intensity of light by the same observer.

If it should be found that there is an individual difference in the amount and distribution of lag, the result would supplement very nicely the explanation why a much higher degree of reproducibility is gotten by the method of flicker than by the method duction in the action of the standard and comparison lights on the eye. If this reduction were equal in amount, quite enough difficulty would be encountered. But it is not

of equality of brightness only when the results of a single observer are considered (see footnote, p. 115). That is since the factor of color difference which so disturbs the judgment in the equality of brightness method is eliminated in the method of flicker, we should get correspondingly a higher degree of reproducibility for a single observer and for different observers, were there not some factor present in the method of flicker and not in the equality of brightness method, which varies from individual to individual. So supplemented the explanation would be as follows. In the method of flicker the

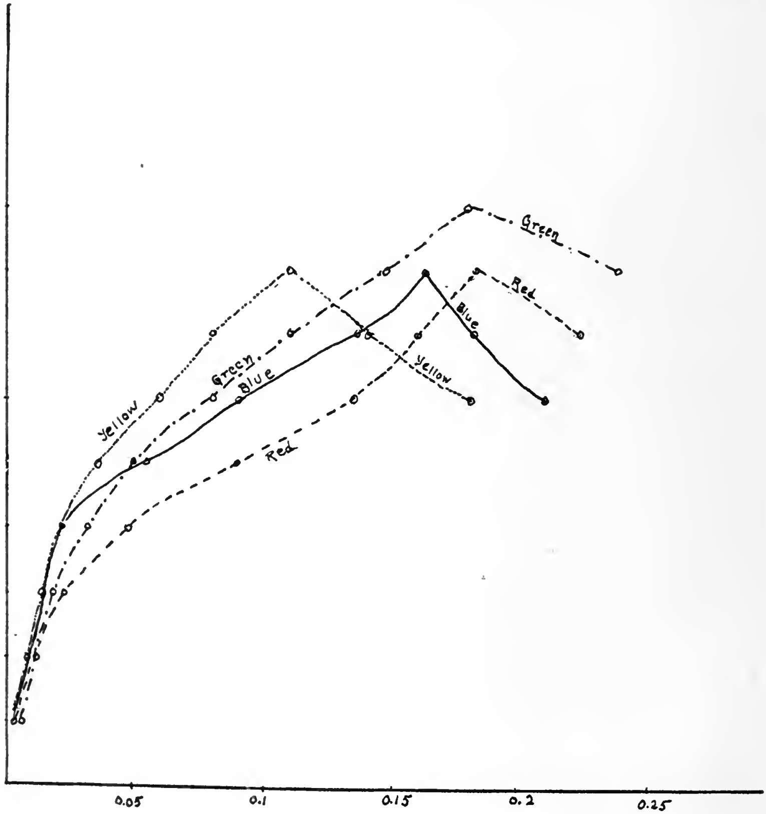

Fig. 3

judgment for a single observer shows a higher degree of reproducibility than in the equality of brightness method because of the elimination of the disturbing factor o color difference; but a false balance is established by the method, the deviation from the true balance depending in direction and amount for different observers upon the difference in the amount and distribution of lag in the rise of the sensations towards the maximum. The difference in the amount and direction of this deviation from the true balance from observer to observer is the cause of the relatively low degree of reproducibility of results when the work of different observers is compared.

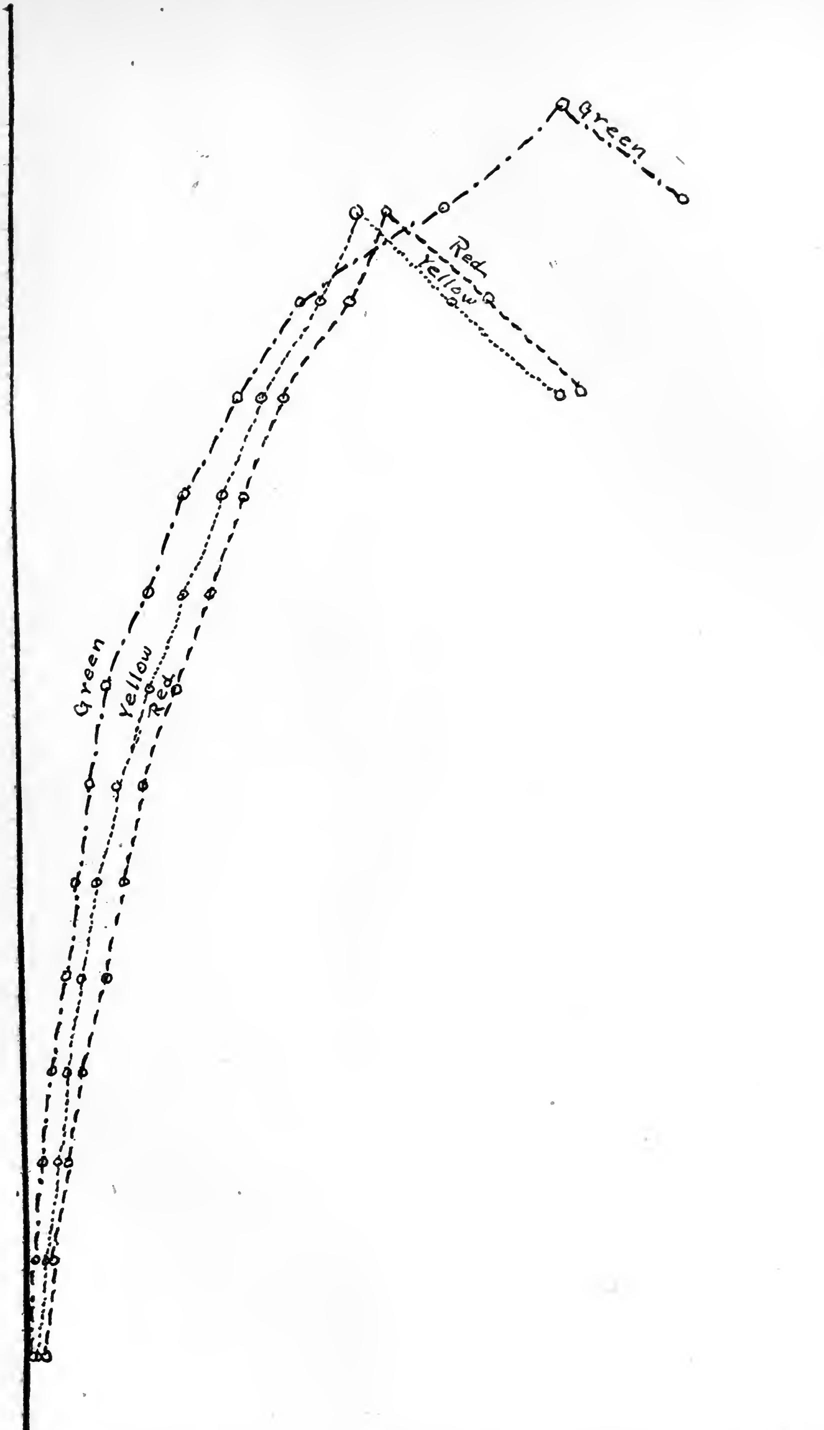

FIG. 4

equal in amount, and we have no adequate information as to the amount of the inequality. Until we have more information with regard to the amount of the reduction and the effect it produces, any successful attempt to regulate the relative duration of the exposure to colored and colorless light, even for a single color at a single intensity, can scarcely be more than the result of chance. Surely to expect to accomplish this for all intensities of all colors by a single ratio of exposure, more especially by means of a 1:1 ratio, as has been the practice in the past, is, it would seem to the writers, to ignore the sensation principles which underlie the method. Obviously this ratio requires calibration, and to give the method the sureness of principle required, it would seem that the calibration might have to be made for each color at the particular intensity at which the work is to be done. This calibration might possibly be accomplished by means of an accurate knowledge, if that knowledge could be secured in sufficient detail, of the temporal course of visual sensation as it rises to its maximum for the given intensity of light used; or it might be done by comparing the results obtained by the method of flicker with the results obtained by some other method adopted as a standard. So far the tendency seems to have been to look for this standard within the subject of photometry itself. As has already been stated, several writers have signified a desire to make the equality of brightness method a standard, and comparisons have been made of the results obtained by the method of flicker and the equality of brightness method.

A consideration of the results of these comparisons together with the data collected by one of the writers in ten years use of the flicker and equality of brightness methods in making the brightness matches needed for the work in color sensitivity, has influenced us to open the question anew in the interests of the work on color sensitivity. In the work with these two methods all done at comparatively high illuminations and with a large number of observers, agreement has been rare. For this reason the chief incentive to make the present study has not been to establish disagreement, but to investigate further the causes of disagreement. The results of this study seem to indicate that the type of exposure of the eye to its stimulus by the method of flicker is an important cause of disagreement.

So much for the theoretical considerations relating to the method of flicker. To test the accuracy of some of the more important points that have come up, a plan of experimentation has been formulated and in part carried out. So far as the results of that experimentation will be reported upon in this paper, the following things will be shown. (1) By comparing the time required for the sensations aroused by colored and colorless light to reach their maximum of intensity, we have already shown that as a general case the eye is very much underexposed to its stimulus by the method of flicker, and we have concluded that the effect of this underexposure on the brightness component of sensation will be unequal in amount for colored and colorless light, and should lead, therefore, to a false estimation of the brightness of the colors. That this conclusion is justified so far as our work is concerned, will be demonstrated in part by comparing the results of the method of flicker with those obtained by the equality of brightness method, in which case the eye is fully exposed to its stimulus, and showing that for the method of flicker there is for our observers for the intensities of light used and for the rate of rotation of the photometer head required for these intensities, a characteristic underestimation of the brightness of red and yellow and overestimation of the brightness of blue and green. That this characteristic deviation is due to the type of exposure used in the method of flicker and not to some other factor will be further shown in the consideration of our second point. (2) We have said that the ratio of the time of exposure should be considered as a factor influencing the results obtained by the method of flicker. In order to confirm this judgment of the case, we have varied this ratio, keeping the other conditions constant, and have found that a corresponding variation is produced in the results. That is, by changing the value of the colored and colorless sectors in the rotating disc we have used to regulate the time of exposure in the method of flicker, corresponding variations are obtained in the characteristic underestimations of red and yellow and the overestimations of blue and green. These variations, it will be shown, moreover, are very much greater than the changes in luminosity that are required to be detected by the method of flicker, and are, therefore, worthy of being taken into account in an evaluation of the usefulness of the method, whatever method be adopted as a standard for comparison. And (3) we have contended that if the equality of brightness method be adopted as the standard for work in color photometry, the method of flicker does not satisfy the requirements, for it does not give results which agree in the average with those obtained by the equality of brightness method. This will be shown both from results of our own work and from a preponderance of the work done by others who have made the comparison. In our own work the comparison has been made for a series of intensities which may be considered as at least fairly representative of the higher intensities, they being considered more favorable to agreement by Dr. Ives.[1] Especial care has been taken in this series to duplicate at one point the intensity which Dr. Ives finds the most favorable to agreement.

The remainder of the paper will be taken up with the demonstration of these three points.

I. The Underestimation of the Luminosities of Red and Yellow and the Overestimation of the Luminosities of Blue and Green

Special tables have not been prepared for this point because the results can readily be seen in the tables for points II. and III. In these tables taken collectively the comparison will be shown for a representative series of variations, both of the ratio of the time of exposure to colored and colorless light and of the intensity of the lights employed. In every case underestimation is found to be characteristic for red and yellow, and overestimation for blue and green.

[1] Ives, H. E., 'Studies in the Photometry of Lights of Different Colors, *Philos. Mag.*, 1912, *24*, Ser. 6, pp. 149–188.

II. The Variation of the Ratio of the Time of Exposure to the Colored and Colorless Light Causes a Corresponding Variation in the Characteristic Underestimation of Red and Yellow and Overestimation of Blue and Green

As has already been stated, the work under this heading has been undertaken in part to show the preceding point, and in part to show that the amount of this underestimation and overestimation is a variable function of the ratio of the time of exposure to the colored and the colorless light. The effect of the variation was determined both when the comparison was made between colored and colorless pigment surfaces, and between colored and colorless lights. For the pigment surfaces the standard red, green, blue, yellow, white, and black of the Hering series of papers were used. For the colored lights, two sources have been used: the Wratton and Wainwright color filters, and the light of the spectrum. Since the work with the spectrum as source has not yet been finished, results will be given at this point from the work with the filters. Of these filters, only the *Alpha* and *Eta* were used. The former transmits a band of red from the end of the spectrum to .65 μ, the latter, a band in the blue-green from .52 μ to .465 μ. These two alone were used for the following reasons: (1) They are fairly representative of the colors that show a relative change in luminosity with change of intensity. And (2) the yellow, green, and blue filters each transmits components that undergo opposite luminosity changes with a change of intensity of the source. That is, the best yellow of the series transmits also a green component; the best green, a yellow component; and the best blue transmits some of the violet.

The photometric apparatus employed was for the sake of comparison made to conform very closely in its essential features to that described by Dr. Ives.[1] The general plan of our apparatus is indicated in Fig. 5. It consists of a photometer bar carrying the standard white light (*A*), a second bar carrying the colored light (*B*), a sectored disc (*C*), and a

[1] Ives, H. E., *op. cit.*, p. 161.

screen (*D*) provided with a small aperture (*O*) through which the light comes to the eye. The standard white light was enclosed in a black light-proof box (*E*), which was provided

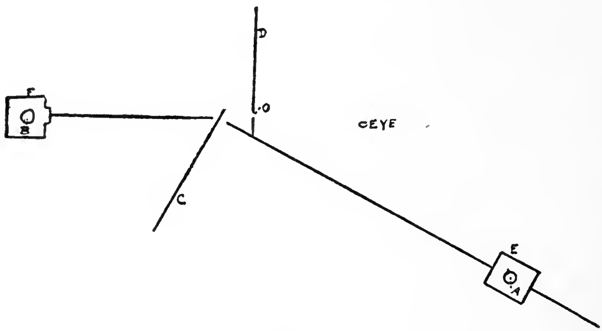

FIG. 5

in front with a circular opening 4 cm. in diameter for the transmission of the light. In passing to the sectored disc, the light was screened both from the observer's eye and from the colored source by black screens properly placed. The light which was passed through the colored filters was placed in a similar light-proof box (*F*) provided with an opening 4 cm. square for the transmission of the light. Above and below this opening were grooves into which the color filters were slid. The sectored discs were made of aluminum. The edges of these discs were carefully bevelled and the surface was kept freshly covered with magnesium oxide deposited from the burning metal. The aperture in the screen through which the light passed to the observer's eye was 3 mm. square. The visual angle subtended by this aperture at the observer's eye at 20 cm. distance was very small. A small angle was needed to guard against the unequal sensitivity of the central and paracentral portions of the retina to flicker, and against the difference in their brightness sensitivity to colored and colorless light. A 13-candle-power Mazda lamp was used as source for the colorless light, and 13-cp., 52-cp., and 130-cp. lamps for the colored light. These lamps were operated on a 110 D.C. circuit in series with an ammeter and finely graduated rheostat to guard against fluctuations in the current and loss of efficiency in the lamps. Also fresh lamps were substituted at the beginning of each series of observations. As a check on the results obtained from these lamps, several series of observations were made using a standardized tungsten lamp, street series, 16.6-cp. operated at 11.43 volts by a storage battery for the colorless light, and a similar lamp of 67-cp. operated by a storage battery at 10.35 volts for the source of the colored light.

The method of making the flicker judgment was as follows: A preliminary determination was made of the approximate setting of the light which was being moved, to give equalization. The speed of rotation of the sectored disc was then reduced until flicker was obtained. The position of the light was again adjusted until no flicker was obtained, and so on. This variation in the speed of rotation of the disc and the position of the light was continued until the position was ascertained that gave no flicker for the lowest speed of rotation. The final determination of this point was made by moving the light in both directions until noticeable flicker was obtained, and taking the average of these two readings. The movement required to give flicker on either side of this average position ranged usually from 2 to 9 mm. depending to some extent upon the observer and the intensity of illumination used. Employing the above apparatus and method, results were obtained for the highest intensity of colored light used for a total open sector of 315°, 270°, 225°, 180°, and 45°; and for the other intensities, for a total open sector of 315°, 180°, and 45°. In making the comparisons by the equality of brightness method, the disc was rotated until one of its edges bisected horizontally the photometric field. The results are shown in Tables II.–V. They will be summarized briefly as follows: (1) For all values of open sector and for all intensities of light, there was an underestimation of the luminosity of the red light and an overestimation of the luminosity of the blue-green. (2) As the size of the open sector was decreased, there was a corresponding increase in the amount of the underestimation of the luminosity of the red for all the intensities employed, and of the overestimation

Table II

Observer *A*

Showing that the Underestimation of Red and the Overestimation of Blue-green is a Characteristic of the Method of Flicker, for Lights of the Intensity Used in this Work, and that the Amount of this Underestimation and Overestimation is a Variable Function of the Ratio of the Time of Exposure of the Eye to the Colored and the Colorless Light

Source of White Light	Source of Colored Light	Color	Equality of Brightness Method	Flicker Method		Difference by Equality of Brightness Method and by Flicker Method with 180° Open Sector	Change Produced by Varying Sectors	Amouut of Change that Can be Detected by		No. of Revolutions per Second, Flicker Method
			Distance of White Light Giving Equality of Illumination	Value of Colored Sector	Distance of White Light Giving no Flicker			Equality of Brightness Method	Flicker Method	
13 cp.	13 cp. 151 cm.	Red	125.5 cm.	315°	133.9 cm.	−10.4 cm.	−3.4 cm.	3.5 cm.	.4 cm.	3.6
	distant from			180°	135.9				.3	4.7
	photometric			45°	137.3				.3	4.1
	screen	Blue-green	191	315°	183.45	+10.55	+5.4	4	.35	4.2
				180°	180.45				.35	6
				45°	178.05				.25	5.2
	52 cp. 151 cm.	Red	93.4	315°	102.05	−11.1	−4	3	.25	3.7
	distant from			180°	104.5				.2	5.9
	photometric			45°	106.05				.25	5.4
	screen	Blue-green	103	315°	94.5	+13	+5.65	4	.3	4.1
				180°	90				.3	6.1
				45°	88.85				.35	5.9
	130 cp. 89 cm.	Red	75.3	315°	82.9	− 9.7	−4	3	.4	6.3
	distant from			270°	84				.45	7.4
	photometric			225°	84.6				.4	9.2
	screen			180°	85				.5	9.2
				45°	86.9				.5	8.9
		Blue-green	82	315°	76	+ 7	+3.25	3.5	.5	6.6
				270°	77				.3	7.4
				225°	76.3				.3	8.1
				180°	75				.3	7.7
				45°	73.75				.35	7

TABLE II.—*Continued*

OBSERVER *A*

Source of White Light	Source of Colored Light	Color	Equality of Brightness Method: Distance of White Light Giving Equality of Illumination	Flicker Method: Value of Colored Sector	Flicker Method: Distance of White Light Giving no Flicker	Difference by Equality of Brightness Method and by Flicker Method with 180° Open Sector	Change Produced by Varying Sectors	Amount of Change that Can Be Detected by: Equality of Brightness Method	Amount of Change that Can Be Detected by: Flicker Method	No. of Revolutions per Second, Flicker Method
16.6 cp. standard lamp	67 cp. standard lamp, 89 cm. distant from photometric screen	Red	86.5	315°	96.1	−13	−4.2	2.5	.5	6.1
				180°	99.5				.3	8.7
				45°	100.3				.4	8.2
		Blue-green	85	315°	77.4	+11.55	+5.4	3	.3	5.6
				180°	73.45				.45	7.2
				45°	72				.5	6.9

TABLE III

OBSERVER *B*

Source of White Light	Source of Colored Light	Color	Equality of Brightness Method: Distance of White Light Giving Equality of Illumination	Flicker Method: Value of Colored Sector	Flicker Method: Distance of White Light Giving no Flicker	Difference by Equality of Brightness Method and by Flicker Method with 180° Open Sector	Change Produced by Varying Sectors	Amount of Change that Can Be Detected by: Equality of Brightness Method	Amount of Change that Can Be Detected by: Flicker Method	No. of Revolutions per Second, Flicker Method
13 cp.	52 cp. 151 cm. distant from photometric screen	Red	88 cm.	315°	99.18 cm.	−17.5 cm.	−8.82 cm.	3.5 cm.	.8 cm.	7.
				180°	105.5				.85	7.8
				45°	108				.9	10.9
		Blue-green	98	315°	96.2	+ 7.4	+7.7	4	.6	7[illegible]
				180°	90.6				.8	7.2
				45°	88.5				.9	6.15
16.6 cp. standard lamp	67 cp. standard lamp, 89 cm. distant from photometric screen	Red	86	315°	96.5	−15	−6.5	3	.8	9
				180°	101				.85	11.8
				45°	103				.8	11.4
		Blue-green	84.3	315°	78.5	+10.8	+8	4.3	.9	5.3
				180°	73.5				.85	7.4
				45°	70.5				.88	7

of the blue-green. (3) The amount of change in the photometric value of the color produced by varying the ratio of exposure to colored and colorless light was many times the smallest amount of change that can be detected by the method of flicker, and, therefore, must be considered of consequence in relation to the application of the method to practical work.

Column 1 of these tables represents the source of white light; Column 2, the source of colored light; Column 3, the color used; Column 4, the distance of the white light from the disc when judgment of equality is given by equality of brightness method; Column 5, the value of the colored sector for the method of flicker; Column 6, the distance from the disc at which the white light has to be placed to give the judgment of no flicker; Column 7, the difference in the distance the white light was placed for the equality of brightness method and the flicker method with 180° of colored sector; Column 8, the change in the distance of the white light produced by varying the value of the colored sectors in the method of flicker; Column 9, the distance the white light has to be moved in the equality of brightness method to change the judgment from equality to just noticeably lighter or darker; Column 10, the distance the white light has to be moved in the flicker method to change the judgment from no flicker to just noticeable flicker; and Column 11, the number of revolutions per second of the sectored disc for the method of flicker.

Tables IV. and V. represent the results of Tables II. and III. expressed in percentage of luminosity at the photometric screen.

Pigment papers are still used in a great many laboratories for the investigation of color sensitivity, and because of their convenience and ease of manipulation, they probably will be used for many years to come for preliminary work and for a certain class of investigations in which only comparative results are wanted. In estimating the brightness or luminosity of these pigment colors, the method of flicker is now much more extensively used perhaps than any of the other methods of making brightness comparisons. For this reason we have considered it worth while to extend our work to the

Table IV

Observer *A*

Showing the Results in Table II. Expressed in Percentage of Luminosity

Source of White Light	Source of Colored Light	Color	Disagreement Between Equality of Brightness Method and Flicker Method with 180° Open Sector	Change Produced by Varying Sectors	Amount of Change that Can Be Detected by: Equality of Brightness Method	Amount of Change that Can Be Detected by: Flicker Method
13 cp.	13 cp. 151 cm. distant from photometric screen	Red	−15 %	− 4.8%	5 %	.4%
		Blue-green	+12.3	+ 6.3	3.7	.6
	52 cp. 151 cm. distant from photometric screen	Red	−20	− 7.5	7	.4
		Blue-green	+30	+13	7.3	.6
	130 cp. 89 cm. distant from photometric screen	Red	−21	− 9	7	.9
		Blue-green	+19.7	+ 7.3	6.8	.7
16.6 cp. standard lamp	67 cp. standard lamp, 89 cm. distant from photometric screen	Red	−23.6	− 8.3	8.7	1
		Blue-green	+33	+ 8.3	8.5	.8

Table V

Observer *B*

Source of White Light	Source of Colored Light	Color	Disagreement Between Equality of Brightness Method and Flicker Method with 180° Open Sector	Change Produced by Varying Sectors	Amount of Change that Can Be Detected by: Equality of Brightness Method	Amount of Change that Can Be Detected by: Flicker Method
13 cp.	52 cp. 151 cm. distant from photometric screen	Red	−30%	−16%	7.5%	1.7%
		Blue-green	+17.	+18	7.7	1.2
16.6 cp. standard lamp	67 cp. standard lamp, 89 cm. distant from photometric screen	Red	−28.	−12.4	6.5	1.6
		Blue-green	+31.7	+24.	9	2

investigation of the effect of varying the value of the colored and the colorless sectors on the brightness of the pigment colors as determined by the method of flicker. Of the devices available for applying the method to these colors, the Schenck apparatus was selected as best suited to our purpose. As colors to be investigated, the red, green, blue, and yellow of the Hering series of standard papers were chosen. Sectors of the value of 180°, 270°, and 300° were used. Values lower than 180° were not used because they could not be accurately obtained with the type of photometer employed. Two intensities of illumination were used, one of 390 foot-candles (vertical component) received directly under a skylight and diffusion sash of ground glass; the other, 5 foot-candles, the illumination of a room lighted by windows. Space will be given here only for the results for the higher illumination. This illumination was carefully chosen far above the range of intensities at which the Purkinje phenomemon occurs when the eye is fully exposed to its stimulus, in order to subject our demonstration to a rigid test. We were seeking, for example, to ascertain whether an intensity might not be found so high that the underexposure of the eye to its stimulus by the method of flicker would not cause an underestimation of the brightness of red and yellow and an overestimation of the brightness of blue and green.[1] That these underestimations and overestimations occur at this high illumination and by amounts many times the smallest brightness difference that can be detected by the method, will be shown in Table VI.

Column 1 of this table shows the color used; Column 2, the black-white value of the color estimated by the equality of brightness method; Column 3 gives the value of the colored sector; Column 4, the white-black value of the color estimated by the flicker method; Column 5 gives the difference in the result by the equality of brightness and flicker method with 180° colored sector; Column 6 gives the change produced in the result by the method of flicker by varying the size of the colored sector; Column 7 gives the amount of change that can be detected by the equality of brightness method; and Column 8, by the flicker method.

III. The Method of Flicker Does not Give Results Which Agree in the Average with those Obtained by the Equality of Brightness Method

Nothing will be added in this section except to make our comparisons at the intensity of illumination found to be most

[1] We have been careful to choose high intensities because Dr. Ives has contended that at high intensities the disagreement between the methods of flicker and equality of brightness tends to disappear.

TABLE VI

OBSERVER *A*

Showing that the Underestimation of Red and Yellow and the Overestimation of Blue and Green is a Characteristic of the Method of Flicker for Light of the Intensity Used in this Work, and that the Amount of this Underestimation and Overestimation is a Variable Function of the Ratio of the Time of Exposure of the Eye to the Colored and the Colorless Light.

Color	Equality of Brightness Method	Flicker Method		Difference by Equality of Brightness Method and by Flicker Method with 180° Colored Seetor	Change Produced by Varying Sectors	Amount of Change that can be Detected by	
	White-black Value	Value of Colored Sector	White-black Value			Equality of Brightness Method	Flicker Method
Red	White 64° Black 296°	300°	White 58.9° Black 301.1°	−17.4°	−12.3°	8°	1.8°
		270°	White 56.2° Black 303.8°				1.2°
		180°	White 46.6° Black 313.4°				1.8°
Yellow	White 332° Black 28°	300°	White 328.3° Black 31.7°	−18.6°	−14.9°	9.5°	2°
		270°	White 321.7° Black 38.3°				1.8°
		180°	White 313.4° Black 46.6°				1.8°
Green	White 88.5° Black 251.5°	300°	White 99° Black 261°	+26.4°	+15.9°	9°	.45°
		270°	White 105.5° Black 254.5°				.9°
		180°	White 114.9° Black 245.1°				.45°
Blue	White 12.5° Black 347.5°	300°	White 14.5° Black 345.5°	+10.7°	+8.7°	5.3°	2.2°
		270°	White 19.1° Black 340.9°				1.4°
		180°	White 23.2° Black 336.8°				.9°

favorable for agreement by Dr. Ives.[1] The plan of the apparatus used in this work is indicated in Fig. 6. A spectroscope was used to give the colored light; a 32-cp. carbon lamp (*F*) was used as the source of the colorless light. This lamp gave a light of the same quality as that used by Dr. Ives, namely, the quality of the carbon standard of 4.85 watts per mean spherical candle. When placed at 32.6 cm. from the sectored disc (*D*), 270 meter candles of light were reflected

[1] Ives, H. E., *op. cit.*, p. 173.

from the disc. The eye piece was removed from the spectroscope and a lens system was used in its place consisting of two lenses (*A*) and (*B*), one to render the light emerging from the objective slit (*C*) parallel, and the other to focus it on the eye 30 cm. distant. Between the eye and the focusing lens (*B*) was interposed the sectored disc (*D*). Thus the light reflected from the sectored disc suffered no absorption in passing to the eye. A stimulus-opening (*E*) 16 mm. in diameter was placed in front of the disc 20 cm. from the eye. This subtended the

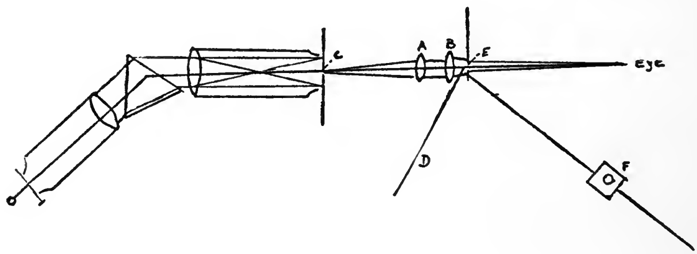

FIG. 6

same visual angle as the field size that Dr. Ives found to be the most favorable. A pupillary aperture 1 mm. square placed in front of the eye reduced the light reflected from the white disc to the intensity called by Dr. Ives 270 illumination units.[1] The colors used were a very narrow band of the spectrum in the region of .68 μ, .57 μ, .52 μ, and .47 μ, giving the four pure colors red, yellow, green, and blue. The method of making the comparison was as follows: The sectored disc was turned so that its edge bisected horizontally the photometric field, and the luminosity of the colored field was altered by changing the width of the collimator-slit until it equalled by the equality of brightness method the 270 illumination units. Using this slit width, then, the disc was rotated and the position of the white light was adjusted until no

[1] By using a pupillary aperture 1 mm. square, Dr. Ives has reduced the light entering the eye by an amount which, so far as we can see, can not be determined. He has established an arbitrary unit which he calls an illumination unit. We can not, therefore, compare the intensities of light used by us in the preceding experiments (pp. 134 ff.) with the 270 illumination units used by Dr. Ives. If one were to judge, however, by the apparent brightness of the disc in the two cases, he would have to say that the amount of light entering the eye was considerably greater for our

flicker was obtained. The flicker determinations were made with 315°, 180°, and 45° total open sector as before. The results are shown in Tables VII and VIII.

Table VII

Observer *A*

Showing that the Underestimation of Red and Yellow and the Overestimation of Blue and Green is a Characteristic of the Method of Flicker for Lights of the Intensity Used in this Work, and that the Amount of this Underestimation and Overestimation is a Variable Function of the Ratio of the Time of Exposure of the Eye to the Colored and the Colorless Light. Intensity same as was used by Ives

Wave-length	Equality of Brightness Method	Flicker Method		Difference by Equality of Brightness Method and by Flicker Method with 180° Colored Sector	Change Produced by Varying Sectors	Amount of Change that Can Be Detected by		No. of Revolutions per Second, Flicker Method
	Distance of White Light Giving Equality of Illumination	Value of Colored Sector	Distance of White Light Giving no Flicker			Equality of Brightness Method	Flicker Method	
.68 μ	32.6 cm.	315°	41.5 cm.	−11.6 cm.	−3.6 cm.	2.4 cm.	.5 cm.	9.2
		180°	44.2				.45	12
		45°	45.1				.5	11
.57 μ	32.6	315°	37.4	− 6.9	−3.4	2.8	.4	9.8
		180°	39.5				.4	14
		45°	40.8				.4	12
.52 μ	32.6	315°	23	+13.6	+4.9	2	.5	9.7
		180°	19				.4	13
		45°	18.1				.4	11.7
.47 μ	32.6	315°	23	+13.8	+5	2.3	.45	9.9
		180°	18.8				.4	14.1
		45°	18				.45	12

higher intensities than the 270 illumination units used by Dr. Ives. Thus it seems probable that most of our preceding tests were made with an intensity of light equal to or greater than that used by him. His claim, it will be remembered, was that one of the two causes of disagreement between the results obtained by the methods of flicker and equality of brightness in preceding experiments is the low intensity of the lights used. (The other was the lack of proper regulation of the size of the photometric field.) We do not believe that either one of these factors is the fundamental cause of disagreement, as is attested in our experiments by the fact that strong disagreement remains when both of them have been eliminated, at least, as completely as they were eliminated by Dr. Ives. A consideration of the functioning of the eye under very short exposures to light, shows, we believe, a much more fundamental cause of disagreement, namely, the difference in the way in which the eye responds to light stimuli when presented for the lengths of time used in the two methods.

TABLE VIII

OBSERVER *B*

.68 μ	32.6	315°	41.3	−11.6	−4	3	.8	12
		180°	44.2				.7	14
		45°	45.3				.8	13.4
.57 μ	32.6	315°	38	− 8.9	−4.3	2.9	.9	12.5
		180°	41.5				.6	14.2
		45°	42.3				.8	13.1
.52 μ	32.6	315°	23.4	+12.1	+3.9	3.3	.8	12
		180°	20.5				.7	14
		45°	19.5				.7	12.2
.47 μ	32.6	315°	22.8	+12.6	+4	3.5	.9	11.1
		180°	20				.7	14.4
		45°	18.8				.8	12.8

It was stated in the beginning of the paper that disagreement between the results of the method of flicker and equality of brightness would be shown from a preponderance of the work done by others who have made the comparison. As a general case the fact scarcely needs more than the pointing out. Before the work of Ives, disagreement was pretty generally admitted. Bell[1] says: "That the flicker and equality of brightness methods do not give coincident results when we consider the general case of flicker photometers, as compared with equality of brightness photometers, is a fact that has been too long familiar to photometrists to admit of a discussion." Comparisons of the two methods have been made by Whitman, Wilde, Dow, Bell, Stuhr, Luckiesh and Ives. Whitman[2] compared the luminosities of a red and green light placed 6 ft. apart on a photometer bar. He found that the setting of the photometer for equality of illumination differed for the equality of brightness and flicker methods by 1.2 ft. for one observer, and .8 ft. for another. Wilde[3] photometered a tungsten lamp against a carbon by the methods of flicker and equality of brightness, and found a difference of 6 per cent. in the result. Bell[4] compared the ratio of lumin-

[1] Bell, L., 'Acuity in Monochromatic Light,' *Electrical World*, Sept. 9, 1911, *58*, p. 637.

[2] Whitman, F. P., 'On the Photometry of Differently Colored Lights and the Flicker Photometer,' *Physical Review*, 1896, *3*, pp. 241-249.

[3] Wilde, L. W., 'The Photometry of Differently Colored Lights,' *The Electrician*, July 16, 1909, *63*, pp. 540–541.

[4] Bell, L., 'Chromatic Aberration and Visual Acuity,' *Electrical World*, May 11, 1911, *57*, pp. 1163–1166.

osities of a mercury vapor lamp with that of a tungsten lamp by means of the flicker method and found it to be 5.42. These same lights by the equality of brightness method gave a ratio ranging from 6.86 to 10.93 for different observers. Stuhr[1] compared red and green lights by several methods including the method of flicker and equality of brightness. He found that the mean deviation of the values obtained by the method of flicker from those obtained by the equality of brightness method amounted to 14.14 per cent. Luckiesh[2] photometered a red against a blue-green light by the methods of flicker and equality of brightness, and found a difference of 62 per cent. in the ratios of the luminosities of the two lights by the two methods.

Two factors have in the main been assigned to the cause of the disagreement: the effect of intensity and of size of the photometric field. Lauriol, Dow, Millar, Ives, and Luckiesh have investigated the former factor, and Schenck, Dow, and Ives the latter. These are both factors which affect the results of both methods. In comparison little attempt has been made to find the factors that affect the results of each method alone. As a general case these, it would seem, might be more apt to prove a source of disagreement than those which affect both methods.

With regard to the intensity of the light as a factor, Lauriol[3] and Dow[4] claim that the relative shift in the brightness of the different colors at low illuminations is shown by both methods. The shift for Dow, however, is more pronounced in the equality of brightness than the flicker determinations. For Lauriol the shift for the different colors varies in magnitude by the two methods and in some cases in direction. Millar,[5] on the other hand, claims that the

[1] Stuhr, J., 'Ueber die Bestimmung des Aequivalenzwertes verschiedenfarbiger Lichtquellen,' Kiel, Philos. Diss., Vol. *19*, Okt., 1908, p. 50.

[2] Luckiesh, M., 'Purkinje Effect and Comparison of Flicker and Equality of Brightness Photometers,' *Electrical World*, March 22, 1913, p. 620.

[3] Lauriol, 'Le photomètre à papillotement et la photométrie heterochrome,' *Bull. Soc. Intern. des Électriciens*, 1904, pp. 647–652.

[4] Dow, J. S., 'Color Phenomena in Photometry,' *Philos. Mag.*, 1906, *12*, Ser. 6, p. 131.

[5] Millar, P. S., 'The Problem of Heterochromatic Photometry,' *Trans. Illuminating Engineering Society*, 1909, *4*, p. 769.

Purkinje phenomenon is not shown at all by the flicker method at low illuminations, while Ives[1] and Luckiesh[2] go to the other extreme and declare that a reverse Purkinje effect is obtained by the flicker method. With regard to size of field as a factor, Schenck[3] found that a decrease in size lowered the mean variation for the flicker method and decreased the luminosity value obtained for all the colors. Dow[4] found that as the size of the field was decreased, red and yellow lightened relatively to green and blue. This effect was more pronounced for the equality of brightness than for the flicker method. Ives[5] found this effect for the equality of brightness method, but the reverse effect for the flicker method.

Ives, admitting the disagreement between the two methods and accepting size of field and intensity of the stimulus as the cause of the disagreement, sought to determine whether a field size and intensity could not be found for which the two methods agree. He photometered different portions of the spectrum against carbon lamps at a number of intensities and with a number of field sizes. He found in general for five observers that the luminosity curves obtained by each method differed. This difference, however, was less for high intensities than for low.

A table is appended (Table IX) in which is shown in percentage the difference in results gotten by the five observers used by Dr. Ives at the intensity of light which he calls most favorable to agreement for the two methods (250 Illumination Units). It will be seen that the disagreement for these observers is in the average as great, if not greater than was gotten by our own observers. Percentage of overestimation by the method of flicker is designated by +, and underestimation by −.

[1] Ives, H. E., *op. cit.*, p. 171.

[2] Luckiesh, M., *op. cit.*, p. 620.

[3] Schenck, F., 'Ueber die Bestimmung der Helligkeit grauer und farbiger Pigmentpapiere mittels intermittirende Netzhautreizung,' *Pflüger's Archiv*, 1896, *64*, pp. 607–628.

[4] Dow, J. S., *op. cit.*, pp. 130–134; 'Physiological Principles Underlying the Flicker Photometer,' *Philos. Mag.*, 1910, *19*, Ser. 6, pp. 58–77.

[5] Ives, H. E., *op. cit.*, p. 172.

Table IX

Showing in percentage the difference in results between the methods of flicker and equality of brightness for the five observers used by Dr. Ives at the intensity of light which he calls most favorable.

λ	H. E. I.	M. L.	P. W. C.	C. F. L.	F. E. C.
.653μ	−12. %	+29. %	−18. %	−51. %	−50. %
.643μ	− 3.6	+56.	− 7.0	−31.	−23.7
.632μ	− 4.3	+20.	−15.5	−45.5	−12.9
.622μ	− 7.3	+12.5	− 4.2	−42.	−15.9
.612μ	−10.	+ 8.3	− 6.5	− 7.5	+ 0.3
.594μ	− 1.	− 0.5	− 0.5	+ 7.5	+ 5.8
.574μ	+ 0.5	− 2.4	− 2.5	+27.	− 5.9
.555μ	− 0.4	− 8.0	−11.9	− 4.8	+ 8.9
.545μ	− 3.1	− 8.4	−13.8	+ 3.	+ 8.
.536μ	− 1.9	− 4.0	−12.6	−12.	+14.3
.526μ	0	− 8.7	−21.4	− 3.	+33.5
.517μ	+ 0.6	−10.8	−13.8	−33.	+30.0

It has been our purpose in general in this part of the paper to indicate a field of investigation in the department of physiological optics about which little is known as yet with certainty, rather than to report a finished piece of work or to attempt to draw positive conclusions. When functioning under the conditions imposed by the method of flicker, too little is known of the characteristics of the eye, we believe, to render safe its use as a measuring instrument. Our purpose in particular has been to point out and show the effect of a factor which we believe to be an important source of disagreement between the equality of brightness and the flicker methods, and to suggest that a more careful study be made of the factors that influence the method of flicker before it is adopted in its present form as the method for the standardizing laboratories. Just as one factor has been overlooked, so there may be others the influence of which should not be ignored.

Appendix

Three other points which may be of interest in connection with the above work are appended here. The first two were discussed by Dr. Ives in a series of articles on the method of flicker in the *Philos. Mag.*, 1912, *24*, Ser. 6, pp. 149–188, 352–370, 744–751, 845–853, 853–863. (1) In the third of his series of articles, he apparently wishes to show that the cause of the disagreement between the results of the methods of flicker and equality of brightness lies on the side of the latter method. That is, the difficulty of making the judgment is so great that not an equalization, only an 'appraisement' is accomplished. To demonstrate this, he attempts to get rid of the disturbing factor of color difference in the equality of brightness method by making his comparisons always between lights differing only slightly in composition. That is, a green is compared with a green slightly shifted toward the yellow or blue, etc. (See his work with the 'cascade' method, p. 748.) A curve of luminosity for the spectrum obtained in this way is found to agree more closely with the flicker curve than one obtained in the ordinary way. The following things may be said of this demonstration, however. In the first place, he states that the cumulative errors are so great in the method that he could not begin at one point in the spectrum having a given luminosity and work in a given direction, then reverse this direction of working and obtain at all a close approximation to the luminosity value for the point at which he started. For this reason he drops the point by point procedure of the 'cascade' method, and plots his curve by taking his observations at twelve points in the spectrum. From the observations of these points the whole curve is constructed. In the second place, his method does not entirely accomplish his purpose of getting rid of all difference in color quality between the lights compared. In order to add some further data bearing upon the question whether the lack of agreement hitherto found between the results obtained by the equality of brightness and flicker methods could have been due to the difficulty of making the equality of brightness judgments of fields differing in color quality, we have thought it worth while to make the comparison using an equality of brightness method which for the purposes of this investigation presents, we believe, some points of advantage over the method used by Dr. Ives.[1] That is,

[1] We do not, however, mean to propose this as an entirely satisfactory method of heterochromatic photometry for the reason given in the discussion of the relation of the method to the Talbot-Plateau law (see footnote p. 149). We are using the method here merely to show that when the disturbing factor of color difference in the

the method we have used offers even less chances for errors in judgment, is simpler, and entirely eliminates the presence of a second color in the fields to be compared. The method is as follows: The sectored disc was adjusted so that its outer edge bisected vertically the photometric field. A standard colorless light was moved to the position on the photometer bar that gave the judgment of equality by the method of flicker, and the disc was rotated at the fusion rate. Half of the field was thus of color of the original saturation and luminosity, and the other half was a fusion of the colored sector of the original saturation and luminosity and a gray sector of the luminosity of the color as determined by the method of flicker. Now, if the luminosity of the color by the method of flicker were the same as by the equality of brightness method, the two halves of the photometric field should match in luminosity (within the limits imposed by the Talbot-Plateau law).[1] That is, the addition of the colorless

fields to be compared is eliminated from the equality of brightness method, there is still a large, in fact an apparently undiminished characteristic difference between the results of the equality of brightness and flicker methods, which, so far as one can see, can in no way be ascribed to the equality of brightness method employed. The degree to which the influence of color difference on the judgment of the brightness equality of the fields compared is removed by this method is shown by the greatly increased reproducibility of the judgment. For our observers, the reproducibility is almost as great as it was for the method of flicker. There was thus but little more of the element of appraisement in this method than there was in the method of flicker, while the characteristic difference in the results obtained by the two methods was not, so far as could be determined, appreciably lessened.

[1] A few words are needed to explain what is meant above by "within the limits imposed by the Talbot-Plateau law." It could scarcely be expected from a consideration of this law that the two fields would match especially under the dark-room conditions under which photometry is done, even when the gray sector was chosen equal in brightness to the color by the equality of brightness method. That is, when the colored is mixed with the gray sector by the method of successive impressions, there is a reduction of the intensity of each impression which is the same as would be gotten were the intensity of each light to be reduced in proportion to the time of exposure of the eye to each light to the total time of exposure of the eye to both lights. (See the discussion of the Talbot-Plateau law, p. 121.) That is, if the value of each sector is 180°, the impression made upon the eye by each light is the same, according to the Talbot-Plateau law, as if both lights were reduced one-half in intensity. But in suffering the reduction, the luminosity of the colored sector is not changed the same in amount as is that of the gray sector. If it is blue or green, for example, its brightness is not reduced so much as is that of the gray sector, and its fusion with the gray sector tends to lighten that sector and to make the second half of the field lighter than

to the colored sector would produce no change in its luminosity, and the two halves of the field would present a fully saturated color of a given luminosity and a less saturated color of the same luminosity (within the limits imposed above). But if there were an underestimation or an overestimation of the luminosity of the color by the method of flicker, the brightness of the second half of the field would be modified in this direction in proportion to the value of the colored and colorless sector; and if the underestimation or overestimation were great enough the two halves would not match. In proportion as the colorless sector is made larger in the second half of the field, the color of the mixture loses saturation, and the comparison with the fully saturated half of the fields becomes more difficult to make. On the other hand, in proportion as the colored sector is made larger, the effect on the brightness of the mixture of the difference between the flicker value and the true sensation value, if such a difference exists, is lost. After considerable preliminary investigation it was decided to use in turn colored sectors of the value of 300°, 270°, and 180°. The comparison was made for lights of the intensities specified in the preceding sections of the paper. In all cases when the color was red or yellow, the

the first. If, however, the colored sector is red or yellow, it is reduced more in brightness than is the gray sector, and its fusion with that sector tends to darken it and so to render the second half of the photometric field darker than the first. We have conducted experiments to determine whether the above effect, which is a direct corollary of the Talbot-Plateau law, actually takes place in observable amounts. When the light of the spectrum or light of the purity given by the Wratten and Wainwright filters was used, we found that it did. That is, when the second half of the field was green or blue and was fused with a gray of the luminosity of the color employed, determined by the equality of brightness method, this half of the field was observably lighter than the first half. Conversely, when red or yellow was used, the second half of the field was darker than the first. The effect, however, was not nearly so great as it was when the gray sector was made of the brightness of the color as determined by the method of flicker. That is, if two experiments are conducted, one in which the second half of the field is made by fusing the colored sector with a gray sector of the brightness of the color as determined by the equality of brightness method, and the other in which this half of the field is made by fusing the colored sector with a gray sector of the brightness of the color as determined by the method of flicker, the difference in brightness between the two halves of the field is quite appreciably greater in the second case than in the first. For example, when the colors are green and blue, the second half of the field is more too light in the second case than in the first; and when red and yellow, it is more too dark.

second half of the field was darker than the first; and when either blue or green, was lighter than the first half of the field. Determinations were made also of how much the colorless light had to be moved to make the two halves of the field match. These distances were not much different from those contained in the tables in the preceding sections of the paper expressing the difference in the estimation of the luminosity of the colors by the methods of flicker and equality of brightness (see pp. 136, 137),—certainly not any more than should be expected when it is remembered that a part of the effect of the difference is lost by mixing the colorless light representing the flicker determination with a sector of the colored light in its true luminosity value. The work was done also with pigment papers with a similar result. Thus it seems reasonable to conclude that the cause of the disagreement between the two methods can not be attributed entirely at least to the difficulty of making the equality of brightness judgment due to the difference in color quality between the fields compared, for in the above cases the color quality of the lights compared was the same. In the third place, disregarding the results of the above experiments, the writers scarcely need point out that it would be extremely difficult to explain such a systematic drift of luminosity in one direction in one part of the spectrum, and in the opposite direction in the other part, as we obtained, in terms of errors due to a false judgment of the sensations actually aroused. Moreover, it would be just as difficult to explain Dr. Ives's own reverse Purkinje effect in terms of a false judgment of the actual brightness values presented in sensation; or the closer agreement he obtains between the results by the methods of flicker and equality of brightness at high illuminations, in which case there is the maximum amount of color present and, therefore, the maximum color difference to disturb the equality of brightness judgment between colored and colorless light. Moreover, the kind of errors that one finds as due to uncertainty of judgment is a deviation on either side of a mean. This occurs when all other factors are eliminated if several judgments of the same sensation are made. Such errors are compensated for by taking the average or mean of the determinations. If it is not conceded that they are compensated for, how, for example, can the average of the results by the equality of brightness method be taken as a standard in terms of which to evaluate the results obtained by other methods? (See Whitman, Schenck, Wilde, etc.).[1] Surely this should not be allowed if there were a consistent deviation in any one direction from the true brightness value for a given color due to errors in judgment. Moreover, such a characteristic drift due to errors in judgment is unknown in all previous work in psychophysics, and not only unknown, but unsuspected.

(2) In the fourth paper of the series,[2] Dr. Ives applies as a test to the method of flicker what he calls two axioms of measurement. These are (*a*) things which are equal to the same things shall be equal to each other; and (*b*) the whole shall be equal to the sum of its parts. He finds that the method of flicker satisfies these axioms better than the equality of brightness method. We would point out that these tests would not be expected to reveal to any considerable degree the influence of the factor we are discussing. They are tests which would apply as a check on the power to make the judgment of the brightness of the sensation properly, or to any tendency of this brightness equality to drift in one direction in any part of the spectrum without a compensating drift in the opposite direction in some other part of the spectrum; but they are not tests that could be expected to show whether or not there is underestimation in one half of the spectrum and overestimation in the other half. For example, the area of the curve of the spectrum plotted by the method of flicker might very well sum up to the value of the reassembled white light because of the compensating effect of the underestimation of one half of the spectrum and the overestimation of the other half.

(3) Since the foregoing paper was presented, the writers

[1] While Dr. Ives does not explicitly state that he takes the equality of brightness method as a standard in terms of which to evaluate the correctness of the results by other methods, the point of view is strongly implied in his first paper (*loc. cit.*).

[2] *Philos. Mag.*, 1912, *24*, Ser. *6*, pp. 845–853.

have met with the contention from a prominent advocate of the method of flicker that the effect of a reduction of intensity is not given by the method of flicker because each individual impression is carried over until the next is given, with sufficient intensity to preclude the effect of reduction. Whether or not each individual impression can be considered as carrying over with sufficient intensity to preclude the effect of reduction is an important point and should, lest the issue be in doubt, be included in a discussion of the principles underlying the method of flicker. It may not be out of place, therefore, for us to consider the question here briefly, even though it has not as yet, so far as we know, been discussed in print.

As evidence that each individual impression should be considered as carrying over with sufficient intensity to preclude the effect of reduction, it was contended, as the case was presented to us, that the rate used in the method of flicker is the fusion rate of the two impressions. Two reasons were given for considering this rate as the fusion rate. (1) If the two impressions be red and green, for example, yellow is produced at the rate of succession used in the flicker method. Yellow, it was pointed out, is a fusion of red and green, and, therefore, the rate used must be considered as the fusion rate for these colors. In answer to this point we would again call attention to the phenomena (see p. 116) which are produced in sensation when two impressions differing in color and brightness are given to the eye successively at different rates of speed.[1] When the rate is very slow, the effect of separate and distinct impressions is given, each in its proper color and brightness. When a little faster rate is used, the impressions become confused and a flickering effect is produced both in the color and brightness components of the sensation. When the rate is made still faster, the flickering of color dies out, leaving only brightness flicker; that is, the color components of the two sensations have been fused. That the brightness

[1] We wish at this point to state very emphatically that our account of the fusion of the color and brightness components of sensation at different rates of speed is not based on any theoretical conception of a separate brightness and color sense, but upon actual observation of the phenomena that take place when light impressions differing in color and luminosity are combined at different rates of succession. These phe-

components have not been fused, however, is attested by the presence of brightness flicker, which is now left outstanding in a field uniform as to color quality. As the rate of succession is made still faster, brightness flicker becomes less and less pronounced and finally disappears.[1] The rate at which this disappearance takes place is the fusion rate for the brightness components for the two sensations, and is much higher for all the colors than is the rate at which the fusion of the color components takes place.[2] (Interpreted in terms of the

nomena may be readily demonstrated by any kind of flicker photometer head if a sufficiently sensitive control of speed of rotation is had. (We have used for the control of speed of rotation a rheostat and motor especially constructed to give fine changes.) It can be very plainly and perhaps most conveniently demonstrated by rotating sectors of pigment papers at the proper gradations of speed in a good daylight illumination.

[1] We find that Krüss (*Physical. Zeitschr.*, 1904, *5*, p. 67) gives a description of the phenomena that take place in sensation when two impressions differing in color and brightness are given to the eye successively at different rates of speed, very similar to that we have given here. He says: "If we slowly alternate the illumination from two differently colored light sources, for example, from a Hefner lamp and a gas burner, we clearly distinguish a succession of reddish and bluish bands with weak washed-out limits between them. As the rate of succession is increased it becomes progressively more difficult to distinguish the two colors from each other. At a comparatively low rate they begin to lose themselves in each other. At a slightly higher rate the difference in color disappears altogether and we have a color mixture. In this mixture, however, a brightness succession, a flicker, is observable which disappears only by a further increase in the rate of succession. Physiologically, it is of great interest that the distinguishing of separate colors ceases at a much slower rate of succession than the rate at which completely continuous sensation begins."

[2] The following values will serve to give a rough comparative showing of the rates at which the phenomena described above take place. The colors used were red and green. They were obtained from pigment papers of the Hering series of standard papers and from gelatine filters. Two intensities of color were employed in each case. The brightness of the Hering green for the lower intensity of illumination was .000814 cp. per sq. in.; of the red, .000594 cp. per sq. in. The phenomenon of separate impressions occurred from the lowest speed up to 6.9 revolutions per second. The impression of an intermingled color and brightness flicker was given from this rate up to 9.6 revolutions per second, at which rate the color components of the sensation fused, giving a field uniform as to color quality but with a strong outstanding brightness flicker. Brightness flicker was present until a speed of 22 revolutions per second was obtained. At this speed the brightness components in sensation were completely fused and the rotating disc presented a surface uniform both as to color and brightness. In making these determinations, the same sized field was used as was employed in our work with the method of flicker, *i. e.*, the disc was viewed through an aperture 3 mm. × 3 mm. in a gray screen (Hering No. 24) 20 cm. from the eye. For the higher intensity the green surface was illuminated to a brightness of .00242 cp. per sq. in.; the

duration of the impression after the light has been cut off, this means, of course, that the brightness component in the sensation does not carry over under these conditions with as little loss of intensity as does the color component.) It is evident, then, that the rate of succession which is used in the method of flicker is at or near the fusion rate for the color components of the two sensations, not for the brightness components; nor is it anywhere near the fusion rate for the brightness components. But it is the brightness components in which we are interested in photometry. That is, it is in terms of the brightness component that all photometric judgments are made. The color components, when they differ in tone, only serve to confuse the judgment. It is, therefore, our object in all methods of photometry as much as possible to get rid of difference in the color components. This can be accomplished in the method of flicker only because of the fact we have just pointed out, namely, that the fusion of the color component in sensation comes at a much lower rate of succession than the fusion of the brightness component. That is, all color differences, whether sensed as distinct or as flickering sensations, disappear at a rate of succession that has little or no effect on eliminating the brightness factor, or in this case the equivalent of this elimination, the fusion of the brightness components of the two sensations. In fact, if there were no difference in the fusion rate of the color and brightness components, the flickering color impressions would so mask the presence of brightness flicker at any rate of succession that could be used, that the method would doubtless

red, .00167 cp. per sq. in. The phenomenon of separate impressions occurred from the lowest speed to 6 revolutions per second, at which rate color flicker began. Color fusion took place at 12.4 revolutions per second, and brightness fusion at 29.3 revolutions per second. At the lower intensity for the filters, the brightness of the green was .154 cp. per sq. in.; for the red, .099 cp. per sq. in. As compared with their brightness these colors were much more poorly saturated than were the Hering pigments. The phenomenon of separate impressions ceased and color flicker began at 6.5 per second. Color fusion took place at 11.5 revolutions per second, and brightness fusion took place at 35.4 revolutions per second. At the higher intensity for the filters the brightness of the green was .22 cp. per sq. in.; for the red, .143 cp. per sq. in. Color flicker began at 7 revolutions per second; color fusion took place at 12.9 revolutions per second; and brightness fusion was complete at 38.3 revolutions per second.

have little if any greater sensitivity than the equality of brightness method. (2) The second point that was cited in support of the contention that the rate used in the method of flicker is the fusion rate for the two sensations aroused, is that no brightness flicker is present when in terms of the method the two impressions are adjudged of the same brightness. This to the present writers seems indeed a strange confusion of meanings. Fusion is a term used to represent what takes place when two impressions or sensations differing in quality are combined into one, the same or homogeneous as to quality. This combination may be obtained in case of light stimuli, for example, by mixing two lights evenly and allowing them to act simultaneously on the eye; or it may be obtained by giving two lights to the eye in succession at such a rate that the sensation aroused by the one lasts over until the next one is set up with a sufficient degree of intensity to give the effect of continuity or homogeneity of quality. It may add to the clearness of our discussion, then, to consider what takes place in this regard when two impressions differing in brightness are given to the eye at the different rates of succession mentioned in the preceding paragraph. At the rate at which distinct and separate impressions are given, each sensation obviously dies away completely before the next one is aroused. If a rate slightly faster than this is selected, the sensation does not die away completely before the next one is set up.

There is a slight lasting-over from one impression to the next. This when the two impressions differ in brightness gives the effect of a wavering or flickering sensation. At the lowest speed at which flicker is produced, the effect of this lasting-over has its minimum value. As the speed is further increased it becomes greater and attains its maximum value at the rate of complete brightness fusion.[1] (See discussion of Talbot-Plateau law, pp. 121.) It is obvious, then, that the rate of speed employed in the method of flicker, which is, roughly speaking, the lowest rate at which brightness flicker

[1] At the fusion rate neither sensation rises to its maximum value, for example, nor has a chance to die away until the next one develops. The effect is that of a continuous sensation homogeneous as to color and brightness.

can be obtained unmixed with color flicker, is not the fusion rate for the brightness components in sensation nor is it anywhere near this rate.[1] It is equally obvious also that the absence of flicker when the final adjustment of the lights has been made for a photometric balance, can not be adduced as any evidence that this rate is the fusion rate for the brightness component of the two sensations, or, what is more significant in relation to the above mentioned claim, that it is a rate to which more than a minimum of lasting-over effect from impression to impression can be ascribed. Flicker is absent merely because, in accord with the purpose of the method, such an adjustment of the distance of the lights from the photometer head is made that the sensations aroused by the two lights are of equal brightness. Such sensations do not flicker whatever may be their rate of succession. It can, therefore, be considered as little more than absurd to adduce the absence of flicker when the photometric balance has been attained as evidence that the rate used is the fusion rate for the brightness components of sensation, and to pass from this to the conclusion that the same amount or anywhere near the same amount of carrying-over effect is present for this rate as obtains when the fusion rate is used. In fact, if this carrying-over effect were present to any considerable degree, the whole point of the flicker method would be lost. That is, it is the purpose in the method of flicker to select a rate of succession that will give the eye the maximum of sensitivity to brightness difference (or flicker), namely, the lowest rate at which flicker can be produced, rather than a rate that will fuse out this difference in sensation.

But supposing it could be established, as was contended, that we have in the rate used in the method of flicker a complete color and brightness fusion of the sensations aroused by the two lights, little would be gained for the claim that there is no reduction in the effect on sensation of the two lights employed, if it be granted, for example, that the Talbot-Plateau law is true. In substance this law is as

[1] Flicker and fusion are in fact antithetical terms, and the rates of succession which are favorable for each are widely separated in the scale of frequencies.

follows. When once the rate of rotation is sufficient to give a uniform sensation, the color and brightness of the disc are the same as they would be if all the light reflected from the sectors were evenly distributed over the surface of the disc; and no further increase in rapidity produces any effect on its appearance.[1] In terms of this law it is seen that the effect on

[1] See H. F. Talbot, 'Experiments on Light,' *Philos. Mag.*, 1834, Ser. 3, *5*, pp. 321–334.

Talbot phrases this law as follows (pp. 328–329): "Since then these two things—the intensity of light and the time of the body's remaining in any given part of the circle—are each inversely proportional to the circumference of the circle it describes, it follows that they must be directly proportional to each other; that is to say, an irregular intermittent luminary whose observations are too frequent and too transitory for the eye to perceive, loses so much of its apparent brightness from this cause as is indicated by the proportion between the whole time of observation and the time during which it disappears." "The rapidity of the rotation does not affect the argument." To verify this reasoning, Talbot conducted experiments with reflected light using pigment surfaces and mirrors to send the light to the eye; and with transmitted light using sectored discs to cut down the time of exposure of the eye to various luminous sources.

In 1835 Plateau repeats and verifies Talbot's experiments. ('Betrachtungen über ein von Hrn. Talbot vergeschlagenes photometrisches Princip,' *Poggen. Annal.*, 1835, *35*, pp. 457–468). He concludes from his experiments as follows (pp. 462–463) "Nun muss zufolge des am Anfange dieses Aufsatzes dargelegten Princips die scheinbare Helligkeit der Scheibe sich zu der des Papiers verhalten wie die Vorübergangsdauer eines weissen und eines schwarzen Sectors; odor was dasselbe ist, wie die Winkelbreiten eines weissen Sectors zur Summe der Winkelbreiten eines weissen und schwarzen Sectors, oder endlich, was auch noch dasselbe ist, wie die Breite sämmtlicher weisser Sectoren zum ganzen Kreisumfang."

Swan, apparently working in ignorance of the writings of Talbot and Plateau, in substance formulates the law anew in 1849 (see W. Swan, 'On the Gradual Production of Luminous Impressions on the Eye and Other Phenomena of Vision,' *Trans. Roy. Soc. Edinb.*, 1849, *16*, pp. 581–603. See also F. Boas, 'Ein Beweis des Talbot'schen Satzes und Bemerkungen zu einigen aus demselben gezegonen Folgerungen,' *Wiedem. Ann.*, 1882, *16*, 359–362; A. M. Bloch, 'Expériences sur la vision,' *Compt. Rend. de la Soc. de Biol.*, 1885, *2*, p. 495; A. Charpentier, 'Loi de Bloch relative aux lumières de courte durée,' *ibid.*, 1887 *4*, p. 5; etc.

For a more modern statement of this law and one also more consistent with the relation of changes in light energy to changes in sensation, see Helmholtz, 'Handbuch der physiol. Optik,' zw. Aufl., 1896, p. 483, "Wenn eine Stelle der Netzhaut von periodisch veränderlichem und regelmässig in derselben Weise wiederkehrendem Lichte getroffen wird, und die Dauer der Periode hinreichend kurz ist, so entseht ein continuirlicher Eindruck, der dem gleich ist, welcher entstehen würde, wenn das während einer jeden Periode eintreffende Licht gleichmässig über die ganze Dauer der Periode vertheilt würde"; or E. C. Sanford, 'Experimental Psychology,' 1898, p. 146, "When once the rate of rotation is sufficient to give a uniform sensation, the color and brightness of any concentric ring are the same that they would be if all the light reflected sensation is the same as is gotten by reducing the intensity of each light by an amount proportional to the ratio of the exposure time of that light to the total time of exposure to both lights; or in case the photometer head is a sectored disc, in proportion to the value of the given sector or set of sectors to 360°. That is, with a total value of each sector or set of sectors of 180°, the effect on sensation is the same as if each light were reduced one-half in intensity; if the total value of one sector or set of sectors is 90°, the effect on sensation is the the same as if the light illuminating that sector were reduced to one-fourth of its intensity; if the total value were 45°, the same effect is produced as if the light were reduced to one-eighth of its intensity; etc. Thus, even if the rate of succession that is used in the method of flicker could be considered as the fusion rate for the brightness component of the sensation aroused, little advantage could be gained for the position in question. For the conclusion most certainly could not be avoided that the effect on sensation would be the same as if the lights were reduced in intensity, and by an amount proportional to the ratio of exposure time of each light to the total time of exposure to both lights.

The position under discussion seems also to involve to some extent a confusion of principle of the method of flicker with the method of critical frequency. For example, in the method of critical frequency, the impressions are given to the eye at the fusion rate. We need scarcely call to mind the procedure. One sector or set of sectors of the disc is illuminated by one of the lights to be compared and the other is black or of a very low luminosity. The disc is rotated at a rate which completely fuses the sectors in sensation. This light is then removed and the other light to be compared is substituted for it. The distance of this light from the disc is then adjusted until the rate of rotation required to produce fusion is the same as it was in the previous case. When this adjustment is obtained the intensity of illumination of the disc by the two lights is said to have been the same, and the

from it were evenly distributed over its surface, and no further increase in rapidity produced any effect on its appearance."

relative brightnesses of the lights themselves are calculated by the law of inverse squares. The situation is, however, quite different for the method of flicker. Both sectors or sets of sectors of the disc are illuminated by the lights to be compared, and the rate of rotation is to be made such that if there were any brightness difference between the sectors, the maximum of flicker, not fusion, would be produced. If a rate were used that would produce fusion, for example, for any given amount of brightness difference, it is obvious that no difference in brightness equal to or less than this amount could be detected by the method. That is, the whole point of the method is to use a rate of speed that could not possibly be the fusion rate for any appreciable amount of brightness difference between the impressions to be compared; and in so far as this purpose can be realized in the different cases in which the method is employed, sensitivity for the method is obtained.

What our critic really needs to establish in order to support his position is that summation instead of fusion takes place. That is, if the total effect of each light on sensation is to rise to a higher level than is given by each individual impression, the individual impressions must in proportion to the rise summate or add their individual intensities. To produce this effect of summation, each individual impression would have to last over in sensation until the next impression of its kind is received, which, since the impressions alternate, could be the next impression but one. For example, when red and green lights are being compared, if the value of the red sensation is to rise to a higher level than that given by a single impression, the sensation aroused by one exposure to red would have to last over until sensation is aroused by the next exposure to red; that is, would have to last through the interval of exposure to green and into and wholly or partly through the succeeding interval of exposure to red. How highly improbable it is that this could happen to any degree that would be of saving consequence to the method, is shown by the following two considerations. (*a*) The wavering character of the sensation which we call flicker is due to the fact that a given sensation does not carry over without a great loss of intensity through the next succeeding interval, let alone through the next interval but one. And (*b*) even at the rate at which complete color and brightness fusion takes place, there is according to the Talbot-Plateau law no effect of summation great enough to cause each individual sensation to attain to a higher intensity than that fixed by the ratio of the time of exposure of its stimulus light to the total time of exposure of both lights, nor to produce a noticeable change in this intensity, however great is the speed of the succession. That is, we have a reduction of the intensity of the sensation aroused by each light which is the same as would be gotten were the intensity of each light to be reduced by an amount proportional to the ratio of the time of exposure of that light to the total time of exposure of both lights, and no further increase in the rapidity of the succession produces any change in this effect.[1]

With regard to the method of flicker, then, the case apparently stands as follows. The individual impressions are so short that the eye is very much underexposed to its stimulus, and the rate of succession is so slow that there is

[1] If one were permitted to interpret the Talbot-Plateau law with regard to what takes place when a rate of succession is employed greater than the fusion rate for both the colored and brightness components of sensation, two possibilities would be opened for explaining why no change in sensation is produced as the rate of succession is increased, and the length of each individual exposure is correspondingly decreased. (1) Either the increase in the reduction of the exposure-time causes no further reduction in the sensation aroused by the individual exposures; or (2) there is, owing to the increased rate of succession, a summation effect which just compensates for the reduction of the individual impressions. Now even if we were to accept as true the one of these alternatives which is the more favorable for the case of flicker, namely, that a compensating summation action takes place, and assume that this compensating summation obtains clear down to the rate of succession that is used in the method of flicker, we would have to expect as much reduction in the sensation aroused by each of the lights as is expressed by the Talbot-Plateau law. That is, the reduction for each would be the same as would be gotten were the intensity of each light to be reduced in proportion to the exposure-time of each to the exposure-time of both. As we have already pointed out, however, it is extremely improbable that there could be a compensating summation action at the flicker rate great enough to be of any considerable consequence to the method, because the wavering character of the sensation which we call flicker is due to the fact that a given sensation does not carry over without great loss until the next one develops, let alone until the next but one develops, which it would have to do to produce any summation effect.

not enough carrying-over from impression to impression to produce fusion, let alone the summation effect which is needed to cause the intensity of the sensation to rise to its full value or perhaps even to a higher level than would be given by a single exposure. Moreover, according to the Talbot-Plateau law a summation effect great enough to cause the sensation to rise to its full value is never produced, however fast is the rate of succession; for once the fusion rate is obtained, there is a reduction of the intensity of the sensation aroused by each light which is the same as would be gotten were the intensity of each light to be reduced in proportion to the time of exposure of that light to the total time of exposure to both lights, and there is no change in this effect however much the rate of succession is increased.

[1] Since the above discussion was presented to the Illuminating Engineering Society, Ives in collaboration with Kingsbury, has published a sixth article on the method of flicker (*Philos. Mag.*, Nov., 1914, *28* (167), pp. 708–728) in which a theory of flicker photometry is developed based on an analogy drawn between the response of the eye under successive stimulation to the action of incandescent lamp filaments under a fluctuating current. The gist of the article is that if the eye behaves under the conditions obtaining in flicker photometry as do lamp filaments (subject to certain modifications which are not in accordance with what is known of the functioning of the eye) under a fluctuating current, the method of flicker should give with high intensities of light at the photometer screen the same results on the average as the equality of brightness method. It is our purpose here merely to note the article, not to give a detailed discussion. The theory will be discussed in a later paper in connection with further experimental data. It may not be out of place to state at this time, however, that the analogy of the eye and the incandescent lamp filament is not based on experimental examination of the eye's manner of response, but is assumed. Moreover, considerable evidence is offered in the present paper, we think that the eye does not react to its stimulus given to it in succession at the flicker rate according to the laws which govern the temperature response of lamp filaments, more especially when the impressions differ widely as to wave-length. It has not been claimed, for example, that the flicker method does not give the same results as the equality of brightness method when the lights compared do not differ as to wave-length.

DISCUSSION

THE FUNCTION OF INCIPIENT MOTOR PROCESSES[1]

There is no doubt that such a theory as the author discusses is of important advantage, yielding a base for a fair understanding of nervous functions. In regard to the assumption that the discharge of a motor center may induce the discharge of a cortical center that is tributary to it, there is evidently something left to the imagination of the reader. It is possible that from the point of view of practical or quantitative science, so to speak, some other hypothesis may be found more defensible.

With this in mind the writer will venture to describe the nervous mechanisms that produce the image, holding to the author's first assumptions but departing from the induced discharge assumption. To bring out the point quickly, let us begin by considering the following case that is easy of explanation, and lead up gradually to the functions under controversy.

If a child sees a red ball and utters the word ball, and then makes a forward movement, certain associations will be formed. At a later time the child is prompted to utter the word ball but the movement is only partially carried out and the word is inaudible. An image of the red ball appears in the child's mind. Now in the first occurrence which we may term the experience, we may say that there are afferent impulses due to the sight of red, to the shape of the ball and to the sound of the uttered word. These go to the cortex, or at least a part of each kind does so. There are also kinæsthetic impulses from the eye muscles and from the throat and lips, and a part of these causes excitation in the cortex.

In the second occurrence, which we may term the recall, there are kinæsthetic impulses from the muscles used for the word and no doubt some of these will be just the same as if the word had not been suppressed but uttered aloud. Now if conditions are right, these latter excitations will reach the cortical centers which were excited in the experience. The author's first assumptions and theory of association are the explanation. Take the color red for example. In the experience, the excitation starts in the retina,

[1] M. F. Washburn, Psych. Rev., Vol. XXI., No. 5.

thence goes to the cortical center for red, thence it flows to the motor centers in activity. Within a moment the excitations from the speech muscles pass through the cortex and perhaps follow the identical neurons just stimulated by the color. By the rules, the common pathway will have its conductivity increased by the experience.

When the recall comes, the flow from the receptors in the muscles will follow this line of increased conductivity, pass through the cortical center for red and hence an image of red will arise. The flow will proceed from the cortical center by the pathways that are open to some motor center or centers. Thus we see that the suppressed utterance of the word ball has brought up an image of the color red and yet there has been no induced discharge such as the author describes. For the above demonstration, it is essential that in the recall some of the muscles be partially contracted so as to cause the kinæsthetic excitations which we assume to follow from muscular contractions.

Let us now go a step further and suppose that some time later there is an occurrence we will term the secondary recall. The child is prompted to utter the word but there is no movement and no real contraction nor even a noticeable change of tone in any muscle. Again an image of the red ball appears. It is probably fainter than in the previous case but it is still clear.

To explain the secondary recall, we will advance the *theory of strain signals.*

Beginning with the motor discharge which prompts the utterance but is not of sufficient intensity to cause muscular contraction, we are brought to the motor terminal in the muscle. Let us here make the following assumption:

When a nervous discharge to a motor terminal is too weak to cause contraction it will produce a chemical or molecular change in the muscle substance which spreads to the sensory terminals, causing an excitation of certain neurons, which we will term a strain signal. This change in the muscle substance requires about the same time as a muscular contraction.

With the aid of this assumption our explanation of the secondary recall will follow the same course as for the other recall. The strain signal acts upon the cortex just as a kinæsthetic excitation would and stimulates those very sensitive cortical neurons which give rise to the image. Thus we see again that the incipient utterance of the word ball has brought up an image of the color red. It is worth noting that between the two types of recall that we have discussed, there are possible stages where kinæsthetic impulses from some muscles are joined by strain signals from others to arouse the image.

We find that the theory of strain signals is in some conformity with the good old rule that what is true approaching a limit is true at the limit, for the image arises from a discharge coming from the muscle to the cortex, both when there is some contraction occurring and when the contraction is incipient only. Moreover the theory appears to be borne out by introspection as when you recall a song, the words seem to sound in your ears at about the same rate of succession as if you were singing them. Again, the intimate relation of nerve to muscle would indicate that a disturbance of the motor nerve, however faint, would cause a change of some kind in the muscle as the theory requires. It may be only a sort of ripple like a sound wave that traverses the muscle. If the reader has given much thought to such matters, he will be able to find other arguments in favor of the theory of strain signals.

It would take too long to discuss fairly the matter of "imageless" conscious processes or degrees of clearness or faintness of images. We may briefly note, however, that in considering these matters, one should keep in mind the rules for the formation of associations and the changes that occur during the development of a movement system or performance. As the performance is being perfected by practice, unnecessary movements are dropped. But the dropping of movements means the elimination of kinæsthetic impulses and also certain changes in the excitations due to the reaction of the environment. These eliminations and changes will naturally result in the fading and disappearance of images and in the formations of new associations. By way of illustration, remember that the images of Tuesday reflect the movements of Monday and prevail over the faded images that reflect the movements of Sunday. In the final stage when the performance has become automatic, the only paths of high conductivity will be those connecting the movements or essential to the performance.

Finally it is submitted that the considerations brought out by the author regarding attention would be met by the theory of strain signals equally well as by the author's theory of incipient motor processes involving induced discharge.

Comparing the two theories, we observe that the author's theory assumes that a motor discharge that is too faint to cause contraction of the muscle is strong enough to induce a discharge in the extremely sensitive tributary cortical center. The theory of strain signals assumes that a motor discharge that is too faint to cause contraction is strong enough to excite certain sensory terminals in the muscle which have communication with cortical centers.

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THE PSYCHOLOGICAL REVIEW

THE THEORY AND PRACTISE OF THE ARTIFICIAL PUPIL

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So far as the writer can ascertain the references in the literature to the theory and application of artificial pupils, although not infrequent, are quite unenlightening. Yet in all work upon the visual processes in which the amount of light energy striking the retina has to be controlled the artificial pupil would seem to be an indispensable accessory. Its value in exact studies upon visual acuity is also self-evident.

The intensity of the light which strikes the retina at any time is determined not only by the intensity and distance of the source, but also by the size of the pupil. It is useless to experiment upon the effects of lights of different objective intensities upon the retina if the reaction of the pupil to these lights is disregarded, for as soon as the objective intensity is increased the pupil contracts, and *vice versa*, so that there is a tendency for the retina to receive a constant illumination, independent of changes in the intensity and distance of the stimulus. This tendency fails to be effective only for very bright or for very dim lights, for which the pupil has attained, approximately, its minimum or maximum opening.

In addition to the compensating effect just mentioned the behavior of the natural pupil offers another difficulty to the student of retinal physiology in the continued fluctuations in opening which it exhibits even for a constant illumination. These fluctuations are periodic in character, but they follow no definite law, and the average aperture about which they hover varies for different persons and for the same person at different times. Such variations are sufficient to render impossible accurate comparative tests of retinal sensitivity without the introduction of some further artifice.

A partial solution of the difficulty lies in temporarily disabling the pupillary reflexes by the use of such drugs as homatropin and pilocarpin. This procedure, however, is not feasible in extensive researches on account of the discomfort which it entails for the subjects. Moreover, it does not insure the same pupillary opening for different persons, or even, at different times, for the same person, so that if results are to be made comparative the size of the pupil must be measured for each series of observations.

The simplest and surest way in which to eliminate the influence of the pupil upon retinal measurements would seem to be to place in front of the natural pupil a diaphragm the aperture of which is smaller than the smallest aperture of the natural pupil, and which is concentric with the latter. It is the purpose of the present paper to discuss the theory and practice of such an "artificial pupil."

The three important problems which are involved in the use of the artificial pupil consist in the determination of the proper size of the diaphragm, of the distance from the eye at which it must be placed, and the invention of some means of insuring the coincidence of the axis passing through the center of the stimulating field and that of the diaphragm with the center of the natural pupil. The geometrical and optical conditions under which the artificial pupil must be employed make it necessary for the stimulus to be distinctly limited in angular size, and thus make impossible its application in the study of vision in the extreme periphery.

The necessary size of the aperture of the artificial pupil depends upon four variables: (1) the diameter of the surface used as a stimulus, (2) the distance of this stimulus from the eye, (3) the minimal size of the natural pupil under this sort of illumination, and (4) the distance between the artificial and natural pupils. The main condition for the successful use of the artificial pupil is that it should be so adjusted that none of the light from the stimulus is intercepted by the iris of the eye itself.

Although the refraction which occurs as the light passes through the surface of the cornea narrows the pencil of rays, practical considerations nevertheless demand that the artificial pupil be smaller than the natural one at any time. In the ensuing discussion we shall neglect the effect of refraction at the corneal surface since the error which such neglect introduces into our calculations merely contributes to the large margin of safety which is necessary, at all events in the use of the artificial pupil. The argument becomes more

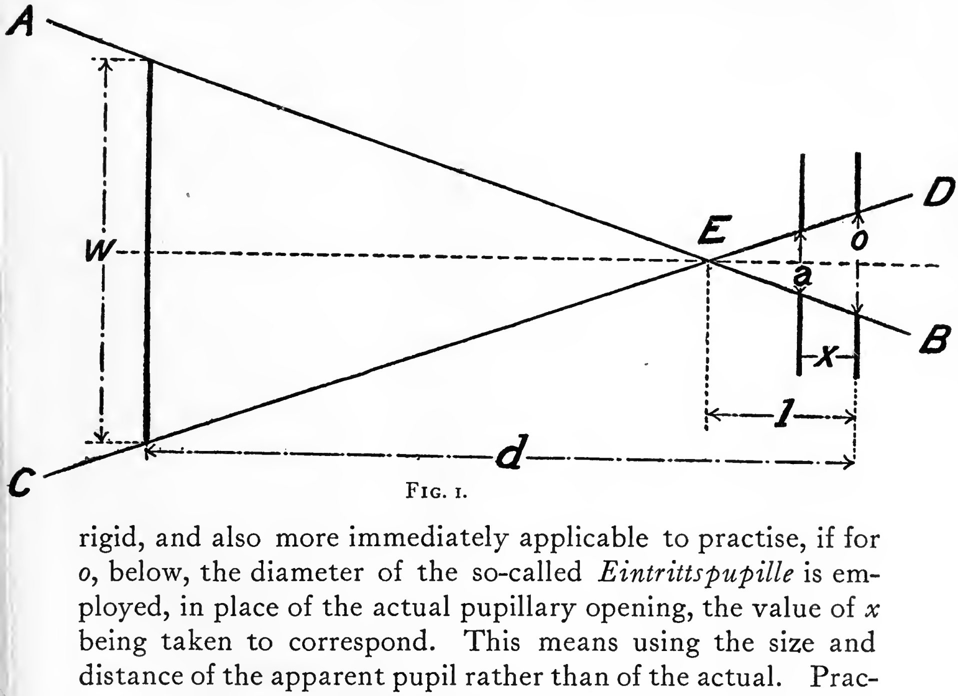

FIG. 1.

rigid, and also more immediately applicable to practise, if for o, below, the diameter of the so-called *Eintrittspupille* is employed, in place of the actual pupillary opening, the value of x being taken to correspond. This means using the size and distance of the apparent pupil rather than of the actual. Practically, however, the difference between these two cases may be neglected.

The accompanying diagram, Fig. 1, represents in cross-section the arrangement of the artificial pupil with respect to the natural pupil and the stimulating surface. a is the diameter of the artificial pupil, o that of the natural pupil, w that of the stimulus. d is the distance from the plane of the stimulus to that of the iris, while x is the distance between the iris and the artificial pupil. The lines AB and CD represent those light rays which form the *critical* boundaries of the rays passing through the artificial pupil. (It may not be immediately obvious why these lines are the ones which it is important for us to consider, rather than the *external* boundaries of the whole pencil of rays, but a study of the diagram will make this clear.) The point of intersection, E, of the lines in question has critical significance, and may be called the *crossing-point*, the distance of this point from the plane of the natural pupil being l.

Inspection of the diagram shows the following relations to be true:

$$\frac{o}{w} = \frac{l}{d - l}, \tag{1}$$

$$\frac{o}{a} = \frac{l}{l - x}. \tag{2}$$

If we solve for l in (1), and for a in (2), and then eliminate l, we get:

$$a = \frac{o(d - x) - wx}{d}, \tag{3}$$

which gives us the maximum diameter of the artificial pupil which will satisfy the prescribed conditions.

On account of the relative convergence of the pencil of rays after it has passed through the cornea the diameter calculated by the above formula would not, strictly speaking, be the largest available opening. However, in practise it would be very unsafe to utilize an opening closer to the maximum. There are two reasons for this: first, the fact that slight accidental movements of the eye and head are unavoidable, even with the best of head-rests and fixation, and, second, the fact that the natural pupil is subject to constant fluctuations in size. Ordinarily it is advisable to work with an opening at least two millimeters smaller than the maximum for the smallest aperture of the natural pupil which is to be expected in the course of the observations.

The distance, l, of the crossing-point from the plane of the iris may be calculated from the formula:

$$l = \frac{do}{o + w}, \tag{4}$$

which follows from (1), above. The position of this point depends upon the size and distance of the stimulus and upon the aperture of the natural pupil. A knowledge of it is important, since if the artificial pupil is placed in front of the crossing-point it will necessarily fail in its purpose, no matter how small it is made. The distance of this point from the eye is, in general, of sufficient magnitude so that the artificial pupil may be placed in a position comfortable to the observer. For example, when the diameter of the stimulus is 5 centimeters, its distance 1 meter, and the natural pupil 3 millimeters, l is approximately 6 centimeters.

To determine the maximum admissible diameter of the stimulus under given conditions, the original equations may be solved for w, with the result:

$$w = \frac{d(o - a) - ox}{x}. \tag{5}$$

With, for example, a pupil aperture of 4 millimeters, an artificial pupil of 1 millimeter, and the values of x and d used in the example above, we find w to be approximately 30 centimeters. The corresponding angular aperture is 17°. It is clear that if we wish to increase the angular size of the stimulus we must decrease a or x or both. The limit for x is the distance from the iris to the cornea, *viz.*, a little under 4 millimeters, that of a is zero. Substituting these values in place of those first employed we get: w = (approximately) 1 meter. This corresponds to an angular opening of about 53°. It would appear to be the maximal size of stimulus in connection with which the artificial pupil can be used under ordinary conditions.

Evidently, however, this maximum is of such a character as to make the artificial pupil available in the study of all of the color perceptive regions of the retina, since these do not extend, in general, beyond 50°. In practise, of course, it would be necessary for the pupil to have a finite aperture and to be removed from the cornea by a distance somewhat greater than that allowed in the above calculation (*viz.* .5 mm.).

The best possible conditions for the use of the artificial pupil would be those which go with complete mydriasis. Under these conditions the aperture of the natural pupil is about 7.5 millimeters. With an artificial pupil of 1 millimeter aperture, and a distance from the iris of 8 millimeters, the angular size of the largest available stimulus would be about 46°. The worst possible conditions are those of complete miosis, which give a natural pupil of about 1.5 millimeters. The maximum angular size of the stimulus for the latter conditions is approximately 11.5°. These maxima include the margin of safety, introduced by corneal refraction, which was mentioned at the outset. In general, of course, the artificial pupil would not be used in connection with the drugs which are customarily employed to produce mydriasis and miosis, and consequently it is necessary to base one's calculations upon the so-called physiological pupil, which lies between 3 and 4 millimeters for a considerable range of intensities of the stimulus.

The final, and perhaps the most difficult problem which must be solved in the use of the artificial pupil is that of securing what may be called *register* between the natural and the artificial diaphragms. Perfect registration may be defined as a disposition of the eye with reference to the artificial pupil such that the axis passing through the centers of the stimulus field and the artificial pupil, and perpendicular to the planes of these, also passes through the center of the natural pupil, and is perpendicular to the plane of the iris. Under these conditions the projection of the natural pupil on the plane of the artificial pupil is concentric with the latter.

The conditions accompanying the use of the artificial pupil are such as to make it difficult, if not impossible, to secure or test registration by objective observations. Approximate registration can be obtained by moving the head slightly with the eye in position, since when the artificial and natural pupils do not coincide the intensity of the stimulus appears to be reduced. However, in most work in which the artificial pupil is helpful it is desirable that registration should be secured before the eye is exposed to the action of the stimulus.

One method of securing accurate registration would be to place two dimly illuminated diaphragms concentrically on the axis passing perpendicularly through the center of the stimulus field, these diaphragms to be at different distances from the latter and of such size that, when the eye is in position, the edge of the farther one can be seen framed in that of the nearer. If the artificial pupil now be placed concentric with the diaphragms the eye will be in register when the framing of the one diaphragm in the other appears concentric.

A neater and somewhat simpler method of insuring registration—the one now in use by the author—is the following:

It is a well-known fact that if a small source of light be held very close to the eye it will be seen not in its true form, but as a relatively large "diffusion circle,"[1] fluctuating in size with the constant changes which are occurring in the size of the pupil. Such a circle is in reality a luminous shadow of the natural pupil, and if the point of light be on the line of sight the diffusion circle which it produces will be concentric with the fovea, provided, of course, that the pupil itself is not eccentric. If, now, a true circle be placed concentric with the same axis, but at a greater distance, so that a more or less distinct image of it can be formed on the retina, this image will be seen to be concentric with the diffusion circle, but if it is displaced it will become eccentric with respect to the latter.

If an adequately small artificial pupil be placed in front of the eye and in register with the natural pupil the size of the diffusion circle will be determined by the former instead of

[1] On the theory of the "*Zerstreuungskries*," see: Helmholtz, "Handbuch der physiologischen Optik," 3d ed., 1909, Vol. 1, pp. 101–120.

the latter. With perfect registration the *Zerstreuungskreis* in question will be seen to be concentric with the image of the second circle mentioned above, but when the registration is imperfect the two will be eccentric with respect to each other. We are thus provided with a very accurate means of securing and of testing registration.

Fig. 2 represents, somewhat diagrammatically, an element of the artificial pupil apparatus which the writer has been using in his investigations concerning retinal fatigue, and

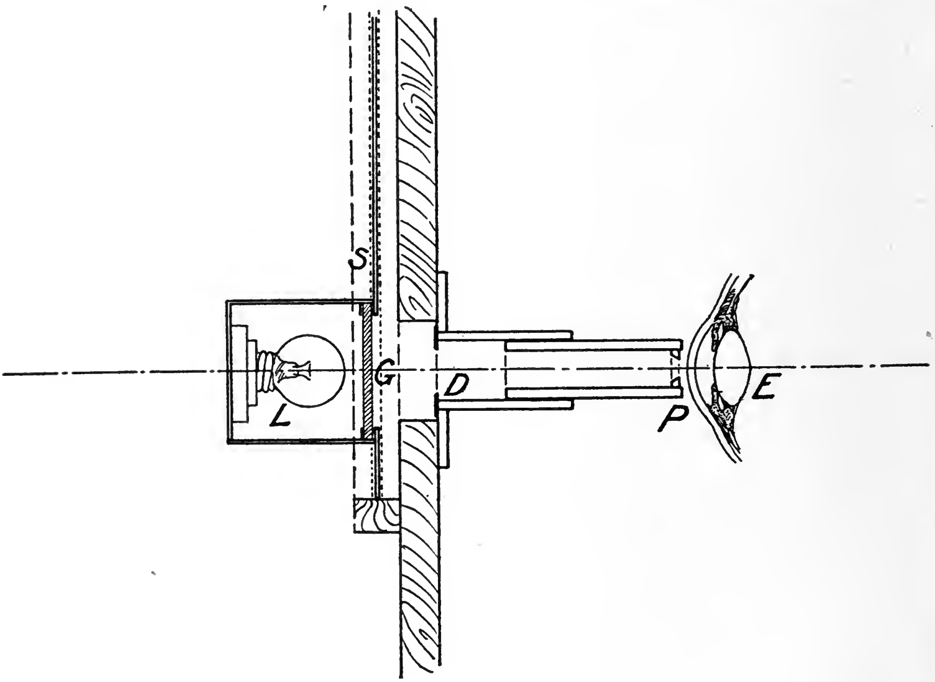

Fig. 2.

which embodies in a general way the principle just described together with the others previously discussed. The pupillary diaphragm, *P*, is held at the end of a small telescoping tube before the other end of which a small electric light, *L*, carrying in front of it a piece of opal glass, *G*, can be let down by the movement of a shutter, *S*. When this light is in position and the eye is held opposite the artificial pupil, the reflection of the light from the interior walls of the tube near to the eye produces a large diffusion circle upon the retina. Within this circle of light is seen a smaller, dark, circular ring which is constituted by the image of the circular diaphragm, *D*, at the far end of the tube. When these two circles are seen to be concentric the line of vision must coincide with the axis of the artificial pupil and the diaphragm in question. The conditions described are those of approximate registration. In practise, registration is secured by a brief exposure of the eye of the subject to the test light—which can and should be very dim—during which exposure he adjusts his head so that the two circles are seen as concentric, fixation being directed to the center of the inner one. The head is then held firmly in position, the test lamp is extinguished, and the eye is given a period of rest sufficient to remove the effects of the faint stimulation which it has thus received. When the absence of such effects is insured the shutter is raised and the stimulus proper is exposed. When the observation is completed the shutter is again dropped, the test lamp lighted, and the state of concentricity or eccentricity of the circles is again noted. The author has found that his subjects have little difficulty in maintaining practically perfect registration in this way during periods of several minutes' duration, a simple head and chin rest being employed.

The diaphragm, *D*, acts to prevent the relatively small stimulus field from illuminating the walls of the tube, although it does not interfere with such illumination by the registration lamp. The interior of the tube should, of course, be painted black, and the current supplied to the lamp should pass through an adjustable resistance so that the intensity of the light may be easily reduced to a minimum. The apparatus as figured is not applicable to maximally large stimulus fields.

Neither of the two methods described above can be relied upon to give *perfect* registration unless a possible anatomical eccentricity of the natural pupil is taken into consideration. According to Gullstrand,[1] such eccentricity is the rule rather than the exception. Usually, however,

[1] A. Gullstrand, Appendix to Helmholtz's "Handbuch der physiologischen Optik," 3d ed., Vol. 1, 1909, pp. 270–272.

the lack of concentricity is not marked, so that if the artificial pupil employed is relatively small—say one half the diameter of the natural pupil—registration of the line of sight, which is obtained by the methods in question, may be relied upon for practical purposes.

In careful work, however, the pupils of the subjects should be measured to determine their eccentricity. Registration may then be effected by having each subject so place his eye that the inner circle is eccentric with respect to the diffusion spot in such a direction and to such a degree as to correct for the eccentricity of the pupil.

One way of testing the concentricity of the natural pupil, which will at the same time educate the subject in the amount of eccentricity necessary to correct the registration of each eye, is the following. A series of trials may be made in which the *stimulus* is viewed through the artificial pupil. Each time the head is adjusted by trial and error so that the stimulus field appears as bright as possible, a position being found which is as close as may be to the center of the range of maximum brightness, then this range has an appreciable magnitude. When this position has been secured the head is held rigidly against the head-rest and the registration lamp is dropped into place, the degree of eccentricity of the two circles being noted and recorded. The average of a number of such determinations may be used for correcting the registration.

The diffusion-circle method of securing registration permits a very simple qualitative test as to the adequacy of the register in a given instance, since the outline of this circle is determined by the effective pupil. If the registration is inadequate, fluctuations in the outline of the circle will be apparent, due to the pulsating contractions and expansions of the natural pupil. The writer has found that with an artificial pupil of two millimeters (diameter), using a small but bright stimulus in a dark room, registration of the line of sight is adequate for most subjects, although in no subjects which he has examined has such registration proven itself perfect.

THE TEMPORAL RELATIONS OF MEANING AND IMAGERY

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I. The Problem

The experiments here reported constitute a part of a more extensive study of memory and perception, which will probably be made public in the future. The work was done in the laboratory of Professor Külpe at Munich. The part now published cannot, however, be properly evaluated without some indication of the nature of the results obtained in the first section of the more extensive study. This first part consisted in an introspective investigation of the mental processes involved in perception and recall.

The material for experiment in the unpublished section consisted of spoken words, printed words, printed pictures and real objects. A series of eight words, pictures or objects were presented to the subjects. Their task was to repeat what they had seen or heard and then to give an introspective account of the mental processes they had experienced during the perception of the series and during their attempts to reproduce the same from memory.[1] The subjects were asked particularly to give an account of the temporal sequence of events as they had experienced them.

It was rather remarkable that in perceiving, the first thing in consciousness was reported as meaning the second some kind of imagery. Whereas in repeating the first thing was often an image whose meaning was understood and then designated by a word.

A few introspections will bring out more clearly what is meant by this assertion.

[1] A fuller description of the details of the technique will be given when the entire work is made public.

Perception of Printed Words

"I notice now a certain regularity in this process. With the first word, the meaning appeared with the reading, without any clear visual image of the object thereby designated. The same process takes place on the continuation of the series of words. Gradually it goes on so rapidly that during the period of exposition (2 seconds) there is time to apprehend a goodly number of apperceptive complexes, which become associated with the imaged object. The steps in the process —so far as I can notice them—are:

"1. Apprehension of the meaning.

"2. Imagery of the object—generally by means of memory images.

"3. Associations which are connected with the object."

Subject Lehner, Nov. 17.[1]

Perception of Pictures

"I look at the picture and generally have its meaning at once. Often I am not entirely certain, *e. g.*, spoon or trowel. When I have the meaning, its naming follows immediately." —Grüninger, Dec. 17.[2]

"In the perception of the several pictures, I notice that I experienced auditory-motor words in immediate connection with them, and that these words followed with varying rapidity the individual pictures. It lasted some time till I got the word 'Mitre.' In this experience it appeared to me that the rapidity with which the word comes, does not depend as much upon the finding of the words as it does upon the

[1] Ich merke jetzt eine gewisse Gesetzmässigkeit des Prozesses. Bei dem ersten Wort tritt mit dem Lesen die Bedeutung bewusst auf, ohne deutliches Gesichtsbild des darin fixierten Objektes. Derselbe Vorgang vollzieht sich bei der Fortsetzung der Reihe, allmählich mit so grosser Schnelligkeit dass während der Exponierungszeit noch Zeit bleibt eine ganze Fülle von Apperceptionsmassen bewusst zu erfassen, die sich an das vorgestellte Objekt noch knüpfen. Die Stufen so weit ich sie bemerken kann sind: 1. Erfassung der Bedeutung. 2. Vorstellung des Objektes, gewöhnlich durch Erinnerungsbilder. 3. Associationen die sich an das Objekt knüpfen. (17ten. Nov.)

[2] Ich sehe das Bild and und meistens habe ich sofort die Bedeutung. Manchmal bin ich nicht ganz sicher z. B. Löffel oder Kelle. Wenn ich die Bedeutung habe, folgt sofort die Benennung.

recognition of the picture. It is on this account that I would willingly have looked longer at the pictures. The words served as designations for the pictures or if you will the objects represented by the pictures, and had another sense, a more general meaning than their relation to the individual pictures or their objects."—Subject Külpe, Nov. 14.[1]

Repetition of Objects

"On repeating, there comes to me all of a sudden a visual image. When this image comes promptly it is usually complete. But when I must think awhile, there comes to me first of all something striking in the object. Then come further qualities, *e. g.*, to the color the form. As soon as this process of supplementing has developed to a certain point, the meaning is all of a sudden present. As soon as I have the meaning, the object seems to become still clearer. *E. g.*: All of a sudden I see the typical lustre of a pearl. Then there comes to me the round form and then all at once I know what it is."—Subject Grüninger, Dec. 10.[2]

Were meaning in some manner identical with imagery, or were it produced by imagery or the imaginal context of a sensation as Titchener suggests is often the case, we should expect just such introspections as this from our subjects—not however for memory but for perception. That they are

[1] Ich bemerke dass ich bei der Wahrnehmung der einzelnen Bilder sofort akustisch-motorische Wörter in Anschluss an sie erlebt habe, und dass diese Wörter in verschiedener Geschwindigkeit sich an die einzelnen Bilder anschlossen. Bei dem Wort Bischofsmütze, z. B., dauerte es ziemlich lang bis ich es fang. Dabei schien die Geschwindigkeit des Auftretens der Wörter nicht sowohl in der Wortfindung selbst als vielmehr in der Erkennung des Bildes begründet zu sein. Damit hängt es zusammen dass ich einige Bilder gerne länger betrachtet hätte. Die Wörter galten als die Bezeichnungen für die Bilder bzw. die Gegenstände die in ihnen dargestellt waren, und hatten einen anderen Sinn, eine allgemeinere Bedeutung als die Beziehung auf die einzelnen Bilder oder ihre Gegenstände. (14ten Nov.)

[2] Beim Hersagen taucht einfach ganz plötzlich ein optisches Bild auf. Wenn das Bild schnell auftritt, dann ist es meistens vollständig. Wenn ich einige Zeit suchen muss, dann taucht zuerst etwas besonders auffälliges am Gegenstand auf. Dann kommen weitere Qualitäten, z. B. zur Farbe die Form, und sobald diese Ergänzung einen grösseren Grad erreicht hat ist die Bedeutung auf einmal da, und sobald ich die Bedeutung habe, scheint mir der Gegenstand noch deutlicher zu werden. Z. B. Ich sehe auf einmal den eigenartigen Glanz der "Perle." Dann kommt mir die runde Form, und dann auf einmal weiss ich was es ist. (10ten Dez.)

found in memory and not in perception is strong evidence against any such theory. Here the nature of the occurrence points to the fact that an image as such means nothing just as Professor Titchener himself claims. It must be interpreted. It can be interpreted only when sufficient data is present. When this is the case, the subject *knows* what it is. This knowledge of what the image represents is not reported as a sensory element added to the elaboration of the image. A new image would itself have to be recognized. The interpretation of the image is a *knowing*. It is something which follows the awareness of the image just as understanding follows the sensations involved in perception.

Repetition of Pictures

"The repetition took place in this manner: First I thought of the first member of the series. Then without holding more strictly to the order of perception each word was spoken following an imaginal representation of the pictures. When I stopped, I attempted to bring up to myself the series. Only by the rising up of a visual image did I obtain a new word." —Subject Külpe, Nov. 14.[1]

Such introspections as these suggested a further investigation. The subjects had noticed a certain sequence of events in the process of perception. Would it be possible to react to the events that had been noted? If meaning comes before imagery in the perception of printed words, would it be possible for the subject to react, now to imagery and now to meaning? And if so, what would be the quantitative results?

In the experiments here reported this problem was attempted, to investigate, namely, by means of reaction time the temporal relations of meaning and imagery in the perception of printed words and pictures. The experiments were made in the psychological laboratory of Professor

[1] Das Hersagen geshah so dass ich mich zunächst auf das erste Glied der Reihe zurückbesann. Danach wurden die einzelnen Wörter im Anschluss an die anschaulichen Vorstellungen der Bilder ohne die Ordnung der Wahrnehmung strenger einzuhalten ganannt. Wenn ich stockte, suchte ich mir die Reihe wiederzuvergegenwärtigen und bekam erst durch eine neue Auftauchung des Vorstellungsbildes ein neues Wort (Nov. 14th).

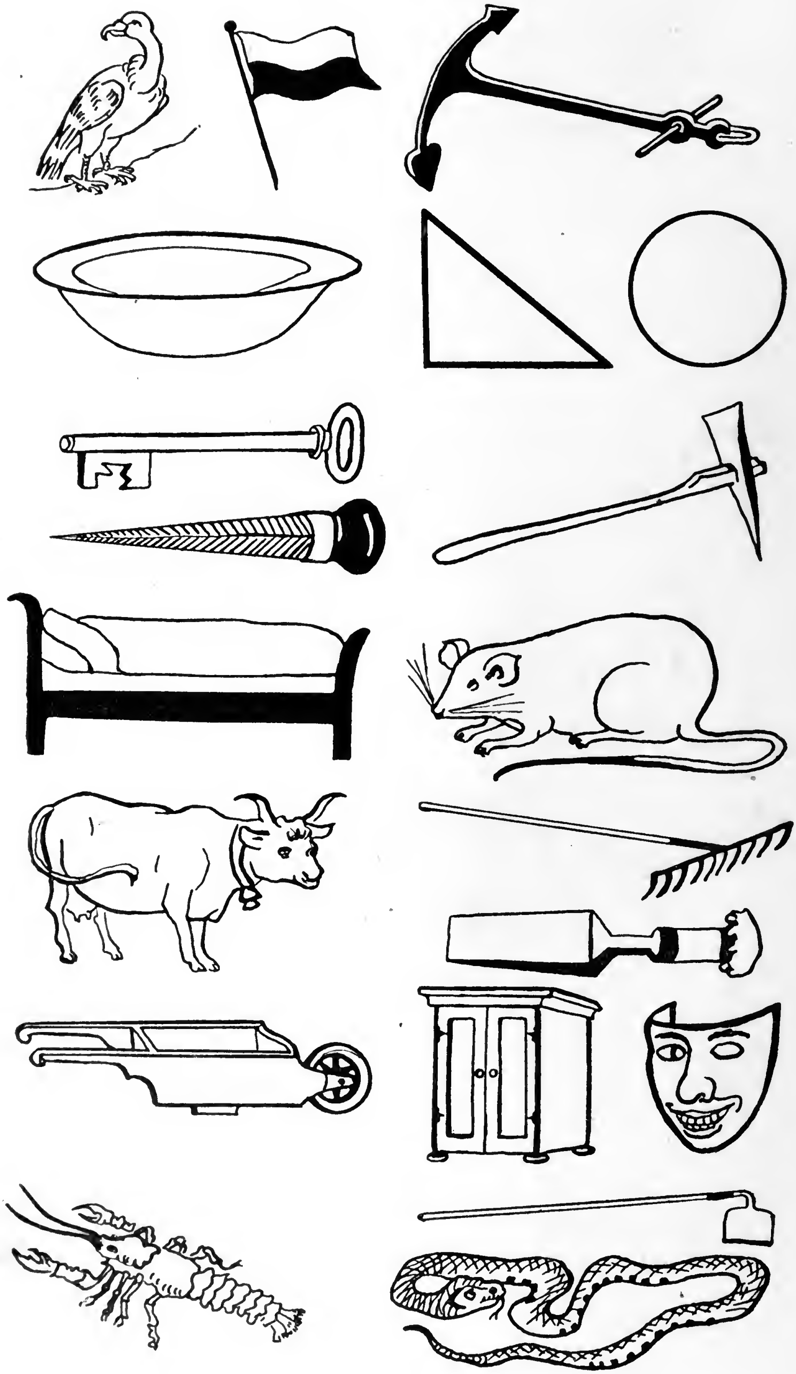
Külpe in Munich during the winter semester of 1913–14 and the summer semester of 1914. The author wishes to take this opportunity to thank Professor Külpe for his great kindness, for his interest and suggestions, and for the sacrifice of his time as subject.

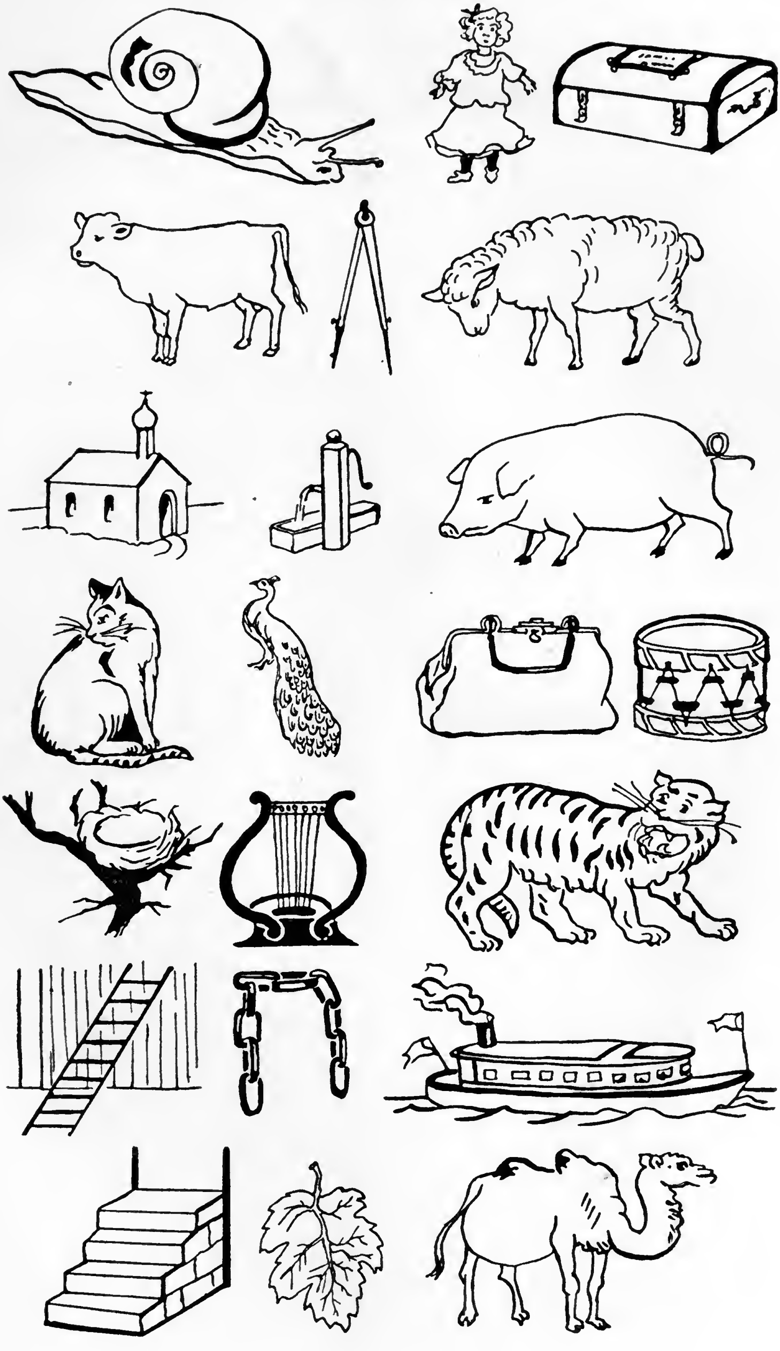
II. Method of Research

The words and pictures used in these experiments designated simple familiar objects—all capable of being visualized; *e. g.*, tree, lamp, knife. Abstract words, prepositions, etc., were not used, in order that conditions might be as favorable as possible for the development of imagery. Had such words been used the difference that was found in reaction time for meaning and imagery would have been much greater. The use of such words would indeed have been justified. For if sensations and images must explain all meanings they must be involved, and exclusively involved, not merely in the perception of things that can be immediately sensed, but also in more abstract mental content. In order, however, to test the theory on the ground where it is best able to stand, it was concluded to forego the use of any words except those that represented familiar sensory objects.

The accompanying plates give an insight into the material used in these experiments. Most of them represent objects that can be named by a one or two syllable German word. The words used were printed on cards in a large legible type.

The use of control words and drawings enabled one to be sure that the subjects were actually reacting to meanings. The controls used in the series of words were nonsense combinations of letters forming one or two syllables. The controls used in the series of pictures were meaningless drawings. In general, the subject was instructed to react (by releasing a telegraph key) in case the word or the drawing represented some real object. The words were exposed by a combination memory and tachistoscope apparatus. The reaction times were measured by a Hipp chronoscope. This was controlled by a pendulum constructed in accordance with a design by Professor Külpe. The variable error in the





chronoscope was negligible—averaging less than 3 σ. The constant error was about 70 σ. Nine subjects took part in the experiments. A preparatory signal (1–½ sec.) was given verbally with the aid of a stop watch.

III. Simple[1] Meaning and Visual Imagery

(*a*) *Quantitative Results*

The instructions to the subject in this experiment will indicate the precise nature of the problem. They are reproduced without translation. The subject read them over at the beginning of each period. A few trial periods were necessary for some subjects in order that they might learn not to react to the control word. These preparatory series were not included in the final results. One of our subjects (Gl.) never did get free from erroneous reactions and his results show a marked difference from the others.

Sie werden nach einem Signal ein Wort zu sehen (bzw.zu hören) bekommen. Ich bitte Sie zu reagieren wenn Sie das Wort verstanden oder seine Bedeutung erfasst, bzw. wenn Sie eine Gesichtsvorstellung von dem durch das Wort bezeichneten Gegenstand gehabt haben.

Die Wörte 'Bedeutung' und 'Vorstellung' werden Ihnen angeben ob das eine oder das andere verlangt wird. Nachher bitte ich mir kurz das Erlebnis zu charakterisieren, and dabei anzugeben, ob die aufgetauchte Vorstellung an die Stelle der Bedeutung gesetzt werden konnte, etwa bloss die konkrete anschauliche Erfüllung dessen war, was in der Bedeutung abstrakt intendiert wurde.

In this series, therefore, the subject reacted either (*a*) To the awareness that the word had a meaning, or (*b*) To the awareness of the visual image of the object.

If there is no difference between meaning and the visual image of an object represented by a word the average of the two series should be approximately the same. The subject ought not to be able to distinguish meaning and imagery

[1] By 'simple meaning' is not meant an absolute simplicity. The word is used to contrast this set of experiments with a later one where the more complex consciousness of purpose was required.

and this should manifest itself in averages for the two sets of reactions that approached each other within the limits of experimental error. If meaning, however, is produced by or is identical with the visual image which accrues to the sensations involved in the perception of the word, the image series should be shorter if anything than the meaning series. The results are given below. The tables are clear without any explanation, except perhaps, that in column *T* is given the

SUBJECT Gl

Words

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
23/VI	Zange	1,182	628	23/VI	Rechen	368	154
	Fernglas	563	9		Besen	563	41
	Pfeil	233	321		Gabel	751	229
26/VI	Messer	639	85	26/VI	Truthahn	686	164
	Lampe	640	86		Esel	676	154
	Sichel	660	106		Trichter	550	28
7/VII	Säbel	472	82		Dreick	602	80
	Stuhl	527	27		Nest	662	140
	Eimer	429	125	7/VII	Aal	558	36
10/VII	Baum	472	82		Geige	338	184
	Trichter	346	208		Fernglas	597	75
	Korb	482	72		Besen	424	98
		12)6,645	1,831		Korb	462	60
		554	152		Maske	249	273
				10/VII	Meissel	351	171
						15)7,837	1,887
					Mean	=522	126
					Median	=563	

Reactions to visual imagery equal or below median = 8.

SUBJECT GRÜNINGER

Words

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
11/II	Auge	902	341	11/II	Schuh	682	18
	Ballon	1,092	151		Nase	787	87
16/II	Kuh	1,660	417	16/II	Fass	661	39
	Bohrer	1,277	34		Schwan	659	41
	Sofa	1,261	18		Schaf	781	81
25/II	Stiefel	1,229	14	25/II	Ring	531	169
	Schuh	1,103	140		Auge	680	20
	Fass	1,462	219		Ballon	705	5
	Nase	1,200	43		Fass	660	40
					Kuh	856	156
		9)11,186	1,377			10)7,002	656
		1,243	153		Mean	=700	65
					Median	=681	

Reactions to visual imagery equal to or below median = 0.

Subject Külpe

Words

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
9/II	Löwe	1,690	790	9/II	Kerze	517	14
13/II	Ballon	944	44	13/II	Schuh	538	7
	Auge	675	225		Ring	434	97
	Rose	1,097	197		Nase	799	268
16/II	Fliege	802	98	16/II	Dampsfchiff	540	9
	Veilchen	753	147		Fass	573	42
	Kuh	848	52		Ballon	708	177
	Kerze	839	61	23/II	Fliege	420	111
23/II	Schuh	897	3		Schuh	577	46
	Nase	813	87		Löwe	607	76
18/5	Ring	800	100		Veilchen	375	156
	Dampsfchiff	649	251	18/V	Rose	460	71
		12)10,807	2,055		Kuh	363	168
		900	171			13)6,911	1,242
					Mean	= 531	95
					Median	= 540	

Reaction to visual imagery equal to or below median=0.

Subject Lehner

Words

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
	Rechen	381	263		Schwan	445	24
16/II	Buch	1,131	487		Bohrer	1,007	538
	Rettich	611	33	16/II	Sofa	663	194
	Nase	604	40		Rose	317	152
	Trichter	584	60		Brief	691	222
	Dreieck	490	154		Palme	500	31
	Blatt	597	47		Baum	399	70
30/VI	Bär	606	38		Hirsch	389	80
	Krone	479	165		Spaten	406	63
	Veilchen	488	156	30/VI	Aal	407	62
	Treppe	631	13		Krug	395	74
	Ofen	572	72		Tasse	489	20
	Säbel	588	56		Schädel	426	43
	Geige	461	183		Ohr	435	34
3/VII	Pfeil	451	193		Spaten	518	49
	Meissel	515	129	3/VII	Hirsch	371	98
	Fernglas	874	230		Baum	483	14
	Flasche	758	114		Palme	524	55
	Krug	691	47		Bär	368	101
	Tasse	736	92		Krone	384	85
7/VII	Schädel	918	274	7/VII	Ofen	429	40
	Ohr	844	200		Veilchen	344	125
	Ballon	803	159		Treppe	398	71
		23)14,813	3,205			23)10,788	2,245
		644	139		Mean	=469	97
					Median	=429	

Reaction to visual imagery equal or below median=1.

Subject Marezoll

Words

Date	Visual Image	T	V	Date	Simple Meaning	T	V
11/V	Ohr	1,308	221	11/V	Käfer	841	254
	Zwicker	1,633	104		Kerze	638	051
	Eule	1,598	69		Storch	831	244
12/V	Schnecke	1,374	155	12/V	Katze	576	011
	Pfau	2,721	1,192		Kuh	822	235
	Käfer	3,868	2,339		Schlange	761	174
	Maus	2,610	1,081	14/V	Löwe	774	187
14/V	Schaf	1,309	220		Stier	458	129
	Hirsch	1,222	307		Geier	325	262
	Zirkel	1,210	319		Ohr	508	079
16/VI	Eimer	1,021	508	16/VI	Eule	·398	189
	Stuhl	947	582		Schnecke	676	089
	Sichel	870	659		Schlange	582	005
	Kerze	1,187	342		Storch	693	106
	Geier	1,367	162		Kuh	650	073
	Stier	988	541	19/VI	Kerze	465	122
19/VI	Löwe	1,607	78		Katze	684	097
	Schlange	1,292	237		Maus	456	131
	Kuh	1,571	42		Pfau	436	151
	Katze	1,146	383		Schaf	697	120
	Storch	1,258	271		Hirsch	384	203
				30/VI	Zirkel	406	181
					Sichel	594	007
					Stuhl	425	162
					Eimer	557	030
		21)32,107	9,812			25)14,687	3,292
		1,529	467			Mean =587	152
						Median=582	

Reactions to visual imagery equal or below median=0.

Subject Moore

Words

Date	Visual Meaning	T	V	Date	Simple Meaning	T	V
	Schuh	839	330		Finger	516	53
	Sofa	1,685	516		Buch	337	126
9/VI	Rechen	1,112	57	9/VI	Schere	444	19
	Auge	1,448	279		Bürste	572	109
	Lampe	1,145	24		Fernglas	563	100
	Dampsfchiff	857	312		Uhr	401	62
	Ochse	1,553	384		Schuh	484	21
12/VI	Frosch	1,009	160		Tiger	758	295
	Kamm	794	375	12/VI	Vogel	259	204
	Bürste	815	354		Rechen	386	77
	Pinsel	1,091	78		Fernglas	396	67
	Besen	1,108	61		Sofa	245	218
	Handbeil	1,188	19		Kamm	572	109
30/VI	Vogel	1,155	14	14/VII	Auge	583	120
	Tiger	1,275	106		Frosch	436	27
	Uhr	1,209	40		Bürste	440	23
	Schere	937	232		Dampfschiff	478	15
14/VII	Schuh	1,445	276			17)7,870	1,645
	Rechen	1,544	375				
		19)22,209	3,992			Mean = 463	96
		1,169	210			Median = 444	

Reactions to visual imagery equal or before median=0.

Subject Scherren

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
22/VI	Krebs	9,807	5,140	13/VII	Fliege	656	104
	Stern	4,537	130		Fächer	988	228
13/VII	Herz	3,370	1,297		Finger	846	86
	Trommel	6,505	1,838	21/VII	Herz	788	28
	Spinne	4,587	80		Fliege	1,034	274
	Koffer	2,531	2,136		Stern	1,080	320
20/VII	Flasche	3,159	1,488		Krebs	963	203
	Hirsch	3,411	1,256		Koffer	552	208
	Säbel	5,354	687		Spinne	697	63
	Traube	5,402	735		Trommel	561	199
	Engel	5,340	673		Säbel	631	129
	Fächer	2,007	2,660		Flasche	969	209
					Engel	378	382
					Traube	501	259
		12)56,010	18,120			14)10,644	2,692
		4,667	1,510			Mean =760	192
						Median=742	

Reaction to visual imagery equal or below median=0.

Subject Stappen

Words

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
12/II	Ring	572	87	12/II	Veilchen	604	63
	Auge	613	46		Fass	743	202
	Ballon	584	75		Kerze	514	27
	Stiefel	656	3		Löwe	509	32
	Schaf	634	25	17 II	Rose	497	44
17/II	Rettich	1,076	417		Kuh	423	118
	Nase	565	94	19/II	Haken	573	32
	Fass	600	59		Lilie	507	34
	Rechen	657	2		Storch	511	30
19/II	Nest	874	215		Sichel	614	73
	Säge	707	48		Lampe	516	25
	Maske	632	27		Sofa	485	56
	Uhr	518	141				
	Esel	537	122				
		14)9,225	1,361			12)6,496	736
		659	97			Mean =541	61
						Median=513	

Reactions to visual imagery equal or below median=0.

reaction times in thousandths of a second. Under *V*, the variations from the mean.

At the bottom of each column the mean reaction times and mean variations have been calculated. The median for the reaction times to meaning have also been determined.

With but one exception, our nine subjects show a marked difference in their reactions to meaning and imagery. The

SUBJECT T

Words

Date	Visual Image	*T*	*V*	Date	Simple Meaning	*T*	*V*
27/VII	Kette	1,129	58	27/VII	Puppe	747	255
	Nest	1,162	25		Bretzel	794	302
	Trichter	1,774	587		Mond	754	262
	Stuhl	890	297		Schädel	546	54
	Anker	1,306	119		Taube	884	362
	Ballon	1,257	70		Zirkel	753	261
	Auge	1,216	29		Hirsch	520	28
	Apfel	905	282		Engel	569	77
	Eimer	1,295	108		Besen	499	7
	Ofen	1,109	78		Pferd	517	25
	Kuh	995	192		Hund	498	6
	Tiger	1,260	73	28/VII	Geige	464	28
28/VII	Fass	1,132	55		Schiff	667	175
	Strumpf	879	308		Tiger	347	145
	Schrank	1,263	76		Schnecke	453	39
	Schlange	866	321		Pfau	554	62
	Fächer	1,259	72		Fliege	470	22
	Herz	1,845	658		Fahne	441	51
	Hahn	947	240		Finger	793	301
	Krebs	2,176	989		Stern	369	123
	Spinne	1,516	329		Trommel	335	157
	Hase	1,072	115		Koffer	525	33
	Schlitten	906	281		Nase	527	35
	Feile	1,541	354		Schlüssel	556	64
	Hand	900	287		Hammer	479	13
	Handschuh	871	316		Spaten	411	81
	Eule	999	188		Bär	401	91
29/VII	Kirsche	921	266	29/VII	Klavier	663	171
	Löwe	802	385		Kerze	419	73
	Leiter	1,415	228		Bleistift	381	111
	Fernglas	1,677	490		Nest	352	140
	Wiege	1,336	149		Wage	367	125
	Wurst	993	194		Bohrer	307	185
	Löffel	806	381		Fliege	304	188
	Haken	2,115	928		Trommel	376	116
	Veilchen	1,315	128		Brille	362	130
	Treppe	919	268		Lilie	369	123
	Bretzel	779	408		Blatt	303	189
	Turm	894	293		Bürste	312	180
	Trichter	1,030	157		Truthahn	301	191
		40)47,472	10,782			40)19,689	4,981
		1,187	269		Mean	= 492	122
					Median	= 474	

Reactions to visual imagery equal or below median=0.

one exception is not to be explained by individual difference in mental type, but rather by an anxiety to react as quickly as possible. At first he reacted to every nonsense word. He was then tried with pictures. Here again, every meaningless drawing elicited a reaction in spite of instructions. The reaction times at first varied around 100 σ. Later he was asked to wait each time and make a judgment that he had fulfilled the task given him. Even under these instructions, he continued to react occasionally to nonsense words—the following reaction being very much retarded. He finally gave up the experiments. What would have resulted had he by practice become entirely free from erroneous reactions one cannot say. It would seem, however, more fair to a just conclusion to exclude rather than include his results in our summary. Leaving aside the results of this subject, the reaction times to visual imagery were all but one above the median of the reaction time to meaning. It is worthy of note that this single exception is the first recorded reaction of this subject. (He had made several practice series before.) Our subjects made 150 reactions in all. Were it merely a matter

SUBJECT MAREZOLL

Wörter

	Visual Image	Simple Meaning	*D*
Ohr	1,308	508	+ 800
Eule	1,598	398	+ 1,200
Schnecke	1,374	676	+ 698
Pfau	2,721	436	+ 2,285
Käfer	3,868	841	+ 3,027
Maus	2,610	456	+ 2,154
Schaf	1,309	697	+ 612
Hirsch	1,222	384	+ 838
Zirkel	1,210	406	+ 804
Eimer	1,021	557	+ 464
Stuhl	947	425	+ 522
Sichel	870	594	+ 376
Kerze	1,187	638	+ 549
Geier	1,367	325	+ 1,042
Stier	988	458	+ 530
Löwe	1,607	774	+ 933
Schlange	1,292	761	+ 531
Kuh	1,571	822	+ 759
Katze	1,146	576	+ 570
Storch	1,258	693	+ 565
	20)30,474	11,425	19,259
	1523.7	571.2	962.9

of chance that reaction times to imagery should be longer than those to meaning we could find about 75 longer and 75 shorter. As a matter of fact, we find 149 longer and only one shorter. In spite then of the rather small number of reactions (conditioned by taking the introspective reports) there is overwhelming evidence to show that something more than chance has to do with the difference in reaction time to meaning and imagery. This difference is not due to the words used for imagery and meaning. Not only were the words in both cases representative of sensory objects but care was taken to repeat the same words in the two series. A table is given above comparing the reactions of one subject for meaning and imagery to the same words. Under *D* is given the difference between the two. The reaction time to imagery is always longer than to meaning. With some of our subjects the results are not so unanimous, the meaning reaction being occasionally longer. This is to be explained mainly by the effects of practice, though something is no doubt due to accidental variation.

(*b*) Introspective Data

From the quantitative results that have just been given, it is evident that the subjects give a different response when told to react to meaning or imagery. Were we to stop with the quantitative results we would not know very much about the nature of that difference. Is meaning simply an early stage in the development of the image? Is it a vague confused image? Is it merely the realization of the power to visualize the object? Is it the tendency of a number of images to crowd into consciousness? What is the difference? There can be no doubt that a considerable difference exists and it is of great importance to find out precisely what it is. This can be done by an examination of the subjects' introspective reports of what they experienced during their reactions. The reports were taken down by dictation immediately after the reaction and then re-read to the subject to insure their accuracy. The originals are in German and will be given in German and in English when a complete account of all the experiments is published.

Consciousness of Meaning	Consciousness of Visual Imagery
(1) *The meaning has a general character.* Kerze: "There came to me at once the word 'Light.' This was not a determination of the meaning, but only another word for it. The meaning was entirely general, as if I should say *a* candle, that is, any candle—every possible candle."—Külpe, 9/II.	(1) *The image is particular.* Sofa: "I have a rather good image and I did not pronounce the word. I see with great precision the brown color and the form of the object—but not of the entire object. I could derive several concepts from this one image. It looks like a large reclining chair. The image would not do for all sofas."—Grüninger, 16/II.
(2) *The universality of the meaning is not always absolute.* Ring: "As soon as I saw this word, I experienced an auditory motor stimulus, and immediately in connection therewith the understanding of that which the word signified. This was quite universal without being related to anything in particular—except the limitation to 'finger-ring.' I am distinctly conscious that a finger-ring was intended. I cannot remember an image of any such ring."—Külpe, 13/II.	(2) *The image is at times schematic.* Herz: "I read the word 'heart' and apprehended its meaning. I remembered my task and sought after an image. I projected over the place of the card a heart of regular mathematical proportions. Only the contours were imaged, and these by such an airy line that I question myself whether I had a visual image at all or whether it was an ideal construction, such as one carries out in mathematical thinking."—Scherren, 13/VII.
(3) *The meaning is at times felt to be incomplete, because of an unanalyzed consciousness of what the word signifies.* Schere: "At first, a feeling of familiarity was present and then a feeling of certainty that I know what the word signifies without having analyzed its meaning any further. First, during the reaction itself there came the further thought 'something with which one cuts.'"—Moore, 9/VI.	(3) *The image is at times incomplete in a different way.* (i) *It is partial.* Rechen: "First, the meaning, then the image. Nevertheless, I reacted before the image was clear. I imaged a part of a rake. Already I have noted several times that I image the left lower parts of objects. Here I imaged a wooden rake."—Stappen, 17/II.
Eule: "I knew that the word was something with which I am familiar and knew that from this point I could, at any time, go on and find its more specific meaning. Thereupon I reacted. In the word itself there was something presented to consciousness (mir gegeben) that I cannot further describe."—Frl. Marezoll, 10/VI.	(ii) *It represents a single definite character of an object.* Kuh: "I have the meaning and now I must have an image. I then look at an empty spot—no longer at the word. Then there appears the color of the animal. I see 'brown.' But a satisfactory, complete image, I do not obtain. I must exert myself even to obtain the color. I could not take the image for the meaning. I cannot read anything more out of the image than 'brown'—never the meaning 'cow.'"—Grüninger, 16/II.

Consciousness of Meaning

(4) *The meaning never has sensory characteristics but rather a conceptual determination.*

Geier: "A moment passed before I found the meaning. No auditory-kinæsthetic image was present. I *knew* that it was something that hovers over mountains in the air—even though I did not see the mountains. Visually I imaged only a pair of extended wings and *knew* that something belonged between them."—Frl. Marezoll, 14/V.

Veilchen: Immediately after the word appeared, I had an auditory-kinæsthetic image of it—as I pronounce it. 'Veilchen,' and in connection therewith a knowledge of its meaning (Ein allgemeines Bedeutungswissen), that I can thus explain: a definite species of flower. I dare say that it is this which makes up the content of the meaning—what I actually know about this object during the experiment.—Külpe, 18/V.

(5) *The meaning is often expressed in terms of a definition of general application.*

Dampfschiff: Immediately on the exposition of the word, auditory-kinæsthetic image thereof, and a realization of the meaning in the sense of 'a means of transport by water.' This time there was no trace of any image.—Külpe, 16/II.

(6) *The meaning is never localized.*

Consciousness of Visual Imagery

(4) *The image manifests degrees of brightness, color and clearness.*

Rose: "Immediately after the word came I had the auditory-kinæsthetic image of the word and thereupon an understanding for its general signification. Then first came an image—the image of a blossom. Almost nothing of the stem was seen. Colorless, mere differences of brightness in the blossom and the leaves were perceived. A full blown rose. The common form. Image and meaning did not cover each other."—Külpe, 16/II.

Krug: Meaning then the visual image. It was an earthenware jug, bellied out in front—antique as if it had just been dug up.—Lehner, 7/VII.

(5) *The image is often described in sensory terms that would fit only a very definite object.*

Rettich: This time there came to me the image of a radish of medium size. I saw clearly the little hollows in its skin filled with dirt and myself in the attitude in which I cultivate this beautiful variety in my wife's garden. All at once there came to me a poem of Mörike. It is entitled 'The Radish.'—Lehner, 16/II.

(6) *The image has often a definite position.*

Schuh: I had an indistinct image of a laced shoe—the point to the right somewhat behind the plane of the word. A confused consciousness of meaning was also immediately present, which did not coincide with the image. The meaning was even more general than foot covering. It had somewhat the sense of a piece of clothing without relation to a part of the body.—Külpe, 23/II.

Consciousness of Meaning	Consciousness of Visual Imagery
(7) *The meaning is always pertinent to the word.*	(7) *The image is sometimes recognized as not strictly pertaining to the word.* Rettich: The word appeared very strange to me. I think I read something like 'Bettish.' Only later did I get the correct meaning. There came a visual image. The image did not really represent a radish but rather a kind of turnip.
(8) *The meaning is never looked upon as superfluous.*	(8) *The image is often regarded as unnecessary and of secondary importance.* Ochs: I first understood the word as something familiar, as something that I knew what it was. A further analysis of the meaning did not take place. Under the influence of the task, my attention was directed to experiencing an image and then arose the head of an ox with his horns as drawn in the pictures for these experiments.—Moore, 12/VI.
	Fass: Immediately after looking at the word an auditory-kinæsthetic representation and understanding of its general signification in the sense of a spatial measure. There came also—altogether fleetingly a weak image with a pair of hoops lying on the ground—wholly accessory as if a schema.—Külpe, 16/II.
(9) *The meaning is always present.*	(9) *The image is often lacking.* Such cases could be multiplied indefinitely. Some have already been given.
(10) *The meaning leads regularly to the image.* Nearly always the subjects report the meaning as coming first.	(10) *The image is only occasionally present before the meaning.* Kuh: I believe[1] the image came first—wholly undefined. Very soon thereafter the meaning, immediately after which the reaction.—Stappen, 17/II.
(11) *The meaning comes spontaneously.* Cases enough have already been cited to make this evident. Only occasionally, where the word is read incorrectly, is any effort required to bring out this meaning.	*The image must often be sought.* Flasche: I had the feeling of a considerably retarded flow of imagery, and perceived clearly that I was sharply concentrated upon my task. I then imagined that I went through Amalien Street and

[1] These cases are very rare. I have found them only with this subject and when he does mention them, it is always with reserve. He says 'I believe' indicating that he is not sure of the observation.

Consciousness of Meaning	Consciousness of Visual Imagery
	had the task to represent to myself a bottle. The representation succeeded but rather poorly. Only the image of the material (glass) and the long form was clear.—Lehner, 3/VII.

A careful consideration of these results will show that the difference between meaning and visual imagery does not consist in any possible difference in the original imagery itself.

If meaning were an early stage in the development of the visual imagery, it might be possible to explain in this way the difference in the reaction times to the two events. A candid consideration of the introspections shows that this is not the case. The universality of the meaning cannot be pictured and is something quite different from the schematism of the image. The incompleteness of the image with a fragmentary character and washed out coloring differs profoundly from the imperfect unanalyzed embryonic stage of the meaning. The image has sensory characters which cannot be ascribed to the meaning—the meaning cognative characters which are utterly foreign to the image. The meaning is a 'knowing' *sui generis;* the image is a sensational element with its own specific character.

The meaning is not the potentiality to visualize. It may have an element of potentiality about it, but it always has an element of actuality which extends from the unanalyzed knowledge expressed by the phrase: "I know what that is"—to the more perfect conception expressed by a definition. The potentiality of the meaning when present is not the same for all meanings. It is a definite potentiality in which the elements of a definition of the object are in subconsciousness. It is not the potentiality to visualize, for the potentiality to visualize (1) depends on a meaning to determine what is to be visualized; (2) results in something different from the actualization of the meaning. The actualization of the meaning leads to the consciousness of a definition which may not even be accompanied by imagery of any kind whatever.

Nor is imagery the tendency of a number of images to crowd into consciousness. That tendency is sometimes present especially with one of our subjects, but by him it was recognized as something that came after the meaning.[1]

Meaning is often present and one is definitely conscious of it without being conscious of a tendency of images to crowd into consciousness. Meaning is a consciousness of knowledge that has definite characters foreign to the images that tend to crowd into consciousness. Furthermore, where images do crowd into consciousness they have to be known. This knowledge of what the image represents cannot be explained by another image which would itself have to be known.

Meaning, therefore, appears to be a conscious process *sui generis* distinct from imagery.

IV. Consciousness of Purpose and Kinæsthetic Imagery

(*a*) *Quantitative Results*

The instructions to the subject indicate sufficiently the nature of the investigation in this section of the work. These instructions were as follows:

Sie werden nach einem Signal ein Wort zu sehen bekommen. Ich bitte Sie zu reagieren wenn Sie die Bedeutung des Wortes im Hinblick auf den Gebrauch oder die Funktion des damit bezeichneten Gegenstandes erfasst, bzw., wenn Sie eine kinaesthetische oder kinaesthetisch-optische Vorstellung davon gehabt haben.

The words chosen for reaction stimuli in this set were not merely capable of being visualized but represented objects that most of us have often handled as: brush, bell, hat. To represent a word like 'lion' by a kinæsthetic image is to some subjects a very difficult task. Consequently a more appropriate set of words was chosen. Even under the most favorable conditions the kinæsthetic image comes far too late to account for the meaning. It might, however, be claimed that such an image is identical with the consciousness of the purpose of an object. Accordingly the comparison was made between reactions to the consciousness of purpose and those

[1] Cf. *supra*, p. 178. Subject Lehner.

to the awareness of a kinæsthetic image which concerned the object itself. Mere verbal images were excluded. Seven subjects took part in this set of experiments.

The results are shown in the accompanying tables:

Subject Grüninger

Words

Date	Motor Image	*T*	*V*	Date	Concept of Purpose	*T*	*V*
2/III	Anker	2,084	210	2/III	Stiefel	2,247	699
	Rechen	1,647	647		Koffer	1,389	159
	Gabel	1,929	365		Bürste	1,292	256
4/III	Trichter	2,379	85	4/III	Flasche	1,671	123
	Apfel	2,307	13		Wurst	714	834
	Wiege	2,448	154		Birne	1,255	293
	Eimer	1,690	604		Brief	1,474	74
	Kette	2,398	104		Leiter	1,414	134
	Zwicker	2,101	193		Fahne	2,255	707
	Treppe	2,506	212		Finger	1,458	90
	Hund	3,745	1,451		Kuh	1,474	74
		11)25,234	4,038		Ente	1,942	394
		2,294	367			12)18,585	3,837
					Mean	= 1,548	320
					Median	= 1,466	

Number of times median for concept exceeds reaction time for imagery=0.

Subject Külpe

Words

Date	Kinæsthetic Image	*T*	*V*	Date	Concept of Purpose	*T*	*V*
27/II	Ring	1,100	451	27/II	Stiefel	804	306
2/III	Sichel	1,416	135		Auge	1,180	70
	Ring	1,332	219	2/III	Haken	771	339
	Rechen	1,823	272		Hammer	1,822	712
	Bleistift	1,227	324		Bürste	1,346	236
	Würfel	2,579	1,028		Gabel	1,165	55
	Lampe	1,298	253		Feile	1,153	43
	Pickel	1,638	87		Uhr	808	302
		8)12,413	2,769		Stiefel	944	166
		1,551	346			9)9,993	2,229
					Mean	=1,110	247
					Median	=1,153	

Number of times median for concept exceeds reaction time for imagery=1.

With all of our subjects the mean for reaction time to kinæsthetic imagery is longer than that to the concept of purpose. Examining these results critically we find that with some of our subjects in spite of the small number of

Subject Lehner

Words

Date	Kinæsthetic Image	*T*	*V*	Date	Concept of Purpose	*T*	*V*
	Würfel	1,083	70		Feile	997	71
2/III	Lampe	1,515	362	2/III	Uhr	1,249	323
	Pickel	1,184	31		Stiefel	1,121	195
	Brief	1,693	540		Wiege	684	242
	Leiter	1,145	8		Eimer	1,050	124
5/III	Fahne	1,501	348	5/III	Kette	733	193
	Flasche	1,980	827		Zwicker	815	111
	Treppe	1,162	9		Finger	1,260	334
	Stiefel	760	393		Brief	941	15
	Uhr	804	349		Leiter	1,076	150
	Feile	941	212		Fahne	960	34
23/VI	Wiege	826	327		Flasche	937	11
	Eimer	879	274	23/VI	Treppe	763	163
	Kette	882	271		Würfel	738	188
	Zwicker	927	226		Lampe	721	205
	Finger	1,170	17		Pickel	775	151
		16)18,452	4,264			16)14,820	2,510
		1,153	266			Mean = 926	157
						Median = 937	

Number of times median for concept exceeds reaction time for imagery=6.

Subject Marezoll

Words

Date	Kinæsthetic Image	*T*	*V*	Date	Concept of Purpose	*T*	*V*
14/5	Buch	1,157	493	14/5	Ring	962	353
	Klavier	1,899	249		Schere	1,215	100
	Eimer	2,148	498		Pinsel	1,942	627
19/V	Korb	1,745	95	19/V	Zirkel	1,966	651
	Pinsel	1,643	7		Schlitten	1,404	89
	Zirkel	4,524	2,874		Schere	2,167	852
	Schlitten	1,573	77		Eimer	3,097	1,782
7/VII	Schere	1,071	579		Korb	650	665
	Ring	723	927	30/VI	Buch	857	458
	Anker	1,884	234		Klavier	807	508
	Besen	1,270	380		Bürste	930	385
	Bohrer	1,422	228	7/VII	Bohrer	990	325
9/VII	Bürste	1,218	432		Besen	749	566
	Zwicker	1,557	93		Anker	1,056	259
	Rechen	1,532	118		Haken	939	376
	Horn	1,069	581				
	Meissel	1,616	34				
		17)28,051	7,899			15)19,731	7,996
		1,650	464			Mean = 1,315	533
						Median = 990	

Number of times median for concept exceeds reaction time for imagery=0.

SUBJECT MOORE

Words

Date	Kinæsthetic Image	*T*	*V*	Date	Concept of Purpose	*T*	*V*
	Bohrer	1,453	217		Handbeil	781	36
16/VI	Spaten	1,129	541	16/VI	Säbel	1,162	345
	Haken	1,628	42		Pinsel	592	225
	Messer	1,796	126		Besen	539	278
18/VI	Bürste	1,772	102		Nadel	918	101
	Zange	1,396	274	18/VI	Rechen	815	2
	Handbeil	1,572	98		Feile	1,046	229
9/VII	Pinsel	1,914	244		Bohrer	1,004	187
	Pfeil	1,222	448	9/VII	Spaten	564	253
	Besen	1,480	190		Haken	836	19
	Wage	1,461	209		Hammer	1,215	398
	Brille	1,896	226		Handschuh	412	405
	Feile	2,071	401		Löffel	635	182
	Kerze	1,459	211		Schlitten	1,001	184
30/VII	Ring	2,234	564	30/VII	Leiter	719	98
	Zange	1,722	52		Schlüssel	669	148
	Bohrer	1,564	106		Rechen	967	150
	Haken	1,888	218		Spaten	822	5
	Schere	1,729	59		Pinsel	1,028	211
	Zirkel	2,014	344		Bürste	621	196
		20)33,400	4,672			20)16,346	3,652
		1,670	234			Mean = 817	183
						Median = 818	

Number of times median for concept exceeds reaction time for imagery=0.

SUBJECT STAPPEN

Words

Date	Kinæsthetic Image	*T*	*V*	Date	Concept of Purpose	*T*	*V*
	Kerze	4,308	2,915		Auge	1,199	449
	Haken	528	865		Säge	682	68
28/II	Bürste	644	749	28/II	Sichel	697	53
	Würfel	465	928		Gabel	814	64
	Sense	1,024	369		Feile	362	388
		5)6,969	5,826			5)3,754	1,022
		1,393	1,165			Mean = 750	204
						Median = 697	

Number of times median for concept exceeds reaction time for imagery=3.

experiments we can say definitely that the concept of purpose comes quicker than the kinæsthetic imagery. With these subjects the median for the concept of purpose was shorter than everyone of the reaction times to kinæsthetic imagery. With one subject, one out of eight reactions to kinæsthetic imagery was shorter than the median; with another two out of

SUBJECT TANNHÄUSER

Words

Date	Kinæsthetic Image	T	V	Date	Concept of Purpose	T	V
	Bohrer	1,492	231		Klavier	1,314	456
	Schlüssel	939	322		Bürste	749	109
	Trichter	1,527	266		Fernglas	749	109
	Bleistift	1,367	106		Haken	1,069	211
	Trommel	1,088	173		Leiter	829	29
30/VII	Brille	1,903	642	30/VII	Kerze	885	27
	Rechen	1,327	66		Pickel	1,071	213
	Tasche	1,255	6		Zange	1,816	958
	Wage	1,378	117		Ring	785	73
	Hammer	1,005	256		Schlitten	600	258
	Handschuh	1,158	103		Feile	763	95
	Besen	1,173	88		Spaten	554	304
	Dolch	855	406		Brief	554	304
	Horn	1,011	250		Anker	739	119
	Auge	2,334	1,073		Eimer	1,114	256
	Brunnen	2,216	955		Hut	824	34
	Geige	694	567		Flasche	976	118
	Fass	2,006	745		Glocke	717	141
	Hahn	2,082	821		Fächer	648	210
	Beil	1,576	315		Finger	1,006	148
31/VII	Messer	831	430	31/VII	Fahne	875	17
	Lampe	883	378		Meissel	834	24
	Koffer	797	464		Korb	952	94
	Buch	1,009	252		Kette	817	41
	Hobel	1,007	254		Kamm	816	42
	Handbeil	707	554		Schrank	637	221
	Schuh	848	413		Sense	776	82
	Ofen	849	412		Pfeil	700	158
		28)35,317	10,665		Nadel	720	138
		1,261	381			29)24,889	4,989
						Mean = 858	172
						Median = 816	

Number of times median for concept exceeds reaction time for imagery = 2.

twenty-eight. With one of our subjects the matter looks a little doubtful; six out of sixteen are shorter than the median. With another subject[1] the results are too few and scattered to give any quantitative basis for judgment.

The question is one where individual differences are likely to play a part. Those who readily form kinæsthetic imagery may be able to obtain such an image more quickly than they can *think* of the purpose of the object. To what extent this is true cannot be decided from the present results.[2]

[1] It was impossible to get more experiments from this subject. He left the day after the series above reported and did not return in the summer semester. They are more of the nature of a preparatory series than final results.

[2] When a short abstract from this paper was read last December at the meeting

Taking all the results together only 12 out of 105 reactions to kinæsthetic imagery were shorter than the median of the various subjects' reaction-times to the concept of purpose.

(b) *Introspective Data*

Turning now to the introspective results, we find that the concept of purpose and the kinæsthetic image are very clearly differentiated. The concept of purpose differs from the simple meaning in that it does not come with the same necessity. It is the result of the subject's task—not of the mere exposition of the stimulus. The same is true of the kinæsthetic image. Both follow upon the awareness of the simple meaning. Neither is a necessary prelude nor a sequence of the other. The task "image" or "concept" is the main factor in determining which is to appear.

The following are some of the more noteworthy introspective differences between the two.

(1) *The concept of purpose is expressed in non-sensory conceptual terms.*

Zwicker: "I imaged my own eye glasses and had clearly a consciousness of concave glasses. I was further conscious of the fact that these glasses must refract the rays of light according to a definite law that the image may still fall upon the retina—even though the lens is incapable of doing it this service. I then formulated the purpose of eye-glasses as: 'The correction of an error of refraction.'"—Lehner, 5/III.

(1) *The kinæsthetic image always describes some kind of act involving a use of the muscles.*

Sichel: "Immediately after the word appeared I had an auditory-kinæsthetic image of it. Following this I constructed a visual and weak kinæsthetic image thereof in this manner. I held a sickle in my right hand and made movements therewith as if I were cutting grass. Thereupon I reacted."—Külpe, 2/III.

(2) *The concept of purpose sometimes involves the consciousness of the relation of the object to other things.*

Gabel: "Immediately after the appearance of the word an auditory-kinæsthetic image thereof. Then came the know-

(2) *This was not noted in the description of the kinæsthetic imagery.*

of the Southern Society for Philosophy and Psychology, Professor Ogden stated that he had reported some years ago at one of the meetings a series of experiments similar to the present in all details. He never published his results, but they were identical with my own. In the interests of a better insight into individual differences it is to be hoped that Professor Ogden will some day give us the advantage of his unpublished results.

ledge that the fork is an instrument for eating, accompanied by a weak visual image of a fork. I was also conscious that 'fork' stands in relation to 'knife.'"—Külpe, 2/III.

(3) *The concept of purpose though often restricted to one of various possible ends has always a certain generality.*

Kette: "I pictured to myself a tolerably strong chain and remembered from the days of my youth that such chains were used to tie animals in their stalls. I saw the whole situation of that day rise up before me."—Lehner, 5/III.

(3) *The kinæsthetic image is often perfectly definite and limited to an individual act in a certain time and place.*

Pickel: "I imaged a pick-ax, such as is used for working hard ground and saw myself in my garden in the act of lifting it in the air. The consciousness of the purpose of a pick-ax is a psychological process which cannot be identified with the act of lifting it."—Lehner, 2/V.

Wiege: "The meaning aroused the image of a cradle. I go back in thought to my childhood and feel how I rock my brother. The kinæsthetic image in this case contains a great part of the purpose."—Grüninger, 4/III.

(4) *The consciousness of purpose seldom stops with a means but rests in a concept conceived of as the object's end.*

Uhr: "Immediately after the appearance of the word I had an auditory-kinæsthetic image, then the thought: 'The clock must be wound up!' and then the further thought: 'The clock tells the time!' Then I reacted. Weak visual image of a clock on a wall."—Külpe, 2/III.

(4) *The kinæsthetic image regularly concerns an art which is a means to the object's end.*

Lampe: "I imaged the lamp that I use in my dwelling, and saw clearly that it did not burn brightly enough, and then imaged the turning up of the wick. The kinæsthetic image of the movement cannot be identified with the consciousness of purpose."—Lehner, 2/III.

Trichter: "Immediately after the simple meaning of the word, I had the visual image of a funnel and then the kinæsthetic image of laying hold of it with my right hand and placing it over an opening. Here also the kinæsthetic image falls short of being the fulfilment of the purpose. For I think that the funnel is the instrument by means of which I pour fluid through an opening, and my image is only the placing of the funnel in the opening."—Grüninger, 4/III.

(5) *The concept of purpose, even though delayed, comes as a natural development of thought about the object.*

(5) *The kinæsthetic image is often forced and is superfluous to the understanding of the function of the object.*

Handbeil: "I soon understood the word, but the simple consciousness of meaning was forced into the background of consciousness by the task. I can express this simple consciousness of meaning by the sentence: ' I know well what that is!' Then I asked myself under the influence of my task: 'What purpose does it serve?' Then there came to me the clear concept that it is of use in cutting wood. With this concept of purpose were some broken, confused words. I do not know whether they were German or English. There was also a dark blurred image of an island in Lake George, where I have often cut wood in summer."—Moore, 16/VI.

(6) *The concept of purpose, though at times more or less restricted, never miscarries entirely.*

Fahne: It was rather difficult for me to connect a kinæsthetic image with the word. At first I imaged a flag as I saw one recently waving on a little tower in Leopold Street. But I said to myself at once, 'This waving is not a kinæsthetic image.' Then I imaged to myself how I would place this flag on the little tower. That the purpose of the flag is not covered by my motor image of it, goes without saying."—Lehner, 5/III.

(6) *The kinæsthetic image is not always pertinent to the purpose of the object.*

Bohrer: "Again the meaning first and then a visual image of the object—of a whole situation. I attempted to screw a drill through the wall, and instead of that I lifted the whole wall with the drill."—Frl. Marezoll, 9/VII.

V. Meaning and the Word

(*a*) *Quantitative Results*

In the perception of the meaning of words, subjects often spoke of the meaning being associated with an auditory-kinæsthetic verbal image of the word itself. No attempt was made to find out by reaction time the temporal relations of the verbal image and meaning in the perception of printed words. From the introspective results no definite answer can be obtained. The two are so close together that they appear simultaneous. One might, however, surmise that since the word must be read, in order that it may be perceived and understood, verbal sensations or verbal imagery are likely to come prior to understanding.

On account of the close connection with the sensations involved in reading and the understanding of printed words, such material presents no little difficulty in studying the necessary relations between verbal imagery and meaning. Pictures seemed to offer a more favorable material for study. If meaning is the kinæsthesis of speech, then the knowledge that a picture before me represents a tree should come when I name the picture and not before. A series of reaction times for the naming of pictures and perceiving the meaning of pictures should give approximately identical results. Three of our subjects took part in these experiments.

With all three subjects there is strong evidence that in general it takes longer to react to the word than to the meaning. The means for reaction to the word are, in every case, longer than those for meaning. This excess is also

SUBJECT LEHNER

Pictures

Date	Word	*T*	*V*	Date	Simple Meaning	*T*	*V*
9/III	Baum	845	214	9/III	Säge	492	1
	Uhr	630	1		Katze	419	74
	Lilie	968	337		Hahn	251	242
	Sichel	638	7		Ring	923	430
	Käfer	626	5		Eimer	500	7
	Hammer	794	163		Krone	629	136
	Treppe	601	30		Pfau	416	77
	Sense	726	95	19/V	Hobel	517	24
	Kamel	698	67		Spinne	579	86
	Mitra	775	144		Schlitten	572	79
19/V	Würfel	840	209	26/V	Fernglas	704	211
	Frosch	649	18		Haue	718	225
	Pinsel	689	58		Krug	536	43
26/V	Apfel	708	77		Stuhl	380	113
	Uhr	493	138	20/VII	Baum	673	180
	Wiege	780	149		Uhr	547	54
20/VII	Katze	485	146		Lilie	334	159
	Ring	720	89		Sichel	383	110
	Haken	563	68		Hammer	558	65
	Säge	594	37		Sense	349	144
	Fernglas	410	221		Kamel	392	101
	Pfau	376	255		Käfer	460	33
	Krone	713	82		Pinsel	468	25
	Eimer	375	256		Treppe	327	166
	Hobel	598	33		Frosch	411	82
	Stuhl	373	258		Würfel	400	93
	Haue	623	8		Wiege	391	102
	Schere	560	70		Apfel	553	60
	Krug	474	157		Korb	344	149
					Mitra	572	79
		29)18,324	3,392			30)14,798	3,350
		631	117			Mean = 493	112
						Median = 480	

Number of times median for meaning exceeds reaction time=6.

Subject Külpe

Pictures

Date	Word	T	V	Date	Simple Meaning	T	V
9/III	Horn	1,020	98	9/III	Pferd	525	108
	Sofa	767	155		Bürste	510	123
	Rechen	824	98		Haus	798	165
	Dampfschiff	769	153		Ente	308	325
	Kette	825	97		Fahne	673	40
	Zwicker	738	184		Flasche	967	334
	Finger	843	79		Trichter	570	63
	Stiefel	1,692	770		Bohrer	659	26
	Dolch	837	85		Uhr	524	109
	Kerze	1,718	796		Lilie	957	324
	Lilie	957	35		Hahn	573	60
	Sichel	834	88		Käfer	767	134
	Haken	759	163		Hammer	722	89
	Ring	332	590		Löwe	327	306
		14)12,915	3,391			14)8,880	2,206
		922	242			Mean = 633	157
						Median = 616	

Number of times median for meaning exceeds reaction time to word = 1.

Subject Marezoll

Pictures

Date	Word	T	V	Date	Simple Meaning	T	V
26/V	Korb	959	48	26/V	Zirkel	998	341
	Schere	825	86		Eimer	643	14
	Pickel	1,862	951		Stuhl	463	194
	Apfel	622	289		Sichel	1,009	352
	Uhr	781	130		Trommel	561	96
	Wiege	729	182		Dampfschiff	463	194
25/VI	Tasse	1,175	264	25/VI	Windmühle	526	131
	Maske	873	38		Kirsche	560	97
	Lyra	782	129		Zither	796	139
	Engel	736	175		Kette	739	82
	Ochs	671	240		Fahne	501	156
	Mitra	913	2		Würfel	628	29
		12)10,928	2,534			12)7,887	1,825
		911	211			Mean = 657	152
						Median = 594	

Number of times median for meaning exceeds reaction time for word = 0.

greater than the mean variation. With one subject in 29 reactions to words, only 6 were shorter than the median for meaning; with another, 1 in 14; with another, 0 in 12.

(*b*) *Introspective Data*

Turning to the introspective results we find them in accordance with the quantitative measurements. Time and time again, whether the task were meaning or word, the same sequence of events was perceived, viz., (1) meaning, (2) word, (3) reaction. Often, however, when the task was meaning, the word was reported as coming during or after reaction.

Some special points of difference between the word and the meaning are given below.

(1) *The meaning leads to the word—the designation of the picture.*

Frosch: "The meaning was first present. I felt a strong striving for the word, as it were from various sides of the drawing. The reaction followed after the entrance of the word."—Lehner, 9/V.

(1) *The word never leads to the meaning.*

(2) *A meaning cannot be lacking if the subject names the picture—no matter what the task.*

(2) *The word may be lacking when the task is meaning.*

Eimer: The word did not appear at all. Various memories were in the background of consciousness.—Frl. Marezoll, 26/V.

(3) *The meaning is what it is by its own right. It is never said to have a meaning.*

Pferd: "Immediately after I saw the picture I experienced a tone of familiarity and knew what this picture represented. At the same time, with the reaction came the word 'Pferd.' I did not react to the word. The tone of familiarity was related not to the picture, but to what it signified. The picture was a symbol of real objects and its signification consisted herein, viz.—to point to them."—Külpe, 9/III.

(3) *The word may have a special meaning of its own; e. g., the word has a more general meaning than that of the picture.*

Engel: "Immediately a memory image. After this image came the word. I knew that the meaning of the word was more general than that of the picture."—Frl. Marezoll, 25/VI.

(4) *The meaning is sometimes designated by a word which is known to be inappropriate.*

Lilie: "First I recognized in the picture a flower, then I named it by mistake 'Tulpe.' I knew that 'Tulpe' did not fit the picture. Then through the form of the flower, etc., I was occasioned to say "Glockenblume."—Lehner, 9/III.

(4) *The word is never designated by a meaning.*

VI. Influence of the Observer's Attitude

When a short abstract of this paper was read last December at the meeting of the Southern Society of Philosophy and Psychology, it was suggested that the difference in reaction time to meaning and imagery is to be explained by a difference in the attitude of the subject. He reacts quicker when told to react to meaning, not because the meaning is something different from the imagery but because he himself assumes a different attitude.

This objection implies that there is no real difference between meaning and imagery, but that when we call them by different names the subject, for some obscure reason, assumes such a different attitude that it markedly influences his reaction time. The objector in other words does not wish to admit a difference between meaning and imagery, and refers the difference in reaction time to an unexplained and perhaps inexplicable mystery.

To say the least, this explanation is highly improbable. For supposing there is no such thing as a special 'meaning process' and that the accruing image is identical with the meaning, then the task of the subject in the two sets of reactions is really identical. It is simply called by different names. If that were the case, then the subject ought (1) to have a real difficulty in distinguishing his two tasks. (2) He ought to give introspective reports identifying the two procedures. (3) The reaction times ought to be identical within the limits of the probable error.

None of these things were so, but on the contrary (1) The tasks were readily distinguished. (2) The introspective reports clearly separate the two processes. (3) The reaction times are markedly different.

All this tends to render highly improbable, if not impossible, the explanation which suggested that the difference in the reaction times is not to be explained by a real difference in the tasks, dependent on a difference between meaning and imagery, but is due entirely to the difference in the attitude of the subject. In fact, it is very hard even to imagine a mental mechanism which would produce two separate attitudes with such different effects in the reaction-times, if that to which the subject takes an attitude is in both cases merely one and the same thing that the experimenter calls by a different name.

Let us, however, go a step further. Our subjects reacted to visual and kinæsthetic images. If we wish to compare the reaction times in this case we will find them markedly different. Is it possible to explain that difference by a difference in the attitude of the subject?

If we should argue visual imagery is distinct from kinæsthetic (1) because the subject distinguishes two different tasks when told to react to the one or the other; (2) because the introspective reports clearly separate the one from the other; (3) because the reaction times to visual imagery are much shorter than to kinæsthetic imagery, no one would doubt the validity of the argument. When, however, the same argument is made in regard to imagery and meaning, it is called in question and the attempt is made to explain away the difference by ascribing it to a difference in the attitude of the subject. If, however, the difference in the attitude of the subject is not the real explanation in the latter case, but a real difference between visual and kinæsthetic imagery, then this difference in the attitude of the subject cannot, without any more ado, explain the shorter reaction time for meaning as compared with imagery.

Furthermore, the difference in attitude itself must be explained. Granted that there is a difference in attitude, what is the most likely explanation for the fact? The first thing that comes to mind is that in the two sets of conditions the subject is taking an attitude to two different things. If that is the case then, meaning and imagery must be distinguished. But how distinguished—as two different mental processes or as two aspects of one and the same process?

In the sequence of events that follow the exposition of the stimulus word, there may be, if you wait long enough, not only visual but also kinæsthetic imagery. Are these aspects of one and the same mental processes, or specifically different items in a definite series of events? Reasons have already been given for distinguishing them. These reasons point to events that are qualitatively distinct, and the distinction can scarcely be called in question. But the very same reasons point to meaning as qualitatively distinct from imagery. When, furthermore, one considers the fact that in the understanding of words the meaning process is never absent, but that visual and kinæsthetic imagery may both be lacking, there is an added reason why meaning should not be identified with an aspect of visual or kinæsthetic imagery.

Furthermore, a difference in the attitude of the observer cannot be made the sole reason for the difference in the reaction times.

(i) In the set of experiments referred to in the beginning, the subject's task was to observe and remember a series of words, pictures or objects. Nothing was said about attending to meaning or imagery. He had simply to report what he had experienced—whatever that might be. Here the question of a difference in the "set" of the observer does not enter at all. In these experiments, the subjects reported that in the perception of words, meaning preceded imagery. This suggested the problem of an objective test of the accuracy of the introspection. The reaction time experiments followed, and confirmed with entire satisfaction the introspections of the earlier series.

(ii) In the reaction time experiments no matter what the task—whether the subject is in the meaning attitude or the image attitude, he regularly reports meaning as coming prior to imagery. If the difference in the 'set' of the observer were the sole reason for the difference in reaction time, we should not expect that no matter what his 'set' he would nevertheless observe a rather constant temporal relation between meaning and imagery.

The introspective results and the reaction times are supplementary. When taken together they leave no doubt that we have really been investigating the temporal relation of meaning and imagery.

VII. The Context Theory of Meaning and the Temporal Relations of Meaning and Imagery

It may now be asked: Whom does all this concern? Who maintains that imagery is meaning? In spite of a certain modification of the image theory of meaning, Professor Titchener's context theory cannot account for the experimental facts brought out in his own and other laboratories. From an analysis of his theory it is apparent that he maintains that meaning is often identical with imagery. In fact under the conditions of our experiments the images and words that followed upon the sensations of the stimulus words and pictures were actually the context. Analogous conscious states have been reported by Cornell observers as the meaning under somewhat similar conditions. But they did not take into consideration the temporal relations of meaning and imagery.

A brief analysis of the context theory of meaning will show how intimately it is concerned with the temporal relations of meaning and imagery.

(*a*) *Outline of the Theory*

"Meaning, psychologically, is always context."[1] Such is the definition that Professor Titchener gives to a fact of consciousness with which the modern psychology of thought is now interested.

What is context? Context in English is a word used to signify the setting of a sentence or a quotation—its relation to what the author has written before and after the passage in question. Titchener lays particular stress upon what comes after in the definition of psychological context. "Context, in this sense, is simply the mental process which accrues to the given process through the situation in which the organism finds itself." A sensation by itself has no meaning—neither has an image. When a second mental process accrues to a former one—this second mental process is the meaning of the first one. It does not produce a new something called mean-

[1] 'A Text Book of Psychology,' New York, 1911, p. 367.

ing, it is the meaning. "One mental process is the meaning of another mental process if it is in that other's context."[2]

What are the mental processes that accrue to others and thus constitute their meaning? Originally the secondary process which constituted the meaning was a group of sensations coming from a bodily attitude of the organism. If the animal took an attitude of defence the kinæsthetic sensations thus aroused did not exactly mean—did not signify that something to be feared was at hand. The whole complex of sensations involved constituted the meaning "something to be feared."

At the present day, however, the human mind has passed beyond the elementary stage of the primitive organism. The essential difference between present human intelligence and its early prototype consists in the use of imagery as well as sensations for the constituents of meaning. "Image has now intervened upon sensation and meaning can be carried in imaginal terms."[2] Thus spoken and written language has become possible. A sensation arouses an image and the image—the psychological process accruing to the sensations—is the meaning of the sensation.

Various types of mind exist. Each has a special tendency to form some kind of imagery in understanding sensations. Indeed "If we were to make serious work of a differential psychology of meaning, we should probably find that in the multitudinous variety of situations and contexts, any mental process may possibly be the meaning of any other."[3]

It is Professor Titchener's opinion however that of all the possible types of supplementary mental processes, two are of special importance: kinæsthesis and verbal images. Indeed he pushes the verbal theory so far as to say: "The words that we read are both perception and context of perception, the auditory kinæsthetic idea is the meaning of the visual symbols."[4]

[1] *Op. cit.*, p. 367.

[2] *Op. cit.*, p. 367.

[3] 'Lectures on the Experimental Psychology of the Thought Processes,' 1909, p. 178.

[4] 'A Text-Book of Psychology,' p. 368.

Thus far, Professor Titchener's theory is entirely psychological. But in order to meet all possible contingencies arising from introspections that he or others may report, where meaning shows no trace of a sensory conscious element —a physiological factor is introduced.

Meaning is not always conscious; *i. e.*, the imaginal supplement to the sensation is not always to be found even by the most careful introspection. In such cases the sensory supplement exists—it is a physiological process in the nervous system.

Professor Titchener thus summarizes his theory of perception:

"Our account of the psychology of perception is now, in the author's view, complete. It has embraced four principal points:

"First, under the general laws of attention and the special laws of sensory connection, sensations are welded together, consolidated, incorporated into a group.

"Secondly, this group of sensations is supplemented by images.

"Thirdly, the supplemental group has a fringe, a background, a context; and this context is the psychological equivalent of its logical meaning.

"Fourthly, meaning may lapse from consciousness and conscious context may be replaced by a non-conscious nervous set."[1]

The type of meaning in the third caption is decidedly different from that given a few pages previous. There meaning is context—context is the mental process that follows upon and accrues to another mental process. The examples given are the images spoken of in the second caption. Here we suddenly find that meaning does not lie in the advening images—but in their fringes.

To harmonize this new idea with what has gone before we may suppose that if meaning is conscious (in the sense of being conscious described by Titchener) it is given by the context which may be (*a*) a second group of sensations, (*b*)

[1] 'A Text Book of Psychology,' 1911, p. 371.

an image or a group of images, (*c*) the fringe or background of such images—the fringe itself being always understood as some kind of sensory element or elements, (*d*) various combinations of (*a*), (*b*) and (*c*).

(*b*) *The Evidence for the Theory*

In the interests of simplicity we may leave aside the speculations about meaning in the primitive organism and confine ourselves to the explanation of the fact of meaning as we experience it.

On what then, may we ask, is the statement based that meaning is context—that it is a 'sensory complex *B*, following upon sensation or image *A*.' The points of evidence are:

1. Introspection shows that when a word or a sentence is understood and careful search is made we always find some kind of imagery—verbal, kinæsthetic, visual, etc.

Granted that this is so what does it prove? Nothing more than this. In the complex of mental processes called up by the task of understanding a word or sentence imagery is present. It does not show that this imagery is the meaning —which is the very point in question.

Titchener says: "The meaning of the printed page may now consist in the auditory-kinæsthetic accompaniment of internal speech; the word is the word's own meaning."[1]

He then refers in a note to introspections in the studies of Watt and of Messer which speaks of meaning being simultaneous with auditory-kinæsthetic imagery. But such a citation is not to the point. The fact that one thing accompanies another is certainly no evidence that the two are identical.

2. Analysis shows no evidence of 'imageless thoughts.'

What analysis shows is the fact of meaning. Many observers have maintained that in their consciousness of meaning sensational elements are lacking. Professor Titchener in his analysis finds also the fact of meaning and giving to the students in his laboratory the task of reporting every mental process that they can observe, he obtains experiences

[1] 'Lectures on the Experimental Psychology of the Thought Process,' p. 177.

far richer in sensational elements than are elsewhere found. Given the task, 'find imagery,' and it will certainly come. And if the subject be told to look for imageless imagery, it will not be found. Meaning and imagery however, have been found both by Professor Titchener and a number of other observers. Facts are common property. It remains for Professor Titchener to prove that meaning is identical with the concomitant or subsequent imagery. This he has not done.

The context theory of imagery demands imagery, when meaning is present. If meaning equals imagery, imagery equals imagery. No imagery—no meaning, must be the conclusion to be drawn from this theory. Nevertheless Professor Titchener shrinks from admitting all that is involved in his doctrine. Why? Because he himself has observed that there are times when he experiences meaning and is not conscious of imagery. He himself, therefore, in spite of the ease with which he images things and situations, has experienced the very state of mind the existence of which he denies.

"In rapid reading, the skimming of pages in quick succession; in the rendering of a musical composition, without hesitation or reflection, in a particular key; in shifting from one language to another as you turn to your right or left-hand neighbor at a dinner table: in these and similar cases, meaning has time and time again, no discoverable representation in consciousness."[1] No discoverable representation in consciousness means no sensational element—no sensational or imaginal complex.

What is Professor Titchener's explanation of such "imageless thoughts" that come to him as he skims over the pages of a book? He has found "imageless thoughts," what then is to be done with them? Explain them away and then deny their existence. How explain them away? Refer them to the nervous system? Meaning here is not imagery for no imagery is present. What is it then? A physiological process, without any conscious accompaniment. Why without any conscious accompaniment? Because by hypothesis the

[1] 'A Text-Book of Psychology,' p. 369.

only conscious processes that come into consideration are sensations and these are lacking.

On the one hand, we have an hypothesis; on the other, a fact—the imageless consciousness of meaning (imageless thoughts) in rapid reading. The fact cannot be accounted for by the hypothesis; therefore Professor Titchener denies the fact. My consciousness of meaning is unconscious. I do not think but my nervous system is thinking for me.

The reference of imageless thought to an unconscious physiological process in the nervous system brings us to a third point in the evidence for Professor Titchener's theory.

(3) "Our psychology is to be explanatory and our explanations are to be physiological."[1]

Adherence to this principle and the ruling out of facts that it cannot explain, give to Professor Titchener's theory a certain plausibility.

What can be referred to the nervous system is explained What cannot be referred to the nervous system is not explained. It is in fact inexplicable. There must be a mistake in the observation. It must be explained away. The nervous system with its sense organs and its centres, can apparently take care of sensations and images. It gives us the sensational elements of our conscious life and apparently excludes anything like imageless thinking. If then we are to explain 'imageless thought' we must analyze it in terms of the elements given by the nervous system, or else explain it away altogether.

Such a procedure, however, places empirical psychology not only under the dominion of metaphysics, but subjects it to one particular metaphysical theory. Under such conditions an impartial empirical study of the mind becomes impossible. Let us first study the facts of consciousness and then build up our metaphysical theories.

Professor Titchener is right in demanding that the science of psychology should be explanatory; he is wrong in maintaining that everything must be explained in consonance with a particular metaphysical theory.

[1] 'A Text-Book of Psychology,' p. 370.

As a matter of fact neither Professor Titchener nor anyone else knows the limitations nor the possibilities of the nervous system. Nor does anyone know, for that matter, what the nervous system may be called upon to do if it is to explain the facts of our conscious life. We do not know all about the facts of consciousness and until we do, explanatory psychology must be careful. We do not know all about the nervous system and it is not wise to distort the fact of consciousness to fit the narrow outlines of our present horizon.

Let us first investigate the facts of consciousness without any timidity about their ultimate explanation. Let us first find out what we have to explain, and then explain it.

The context theory of meaning is not based entirely upon such general considerations as we have picked out from Professor Titchener's writings. There are a number of experimental studies that have been put forward as tests in confirmation of the theory.

Of these, we may analyze two, leaving a more complete account of the literature to a full report of our experiments which we hope to publish later.

Helen Clarke,[1] in an article on 'Conscious Attitudes' took up the problem of the understanding of words and sentences. She confirmed the reports of other observers that 'often the images are adequate, irrelevant or even contradictory' (p. 241). The inadequacy she explained by saying that 'we have no criterion save the facts themselves, by which we can decide how clear or complete an image must be in order to carry a meaning' (p. 241). The contradictory character she accounted for by pointing out that in every one of her cases there was 'sufficient connection between the logical meaning of the word, and the psychological context of the act of understanding, for the latter to carry a general meaning' (p. 242). The fact of irrelevancy, she said, was less easy to explain.

Miss Clarke therefore seems to be conscious of the fact that words have a logical meaning which cannot be identified with the imagery that they evolve. She distinguishes be-

[1] *Am. J. of Psychol.*, XXII., pp. 214–249.

tween the word—the imagery that it evolves—and the meaning that is carried. She finds also that imagery is often irrelevant. Irrelevant to what, we may ask? To the meaning. She therefore realizes a difference between the psychological process called an image and another something of which she is also conscious and which may be termed the meaning of the word. Miss Clarke[1] seems to look upon general meaning as a logical something of which no account need be taken in psychology. If, however, the task of psychology is to investigate all conscious processes, logical meaning cannot be ruled out as 'outside the sphere of psychology.'[2] For "logical meaning" is conscious. Its nature is therefore a psychological problem. It is that something to which the imagery is often inadequate, irrelevant and contradictory. Miss Clarke has implicitly at least recognized it as a conscious state, distinct from imagery.

Edmund Jacobson[3] investigated by the Method of Introspection (1) The Perception of Letters, (2) The Meaning of Words, (3) The Understanding of Sentences. The instructions to his subjects (three observers) were as follows:

I. Give a minute account of all the mental processes you experience in their temporal order of sequence.

II. Put direct description of conscious processes outside of parentheses, and statements concerning meanings, objects, stimuli and physiological occurrences inside.

The experiments on the perception of letters showed that under the instructions given their meaning is usually accompanied by the arousal of what Jacobson termed designatory processes, viz., kinæsthetic or auditory sensations or both. Jacobson calls attention to the fact that "The main point to note is that the precise statement of meaning is by no means easy." Nor does he state anything more definite as to what the meaning of a letter is.

The experiments with the meaning of the words were made as follows: "A written word, was laid before the observer for a period of one minute. He was instructed to fixate the

[1] Along with Geissler, *Am. J. of Psychol.*, XXIII., p. 194.

[2] Cf. Geissler, *l. c.*

[3] *Am. J. of Psychol.*, 1911, XXII., pp. 553–577.

word, to utter it with quick repetition and to get at its meaning. The concluding ten seconds were marked off by signals; and the observer's task was to report what occurred in consciousness during the particular interval." The observer reported two kinds of imagery: (*a*) That which appeared as the carrier of the meaning and (*b*) that which appeared as irrelevant. No logical or psychological test could be found to distinguish between the relevant and irrelevant imagery.

The conclusion of Jacobson was "that the conscious meanings brought out in these experiments are not perfect and static logical meanings of definition. . . . Logically, the representation of meaning is inadequate; psychologically, it is adequate to the demands of the occasion" (pp. 568–569).

In his experiments on the meaning of sentences, Jacobson found cases in which (1) an automatic reading was followed by a perception of the meaning identified with images called forth by the experiment. (2) Cases in which the meaning did not come to the subject at all in spite of a wealth of visual, organic, kinæsthetic and tactual sensations: (3) Cases in which the visual and auditory images and sensations from reading were the sole processes present in consciousness —and yet the sentence had meaning. Jacobson concludes: (1) "Wherever there is meaning there are also processes," *i. e.*, sensations and images of one kind or another. (2) "The correlated meanings and processes are two renderings from different points of view of the same experience."

The first conclusion seems established by the introspective reports, but it holds only for the conditions of these experiments where ample time is given for images to appear and the task is set to report primarily mental processes, *i. e.*, sensations and images; and secondarily, in parentheses, to note meaning as it arises.

The second conclusion: (which is really the "crux" of the whole situation) meaning is an aspect of sensation and imagery, is simply stated and the reader is left to judge for himself on what evidence the conclusion is based. The only evidence in his paper for such an indentification is to be sought in the fact that his subjects, as a rule, were not satisfied that they had anything that corresponded with their idea of meaning till relevant imagery was present. This simply shows that meaning in the Cornell sense is not present till such imagery arises. From Jacobson's own data, it appears, however, that meaning in a broader sense must have been present when meaning in the Cornell sense was denied. When Dr. Geissler, instructor in psychology at Cornell, for 3 seconds, looked at the sentence, "Did you see him kill the man?," and then declared at the end "No meaning all the way through," we can only conclude that "meaning" must have been taken in a very restricted sense. When again he looked at the sentence, "The iron cube fell heavily on the floor," reads it as so many meaningless words, and then on rereading obtains the meaning, a very loud sound, the time of the whole procedure being 4.5 seconds, the conclusion is strengthened that during the experiment, he was seeking for a meaning in the sense of an imaginal representation. In this sense, and in no other, is Jacobson's conclusion warranted. An imaginal representation is some kind of imagery. The sweeping conclusion that meaning is an aspect of imagery requires the proof of another proposition, namely that all meaning consists in imaginal representation.

The data of this piece of introspective work is incompatible neither with the data nor the conclusions of the Külpean school. Indeed it has confirmed the fact that the meaning of a sentence may be present when the sole processes present in consciousness are the visual and auditory images and sensations from reading. And if it be true that on certain occasions, as in Geissler's case, these same processes were present and the meaning was really absent, one should conclude that they cannot be identical with the meaning. In like manner, a physician refuses to admit that a definite microörganism is the cause of a disease—if at times it is found when the disease does not occur, and the disease occurs when the organism is absent. Jacobson should therefore have admitted that there are times at least when meaning is not a mere aspect of sensations and images.

Professor Titchener looks upon the chief value of Jacobson's work in making the distinctions between the mere statement that meaning is present and the analytic description of the psychological part of meaning. He says that "He finds no specific 'meaning process' underlying the statement of meaning."[1]

True it is that Jacobson found no special sensory or imaginal process as the habitual carrier of meanings, but he did not prove that meaning is not itself a conscious process. In fact, his experiments seem rather to confirm the conclusion that meaning is not imagery, but something else altogether.

Had the Cornell School taken cognizance of the temporal relations of meaning and imagery, the context theory of meaning would have been profoundly modified. Imaginal terms may accrue to incoming sensations and constitute by definition their context. Do they constitute their meaning? A determination of the temporal relation that imagery bears to meaning shows that this is impossible. What comes after another cannot be said to cause, or constitute it, or be identical with it. Meaning, therefore, is not context. What is it—a mere negation? Not at all. It is a definite mental process *sui generis*. What are its qualitative characters? Some of these have been already indicated. A further development of the concept will be given with the fuller account of these investigations.

[1] "Description *vs*. Statement of Meaning," *Am. J. of Psychol.*, 1912, XXIII., p. 182.

THE SHORTEST PERCEPTIBLE TIME-INTERVAL BETWEEN TWO FLASHES OF LIGHT[1]

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The determination of the minimal perceptible time-interval: the shortest interval between two stimuli which allows the stimuli to be perceived as successive, and not simultaneous is important for many lines of work, including problems of time-perception and rhythm and also problems of rate-perception. Moreover an important theory of psychic synthesis has been supported by interpretations of certain measurements of the time-threshold for disparate stimulations (*i. e.*, stimulations of two modes of sense in succession).

My interest in these several lines of research, and also in certain purely visual phenomena, led me to commence, in the summer of 1912, an investigation of the time-threshold for visual stimulation, and its relation to the 'critical frequency' of flicker and fusion. The result of that summer's work (done at the Johns Hopkins University, with myself as principal observer), encouraged me to attempt further work on the problem with better apparatus. During the next college year (1913–1914) a graduate student was allowed to take up the problem, and obtained some results which seemed important.[2] This student, on leaving the University, took his unelaborated results with him, and I have not since been able to obtain them. Last summer, having the opportunity to work in Dr. Hyde's laboratory, I took up the problem again with specially constructed apparatus and obtained results which are interesting and important. I shall give in the following paper the results of both of my experiments.

[1] From the Nela Research Laboratory, National Lamp Works of the General Electric Company.

[2] This work was done with a pendulum apparatus, giving great accuracy, and having other advantages over the rotation apparatus first used; but with some difficulties of manipulation.

The first work on the visual time-threshold was done by Exner,[1] who worked with electric sparks, and found thresholds of 44 σ at 280 mm. distance, and 21 σ at 640 mm. Weyer,[2] in Wundt's laboratory, found a much lower threshold, 12 σ. Weyer also found, using electric sparks, a flicker-threshold from 25 σ to 87 σ, according to the adaptation and other conditions, and a threshold for separation of a series from 42 σ to 105 σ.

Shortly before my work was begun Bassler[3] published the results of some of his investigations, in which he found the time-threshold (length of shortest perceptible dark interval) to be about 40 σ with two visual stimulations, and the flicker point to be about one third as much (for serial stimulation).

None of these results are very significant, the work with electric sparks suffering from lack of control, and Bassler's being affected by serious defects of method.

Bassler used black discs on which were either two white sectors or a regularly spaced series, and rotated the disc close behind a screen in which was a hole a centimeter and a half in diameter. This hole, in which the alternation of black and white occurred, was observed from an unspecified distance. A student in our laboratory reproduced Bassler's apparatus, as nearly as Bassler's description allowed, and we found that eye movement was a very important factor in the observation, the eye movement being induced or increased by the motion of the black and white edges as they traveled across the aperture.

Preliminary Work[4]

It is obvious that the proper attack on the problem of the visual time-threshold involves control of the intensity and the duration of the flashes of light, and of the adaptation and movement of the eye, as well as of the areas stimulated.

In my first experiment I succeeded in eliminating the most

[1] Exner, *Pflüger's Archiv*, XI., S. 407.

[2] Weyer, *Philos. Studien*, XV., S. 67–138.

[3] Bassler, *Pflüger's Archiv*, 1911, Bd. 43, 245–251.

[4] This work was reported before the Natural Academy of Sciences, November 19, 1913. See abstract in *Science*, 1913, Vol. 38, p. 699.

serious important cause of eye movement, namely the traveling of the illumination across the area of stimulation, and kept the illumination constant and the eye dark-adapted. The first factor which I wished to investigate was the effect of the duration of the flashes, and the second was the effect of the intensity.

For simplicity's sake I adopted the method of rotating sectors, measuring the time interval by determining the speed of rotation, and computing from the angular width of the sectors. This method has two serious disadvantages: first, the change in speed, which is necessary to vary the length of the interval between flashes, varies the length of the flashes also, so that the effect of absolute flash length cannot be easily determined, relative length only being controllable. Second, the pair of flashes is necessarily repeated rapidly again and again unless some special device is used to cut off the exposure on all but one round of the disc; and this repetition is a factor which adds greatly to the difficulty of the determination.

My apparatus consisted of a Nernst glower, enclosed in a metal box, with a lens; a disc of white plaster of paris; a motor of controllable rate, driving, by a reducing belt, a spindle on which discs of adjustable sectors could be rotated; and an Ewald chronoscope for counting the rotations of the spindle during a given time.

The lens, 83 cm. from the Nernst glower, focused the light into an image, of approximately the same size as the glower, in the plane of the surface of the rotating sectors on the spindle, with the long axis of the image in a radius of rotation. When not interrupted by the sectors, the light fell on the plaster surface placed 35 cm. beyond the focus, forming a nearly rectangular spot 3.5 by 5 cm. The brightness of this spot was not measured, but was kept constant by maintaining a constant current through the Nernst glower, and by frequently inserting a Lümmer-Brodhun photometer in the position of the disc, comparing the illumination of the Nernst with that of a standardized 8 c.p. carbon lamp. The two brightnesses used were equal to those produced on the same surface by the 8 c.p. lamp at 36.5 cm. and 67.5 cm.[1] respectively. Since the rotating sectors moved across the beam of light at the focus, 'traveling' of the illuminated areas was nearly eliminated; since the focused image was narrow, the time between the beginning of the illumination (or the dark period) and the full illumination (or complete cut-off) was so small as to be negligible.

The observer (myself in most cases) sat between 75 and 80 cm. from the plaster surface, the angle between his line of sight and the axis of the Nernst beam being 45 degrees. The plane of the plaster surface was so placed that it made equal angles with the axis of the beam and the line of sight.

With this apparatus I made determinations both of the time-threshold for two flashes, and of the critical frequency for a series of interruptions. In the flicker work, a different disc, with the appropriate sectors cut out, was used for each of the different ratios of light to dark interval. In the work on time-threshold, a combination of sectors was used, giving two openings, from 0° to 90° in width, separated by a 5° sector or by a 10° sector. The lengths of flash used with the 5° interval were 5°, 10°, and by 10° steps to 90°: with the 10° interval, flashes of 2.5°, 5° and 10° were used, also several greater lengths, up to 180° for one flash, the other being shorter. The flicker discs (fifteen in number) had each two apertures, with ratios of open to closed ranging between 1/35 to 35/1.

In working on myself the method was as follows: starting with a speed of rotation such that distinct doubleness (or flicker) was observable, the speed was increased by small steps until a single flash (or fusion) was obtained. Then, by depressing a key a circuit was completed through the Ewald, and a circuit-breaker on the spindle, and was allowed to continue for ten seconds: thus the number of rotations in ten seconds was registered. After recording the speed, it was increased somewhat (the amount of increase at this point being purposely irregular), and then decreased by small steps

[1] That is, in the first case, the lamp at 36 cm. gave a brightness clearly brighter than that of the Nernst beam, and at 37 cm. a brightness clearly less.

until the point of doubleness (or flicker) was reached, when the speed was again measured. Three determinations were usually made on each setting of the sectors (or each flicker-disc), before proceeding to the next. The longer series of settings, or series of discs, was gone through with in this way from one to two hours, and as one such series a day was all an observer could endure, the progress of the experiment was necessarily slow.

The observer was instructed to make his judgment each time rather quickly, and then look away until the speed was changed. Continued gazing at the lighted area was found to cause even a pronounced doubleness or flicker to disappear.[1]

The observations were made with darkness adaptation. In working on myself, I took the speed readings, and made the record, by a very dim light, and then waited for a minute or so for readaptation. This procedure undoubtedly had some effect on the determinations, but this effect was probably not large.

The series of settings in the groups were taken in different orders on different days, the several orders being carefully

TABLE I

FLICKER AND FUSION

Observer Dunlap

Sectors in Degrees		Cycles per Second				Durations in Sigmas			
						Flicker		Fusion	
Closed	Open	Flicker	M.V. %	Fusion	M.V. %	Closed	Open	Closed	Open
5	175	24.12	5.86	28.17	5.93	1.15	40.29	0.98	34.48
10	170	28.11	4.20	31.9	4.48	1.97	33.59	1.73	29.53
15	165	31.08	5.51	36.00	5.25	2.68	29.49	2.31	25.45
30	150	38.56	5.16	42.59	5.62	4.32	21.60	3.91	19.56
45	135	42.59	3.51	46.25	4.15	5.86	17.60	5.40	16.21
60	120	43.84	3.55	47.94	4.69	7.60	15.20	6.95	13.90
75	105	44.68	4.00	49.46	4.38	9.32	13.05	8.42	11.79
90	90	45.32	3.07	49.47	4.34	11.03	11.03	10.10	10.10
105	75	45.64	4.07	50.22	4.60	12.00	9.12	11.61	8.29
120	60	44.81	3.70	49.75	4.94	14.87	7.43	13.39	6.69
135	45	44.58	4.23	49.00	4.13	16.82	5.60	15.30	5.10
150	30	41.58	4.72	46.73	3.38	20.03	4.00	17.83	3.56
165	15	38.72	4.57	42.92	5.65	23.66	2.15	21.35	1.94
170	10	35.66	5.39	39.80	4.98	25.48	1.55	23.72	1.39
175	5	29.61	6.42	34.04	6.06	32.82	0.93	28.55	0.81

[1] This 'flicker adaptation' is not due to brightness adaptation, as later work shows.

Table II

Time Threshold. Standard Brightness

Observer Dunlap

1. $A = C$, $B = 5$.

$A^\circ = C^\circ$	Double			Single		
	$B\sigma$	M.V.%	$A = C\sigma$	$B\sigma$	M.V.%	$A = C\sigma$
5	19.8	8.0	19.8	13.8	12.3	13.8
10	14.9	10.7	29.8	11.1	9.9	22.2
20	10.5	7.9	42.3	8.2	7.9	32.8
30	7.3	14.7	43.8	6.2	9.1	37.3
40	6.3	13.5	50.9	5.2	10.7	41.7
50	5.4	11.8	54.6	4.5	13.3	4.59
60	4.8	10.1	58.5	4.1	11.2	50.0
70	4.4	10.7	62.3	3.7	6.3	52.9
80	4.3	9.4	69.6	3.7	7.4	60.4
90	4.0	8.4	72.9	3.5	9.9	63.5

2. $A = C$, $B = 10$.

$A^\circ = C^\circ$	$B\sigma$	M.V.%	$A\sigma$	$B\sigma$	M.V.%	$A\sigma$
2.5	28.1	17.8	7.0	20.9	16.5	5.2
5	27.0	11.6	13.5	19.2	13.8	9.6
10	20.6	8.7	20.6	15.9	11.7	15.9

3. $B = 5$, $C = 5$.

A°	$B\sigma$	M.V.%	$A\sigma$	$B\sigma$	M.V.%	$A\sigma$
10	16.0	11.5	33.1	11.9	6.2	23.8
30	11.8	5.7	71.1	9.4	9.1	56.7
50	8.8	8.1	88.3	7.0	4.6	70.8
70	6.6	7.2	92.5	5.6	8.6	78.9
90	5.3	7.8	96.1	4.6	8.1	84.4

4. $A = 10$, $B = 10$.

C°	$B\sigma$	M.V.%	$C\sigma$	$B\sigma$	M.V.%	$C\sigma$
20	20.8	10.6	41.7	16.3	7.8	32.6
60	22.2	18.4	133.5	16.8	9.2	117.7
100	24.3	8.9	243.6	18.1	7.5	181.3
140	23.9	11.3	335.2	18.0	8.9	252.5
180	21.9	7.5	393.3	15.6	13.9	280.8

planned to distribute the effects of practice over the whole series.

The results of my observations are presented in Tables I., II. and III. In Table I. the average flicker-points and fusion-points for the several ratios of open to closed sectors are given both in cycles per second (*i. e.*, the number of complete changes from dark to light and back to light again in a second); and also in the duration in thousandths of a second, of the individual light and dark periods.

In Tables II. and III. the average durations are given for 'A' (the first flash), 'B' (the dark intermediate interval) and 'C' (the second flash) when the flashes appeared discontinuous ('double'), and when they appeared as one uniform flash ('single'). Table II. gives results of work with the higher brightness described above; Table III., with the lower brightness.

Table III

Time Thresholds, Low Brightness

Observer Dunlap

$A = C, B = 5.$

$A^\circ = C^\circ$	$B\sigma$	M.V.%	$A = C\sigma$	$B\sigma$	M.V.%	$A = C\sigma$
5	20.5	10.7	20.5	12.4	9.42	12.4
30	8.9	7.6	53.9	7.2	5.6	43.3
70	4.5	7.8	63.1	4.0	4.8	56.8

Table IV

Time Thresholds: Low Brightness

Observer G. R. Wells

1. $A = C, B = 5$

$A^\circ = C^\circ$	Double			Single		
	$B\sigma$	M. V.%	$A = C\sigma$	$B\sigma$	M. V.%	$A = C\sigma$
10	19.9	11.0	39.9	13.9	5.7	27.9
30	10.5	9.3	63.1	8.1	10.3	49.1
50	7.1	10.0	71.7	5.5	10.7	55.6

2. $C = 10, B = 5$

A°	$B\sigma$	M. V.%	$A\sigma$	$B\sigma$	M. V.%	$A\sigma$
30	13.1	11.8	78.9	9.9	9.2	59.5
50	11.2	9.0	112.4	8.0	8.6	80.7

3. $A = 10, B = 5$

30	18.8	10.7	113.3	14.9	8.6	89.6
50	19.5	6.3	195.6	15.0	9.7	150.1

In Tables IV. and V. the results of observations of two other persons are given. These observations were made after I had finished mine, and it was not deemed necessary to use all the flash-lengths which I had observed. In these cases I manipulated the apparatus and recorded the measurements, so that the observers worked under better conditions

TABLE V

TIME THRESHOLDS

Observer H. M. Johnson.

1. Standard Brightness

$A^\circ = C^\circ$	Double			Single		
	$B\sigma$	M. V. %	$A = C\sigma$	$B\sigma$	M. V. %	$A = C\sigma$
5	16.6	14.2	16.6	12.1	9.3	12.1
10	9.4	12.1	18.8	7.8	12.2	15.6
30	7.0	10.0	42.2	5.8	8.9	34.8
50	5.2	8.0	52.4	4.3	4.8	43.4
70	4.5	8.6	63.1	3.8	9.1	54.5
2. Lower Brightness						
5	13.2	7.1	13.2	10.3	5.7	10.3
30	7.0	13.1	42.0	5.8	9.2	35.2

than those under which I observed, specifically as regards adaptation.

Each of the values given in Tables I., II., III. and V. are averages of twenty-five thresholds. The values in Table IV. are averages of twenty thresholds.

There are two points of importance which stand out in these data. First, the rise in rate of the 'critical frequency' (flicker and fusion points) from the extreme inequality to equality of open and closed sectors, in both directions (Table II.). Second, the decrease of the time threshold with increase in the length of the first flash (Tables II., IV. and V.). This decrease seems to be altogether a function of the first flash; increasing the length of the first flash with the second flash constant (II., 3; IV., 2) has almost the effect of increasing both flashes; while increasing the length of the second flash (II., 4; IV., 3) alone has practically no effect. The slight increase in the threshold in both these cases is due to the increased difficulty of observation with the unequal length and hence unequal appearing brightness of the flashes.

In addition to these points, it is to be noted that the time thresholds are low, ranging (with equal flashes) from 4 to 20 sigmas. The comparison of these figures with those obtained in other experiments is, however, not now significant, since we have not as yet analyzed the various factors entering into determinations of this sort. In addition to the brightness, in regard to which the above data are not significant, the factor of adaptation is probably extremely potent. These results were obtained with fairly good darkness adaptation; they cannot be compared with results obtained with daylight adaptation.

Among the factors affecting the formation of judgments, the rapid repetition of the pair of flashes was conspicuously disturbing. The simple rotation apparatus is not suited to determinations of this kind.

Work on Brightness and Adaptation

The second set of experiments I varried on, at Dr. Hyde's invitation, in the Nela Research Laboratory during the summer months of 1914. In carrying out these experiments I received much help from the staff of the laboratory, and I am especially indebted to Dr. Hyde, the director of the laboratory; to Mr. Cady, assistant director; to Dr. Lorenz; to Dr. Cobb; and to Dr. Johnson. The readiness of the members of the staff to give their time to my problems, and to release to me apparatus from their own experiments, made possible such work as I was able to accomplish in the short time I was there. The greatest burden of the observations fell on Mr. Eric Martienssen, to whom I am indebted for his careful and willing work, under conditions which were sometimes trying.

My apparatus, which need not be described in detail, consisted of the following units.

(*a*) A double rotator,[1] carrying on one axis of rotation two arbors; one on the main shaft and the other on a sleeve on that shaft, the sleeve being geared to an auxiliary shaft and that back to the main shaft, so that the sleeve made one rotation for nine of the shaft. The arbor on the sleeve carried a large metal disc with a 40-degree aperture. Variable cardboard sectors were carried by the faster moving arbor. When the axis of light is parallel to the shaft of this apparatus,

[1] This piece of apparatus was made under my direction in the Physics workshop of The Johns Hopkins University.

whatever exposures are arranged through sectors on the faster arbor are repeated every ninth revolution of the shaft, being cut out the remainder of the time by the slow moving disc.

The main shaft carried also a loose gear, in mesh with a gear on the driving motor; with an electro-magnetic clutch of my devising, so that the disc and sectors could be stopped for adjustment without stopping the driving motor; and could be started again without jerk by turning the current gradually on the clutch magnet. The same shaft also carried a cylinder of brass and hard rubber, on which rested two brass brushes, so that the rotations could be counted by a step-up mechanism operated by the make-and-break of the circuit.

(*b*) A nitrogen-filled lamp, with the wire in a straight compact coil; operated in these experiments at 85, 120, 200 289 and 400 watts. The image of the coiled wire was focused, by suitable lenses, on a slit, to cut off light reflected from the surface of the lamp bulb; and by other lenses refocused in the plane of the sectors carried on the faster moving arbor of the rotation apparatus described above. The axis of the beam of light was parallel to the shaft of the rotation apparatus, and the long axis of the image was radial to the shaft.

The lamp, the slit and the lenses were enclosed in a large hood of black felt drawn over a wooden framework, with an aperture just large enough for the convergent beam to emerge.

(*c*) A movable screen located just beyond the slow-moving disc of the rotator and operated by a hand lever. By raising this screen shortly before the disc made an exposure, and lowering it shortly afterwards, a single exposure of the interval arranged through the sectors was allowed. The manipulation of this screen required no accurate timing, since the slow disc allowed exposure every ninth rotation of the sectors only.

(*d*) A lens, just beyond the hand screen, decreased the divergence of the cone of light, increasing the brightness of the surface illuminated.

(*e*) A plaster disc, surfaced with magnesia, illuminated by the cone of light. This disc was 12.5 cm. in diameter, and about half the diameter of the light cone at the point of insertion of the disc and had a background of black velvet upon which the light around the disc was negligible. The plane of the disc was vertical, but was at an angle of 30° from the plane perpendicular to the axis of the cone of light.

The observer sat so that his binocular line of sight was perpendicular to the disc, which was about 165 cm. from his eyes.

(*f*) A miniature projection lamp, with a small incandescent bulb entirely enclosed, established out of the range of the observer's vision, cast on the object disc a group of four small dots, which served excellently as a fixation mark.

(*g*) Eight mazda lamps, totalling 600 watts, so disposed in the room that the walls were illuminated, especially the wall in front of the observer—the wall behind the plaster disc—but the lamps were screened from the observer's eyes. These lamps were controlled by a single switch.

(*h*) A single mazda lamp in a long black cardboard tunnel, arranged to throw continuous illumination, when desired, on the disc.

(*i*) An Ewald chronoscope, for counting the rotations of the sectors, as a control of the accuracy.

(*j*) A synchronous motor[1] for driving the rotator. This had eight poles, and working on 60 cycle A.C. current gave 15 rotations per second. This motor was geared to the main shaft of the rotator (*a*), the ratio to the gear on the motor shaft being 1 to 3. The main shaft therefore made five rotations per second, so that for the sectors carried by the arbor on the main shaft 9° equalled 5 σ. The variations in speed, due to variations in current frequency, were negligible during the periods of work.

(*k*) A small D.C. motor for starting the synchronous motor. This starting motor was belted to a one-flanged pulley on the shaft of the synchronous motor so that the belt could be thrown off when the synchronous motor was working properly. A stroboscope disc mounted on the same shaft, and illuminated by a 15-watt lamp on the A.C. current,

[1] This motor was one which Dr. Lorenz had constructed for his use. The stroboscopic method of starting the motor was also suggested by him.

indicates the proper moment for turning on the synchronous motor.

The five wattages used on the lamp gave brightnesses on the object disc of 3, 10, 36, 82 and 168 candles per square meter. This range of illuminations seemed adequate for the investigation of the effects of brightness, which was the first point I had planned to attack.

The results of the preliminary experiment, reported above, had shown clearly that the threshold for doubleness (measured in terms of the dark interval) depends on the length of the flashes, especially of the first flash, although the absolute magnitudes of the thresholds as determined in those experiments could not be supposed to be very significant. It would therefore be possible, theoretically, to determine thresholds in either of two ways: first, by keeping the dark interval constant and varying the flashes; and second, by keeping the flashes constant and varying the dark interval. It would seem equally useful to work out the thresholds in flash length for several fixed dark-interval lengths, and to work out the thresholds in dark-interval length for several fixed flash-lengths. In either case the effect of the brightness, and of adaptation could (it would seem) be worked out in an adequate way.

In the manipulation of apparatus, the first procedure is far the simpler. The sector adjustments are not so complicated, and hence the progress of the experiment should be more rapid. Realizing that the work would at best be slow, I chose the plan which offered this important advantage.

Observations were made at first with dark adaptation exclusively. The subject was kept in the room from ten to twenty minutes before commencing work, according as he had come in from outdoors, or from more or less dimly lighted work rooms. No warning signal other than the normal sound of the electro-magnetic clutch in taking hold, was needed by the observer. The motor ran continuously, and the clutch was thrown in when an observation was desired. The rotator 'picked up' full speed in less than a second; the hand screen was lifted about two seconds after the clutch was thrown in. The four dots of light in the center of the disc fixed the line of sight before the flashes occurred. Four repetitions of the exposure were given in succession, but the observer usually gave his judgment after the second or third.

Observations were carried on for some time by Martienssen and myself by this method, using the procedure of 'serial groups,' but the results, although interesting, were of little value for the purposes of the experiment. Variations in the durations of the flashes produced variations in the apparent brightness and apparent color of the disc, which were at first extremely confusing, and on which finally the judgments came to depend, rather than on any real appearance of 'doubleness' or 'singleness.' One set of five hundred judgments by Martienssen, which are typical, are given in Table VI.

Table VI

Martienssen

Dark interval 25σ. Brightness 82 c. per sq. m.

Two Flashes	Double	Single	One Flash	Double	Single
70σ	48	2	140σ	0	50
60	36	14	120	0	50
60	38	12	100	3	47
40	32	18	80	11	39
30	41	9	60	12	38

In this table the first column gives the length of flash where two were used, the second and third columns giving the number of judgments of double and single for each flash-duration. The fourth column gives the durations of the single flashes, each equal to the sums of the two in the corresponding pair; the fifth and sixth columns giving the number of judgments of single and double respectively for each of these single flash durations.

The increasing difficulty of discrimination is here shown, not so much by the increased tendency to call the double flashes single, as by the tendency to call the single flashes double. Obviously, no definite threshold can be determined when this tendency is present.[1] This tendency, it must be noted, is not due to mere confusion; as we shall see later, a single flash often appears distinctly double, and with the same sort of doubleness as is noticed in a really double flash. In this particular set of observations, however, the judgments, according to the observer's report, were based largely on differences in apparent brightness and color; at least this seemed to him to be the case in the latter part of the set.

Table VII

Martienssen

Dark interval 25σ. Brightness, 3 c. per sq. m.

Two Flashes	Double	Single	One Flash	Double	Single
50σ	26	4	106σ	3	23
50	16	4	80	4	16
30	18	2	60	2	18
20	15	5	40	4	16
10	15	5	20	11	9

The results of a set of observations by Martienssen with lower brightness are given in Table VII. Results of a set of observations by Dr. Johnson on the moderate brightness are given in Table VIII. Other sets with different brightnesss gave results of the same order.

Table VIII

Johnson

Dark interval 25σ. Brightness, 82 c. per sq. m.

Two Flashes	Double	Single	One Flash	Double	Single
50σ	20	0	100σ	1	19
40	17	3	80	7	13
30	23	7	60	4	26
20	10	10	40	5	15

In this set of observations, the observer's judgment was influenced very largely by the apparent duration of the total

[1] This condition is similar to that found in attempting to determine the 'two point' threshold by simultaneous stimulation of the skin. No threshold can be determined, since *one* stimulation frequently is perceived as two, and hence the least separation of two points giving a certain percentage of perception of two has no definite significance.

exposure; the greater duration of the two flashes was noticed, especially with the shorter flash-lengths, and this tended more and more to become the criterion of doubleness.

The procedure by groups ('method of serial groups'), it was clear, could not be used in this experiment. The secondary criteria—in this case the differences in brightness, color, and duration—are made maximally conspicuous by this method, and judgments strictly on the points under examination are made practically impossible. I therefore attempted to use the shuffled series procedure, still clinging to the method of constant dark interval. A few series, however, showed that this method was not practicable, even when the better procedure was employed, since the differences in brightnesses and color still were very conspicuous. The regular progression procedure ('method of minimal change') accentuated these secondary criteria still more.

The effects of the total duration had been foreseen, and I had expected to introduce variations in which the single flash should be equal in length to the two flashes *plus* the dark interval. This variation was found to be inapplicable because it would have accentuated the brightness differences. For example; the greater brightness of the 100 σ flash as compared with the two successive flashes of 50 σ with 25 σ dark interval, would be still greater if the lengths of the two flashes were reduced to 37.5 σ each.

The next attempts were made by the method of constant flash-length, using the 'shuffled series' procedure. With this method the differences in apparent brightness are not so marked as with the constant dark-interval method, and by this procedure these differences and the differences in duration are not so disturbing as they are in the serial group procedure. It is possible, in other words, to form judgments on the apparent doubleness or singleness alone of the flashes, although it required a high degree of training in order to eliminate absolutely other criteria.

The results of these next observations by the shuffled series procedure are given in Tables IX., X., XI. and XII. In these tables the first column gives the separation of the two flashes, and the other columns give the number of judgments of 'single' and 'double' for each of the five brightnesses. The observations with all of the brightnesses were obtained on the same days, a series being taken with each brightness during each experimental period, the order of brightness being altered from day to day in a regular way.

TABLE IX

Martienssen

Flash = 50σ

Intervals, σ	Brightnesses, Candles per Sq. Meter									
	3		10		36		82		168	
	d.	s.	d.	s.	d.	s.	d.	s.	d.	s.
0	11	13	5	15	9	15	11	9	5	19
5	8	4	5	5	9	3	5	5	7	5
10	6	6	7	3	5	7	7	3	8	4
15	9	3	5	5	9	3	6	4	9	3
20	10	2	4	6	11	1	8	2	6	6
25	11	1	8	2	10	2	10	0	8	4

TABLE X

Johnson

Flash = 50σ

Intervals, σ	Brightnesses, Candles per Sq. Meter					
	3		10		168	
	d.	s.	d.	s.	d.	s.
0	4	16	0	20	0	20
5	5	5	0	10	2	8
10	7	3	2	8	3	7
15	8	2	4	6	8	2
20	10	0	9	1	9	1
25	10	0	8	2	10	0

Other series were taken with light adaptation. In this work the room was lighted by the mazda lamps referred to under (*g*), and the observer was adapted to the brightness of the plastered wall due to this illumination. When ready to make the observation, the lights were switched off, approximately 1.5 seconds before the exposure of the flashes (or flash). This interval was timed by watching the exposure on the hand-screen; and turning off the lights immediately after such exposure. Then the hand-screen was lifted, and since the exposure occurred every 1.8 seconds (the rotation-period of the slow moving disc) the interval between the turning off of the mazda lamps and the exposure on the object disc was timed sufficiently well.

Table XI

Martienssen

Light Adaptation. Flash = 50σ

Intervals, σ	Brightnesses, Candles per Sq. Meter					
	3		36		168	
	d.	s.	d.	s.	d.	s.
0	5	25	6	24	3	27
5	2	17	12	13	6	14
10	15	6	15	5	14	16
15	17	2	18	2	19	1

Table XII

Johnson

Light Adaptation. Flash = 50σ

Intervals, σ	Brightnesses, Candles per Sq. Meter					
	3		36		168	
	d.	s.	d.	s.	d.	s.
0	0	25	2	23	5	20
5	5	15	12	13	9	11
10	13	6	10	10	18	2
15	24	1	16	3	19	1

This method of working with light adaptation seems quite satisfactory. An interval must be allowed between the turning off of the adaptation light and the beginning of the stimulus light, to allow muscular recovery. The one-and-a-half second period seemed to be about the shortest which could be used. Of course a slight amount of adaptation occurs within this period, but this is kept constant throughout.

Series with darkness adaptation followed the work with light adaptation. Results of one group of series on Martienssen are given in Table XIII. The remainder of the work on this observer and on Dr. Johnson was directed to 'feeling out' methods, and does not lend itself to tabulation.

TABLE XIII

Martienssen

Dark Adaptation. Flash = 25 σ

Intervals, σ	Brightness, Candles per Sq. Meter					
	3		36		168	
	d.	s.	d.	s.	d.	s.
0	7	7	3	11	1	13
15	9	5	9	5	9	5
20	13	1	13	1	8	6
25	14	0	14	0	11	3

From the results, as tabulated, little can be inferred as to the effect of brightness. It is evident that adaptation is an important factor. The factor of greatest consequence, however, is the tendency to see the single flash as double. The effects of this tendency are found in the tabulated results, especially with the lowest brightness, and were still more evident in the work not tabulated.[1] Attempts to use flashes longer than 50 σ proved fruitless on account of this tendency. At 75 σ, for example, there was a large increase in the number of 'double' judgments on single stimuli. There is a limit, however, beyond which the double appearance is not found. It may be useful, later, to determine both the upper limit and the lower limit for the fallacious doubling, but this is a determination of the most difficult sort.

The double appearance of the single flash may, with practice, be distinguished from the true 'doubleness.' That is, there are times when the 'doubleness' of a single flash is clearly different from the 'doubleness' of two successive flashes, if the one and the two are shown with but little pause between. This discrimination is apt to be lost at any time, however, and the pseudo-'doubleness' taken for real 'doubleness.'

As an illustration of the discrimination, the following observation of Dr. Johnson will serve.

[1] In many cases, both with one flash and with two flashes, the appearance was 'double' on first exposure and 'single' on the succeeding exposures.

1. With the brightness = 36 (c. per sq. meter), two 40 σ flashes were distinguished from one 80 σ flash when the interval was 20 σ; with 15 σ interval, the one flash and the two flashes looked *equally double*.

With brightness = 3, the two were distinguished from the one with 15 σ interval.

With brightness = 168, discrimination was clear at 25 σ; not at 20 σ.

2. With two 25 σ flashes, and one 50 σ flash, the difference was clear when the interval was 35 σ, with all the brightnesses, equal 'doubleness' at 30 σ.

3. With two 10 σ and one 20 σ flashes, the discrimination was clear when the interval was 55 σ for the 3 and 36 brightnesses, and 40 σ for the 168 brightness. Below these points the 'doubleness' was the same.

Similar observations by Dr. Cobb gave the following results:

1. Two 25 σ flashes and one 50 σ flash, with brightness = 3, 'doubleness' clear at 40 σ interval. With brightness 36 and 168, 'doubleness' clear at 30 σ interval.

2. Two 50 σ flashes and one 100 σ flash, with brightness = 3, clear at 20 σ.

With brightness = 36, clear at 25 σ.

With brightness = 168, clear at 30 σ.

3. Two 10 σ flashes and one 20 σ flash, with 3 and 36 brightnesses, not clear below 50 σ (no longer interval used): with 168, clear at 45 σ.

4. Two 75 σ flashes and one 150 σ flash, brightness = 3, clear at 10 σ interval. Brightness = 36 and 168, clear at 15 σ interval.

On the whole we cannot conclude that increasing the brightness of the flashes increases the distinction of the doubleness of two. This is a matter that is dependent upon the absolute length of the flashes. In subsequent work, carried out on the two observers listed above, and on Dr. George R. Wells, the effect of brightness was brought out directly by trying various intervals in succession with the same flash-lengths. This work, while agreeing with that reported above, brought out the further fact that the effect of intensity variations on successive flashes which are hardly distinguished at best because of the shortness of the interval, is not the same as the effect on succession with longer intervals.

These observations are not consistent with the tabulated results, but there is no reason why we should expect them to be so, since the conditions of observation were entirely different. We must always distinguish in problems of this kind variations in the actual observable phenomena established by the experimental conditions, and the variations in the observations of these phenomena which may be due to the same conditions. For example: the sensible content from two (successive) stimulations may be different from the sensible content due to a single stimulation, and yet on account of the circumstances of observation, the difference may not be noted. On the other hand, a sensible content of a certain sort may now be judged like, now be judged different from, a content from which it differs slightly, according as the conditions of observation throw this difference in relief, or minimize it.

My Own Observations

During the course of the experiments reported above, I acquired a considerable facility in observation, since I watched the flashes while having full knowledge of the stimulus conditions. I did not record my observations during the work with the other observers, since the necessity of conducting the experiments for them, and especially the attention to speed and accuracy in the adjustment between exposures, was a disturbing factor.

Later I made observations (with knowledge) myself under satisfactory conditions. In these cases I worked with the 'progressive procedure,' starting alternately with a setting (width of dark interval) giving *no doubleness*, and one giving *doubleness*.

This work was done at night, and the results on different nights did not agree absolutely. There was, however, a general uniformity, such as is indicated in Table XIV., in which are given the results on four nights during August. The figures given are not averages, but absolute values in the scale of 5 σ steps; the points at which (and above which) the flashes were always seen 'double' (d.) and at which and below which they were seen 'single' (s.) on that night under

Table XIV

Dunlap

Flash = 50 σ

1. Aug. 4

Brightness	Dark Adap.		Light Adap.		Constant Light	
	d.	s.	d.	s.	d.	s.
10	40	30	20	5	20	5
36	40–50	30	20	5	20	5
82	40–50	30	20	5	20	5
168	40	30	10–30	5	20	5

2. Aug. 22.

Brightness	Dark Adap.		Light Adap.	
	d.	s.	d.	s.
3	25	15	20	10
10	25	15	15	5
36	25	15	5	?
82	25	15	5	?
168	25	15	10	5

3. Aug. 23

Brightness	Dark Adap.		Light Adap.		Constant Light	
	d.	s.	d.	s.	d.	s.
3	20	10	20	5–10	5	2.5
10	20	10	10	?	5	2.5
36	20	5–10	5	?	10	5
82	20	15	5	?	10	5
168	20	10–15	5	?	10	5

the conditions indicated. When the threshold varied during the test, the variation is indicated. The observation lasted from one to two hours, with periods of rest for the eyes.

In certain cases, no definite determination was made for the 'single' point. This is indicated by a question mark.

The series with dark adaptation and light adaptation were taken as in the work on other observers. The results in the columns under 'constant light' were obtained while the object disc was illuminated by the 'tunnel lamp' described above, (*h*). In this case, the flashes were superimposed on a constantly lighted surface. Except for the illumination of the disc, the room was dark during these observations.

The observations included in Table XIV. were with 50 σ flashes only; with 25 σ the results were more uniform; for all brightnesses, with dark adaptation, the double point was at 40 σ, the single, at 30 σ; with light adaptation, the points were 20 σ and 10 σ respectively; with light adaptation and constant light in the disc, 20 σ and 5 σ. With dark adaptation and constant illumination, the single point was 5 σ, but the double point was variable (10 σ–20 σ). Flashes above 50 σ (up to 75 σ) gave more variable results.

The general influence of light adaptation and constant illumination was demonstrated on a number of persons, including the observers listed above, by a simple method. The sectors were set so that with dark adaptation the two flashes appeared 'single,' or the judgment was 'doubtful.' Then the eye was light-adapted for a short time, and observation showed a striking change, it being possible with any observer to change the judgment from 'distinctly single' to 'distinctly double' by this means. The addition of a constant illumination served the same purpose. With certain settings of the sectors, and a faint constant illumination on the disc, the two flashes appeared 'single'; by increasing the constant illumination a point was reached at which the appearance was clearly double. This point varied with different observers, and at different times.

The Sources of Difficulty

The results of the investigations of the visual time threshold up to this point are as follows:

1. The effects of *brightness* of the light are variable, depending on the other factors in such a way that no conclusion can be drawn as yet concerning their effects.

2. The threshold is lower for the light-adapted eye than for the dark-adapted eye. This holds, at least, for certain light-adaptations.

3. The threshold is lower for an interval marked by flashes added to a continuous stimulation, than flashes in a dark field. This holds for a wide range of constant illumintion, the threshold varying usually with the brightness of the constant illumination up to the point where the additions lose in distinctness.

4. A single flash is frequently seen as a succession of two, and although this 'twoness' may, under proper conditions, be discriminable from actual 'twoness,' these conditions are not easily actualized in quantitative work.

In consequence of this (and, possibly, other factors) quantitative work by the standard methods is not possible; at least the results of such work are unreliable. Special methods must be devised.

5. It is impossible to train observers on the light threshold problem in a limited time (two or three months). Observations are of value only if made by persons having a long training in that particular work. In this respect, the time-threshold problem differs markedly from certain other problems, *e. g.*, of flicker. The length of training required cannot be specified, but possibly should extend over a period longer than a year.

The most interesting question coming out of these observations concerns the apparent doubleness of a single flash under certain conditions. This doubleness of appearance is unquestionable; the flash has at times a striking 'one-two' progression.

This fictitious doubleness is not exclusively a dark-adaptation phenomenon, although it is less noticeable with light-adaptation. Constant illumination, on the other hand, even of relatively low brightnesses, completely abolishes it. We might therefore suppose it to be due to an iris-reflex: the stimulation beginning with dilated iris causes a strong contraction and immediate relaxation, so that the light-flux entering the eye drops and rises again causing a depression ('dimple') in the excitation curve of the retinal process in the same way as in a rapid succession of two flashes.

The occurrence of the flash provokes a strong visual reflex, noticed by every observer. One feature of this reflex is an increase in accommodation: at the end of the stimulation, this accommodation is for a point nearer than the object-disc, and the relaxation necessary to re-accommodate for the disc is easily noticed. Since accommodation and iris-contraction go together this may be taken as indicating the iris factor suggested above.

On the other hand, the chief factor may be retinal. The retinal process may rise to a point higher than its 'normal' for the intensity of stimulation, and then drop back.[1] The drop may be below normal, with an immediate second rise; thus the 'dimple' which normally produces the appearance of doubleness may occur independent of iris-activity.

It is possible that no 'dimple' may be required. The two drops in the sensation,—one following the excessive rise, and the other at the end, may be interpreted as 'twoness.'

The motor-process—adjustment of the eyes—may be connected with the fictitious doubleness through an actual inhibitory discharge to the retina accompanying the discharge to the ciliary muscle. Efferent fibers to the retina are known to exist, although their function is not known.

The motor-process is probably the cause, or connected with, the severe effect of the observations. Both Martienssen and myself felt the effect to a marked degree, the eyes becoming very irritable, and necessitating frequent interruptions of the work.

Instead of being towards the end or in the middle of a very simple experiment, or small group of simple experiments, we are now at the place where it is necessary to take up a large number of points, not so clearly connected with each other as they are contributory to the solution of our initial problem. If any light is to be thrown on these problems, it can come

[1] Such action of a light stimulation on the retina is called by physicists the 'over-shooting of the sensation.'

only through the solution of these various problems, each of which involves an extended investigation.

The problem, or group of problems, which stand out above the others in importance, concerns adaptation. I am now installing apparatus and developing methods which may throw new light on this topic.

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THE PSYCHOLOGICAL REVIEW

AN EXPERIMENTAL CONTRIBUTION TO THE INVESTIGATION OF THE SUBCONSCIOUS[1]

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The subconscious is so often referred to and so little attention has been given to investigating it experimentally that it has seemed to me a condensed summary of a recent investigation I have made, might possibly be of some interest.

In making this study the image method was employed. That is, to state very briefly the mode of procedure: in one half of the experiments the observer, usually with his eyes closed or blindfolded and seated opposite the experimenter, was instructed to sit in a relaxed position and let an image (visual or auditory, memory or imaginative, etc., depending upon what was desired by the experimenter), arise of itself. The observer was not only not to arouse the image but he was not even to know its content until he saw it before him, and only those images were noted where such instructions had been entirely complied with. In the other half of the experiments, the observer was directed to arouse the image, that is, for example he was instructed to decide on the particular thing he wished to visualize and to arouse the corresponding image.

Stanford and Munich University students acted as observers.

An examination of the data regarding the content of the images, their mode of arising, etc., shows:

[1] For fuller details of the investigation as to the theory underlying it, the methods used, the experimental data, etc., see Martin, 'Ein experimenteller Beitrag zur Erforschung des Unterbewussten (Barth) and Über die Abhängigkeit visueller Vorstellungsbilder vom Denken,' *Zeit. für Psych.*, *70*, 212.

1. The subconscious mental activity reveals itself through the arising of images where the observer did not previously know whether anything would be imaged, or if so, what it would be ; also, in the arising of unwilled (spontaneous) images in connection with those willed.

2. Evidently, sometimes and in some persons, the subconscious thinking responds more quickly to the task set than does the conscious. This is shown by the spontaneous images arising more promptly than do the willed. That is, the spontaneous image is before the observer before he has decided what image to arouse or it arises in place of it.

3. The images show that not only the conscious but the subconscious mental activity differs in richness of content in different individuals.

4. In case of all the observers—but in some of them more than in others—some of the material stored away under the threshold has evidently remained as originally grouped, as for example, when the visual image of a particular man in a particular environment arises simultaneously and at once. On the other hand, some of the material has evidently been more or less broken up, as for example, where an eye arises spontaneously, when an imagination image of a face is asked and no other features followed it until aroused by a special act of will on the part of the observer. In case of some of the observers the broken-up memory material, the memory elements, have been unconsciously (as shown by the observer's great surprise at the content of the visual images which arise) recombined under the threshold into complicated and appropriate new groups. There has been not alone a breaking up of memory material but, to use Ribot's words, an 'unconscious elaboration' of it. In the observers with whom I have experimented, the memory activity evidently predominates both below and above the threshold of consciousness.

5. The memory and imagination material under the threshold is evidently not all on the same stratum or level as regards consciousness, for some of it arises much more spontaneously and quickly and has a different content. Here too individuality plays a great rôle.

6. From what has been said it will be seen, that the image method makes it possible to obtain information regarding the past life of the individual, the general character and the personal peculiarities of the thinking going on in his mind, not alone above but also below the threshold of consciousness. The applicability of this method in the case of a particular person will of course depend upon his ability and habit as regards the imaging of his conscious and subconscious thinking.

7. The introspections show that the spontaneous images are sometimes the point of departure of the willed images, that is, the involuntary image that arises before the observer has decided what to will acts in the way of suggestion. This shows how important the spontaneous images must be in our daily life. Where the spontaneous images are in the direction of the work in hand, they must save time in that they arise immediately and furnish material already elaborated. On the other hand, if they are not of such a character that they can be used directly in the intellectual work being carried on or as points of departure for conscious thinking along the desired line, they must be an interruption and even a hindrance in the continuing of such thinking. The results show also that the spontaneous images may furnish ideals as regards action. In this respect they may and may not be entirely helpful. One of the observers who took part in these experiments, has very strong and insistent spontaneous auditory images. So insistent are they, that she tells me that they led to her giving up the study of music to which she had devoted several years, and turning to a totally different field of work. She says, that whenever she plays on the piano the spontaneous auditory images precede what she is playing and show her how imperfect is her execution.

8. A comparison of the content of the voluntary images with that of those which are spontaneous, shows that in the case of the visual images of a given observer what is above and below the threshold of consciousness is not materially different.

This result does not support Binet's[1] theory regarding the nature of the subconscious, which is, that there are two personalities running side by side, one above and the other below the threshold of consciousness, as what is above and below the threshold of consciousness, as was said, seems in the case of these observers not to be materially different. It may be otherwise in pathological persons, of course. Cases of double personality certainly suggest this. But such special cases do not give Binet's theory any great universality. Nor does Meyers's[2] theory, which has found support among workers in psychical research, that the subconscious is an expression of the infinite mind, and the conscious an individual matter or a very limited expression of the infinite, get support, for, as was just said, what is under the threshold does not seem enormously richer in content than what is above. Nor do I find anything in these results which leads me to suppose that under the threshold a mental condition exists which makes it necessary to suppose that communication between different persons (telepathy) is possible and which would more or less support Meyer's theory. The results do support Prince's[3] theory that what is under the threshold is an expression of the observer's previous experiences.

9. The results have a farther interest from the standpoint of general psychology.

A. They show that the differences and likenesses between spontaneous and voluntary images ought not to be overlooked in psychology, as has been the case in the past, since through the study and comparison of such images we may go below the threshold of consciousness and get information regarding what is going on there.

B. They throw light on what is called inattention and vacillation of attention. We see that, sometimes at least, this grows out of the fact that the person has a flood of spontaneous images and ideas, which impede and even crowd out voluntary images and ideas. They explain why the genius is so impatient of restraint and may sometimes actually get

[1] On double consciousness, etc.

[2] 'Human Personality,' I., 34 ff., 1904.

[3] 'The Subconscious,' 1 ff., 1914.

on faster by letting himself go, and also why the student in a field of an exacting and foreign character as regards his natural thinking, must take himself in hand or fail altogether in his work. I take the following in the way of illustration from what one of the observers gave to protocol:—

"Als sich mein Studium begann, war es mir kaum möglich, mich in einer Vorlesung irgendwie zu konzentrieren, weil ich beständig durch spontan auftretende V. gestört wurde. Ich have dann versucht, die spontanen V. zu verdrängen und grosse Mühe darauf verwendet und habe es darin bis zu einer gewissen Fertigkeit gebracht, so dass ich jetzt spontane V. willkürlich haben oder nicht haben kann. Sobald ich mich aber etwas gehen lasse, sind die spont. da, und ich bin ziemlich machtlos dagegen."

C. The data obtained lead one to ask whether in future memory investigations along quantitative lines the task of the investigation will not be something more than a filling in of the gap left in the work of an Ebbinghaus and a Müller, something more than a building upon the results already obtained by them. May we not possibly be obliged to begin again at the very bottom and repeat the work in order to feel sure of its foundations. It would seem from these results that instructions given by an experimenter favorable to voluntary effort, or the belief on the part of the observer that he must put forth his will in connection with the task set, while favorable to voluntary memory may have been detrimental to spontaneous memory and vice versa. In short, it does not seem entirely impossible that two persons may have equally good memories as regards the amount that can be reproduced, but that like instructions, as for example, that effort (resp. no effort) is to be used in reproducing a given material, may make it appear that one person has a much better memory than the other or indeed that neither has a good memory.

D. Again, these results put in question the results of certain experiments of Rux,[1] which were inspired by Ach. Rux has attempted to measure the strength of will by using

[1] 'Ueber das assoziative Aequivalent der Determination,' *Untersuchungen zur Psychologie und Philosophie*, Bd. II.

the quantitative data derived from memory experiments without apparently making any attempt to show how much of the work done was accomplished by voluntary and how much by spontaneous memory.

10. The results have a pedagogical interest. .

A. In that they show that it is possible to educate and enrich the subconscious.

B. In that they lead one to ask whether we may not sometimes be placing too much emphasis on the employment of will in connection with the intellectual work to be done. When the student's work is of a creative nature or along the line of discovery and his spontaneous thinking and images are in harmony with the field in which he is working, one can think that the director of a leading institution in America which is devoted to scientific research, showed psychological acumen, when he urged the investigators working under him to take each day some time away from their work not only to give their minds rest but to free themselves from the restraint of thinking in one particular narrow line.

The Image Method Versus the Automatic Writing and Speaking Methods of Penetrating Below the Threshold of Consciousness

Binet and others have used the automatic writing method, in investigating the subconscious. As the image method will naturally come in competition with the automatic writing method in investigations along this line, I have thought it desirable to make some experiments by this method to ascertain how it compares as regards the amount of data yielded with the visual image method in the getting of information of what is going on under the threshold of consciousness.

The experimental results I have given in the work of which this paper is a summary. They show (1) that while theoretically the subconscious experience is reproduced through automatic writing without entering consciousness, to be certain that this actually occurred, that is, to be certain that the experience did not enter consciousness and after such entrance more or less influence and direct the writing, one must have observers who have the ability and the training to introspect very accurately. (2) That the image method has a much wider applicability, as it can be employed with any one who has visual and other images, while the automatic writing method, as is shown by these experiments and by others, is very limited in its application. In these experiments only 2 out of the 19 persons were really able to respond to the task set. (3) The image method gives more information in a given period of time and thereby decreases the difficulty of the introspection. (4) In the image method the experience is brought above the threshold and the observer is encouraged to give his full attention to what occurs, and he may be directed to observe particular things. (5) In the image method it is not necessary to direct the movement connected with the giving of the information into an entirely new channel by substituting the action of lower nerve centers (centers connected with subconscious thinking) for the higher (centers connected with conscious thinking) which usually largely direct it. (6) In a confirmatory way the writing method may be made very useful. The great richness, for example, of what is under the threshold of consciousness in case of M. and O. is shown by both methods. (7) Each method also brings things to the attention not brought out by the other method. The tendency of the writing movements to be at the disposal of what is in consciousness is, for example, very noticeable in case of some observers. In case of M. and O., what is below the threshold evidently plays also a rôle as regards the writing.

Automatic Speaking Method Versus the Image Method

Of some special cases of automatic speaking I have given illustrations in my study entitled 'Die Projektionsmethode' (p. 5, 105). From what is heard by the patient himself or by the experimenter, an idea can be obtained of course of what is going on under the threshold of consciousness. The words occasionally unconsciously spoken by a normal person give one a similar idea. It will be at once evident, however, without any comparative experiments that the image method has a very much broader field of usefulness because of the difficulty of getting an adequate distraction in using the automatic speaking method.

The Image Method Versus the Pathological and the Psychoanalytical Methods of Investigating the Subconscious

The other methods of investigating the subconscious I find less satisfactory than the automatic writing and speaking methods. The objection to the pathological method, where the data regarding the subconscious is obtained for example from cases of double personality, is the feeling of doubt and even mistrust with which one often collects and examines such data.

The objection to the method of psychoanalysis is that the instruction given to the patient to speak out everything that comes into his mind, gives a mass of data which contains not only what is below but what is above the threshold and farther that in applying this method no systematic effort is made, as in the case of the image method, to separate out and classify such data.

Taken all in all, it seems to me, the results show that the image method offers a mode of penetrating below the threshold of consciousness which is at least comparable if not superior to that offered by other methods.

EMOTIONAL POETRY AND THE PREFERENCE JUDGMENT

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In a former study[1] the writer reported somewhat extensive experiments upon the imaginal reaction to poetry and the influence of the various forms of the image upon the affective and the æsthetic judgment. The poetic fragments utilized in this experiment were selected largely because of their imaginal suggestiveness. Occasional comments of reagents upon certain fragments indicated that had highly emotional poetry been employed instead of imaginal poetry different results might have been obtained.

Accordingly, a second series of experiments was planned in order to test the emotional factor in poetry. Twenty-four fragments of poetry, somewhat longer than those of the preceding test, chosen because of their emotional content,[2] were utilized. The judgments obtained are not, however, directly comparable with those given in the preceding series since instead of a grouping of the emotional fragments on the basis of pleasantness-unpleasantness, a grouping of the fragments into eight groups according to preference was asked for. In group 1 were to be placed, according to type-written instructions placed before every reagent, the fragments liked best; in group 8, those liked least; the other fragments in the intermediate groups. After this grouping the reagents were instructed to shade the fragments in each group, placing first in each group the fragment most liked and shading from that to the one liked least. Such an

[1] "The Imaginal Reaction to Poetry," Univ. of Wyom., Department Psychol., Bulletin No. 2.

[2] That the fragments utilized were actually less imaginal in content than those employed in the previous test is shown by the fact that in proportion to the number of fragments and for the same number of reagents they aroused only half as many images.

arrangement was repeated five times at week-intervals. Each reagent was instructed to record his mood before beginning his grouping and to record it again at the close of the experiment. After the first and the fifth preference arrangement the reagent was instructed to rearrange the fragments, on the basis of the vividness of his emotional reaction to them, in four groups: III. Reaction vivid; II. Reaction moderately vivid; I. Reaction slight; O. No emotional reaction. After the number of each fragment on the second record the reagent was instructed to write a word or phrase descriptive of the emotional content of the fragment. In connection with the second preference arrangement, a rearrangement into four groups as before on the vividness with which the reagent projected himself into the content was asked for, with a complete account of the kind of self-projection observed in any case. With the third preference arrangement a grouping of the fragments with reference to the nature of the inner speech was asked for, together with comments upon the form of the inner speech for each fragment. A fourfold grouping on the basis of the vividness of the concrete imagery aroused by reading was requested in connection with the fourth preference arrangement.

Some four weeks after the last preference arrangement a grouping of the fragments into eight groups according to their beauty was obtained. In group 1 were placed the most beautiful fragments; in group 8 the least beautiful; in the intermediate groups the other fragments. As before, the fragments were shaded within the groups. In connection with this grouping answers to the following questions were obtained:

1. What do you mean by 'beautiful'? Answer on the basis of your experience while arranging the fragments.

2. In your opinion is an arrangement on the basis of beauty equivalent to an arrangement on the basis of preference? Why?

3. Is an arrangement on the basis of beauty equivalent to one on the basis of pleasantness?

4. Would an arrangement on the basis of pleasantness be equivalent to one on the basis of preference?

5. What kind of emotional appeal do you prefer in poetry? Can you give any reason for your preference?

6. What kind of emotional appeal do you consider most beautiful? Why?

Seven reagents took part in the experiment; all had had considerable practise in introspective work.

A brief description of the poetic fragments employed seems necessary. The descriptive terms used are taken from those given by the reagents in connection with the two groupings of the fragments made by them on the basis of their emotional vividness. An estimate of the emotional value of each fragment was obtained by adding the numbers of the groups in which a particular fragment was placed by each of the seven reagents for each of the two groupings. The greatest sum obtainable was 42; the least, 0. The sum actually received is given for each fragment in parenthesis after the descriptive summary.

The fragments were as follows: 1, twelve lines, from Browning's 'Saul,' expressive of the joys of living, beginning, 'Oh, our manhood's prime vigor!' (28); 2, nine lines, verse XCII., Canto Third, Byron's 'Childe Harold,' descriptive of the exultation and awe aroused by a mountain storm (33); 3, nine lines, verse XXI. of Shelley's 'Adonais,' expressive of inevitability, futility, grief (20); 4, twelve lines, a lyric expressive of companionship (31); 5, sixteen lines, the eleventh verse of Swinburne's 'The Garden of Proserpine,' with an emotional toning of desire for death, annihilation (17); 6, ten lines from Shelley's 'Prometheus Unbound,' expressive of defiance, beginning 'Fiend I defy thee' (29); 7, fifteen lines from Browning's 'Andrea Del Sarto' beginning 'A common greyness silvers everything,' lines that voice a twilight mood of sadness and resignation (28); 8, ten lines, a translation by Symons of one of Mallarmé's exquisite word-pictures, expressing vague aspiration, calm (13); 9, nine lines, fifth stanza of Swinburne's 'A Forsaken Garden,' expressive of barrenness, weariness (19); 10, eight lines, Stevenson's 'Under the wide and starry sky,' completion (23); 11, twelve lines, fifth stanza of Browning's 'Love Among the Ruins,' descriptive of expectancy and love (25); 12, fifteen lines, from Tennyson's 'The Princess,' 'Tears idle tears' (27); 13, eight lines, Galsworthy's gay wind-song, 'Wind, wind—heather gypsy' (25); 14, eight lines, Blake's 'When the voices of children are heard on the green,' expressive of quiet happiness (17); 15, twelve lines, Henley's famous 'Captain of my soul' verses (34); 16, thirteen lines, first two stanzas of Poe's 'To One in Paradise,' voicing despair (27); 17, sixteen lines, a descriptive piece by Galsworthy, 'We'll hear the uncompanioned murmur of the swell,' expressive of 'God's own quietude of things' (24); 18, seven lines, Yeats' 'Be you still, be you still, trembling heart,' voicing mystical courage (14); 19, eleven lines from Tennyson's 'Lotus-Eaters,' beginning 'There is sweet music here that softer falls,' word-pictures suggesting peace (31); 20, twelve lines, Marston's 'All my roses are dead in my Garden,' expressing despoilment, hopelessness (27); 21, fifteen lines, the hunger for pursuit (16); 22, twelve lines, Yeats' mystical 'Outworn heart, in a time outworn' (14); 23, nine lines, the first two and the last stanza of Moody's 'Heart's Wild-Flower' (33); 24, fourteen lines, Moody's 'Grey drizzling mists the moorlands drape' (23). These fragments were typewritten on separate sheets of paper, convenient for handling. The poet's name did not appear on the fragment and only a few cases of recognition occurred. The fragments, except 4 and 21, were of accepted literary excellence, many of them being classic productions.

On the basis of the data gathered the following points may be discussed: I. The variability and character of the group preference judgment and its dependence upon such factors as the emotional content, self-projection, concrete imagery, the waxing and waning value of the separate fragments; II. The variability and character of the individual preference judgment and its dependence upon peculiarities in the individual reactions; III. The relation of the preference judgment to the judgment of beauty.

I. The Group Preference Judgment

The average position of each fragment for each of the five arrangements by the seven reagents was calculated with the average M.V. for each arrangement. There is a decrease in the average M.V. from the first to the fifth trial, although not a constant decrease, as follows: first trial, 4.839; second trial, 4.535; third trial, 4.783; fourth trial, 4.629; fifth trial, 4.166. The average M.V. for the first arrangement is somewhat high, although not higher than that given in certain other reports on the subjective judgment. It is, relatively to the number of possible positions, higher than the average M.V. in a first arrangement of imaginal poetry on the basis of pleasantness-unpleasantness. It is tempting to attribute this increased M.V. to the emotional nature of the poetry and very probably it should be so attributed. But it should not be forgotten that increased subjectivity is not the only possible cause of increased variability.

With repetition of the arrangements there is lowered variability. There was at first, as has been pointed out by other investigators of the subjective judgment, a greater agreement on the unpreferred fragments with a shift in the last three trials to greater agreement on the preferred fragments. The average M.V. of the fragments in the first six positions for the five different arrangements is as follows: first trial, 4.01; second trial, 4.62; third, 4.04; fourth, 4.22; fifth, 3.27. The greatest agreement is seen to occur on the fifth trial. The average M.V. for the fragments in the last six positions should also be noticed: first trial, 3.77; second, 3.94; third, 4.53; fourth, 4.52; fifth, 4.41. The difference between the M.V.'s for the first and the last six positions is greater for the fifth than for any other trial.

The increasing agreement of the group with repetition of the test is shown by the extent to which every arrangement is correlated with every other arrangement as given in Table I. The progressive increase in coefficient values for successive arrangements is evident, reaching a final value of .89 for the last two trials.

Table I

Correlations between Group-Arrangements.

Trial	I	II	III	IV	V
I		.758	.764	.739	.773
II	.758		.750	.699	.820
III	.764	.750		.820	.863
IV	.739	.699	.820		.890
V	.773	.820	.863	.890	
Av.	.758	.757	.799	.787	.837

Study of the records suggests no explanation for this other than growing objectivity of judgment with increased familiarity with material. With such familiarity the individual judgment would seem to be steadied by social standards.

Table II

Effect upon Preference of Vividness of Emotion, Self-Projection, and Imagery (Combined Records. 7 Reagents).

	1	2	3	4	5	6	7	8	Totals
III									
Emotion—1	16	9	10	5	4	2	5	2	53
Self-projection	25	9	6	3	2	2	1	4	52
Imagery	19	8	11	3	5	0	2	5	53
Emotion—2	13	7	7	1	5	2	1	7	43
Total									201
II									
Emotion—1	9	12	8	7	5	6	3	3	53
Self-projection	5	6	4	5	3	7	4	3	37
Imagery	3	5	4	7	5	3	4	6	37
Emotion—2	9	10	7	7	1	5	3	4	46
Total									173
I									
Emotion—1	3	4	2	7	2	7	7	4	36
Self-projection	0	4	4	3	7	3	3	6	30
Imagery	3	4	3	2	2	4	8	5	31
Emotion—2	2	10	5	6	5	5	9	5	47
Total									144
0									
Emotion—1	0	1	1	2	7	1	3	9	24
Self-projection	3	7	6	6	4	7	7	9	49
Imagery	3	10	8	5	5	9	5	2	47
Emotion—2	2	3	4	6	4	5	4	4	32
Total									152

An attempt was made to determine the influence of various factors upon the preference judgment by obtaining as described above a four-fold grouping, twice for vividness of emotional toning and once each for vividness of concrete imagery and of self-projection,[1] and distributing these judgments under the eight preference groups (Table II.).

From this table it is evident that the three factors are about equally potent but that, in general, rich content, emotional, imaginal, and self-projective, contributed to preference. The figures for the second grouping of the fragments on the emotional basis (fifth preference arrangement) indicate some loss of emotional vividness with repetition.

TABLE III

No. of Fragments	Five Preference-Arrangements			Arrangement for Beauty			Emotion 1	Self-Proj.	Imagery	Emotion 2	Emotional Tone
	Position	Av.	M. V.	Position	Av.	M. V.					
19	1	5.20	3.20	1	1.85	.98	17	15	20	14	Peace.
23	2	6.08	2.38	3	5.42	2.03	17	15	15	16	Heart's wild flower.
2	3	7.45	2.86	5	6.57	4.65	18	13	19	15	Exultation.
17	4	8.05	2.56	2	4.57	2.94	13	15	15	11	Quietude, companionship.
4	5	8.74	3.44	10	11.28	5.31	18	9	10	13	"You."
11	6	9.42	2.54	6	6.71	3.10	10	12	13	15	"Love among the Ruins."
7	7	9.60	5.20	4	6.57	2.49	12	15	16	16	Twilight: regret.
15	8	9.82	3.00	12	14.00	3.71	19	9	6	15	Fortitude.
1	9	9.85	4.68	7	7.42	1.51	16	12	12	12	Joy of living.
24	10	12.50	4.67	14	14.57	4.04	13	9	15	10	Weariness; greyness.
12	11	12.85	2.96	8	9.71	3.75	15	11	7	12	"Days that are no more."
22	12	12.91	3.15	11	11.71	2.32	10	6	9	10	Mystical rebirth.
10	13	13.42	4.66	17	16.42	5.35	13	12	13	10	"Glad did I live, gladly die."
16	14	13.51	2.35	13	14.42	3.63	14	10	6	14	Despair.
20	15	14.48	4.55	18	16.71	6.33	16	12	13	11	Hopelessness.
8	16	15.17	1.62	9	10.42	5.46	6	6	11	7	Aspiration.
18	17	15.40	3.20	15	15.57	4.20	7	5	3	7	Mystical courage.
5	18	15.42	2.63	21	17.57	3.46	7	7	4	10	Eternal sleep.
13	19	15.54	6.00	22	18.71	3.55	12	12	12	13	Irresponsibility.
14	20	15.63	3.07	23	18.57	2.08	8	13	9	9	Content.
3	21	15.80	3.54	19	16.85	4.45	12	6	4	12	Futility; grief
9	22	16.48	3.10	16	16.14	4.12	12	11	9	7	Barrenness.
21	23	17.82	3.42	20	17.14	5.22	9	9	12	7	Pursuit.
6	24	18.77	3.73	24	21.00	2.00	13	16	11	16	Defiance.

Table III. gives the position, average and M.V. for every fragment, for five preference arrangements, and for the one arrangement on the basis of beauty, together with numbers representing the vividness of the emotional, self-projective, and imaginal reactions, obtained by adding together the

[1] By self-projection is meant an explicit self-reference in whatever form. Cf. "Literary-Self Projection, PSYCHOL. REV., 19, 299–311 (1912).

number of the groups in which the fragment was placed by the seven reagents. An attempt is also made to describe the emotional tone of each fragment. This table confirms the conclusion that rich content contributes to preference but suggests also that imaginal content is slightly more potent in determining preference than are the other factors studied. This is shown also by grouping together the six fragments that the records show to be most emotional, most conducive to self-projection, and most imaginal with an indication of the position of each fragment in the preference series (Table IV.).

TABLE IV

Most Emotional 1	Position	Most Emotional 2	Position	Most Self-Projective	Position	Most Imaginal	Position
15	8	6	24	6	24	19	1
2	3	7	7	7	7	2	3
4	5	23	2	17	4	7	7
19	1	2	3	23	2	23	2
23	2	15	8	19	1	17	4
1	9	11	6	14	20	24	10
20	15			2	3		

The two fragments most definitely imaginal (19 and 2) rank respectively first and third. Many fragments occur in two or more of the groups.

Putting the matter in another way we see that of the six fragments most preferred 19 and 23 are imaginal, favor self-projection, and are emotionally toned; 2 is imaginal and emotional; 17 is imaginal, and induces self-projection; 4 is emotional; 11 is emotional, imaginal, and favors self-projection.

A comparison of the orders received by the different fragments for the successive arrangements indicates that fragments 17, 11, 7, 24, 10, 8, and 22 (slightly) waxed in value; fragments 1, 16, 20, 13, 14, and 4 (slightly) waned in value; fragments 19, 23, 3, 9, 21, 6, 2, 12, 15 remained relatively static; fragment 18 waxed in value and then fell; 5 waned and then waxed in value. Reference to the waxing and waning value of the fragments will occur later in discussion of the arrangement on the basis of beauty.

II. The Individual Preference Judgment

The effect of the individual reactions upon the preference judgment was evident and makes necessary a summary statement of certain characteristics of the reagents. There were seven of these reagents as stated before. The conclusions, relatively to their general reactions, are based upon extensive acquaintance with the observers in psychological experimentation.

With reference to imaginal tendencies the observers fell into three groups.

The first group includes reagents Rgr and Ele, subjects in whom there is a strong preponderance of visual imagery. Rgr's visual images are vivid and detailed. Ele shows a strong inclination to emphasize form; she is accustomed to changing all sounds into visual forms. Voices she pictures in series of waves and lines at different levels; she compares different pitches by reference to the heights at which the translating lines are placed. She has a great liking for mathematics.

The second group includes Jan, Hne, and Jdo. These subjects show mixed imagery. Although they employ visual imagery to some extent, they appear to be much more dependent upon kinæsthetic and organic material. Auditory content is, however, very potent for Hne.

The third group includes Ado and Tbu, who are strikingly deficient in visual imagery, an incapacity which in Tbu's case is evidently conditioned by very poor eyesight. Tbu relies almost wholly upon inner speech and is strongly inclined to accept the imageless thought proposition. Ado makes much use of kinæsthetic material and in this respect might more properly be classed with the second group.

The form which self-projection assumed was also somewhat characteristic within the same groups.

For Rgr and Ele such self-reference appeared to be highly objective. Rgr projects herself visually within the scene but without dramatic or kinæsthetic participation in the scene. Ele is "there" as a spectator only. She assumes, without visualization of self, a definite orientation toward the scene, always on the outskirts, where she is able to get a good view of the situation.

For Hne and Ado, the self-reference is highly colored. Hne gives a visual self-projection that is fused with kinæsthetic and organic material; she is within the scene. Ado is also within the scene, part of it, but without visualization of self. Her participation is definitely dramatic, emotional. These two reagents "subjectify" the poetic material.

Jan and Jdo identify themselves kinæsthetically or organically with persons or inanimate objects described. Sometimes for Jdo there is a projection of kinæsthesis into a visualized figure, not of self. As distinguished from Hne and Ado, these reagents appear to project or objectify the subjective reaction.

Tbu reports little self-reference except that in inner speech he is at once speaker and listener.

A grouping on the basis of the inner speech effects some changes in the distribution of subjects. This inner speech is auditory for Hne, Tbu, Jdo, Ado, and Rgr. But of these reagents Hne is the only one who heard, to any extent, fragments read in voices other than her own. Tbu, Ado, and Jdo make much of inner elocution, and Tbu is almost wholly preoccupied with this aspect of the reaction.

Ele and Jan were sceptical as to auditory content for their inner speech. Ele reported again curious translations of the inner speech into visual forms.

The average (with M.V.) was calculated for each fragment for the five arrangements by each reagent and the position assigned each fragment on the basis of this average. The average M.V. for each reagent from the average of his five arrangements was calculated and gives us an indication of his individual variability. His average M.V. from the average group judgment was also determined and this indicates the extent to which his judgment was representative of the group.

The variability of each reagent from his own average for the five trials was as follows in the order of least variability: (1) Tbu, 2.21; (2) Rgr, 2.28; (3) Ado, 2.71; (4) Ele, 3.11;

(5) Jdo, 3.71; (6) Jan, 3.56; (7) Hne, 4.09. Increased variability seems, in general, to characterize the more emotional reagents (determined by their fourfold grouping of the fragments), while the effect of the emotional material in increasing the variability is shown by comparison of the individual variability in this test with that found when less emotional poetry was utilized; it is proportionately much higher in the present test.[1]

The average variability of each reagent from the average of the seven reagents for the five arrangements gave the following order: (1) Jdo, 2.75; (2) Ado, 2.90; (3) Rgr, 2.96; Ele, 3.07; (5) Jan, 3.48; (6) Hne, 3.78; (7) Tbu,5.19. The most interesting point in this listing of reagents is Tbu's shift in position, which with high personal consistency indicates a different basis of judgment from that of the other reagents, explanation for which is to be found in his introspective reports. On the whole, it may be noted, the variability from the group average is no more extensive than that found in imaginal poetry.

Table V

Preferred Fragments

	Rgr	Ele	Hne	Jan	Ado	Jdo	Tbu
1	19	17	11	4	19	19	15
2	2	23	4	13	17	23	1
3	7	19	12	23	7	7	2
4	23	2	19	19	23	17	10
5	1	7	1	18	2	10	24
6	4	24	13	17	11	4	5

An arrangement in order of the six fragments which were most preferred by each of the reagents shows at once the effect of the individual differences in reaction (Table V.). We note that of Rgr's preferred fragments, the first four are exactly in the order of imaginal vividness, largely visual. Rgr states very definitely that she prefers poetry which calls up vivid visual images, unless, as in fragments 13, such images are grotesque. Ele's six preferred fragments are just the six

[1] A great variability from her own average in the judgments on emotional poetry in contrast to great self-consistency in judgments on imaginal poetry was shown very definitely by the one reagent (Jdo) who participated in both tests.

most imaginal fragments, although not in the exact order of the group. Ele also expresses a preference for poetry conveying the clearest imagery and is particularly pleased with what she calls sound-pictures.

Hne's preferred fragments are chiefly emotional in tone; 11 and 4, both highly emotional, represent her first and second choice. Jan's preferred fragment is 4, which is emotional in its appeal, but his other choices give some indication of dependence upon imaginal richness. He also prefers 18, a mystic fragment of little sensuous content. Ado's and Jdo's preferred fragments show the influence of imaginal content as well as of emotional toning.

Tbu's preferences are distinctly individual, determined largely by the kind of emotion expressed which Tbu prefers to be strong in nature, expressive of a desire to act, to conquer. Neither 19 nor 23, so generally preferred by other reagents, occur among his first six fragments; the absence of imaginal fragment is very evident.

Table VI

Correlations of Av. reference Arrangement of Each Reagent with that of Every Other Reagent

Reagent	Rgr	Ele	Jdo	Jan	Hne	Ado	Tbu
Rgr		.601	.590	.461	.232	.700	.237
Ele	.601		.410	.325	.209	.702	.103
Jdo	.590	.410		.536	.230	.569	.200
Jan	.461	.325	.536		.256	.241	−.272
Hne	.232	.209	.230	.256		.381	−.059
Ado	.700	.702	.569	.241	.381		.193
Tbu	.237	.103	.200	−.272	−.059	.193	

The tabulation of preferences suggested the working out of the coefficients of correlation for the average preference arrangement of each reagent with every other. These are given in Table VI. It is evident from this table that reactions on the basis of imaginal qualities are most representative (Rgr, Ado, Jdo, Ele) and that the subject most visual in reaction (Rgr) gives the highest average correlation. Should such a conclusion be substantiated by a more extensive investigation it would seem to throw light upon the kind of literary material that would probably have constant value for a long period of time and the type of critic that would best represent the average reaction in the long run.

Certain other factors influencing the individual reactions are evident from the tables and the introspective reports. Table III. indicates that subdued emotions are more generally preferred by this group than are violent emotions. The individual reports confirm this, although the effect of the mood of the day is mentioned by several reagents as influencing their preferences.

Rgr. "In poetry the emotional appeal which I prefer depends largely on my mood.—I like poems about nature as they arouse emotions outside of one's self."

Ele. "An appeal to quiet, drowsy, lesiurely, reminiscential feelings suits me best.—I do not like noise, boisterousness, confusion."

Hne. "The appeal preferred depends upon the mood. Usually prefer something expressing longing unfulfilled, or the joy of living."

Jan. "I do not know that I can select any one emotional appeal; sometimes it's one sort, sometimes another. Pathos perhaps makes the greatest appeal."

Jdo. "I prefer the emotional tone to be in harmony with my mood which varies strongly from day to day. In general I prefer a sad toning."

Ado. "I like an emotional appeal that is melancholy in tone."

Tbu. "I prefer a strong emotional appeal to any of the pleasant emotions and sometimes to those generally considered unpleasant. Usually, however, I prefer such an appeal to emotions as are aroused by fragments 1 and 15, feelings of desire to act, conquer, oppose even unconquerable forces. The reason for this preference so far as I can judge is that such emotions are not common in me. However much I may consider them ideal, I do not possess them. It is their contrary nature that appeals to me."

In order to test specifically the effect of the mood of the day upon the reaction to strongly emotional poetry, the following tabulation was made (Table VII.). The seven fragments most melancholy in tone were selected, 3, 5, 9, 12, 16, 20, 24; three of happy buoyant coloring were chosen, 13, 14, 1; and three of strong aggressive emotion, 2, 6, 15. Next, record was made from the introspective notes of any cases where the reagent reported strongly depressive moods at the time of the experiment. Eight cases of this occurred. The effect of the mood-dominance was then determined by subtracting the position on the day in question for the given fragment and the given reagent from the average for that reagent's five arrangements. A minus sign indicates increased preference for the fragment for the given day; a plus sign indicates decreased preference.

The table would seem to suggest some interesting differences between the reagents as to the effect of mood upon their preferences. Jan and Jdo show very evidently that a mood of depression increased for them the preference for melancholy poetry and in Jdo's case very considerably lowered the liking for buoyant fragments. The effect of the mood upon fragments 2, 6, 15 is less constant. Ado's record indicates a general lowering of values under depression, with, in a few cases, added appreciation of the melancholy fragments. Rgr shows less effect of mood upon preference than any other reagent, and that effect is mainly a lowering of values. The effect of depression is somewhat variable for Ele and Hne, both records suggest that harmony with the mood is likely to increase preference. Under the influence of the given mood these reagents show inclination to stress 2 and 6. The effect of mood upon the emotional reaction is a very important one. The above discussion, however meager, suggests a method by which the problem may be attacked.

Jdo's comments on the effect of mood upon the reaction for any particular day are more complete than those of the other reagents and in certain respects instructive. There were days of æsthetic toning and other times when it required considerable effort to surrender to poetic suggestion. These differences were due to general mental conditions, rather than to experimental conditions. At the close of the experiment Jdo was usually in a more æsthetic mood than at its beginning. The most adverse general criticism upon experimental investigations of this sort she finds in the reduction of the time needed for æsthetic absorption. Short fragments suffer in comparison with the longer productions from which they are taken. Fragment 12, for instance, frequently failed 'to catch fire.' Rereading was necessary. Again, Jdo noted that the first fragments suffered by being read before she had assumed a poetic mood; or, at times of increased susceptibility to outer suggestion, fragments 1 and 2 set the tone for subsequent reactions.

The second arrangement was made under a mood of great depression, heightened by the 'grey toning' of the weather. Jdo recorded in her notes that fragments expressive of sadness and futility were given a higher value than before. She was aware also of a tendency to react against the mood by assigning high value to fragments expressive of fortitude. At the third arrangement a mood of æsthetic sadness again enhanced the value of fragments of melancholy tone. On this occasion the lilt and swift mocking rhythm of 9, 14, and 13 were found very distressing. She reports, "They move too rapidly and lightly to fit in with the tempo of my mood." At the close of the test the pulse-rapidity was found to be 78. The fourth arrangement was made when the subject was in a scientific mood that contrasted strongly with the æsthetic mood of the previous week; there was restlessness present and distaste for taking time for the experiment. The melancholy-toned fragments were conspicuously less pleasing than before. Fragments 13 and 14 now fitted into the rhythm of the day and were shifted from the eighth to the second group. The pulse was 90. The fifth arrangement was made under the influence of a "hurry-mood" (pulse 94); Fragment 13 was felt to express exactly the personal tempo for the day.

Besides investigating the influence of the imaginal and emotional reactions upon preference, the experimenter made an attempt to determine the effect upon preference of the different forms of the inner speech. In particular, an effort was made to determine whether certain fragments encouraged an auditory inner speech and others stressed the kinæsthetic quality of inner speech. To this end the reagents were requested, in connection with the third preference arrangement, to place, if possible, the fragments in two groups, one group being for fragments in which the auditory aspect was the more pronounced in the inner speech, the other for fragments that stressed the kinæsthetic factor. The attempt

Table VII

Effect of Mood on Preference

Mood Reagent	Arrangement							
	1st Disgusted Jan	1st Depressed Ado	3d "Blue" Ado	2d Sad Jdo	3d Depressed Jdo	3d Dissatisfied Rgr	4th Impatient Ele	5th Disheartened Hne
No. of Frag.								
3. Futility Grief	−6.2	+6.8	−9.2	−4.6	−3.6	−4.2	+1.2	+5.4
5. Eternal Sleep	−10.8	+4	−6.0	−6.2	−3.2	0.0	−3.2	−2.8
9. Barrenness	−3.8	−1.6	+1.4	−7.8	+2.2	−4.4	−1.0	−4.4
12. "Days that are no more"	+1.6	+3.8	+1.8	−1.0	−5.0	+3.0	−9.6	−3.0
16. Despair	−3.2	−1.8	+2.2	+2.4	−3.6	+1.4	+0.6	+1.8
20. Hopelessness	−8.8	−0.2	−6.2	+1.4	−3.6	+2.0	+4.2	+0.8
24. Weariness	−1.2	+2.2	+5.2	−11.6	−0.6	+3.4	−1.6	+1.8
13. Gayety	−3.2	+0.8	−1.2	+2.6	+11.6	−0.4	+3.2	+6.0
14. Happiness	−4.2	−0.8	+0.2	+3.0	+9.0	+2.8	−2.8	+0.8
1. Joy of living	+8.2	+1.8	−0.2	+5.6	+6.6	0.0	−3.2	+12.2
2. Exultation	−0.6	+1.6	+4.2	+8.6	−2.4	0.0	−3.2	−10.4
6. Defiance	+2.6	+1.6	+1.6	−2.8	−3.8	−0.2	−15.0	−8.0
15. Fortitude	−5.8	+6.4	−4.6	−8.0	+2.0	−0.6	+0.2	+2.0

proved abortive, although the comments upon the variations of the inner speech were instructive. Only two reagents (Tbu and Jdo) were willing to attempt the grouping suggested. Tbu's choice of fragments in which the auditory side was most stressed was as follows: 5, 8, 9, 10, 12, 13, 14, 16, 19, 20, 21. Jdo's selection was as follows: 4, 10, 12, 13, 14, 19. There are five fragments selected by both of these reagents.

Fragment 21 was written in dialogue form, the two speakers being a mother and son. Nearly all the reagents note a difference in inner speech with the transition from one part to another. Hne hears her own voice for the mother's part, and a man's deep voice for the son's. She finds the fragment pleasing. Tbu, on the other hand, who hears the man's voice in his own, and the mother's voice in a squeaky disagreeable voice, does not like the fragment.

Ele reported that she seemed to see the inner speech. "Inflections are represented by different levels upon a scale." For Jdo, there was a prominence of the visual verbal side (with subordination of the inner speech) for fragments 4, 9, 13, and 17. These two subjects are the only ones referring to visual factors in connection with the inner speech.

III. The Preference Judgment and the Judgment of Beauty

The introspective reports of the reagents, except those of Tbu and Hne, do not indicate much difference in their basis of judgment when they shift from the category of preference to that of beauty. Ele and Rgr assert that there is no difference introspectively between the two forms of judgment. Rgr adds, "I would not prefer those fragments which did not appeal to me as beautiful." Jdo makes the preferred fragments largely correspondent with the beautiful ones although she reports that there may be poetry which is merely pleasing and preferred for that reason. Ado finds little difference between the two categories, except that in a preference arrangement she gives more attention to the thought and in the beauty arrangement more attention to style and form of expression. Hne reports considerable difference between the two kinds of arrangement. She also emphasizes the expression-side as important for the judgment of beauty and the thought-side as important for preference. Tbu makes the following distinction, "By beautiful I mean a fragment that is pleasing in rhythm, rhyme, and meaning. Preference is based on the emotional reaction."

Some differences are given as to the emotional appeal that seems most beautiful in comparison with that which is preferred. Ele, Rgr, and Ado find no difference Ele states that an appeal to quiet drowsy emotions is at once most preferred and most beautiful; Rgr finds an appeal to peaceful emotions most beautiful; so, too, does Jan. Jdo writes, "I feel the appeal to renunciation, to world-sadness, to wistfulness, to serenity most beautiful." And Tbu, "A lack of emotional appeal seems most beautiful to me; something calm, quiet, not at all exciting." Throughout the group there is evident a tendency to reduce the value of personal emotion as a factor in the judgment of beauty.[1]

Correlation of the average arrangement by the seven reagents on the basis of beauty with the average on the basis of preference (five trials) is, however, very high, .894. A correlation of this average judgment for beauty with the average of the first and the fifth preference arrangement gives as coefficients .729 and .941 respectively. This last coefficient may indicate merely that the growing agreement of the group shown in the correlation of successive arrangements for preference (Table I.) continued in spite of the shift in category and the lapse of four weeks; or, more probably, it shows that with familiarity with the fragments preference is determined more definitely by the element of beauty than is evident in the earlier trials. A preliminary arrangement for beauty would have been most valuable in this connection.

The average M.V. for the arrangement on the basis of beauty is 3.61, a lower M.V. than found for any of the five preference arrangements. This lowered M.V. suggests a more objective standard for the judgment of beauty than for a judgment of preference.[2] Tbu, in particular, shifts his basis of judgment. The correlation coefficient for his arrangement on the basis of beauty with the average of the group is high, .783.

In particular, it seems probable that a beautiful fragment is more constant in its value than a preferred one and falls less in value with familiarity in the repeated arrangements. There are several ways in which one may test this assumption. One may, for instance, inspect the order of fragments in the first preference arrangement (group average) and note which fragments show striking discrepancies with the order of

[1] This report is quite in accord with Souriau's conclusion. "La Reverie Esthetique," p. 104 f.

[2] Compare Müller-Freienfels, R. "Psychologie der Kunst," II., p. 171.

fragments in the arrangement for beauty (group average) One might then check this list with the numbers of those fragments that have already been cited as showing evidence of a waxing, waning, or static value in the repeated preference arrangements. Following this out, we find that fragments 4, 13, 14, 15, 16 and 20 give a much higher value (five or more places) in the first preference arrangement than in the arrangement for beauty. With one exception (15) these are just the fragments which were found to fall in value with the repeated arrangements. Fragments 7, 8, 9, 12, and 17 show a much higher value in the arrangement for beauty than in arrangement for preference. Three of these fragments 7, 8, and 17 were fragments that waxed in value with the repeated arrangements. The outstanding fragments are instructive. Fragment 15 is very definitely a preferred fragment rather than a beautiful one; fragments 9 and 12 are judged to be beautiful but in spite of that are not highly preferred.

Again, taking the average arrangement for beauty and dividing it into two sections, the twelve fragments least and the twelve fragments most beautiful, we find that of the twelve most beautiful fragments, five were those which waxed in value in the preference arrangements, five were static in value, two waned in value. Of the twelve less beautiful fragments, three fell in value in the preference arrangements, five were static, two waxed, two (18 and 5) fluctuated.

The conclusion seems justified that the beautiful has a value which holds its own or waxes with familiarity.[1]

IV. Summary and Conclusions

As a general outcome of the experiment we conclude that the group reaction to emotional poetry is slightly more subjective than the group reaction to imaginal poetry. Familiarity with the material reduces the group variability. Rich content, emotional and imaginal, is shown to contribute to preference, with a slight advantage in favor of imaginal content as a determining factor. Certain fragments appear

[1] Müller-Freienfels, *op. cit.*, II, p. 168.

to fall in value with repetition of the judgment; others to increase; others are static.

The effect of individual differences upon preference is so evident that a grouping of the reagents on the basis of type of reaction is instructive. The more emotional subjects appear to be more variable in their judgments. The preferences of four of the reagents show a great dependence upon imaginal content; emotional vividness is potent for two subjects; and kind of emotional content for one. The intercorrelations indicate that preferences based on imaginal content have a more representative value than preferences determined by emotional content. The latter are influenced by the mood present at the time of choice.

A group arrangement on the basis of beauty correlates very highly with the average group preference arrangement and would seem to be, for most of the observers, determined in much the same way. This second judgment is, however, less subjective than the preference judgment. Apparently the more beautiful fragments have a more constant value than fragments that are merely preferred. Since the former do not wane in value as do the latter, in time the two categories closely approximate each other.

In general, the order of merit method, in conjunction with an analysis of individual reports, appears to afford a most excellent means of studying the æsthetic reaction.

AN EXPERIMENT IN ASSOCIATION

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THE method used in this experiment was devised by Dr. J. E. Lough, of New York University. It is simple and easy to execute, and at the same time it is very effective. The experiment is dominantly mental in character.

1. AIM

This experiment was studied in the light of the influence of (*a*) practise, and (*b*) such transient factors as distraction, fatigue, concentration, state of health, etc.

2. METHOD AND MEANS

The experiment was conducted individually, the writer being the subject. The means used were the test and key sheets of the Lough Association Method. Other means were a common lead No. 2 pencil and paper to write the associated letters on. Time was kept by an ordinary watch.

The test sheet contained ten rows of letters; each row contained twenty letters. These letters were in non-alphabetic order. The key sheet had two rows of letters on it, one just above the other. The top row was in alphabetic order, from A to T, inclusive. The bottom row was placed in random order and each of its letters was directly under a letter of the top row. For convenience a test sheet and keys have been inserted. On this sheet seven keys are given, the first in full, but as the top row is the same for all keys only the bottom row of each of the others is given.

3. PROCEDURE

This experiment was taken in three divisions. The morning test (*A*) was taken between seven and eight o'clock, with emphasis on speed, *i. e.*, it was quantitative in nature. The afternoon test (*B*) was taken between one and two o'clock, and was qualitative in nature, the emphasis being put on the quality of the work rather than speed. The evening test (*C*) was taken for both speed and accuracy and came between ten and eleven o'clock. The general course of these three tests, except the aspects already described, was the same in all.

Test Sheet

																				Time
K	C	E	N	O	R	A	F	B	I	L	G	S	M	P	T	D	J	H	Q	
M	K	N	G	O	L	C	A	E	B	T	I	F	Q	J	P	H	R	D	S	
C	D	A	T	G	I	S	K	R	N	J	M	Q	S	E	H	O	P	L	F	
Q	T	C	P	F	J	N	L	I	C	D	R	G	S	A	E	K	H	B	M	
T	P	H	M	J	N	L	S	O	F	D	G	E	Q	L	K	A	N	B	I	
N	I	B	Q	E	H	D	T	J	R	F	A	K	S	L	O	G	M	C	P	
A	Q	I	K	E	G	T	S	J	D	O	H	C	F	M	B	R	L	P	N	
S	A	P	J	Q	M	G	D	F	T	R	K	I	N	H	L	E	O	C	B	
J	E	I	B	D	N	G	S	O	C	M	L	A	Q	F	R	P	T	K	H	
G	N	E	D	K	B	S	R	Q	H	P	C	J	L	T	M	F	A	O	I	

Keys Used

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T
1	U	Y	T	M	B	J	C	Z	C	L	Y	K	A	E	G	F	I	H	N	D
2	T	M	H	I	A	S	C	B	X	D	E	J	F	Y	W	G	O	K	U	L
3	S	X	Y	K	N	J	U	P	T	P	E	C	O	Q	R	A	D	E	Z	L
4	M	T	J	O	D	P	V	A	Y	C	F	B	S	Q	E	W	G	N	H	M
5	Z	U	F	L	Y	M	B	W	Z	E	R	D	H	A	V	J	N	G	I	O
6	C	P	G	Q	K	T	A	X	U	N	Y	O	J	F	R	L	W	I	M	D
7	F	G	X	J	R	W	V	L	D	M	B	J	J	S	C	Y	I	K	U	Z

Making the associations of the letters was done in the following manner: The key, until all associations were thoroughly committed to memory, was kept just above the line, on the test sheet, on which the subject was working. The letters in each line of the test sheet were taken consecutively as they were approached, regardless of their order. Each one was matched with its likeness in the upper row on the key, and under it was written the particular letter which appeared under its likeness in the top row of the key, *e. g.*, A was first in the top row on the key: suppose that under A in the bottom row of the key was Z, then whenever A was found in a line on the test sheet Z was written under it. For further illustration take an actual case. On the test sheet K is the first letter in the first line; by running down the top row of letters on the key (No. 1) we find that K has Y under it; therefore we would write Y under K on the test sheet. All the other letters are associated in the same way with the various letters of the alphabet.

These associations were made very slowly at first. The subject, however, soon learned a few of the associations or equivalents, and learned how to make short cuts from the letters on the test sheet to their equivalents in the key, without going to the first of the key and following it letter by letter till the right one was reached as the tendency at first was to do, and thus continually decreased the time for each test. As these associations were committed to memory, this entire process of referring to the key for the equivalent was, of course, syncopated. When the associations became well fixed in consciousness the sight of a certain letter on the test sheet brought up an immediate image of its equivalent in the key and the motor-writing impulse was discharged, resulting in the hand movement executing the writing of the equivalent.

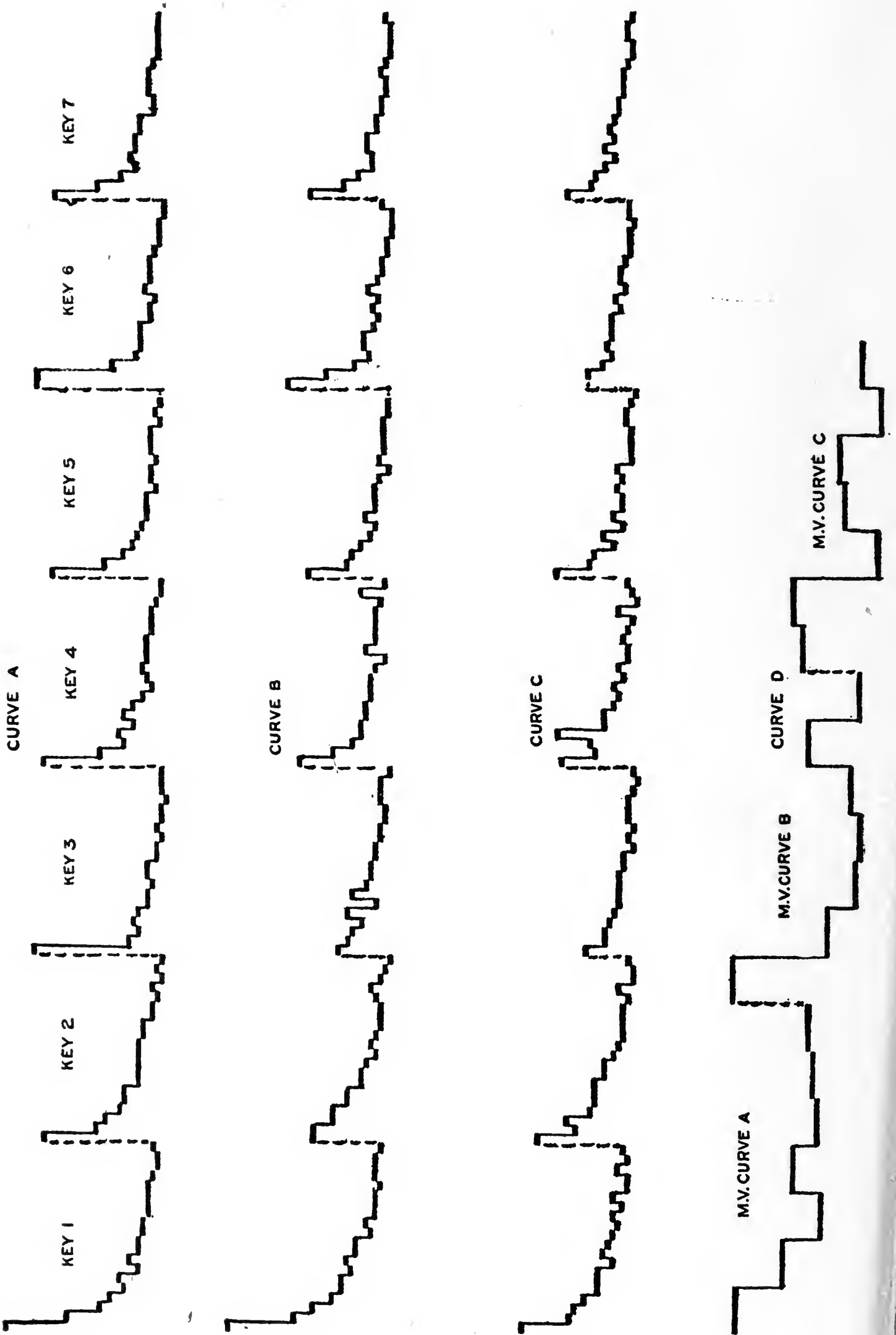
In this experiment the completion of a test sheet consisting of ten lines constituted a test, one of the ten lines constituted a trial.

Twenty tests were made in each key, except the fifth, which by oversight was discontinued after nineteen tests. When one key was finished another was immediately begun. Seven keys were finished, covering a total of one hundred and thirty-nine days. On account of illness one test was missed in both the morning and the evening series. No other break occurred in the entire experiment.

4. Results

The results of this series of tests are very interesting, especially from the standpoint of practise, and the effect of changing keys.

Curves are plotted and tables compiled from the daily


KEY 1
KEY 2
KEY 3
KEY 4
KEY 5
KEY 6
KEY 7
CURVE A
CURVE B
CURVE C
CURVE D
M.V. CURVE A
M.V. CURVE B
M.V. CURVE C

records. These curves and tables show concretely and graphically the learning process or habit formation in this experiment.

The tables show, for each key, the daily record in seconds for the average of ten trials, or one test, the general average, the maximum and minimum trials, and the mean variation for twenty tests.

Curves *A*, *B* and *C* represent the daily series of experiments as heretofore explained. Curve *D* represents the mean variations in the various keys and for the three series. This curve shows that in each succeeding key the mean variation became smaller and smaller.

The subject found that to try to rush either in getting adjusted in order to start at a given time, *e. g.*, when the second hand of the watch reached the thirty seconds mark, or to finish a trial in a shorter period than the previous one nearly always resulted in a loss rather than a gain in time. It seemed that consciousness rebelled against any acts of coercion. Usually when the best records were made the process was almost unconscious, *i. e.*, no special effort for speed was being made.

Some associations in each key were easily made because of their familiarity. That is, they formed initial letters of familiar names and for that reason were easily remembered. After these, the associations of the first and last letters with their equivalents were the easiest to form. The remaining associations then were retained by sheer memory.

At times it was very hard to concentrate the attention, and as a result a very poor record was made. Many times the subject wasted time in trying to recall a certain equivalent instead of looking at once at the key.

In some cases the sequence of letters offered resistance to conscious activity. For instance, in key No. 1 Y is the equivalent of both B and K, and it happens that B and K come together in the last line of the test sheet, thus causing two Y's to come consecutively in the association. In this case the subject had a feeling when writing the second Y that he was either repeating the same association or making a false association which caused considerable distraction. In key No. 7 the letter J comes twice consecutively; its equivalents are L and M; it happens that in the ninth line of the test sheet the letters M and L come together, thus causing a repetition in the association as explained above.

The subject had another source of distraction closely akin to that mentioned above, namely, making a second association and thus writing the wrong letter. To illustrate this process let us refer to key No. 1; D in the top line has its equivalent M in the bottom line; while M in the top line has its equivalent A in the bottom line; when the key became well memorized there was a tendency, upon seeing D, to run ahead and, instead of writing its equivalent M, to write M's equivalent A. This difficulty may have been due to a lack of concentration of attention.

It was also found that a cold, fatigue or unaccustomed noise, persons entering the room, knocking on the door, etc., caused slow work, or an increase in the time record.

The form of the curves *A*, *B* and *C* is very striking. Each one of these composite curves contains curves of the seven keys used. The broken vertical lines between these curves are merely connections and have nothing to do with the experiment.

It is very significant that in these curves for the various keys the greater part of the gain comes during the first day's practise. For instance, in curve *A*, key No. 1, the total gain was 32.5 seconds, while the gain of the first day was 13 seconds. In key No. 3 of the same curve the total gain was 28 seconds, while the gain for the first day was 20 seconds, thus leaving a gain of only 8 seconds during the remaining nineteen days.

For the most part the key curves in curves *B* and *C* make less gains than those in curve *A*. This is due, however, not so much to a failure in reaching low minima as to the fact that the maxima were lower than those in curve *A*. This was expected because of the practise in the same keys in curve *A*. While the mental attitude was different and while the aim was not identical in the three series, nevertheless, the practise had the same effect on consciousness and the habit-forming processes; and, therefore, the second and third series had the benefit of the practise done in the first series.

In passing from one key to another, of course there was a considerable rise in time. It has been contended by some authors that such rises in changing the reaction to certain stimuli are caused by interference; others, Bair,[1] for instance, think not, unless it takes longer to do the new test than it did to do the preceding one.

It seems to the writer that there are two possible explanations: first, if the time taken in doing the new experiment is less than that for the old one the initial rise is due simply to the general law of habit formation and not to interference of associations formed in the previous habit. Because if such interference does exist, and this added difficulty plus the normal difficulty in forming any habit be fused together, it would undoubtedly take a longer time to perform the new experiment the first time than it did to perform the old one the first time; and, secondly, it is probable that there is, on account of similarity and recency when two experiments are related in character, a certain amount of interference which would tend to decrease the time. It is also probable that a certain amount of the learning in the first experiment is transferred to the new one which would tend to decrease the time. With one of these forces tending to increase the time and the other tending to decrease it a resultant is obtained which ordinarily gives as a consequence a lower time record than occurred in the former experiment. It seems to the writer, from his study of general experimental findings and from a close study of his own experiments in the learning process, that the latter hypothesis is the more tenable.

5. Conclusion

In this experiment we have the typical learning process. This process is made graphic by curves which show very rapid progress at first, but finally the rate becomes very slow.

[1] Bair, J. H., "The Habit Curve," Psy. Rev., Monog. Suppl., 1903.

Tables for the Seven Keys

Average Time in Seconds for Each Test of Ten Trials

Key 1 | *Key 2*

Date	*A*	*B*	*C*	Date	*A*	*B*	*C*
Oct. 22	45.5	48.5	37.4	Nov. 11	38.2	30.9	33.8
" 23	33.3	35.2	26.8	" 12	26.7	30.5	26.1
" 24	26.5	31.3	26.4	" 13	25.4	26.6	27.5
" 25	24.2	27.4	24.1	" 14	22.1	26.9	22.6
" 26	21.2	24.7	21.4	" 15	20.9	23.8	21.6
" 27	21.8	24.5	20.9	" 16	21.4	24.4	21.6
" 28	18.5	22.8	20.8	" 17	21.4	21.4	22.6
" 29	20.1	23.7	17.9	" 18	17.9	20.7	20.2
" 30	17.9	21.5	20.0	" 19	18.4	20.1	20.1
" 31	17.7	21.7	19.1	" 20	17.9	18.5	18.0
Nov. 1	17.0	19.1	18.3	" 21	17.8	19.3	17.0
" 2	16.6	20.1	17.3	" 22	17.5	17.7	15.9
" 3	15.8	18.1	17.7	" 23	16.5	17.0	16.2
" 4	16.1	17.7	15.9	" 24	16.5	17.4	16.2
" 5	16.1	18.3	17.5	" 25	14.9	16.5	16.3
" 6	15.6	17.0	15.5	" 26	14.8	18.4	15.1
" 7	15.3	18.1	15.2	" 27	14.1	18.8	16.9
" 8	14.0	17.6	17.3	" 28	14.6	16.9	13.6
" 9	14.3	17.6	14.6	" 29	13.3	15.9	14.4
" 10	14.7	17.6	15.9	" 30	14.5	15.4	15.5
Gen. Av.	20.1	23.1	19.8		19.1	20.8	19.6
Maximum	55.0	55.0	42.0		45.0	39.0	40.0
Minimum	12.0	15.0	13.0		11.0	13.0	12.0
M. V.	5.18	5.30	3.82		4.19	3.33	4.00

Key 3 | *Key 4*

Date	*A*	*B*	*C*	Date	*A*	*B*	*C*
Dec. 1	40.2	25.9	24.0	Dec. 21	37.9	33.7	29.0
" 2	20.1	24.4	29.2	" 22	26.2	26.6	22.2
" 3	17.7	23.2	20.4	" 23	20.9	23.4	22.6
" 4	19.2	21.4	19.3	" 24	22.4	21.4	29.6
" 5	17.8	23.6	18.1	" 25	19.4	21.3	19.8
" 6	15.9	18.8	16.9	" 26	21.2	20.3	20.4
" 7	16.3	22.7	17.6	" 27	19.4	19.9	18.0
" 8	14.6	19.9	17.3	" 28	17.4	18.5	17.1
" 9	15.9	18.6	17.0	" 29	15.2	18.7	17.6
" 10	16.3	19.2	16.3	" 30	15.7	19.2	16.5
" 11	13.9	17.8	15.5	" 31	17.5	17.7	17.2
" 12	13.9	17.7	14.4	Jan. 1	15.6	15.9	15.3
" 13	13.0	16.7	14.5	" 2	15.6	19.7	14.9
" 14	14.4	16.6	14.1	" 3	16.0	16.9	15.7
" 15	13.0	15.8	15.4	" 4	15.4	18.4	15.1
" 16	12.8	16.2	14.6	" 5	15.0	18.1	14.3
" 17	12.4	17.7	15.0	" 6	14.5	18.3	17.1
" 18	13.1	16.5	14.1	" 7	14.1	16.6	13.2
" 19	13.0	16.7	13.4	" 8	12.7	20.5	13.7
" 20	13.2	15.1	13.7	" 9	13.2	16.2	15.3
Gen. Av.	16.3	19.2	16.6		18.3	20.2	17.8
Maximum	57.0	30.0	29.0		42.0	42.0	33.0
Minimum	11.0	13.0	12.0		11.0	14.0	12.0
M. V.	3.36	2.70	2.15		4.06	2.61	2.86

Key 5 | *Key 6*

Date	*A*	*B*	*C*	Date	*A*	*B*	*C*
Jan. 10	35.9	31.7	30.1	Jan. 29	38.6	36.3	...[2]
" 11	24.9	23.5	23.3	" 30	...[2]	28.0	22.9
" 12	21.2	22.5	21.6	" 31	23.2	22.1	19.6
" 13	18.7	20.9	18.4	Feb. 1	18.4	18.5	18.2
" 14	18.0	19.1	20.5	" 2	16.8	20.4	18.6
" 15	16.5	18.4	16.2	" 3	17.4	19.8	18.3
" 16	16.1	20.3	17.7	" 4	16.6	18.4	17.5
" 17	16.0	17.7	16.3	" 5	14.7	16.6	18.1
" 18	15.9	17.7	16.4	" 6	14.7	18.2	16.0
" 19	13.9	17.9	16.7	" 7	14.3	17.0	17.2
" 20	14.9	16.9	15.9	" 8	16.9	19.1	15.0
" 21	15.1	15.3	16.8	" 9	14.8	16.9	15.8
" 22	13.7	17.2	14.0	" 10	14.8	16.2	15.5
" 23	15.1	16.0	14.1	" 11	14.9	15.9	15.3
" 24	15.1	16.0	14.1	" 12	13.9	14.8	13.8
" 25	14.7	15.7	14.7	" 13	13.3	14.7	13.5
" 26	13.1	16.2	14.9	" 14	12.5	13.8	13.8
" 27	14.0	14.8	13.6	" 15	12.3	14.0	13.0
" 28	13.3	15.3	13.3	" 16	12.3	13.8	14.4
" 29[1]				" 17	12.3	15.6	13.7
Gen. Av.	17.1	18.6	17.3		16.3	18.6	16.3
Maximum	38.0	40.0	35.0		43.0	40.0	25.0
Minimum	11.0	13.0	12.0		11.0	12.0	12.0
M. V.	3.52	2.80	2.89		3.61	3.74	2.15

Key 7

Date	*A*	*B*	*C*	Date	*A*	*B*	*C*
Feb. 18	35.3	31.4	27.4	Mar. 2	14.6	15.4	15.2
" 19	25.5	23.6	22.1	" 3	14.9	16.0	14.5
" 20	20.9	21.1	20.9	" 4	13.8	14.6	14.2
" 21	18.1	17.5	18.5	" 5	13.3	14.6	13.2
" 22	19.1	18.1	17.0	" 6	13.2	14.6	13.2
" 23	18.2	19.3	18.7	" 7	13.2	14.1	14.0
" 24	18.1	18.5	17.2	" 8	12.6	14.2	13.9
" 25	17.2	18.5	18.4	" 9	13.3	14.7	12.9
" 26	16.8	17.1	17.2	Gen. Av.	17.4	17.7	16.7
" 27	13.6	16.8	15.6	Maximum	40.0	39.0	30.0
" 28	13.8	16.6	16.0	Minimum	11.0	13.0	12.0
Mar. 1	15.4	15.1	14.6	M. V.	3.69	2.80	2.52

Some of these curves show that the larger part of improvement is done during the first test. Usually, however, there was a constant, though slow, progress in improvement until the end of the twenty tests. Practise showed its influence to the end of the series, with every indication that the averages could, with further practise, be further reduced.

Secondary factors, such as fatigue, concentration of attention, indisposition, distraction, etc., all had great influence

[1] One day short by oversight.

[2] Periods missed on account of sickness.

on the work. The conscious processes were very susceptive to these influences.

In learning the associations of the different keys mnemonics proved helpful, by taking notice that certain association letters formed initials of familiar names, etc., while some had to be retained by sheer force of memory.

In some cases the peculiarity in sequence of the letters or pairs of letters associated caused retardation in association processes.

Regarding the theory of interference upon completing one key and taking up a different one the writer believes that probably there is a certain degree of interference; he also thinks there is a certain amount of learning in the former key transferred to the new one, and that there is a resultant of forces present, which, in general, makes possible a lower time rate. In the present investigation there was constant lowering of the beginning time rate as progress from key to key was made; which, it would seem, shows conclusively that there was a transference of learning from one key to another, and that the evil effects of interference were largely neutralized.

A NOTE ON THE EFFECT OF RHYTHM ON MEMORY

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In spite of the poor standing of the "class" experiment, the writer has been so impressed by the similarity in the results of three such tests, that he thinks the data obtained will be of general interest to students of memory problems—the more so, as some of the conclusions are in direct contradiction to the results obtained in certain other investigations.

The object of the experiment was to test the effect of some of the various kinds of rhythm upon the memory for numbers. The rhythms used were the trochaic, iambic, dactylic, anapestic and amphibrachic forms, together with a non-rhythmic series. The investigation was carried on in the time-honored way. The material consisted of 9 and 10 digits arranged haphazard, for example, 381427695. Such a group was read at a rate of between 90 and 100 per minute, Eight such groups constituted one series. One 9 digit series and one 10 digit series were used for each of the different kinds of rhythm.

The rate of 90 to 100 per minute was used in the endeavor to obviate subjective grouping on the part of the subjects. and because it would be about equally advantageous for the two-part and three-part rhythms. According to Bolton's work upon rhythm, it was found that 75 per minute was the most favorable rate for two-part subjective rhythms, and 130 per minute for the three-part subjective groupings. The intermediate rate was therefore used, for this experiment concerned itself with objective groupings only. The subjects were instructed to write down as many of the numbers as they could remember immediately after the reading of one group. They were told to be sure to get the right number in the right place and to put dashes for the forgotten numbers, so that those given correctly should appear in the correct place in the combination.

The results were recorded in the usual way. A number correctly given and in the right place in the combination was awarded a credit of 100 per cent. A transposition of two numbers was given half credit, or 50 per cent. What might be called a half transposition, or a number which was shifted one place to the right or left with an incorrect number appearing in its place, was given a value of 25 per cent. The results are consequently given in percentages of the total amount recalled.

The series were given in such a way as to obviate as far as possible the effects of practice for any meter. Frequent rests were given the subjects so that fatigue might not interfere with the results.

A total of 180 subjects was used, 80 men and 100 women, all of them coming from the class in introductory psychology at the University of Michigan. The first group, consisting of 50 persons, 25 men and 25 women, performed the experiment during the winter of 1911 and 1912. During the winter of 1912 and 1913 about 80 persons were experimented upon, and the papers of 25 men and of 25 women were selected at random. During the winter just passed (1913–1914) 30 men and 50 women performed the same experiment.

Since certain differences appear in the masculine and feminine types of reaction, it will be well to treat them at first separately and then note the sex differences. The following table shows the results registered by the men in the 9 digit series.

TABLE I

MEN. 9 DIGITS

Group	N. R.	Troc.	Iamb.	Dact.	Amph.	Anap.	Av.
1	74.1	75.0	78.9	85.0	——	89.7	80.5
2	77.0	73.2	71.2	88.5	80.4	83.5	78.9
3	71.0	68.2	72.0	88.7	83.7	88.8	78.7
Average	73.7	71.8	74.0	87.5	82.1	87.3	79.4

N. R. indicates that the numbers were read without accent, or rather with the amount of accent as nearly uniform as possible. The other headings show the rhythm used—trochaic, iambic, dactylic, amphibrachic and anapestic.

Until the results of the third set of papers had been obtained, there was no intention of using them for anything but purposes of illustration in class. The records of two sets had therefore been destroyed before any individual characteristics had been worked out. Realizing that the averages as shown in the tables may be greatly affected by some extreme cases rather than representative of a general tendency, the writer regrets this loss. However, he has worked out the last set of papers not only in percentage values, but also in terms of position by The Order of Merit Method, where the effect of extreme cases is eliminated. There were no substantial differences in the results obtained by these two methods, certainly no differences of kind. There were a few differences of degree.

A study of the table brings out these facts:

1. There is a variation of 6 per cent. in the recall of the unaccented series amounting to 0.54 of one syllable when we compare the best group with the worst. There is no correlation between the ability to recall the non-rhythmic series and especial ability to recall any particular kind of metrical presentation. The one thing which does appear is that the group which had the highest recall value for the unaccented series had the lowest for the average of the three-part meters. This same relation will not hold for the other two groups, however, for the group which was the worst in the non-rhythmic series was not the best in the three-part rhythms, unless we omit the amphibrach. If we do omit the amphibrach entirely, we do find the reverse correlation, that the poorest group in the non-rhythmic series was the best in the three-part rhythms considered as a whole, while the best non-rhythmic group was the worst in the three-part rhythms.

Another interesting result which appears from a study of the table is that the group of men which did best in the non-rhythmic series did better on the falling rhythms, the trochaic and dactylic, than they did on the rising meters, the iambic and anapestic; while those who were the worst in the unaccented series recalled the rising meters better than the falling.

2. In two out of three groups and in the average of the three groups, the trochaic form of rhythm has not as high a memory value as the non-rhythmic series.

3. In two out of the three cases and in the average, the iambic form of rhythm is better for the purposes of recall than the non-rhythmic series. We are justified in stating, then, that the iambic form of presentation is very slightly better than that without rhythm. Considering the average for the two-part rhythms, we find it to be slightly worse than the unaccented series.

But it must be remembered that the series of 9 digits read in two-part rhythm is scarcely a fair test, for one number is left over, making an incomplete foot at the end of the line. When this incomplete foot is unaccented, as in the iambic form, the group has a higher memory value, in two cases out of three, than when the incomplete foot is accented. It is interesting in this connection to call attention to the fact brought out by Miss Rowland in connection with visual rhythms, namely, that a change in the minor element of the series is more disturbing than a change in the major element.

4. The three-part rhythms are all better than the unaccented series. The dactylic and anapestic forms are about equal, with the dactylic very slightly in the lead and the amphibrachic is the worst, but better than the two-part and non-rhythmic series. It is entirely natural that the three-part rhythms should be the best in this part of the experiment, for most of us have been trained from our earliest days in the grade schools to group numbers by hundreds, thousands, millions, etc.

The following table shows the results registered by the women in the 9 digit series.

The study of this table shows the following facts:

1. There is a variation of 3.7 per cent. in the recall of the unaccented series amounting to 0.33 per cent. of one syllable when we compare the best group with the worst. Here we

TABLE I*A*

Group	N. R.	Troc.	Iamb.	Dact.	Amph.	Anap.	Av.
1	76.5	72.6	78.1	91.8	——	99.1	83.6
2	76.0	71.3	70.7	94.4	88.1	95.3	82.6
3	72.8	65.5	70.0	90.8	83.7	90.3	78.7
Average .	74.5	68.1	72.2	92.0	85.0	93.7	80.9

find that the best non-rhythmic group is also the best in the two-part and three-part rhythm series, while the worst non-rhythmic group is also the worst for all forms of rhythmic presentation. The best non-rhythmic group is best in the falling rhythms, while the worst non-rhythmic group is best in the rising two-part and the falling three-part rhythms.

2. In all three tests, the trochaic form is not as good as the non-rhythmic.

3. The iambic form is in general below the non-rhythmic, but is better than the trochaic. Again, as with the men, the series having the extra unaccented syllable has a higher memory value than the series containing the extra accented syllable.

4. The three-part rhythms are considerably better than either the unaccented series or the two-part meters. The anapestic form is best, the dactylic next and the amphibrachic worst.

Averaging the results of the men and women in the 9 digit series, we find that the three-part rhythm is best, the non-rhythmic next and the two-part rhythm the worst of all. There is also a tendency for the rising meters to have a greater memory value than the falling ones.

Considering now the differences between the masculine and feminine types of reaction, we find:

1. The total amount recalled by women is greater than that recalled by men, 80.9 to 79.4 or 100 to 98.1.

2. In the non-rhythmic series, the women recalled more than the men absolutely, but relatively less. For when we consider that the women remembered actually more than the men did taking the series as a whole, the percentage of the series recalled in the non-rhythmic series is less for the

women by a ratio of 92.1 to 93.0. The non-rhythmic form of presentation, then, is relatively worse for the women than it is for the men in spite of the fact that they recalled more actually.

3. The two-part rhythm is better for the men, both absolutely and relatively, than it is for the women, the absolute ratio being 72.9 for the men to 70.1 for the women, and relatively 92.0 to 86.8.

4. The three-part rhythm is better for the women, both absolutely and relatively, by the ratio of 90.2 to 85.6 absolutely, or 112 to 108 relatively.

5. With the men, the dactylic form of meter had a somewhat higher memory value, while with the women the anapestic form was best.

Turning now to the consideration of the 10 digit series, which was given to but two sets of persons, we find a relative rise in the memory value of the two-part rhythms, and a relative decrease in the three-part, showing that the irregularity of the last foot was a determining factor. The total amount remembered in this series was considerably less than in the 9 digit series, as might naturally be expected. Table II. shows the results of the men in the 10 digit series.

TABLE II

Group	N. R.	Troc.	Iamb.	Dact.	Amph.	Anap.	Av.
2	71.0	69.7	72.7	77.1	——	76.9	73.6
3	62.3	63.4	69.0	75.5	71.2	73.0	69.1
Average .	64.4	64.4	70.8	76.6	71.2	74.8	70.4

This table shows:

1. The recall of the unaccented series shows a variation of almost one syllable when we consider the results of the two groups. The best group in this series was absolutely the best in the three-part rhythms, but relatively worse than the other group. There is no evident correlation between the recall of the unaccented series and either the falling or rising rhythms.

2. The trochaic rhythm is equal to the non-rhythmic

series. This is true of the average, the two series differing somewhat. In one, the trochaic is better and in the other worse than the unaccented series.

3. The iambic series is better in both cases than the non-rhythmic. On the average, then, the two-part rhythm with series of 10 digits is better than the unaccented series.

4. All of the three-part are better than the two-part kinds of rhythm, the dactylic holding the highest place, the anapestic second and the amphibrachic coming last, as was the case in the 9 digit series.

The following table gives the results of the women in the 10 digit series:

TABLE II*A*

Group	N. R.	Troc.	Iamb.	Dact.	Amph.	Anap.	Av.
2	63.5	68.5	69.8	72.9	——	74.3	69.8
3	61.0	64.1	65.4	70.4	68.5	72.2	66.9
Average .	62.0	65.5	67.0	71.2	68.5	73.0	67.9

A consideration of this table brings out the following points:

1. There is very little variation between the two groups of women in the recall of unaccented material, only a quarter of one syllable. The group which was absolutely worst in the recall of the non-rhythmic series was relatively better in the recall of the three-part rhythms.

2. In both series, both forms of the two-part rhythm are better than the unaccented series. The iambic form is better than the trochaic.

3. Each of the three forms of three-part rhythm is better than either of the two-part meters. But with the women, the anapestic form of meter is better than the dactylic. The amphibrachic is lowest of all.

Taking into account the sex differences, we find that:

1. The total amount recalled by the men is greater than that recalled by the women, 70.4 to 67.9 or 100 to 96.5. Considered absolutely, the women recall less in all the different forms of rhythm. This is true of the average of the

non-rhythmic, two-part and three-part rhythms. The women did recall slightly more in the trochaic form. Considered relatively, we find the men to be better in the non-rhythmic series, worse in the two-part meters, and slightly better in the three-part rhythms, when we consider all the three-part meters together.

2. The men are best in the dactylic meter and the women in the anapestic.

The task would be incomplete if no endeavor were made to bring together the results of the 9 digit and the 10 digit series. It would, of course, be unfair to average them, but it is possible to take the average recall for each group and reduce the amount recalled for each kind of meter to a percentage of this average. This would do no injustice to any group considered alone and would make comparisons possible. Combining the previous tables for the men, and omitting the amphibrachic form of meter—for it shows nothing striking—we obtain this table:

TABLE III

	Group	N. R.	Troc.	Iamb.	Dact.	Anap.
9 digits..........	1	92.2	93.3	98.0	105.9	111.7
	2	97.4	93.6	90.1	112.0	105.8
	3	90.2	86.6	91.5	112.8	113.0
10 digits..........	2	96.7	94.8	99.0	105.0	103.2
	3	90.1	91.7	99.8	109.2	105.2
Av. of 9 digits......		92.8	91.0	93.3	110.2	110.0
Av. of 10 digits....		91.5	91.5	100.6	109.0	106.3
Difference.........		1.3	0.5	7.3	1.2	3.7

The main value of this table seems to be to show the amount of damage done by introducing irregularity into the series. There is a difference of 1.3 in the non-rhythmic series in favor of the shorter groups. The two-part rhythms are better in the 10 digit series by an average of 3.9 and the three-part rhythms are better in the 9 digit series by an average of 2.45. On the average, the three-part rhythms seem to be affected less by the introduction of an irregularity than do the two-part, 3.9 to 2.45. Moreover the falling rhythms are less affected by the extra measure than are the rising meters, 0.85 to 5.5.

TABLE III*A*

WOMEN

	Group	N. R.	Troc.	Iamb.	Dact.	Anap.
9 digits..........	1	91.5	86.8	93.5	109.9	118.8
	2	92.3	86.6	86.0	114.7	115.9
	3	92.5	83.2	88.9	115.2	114.6
10 digits..........	2	91.0	98.1	100.0	104.4	106.4
	3	91.2	95.8	98.0	105.1	108.0
Av. of 9 digits.....		92.2	83.3	89.4	113.8	116.0
Av. of 10 digits....		91.4	96.5	99.0	105.0	107.6
Difference.........		0.8	13.2	9.6	8.8	8.4

This table shows that there is a slight relative difference —0.8—in the recall of the non-rhythmic series, the difference being in favor of the shorter series. The two-part rhythms are better in the 10 digit series by an average of 11.4 and the three-part rhythms are better in the 9 digit series by an average of 8.6. The two-part rhythm, then, is affected a little more by the introduction of irregularity than is the three-part rhythm. In both the two-part and the three-part rhythms, the falling meter is the more affected, 11 to 9. There is less disturbance of the three-part rhythms than of the two. The other points which are brought out by this table have been considered before, so may be omitted here.

When we consider the sex differences as brought out by a comparison of the results of the 9 digit and the 10 digit series, we find that:

1. The feminine recall is better for the 9 digit series by a ratio of 80.9 to 79.4 or 100 to 98.1, whereas in the longer series the masculine recall is better by a ratio of 70.4 to 67.9 or 100 to 96.5. Considering the whole experiment, then, the men have slightly better memories for numbers than the women, the ratio being 100 to 99.1. This difference is very slight and might be called negligible.

2. The irregularities introduced into the series are more disturbing to the women than to the men, 8.16 to 2.8.

3. The irregularity on the average affects the three part rhythms less than it does the two-part for both sexes.

4. With the men, the dactylic form of meter is the best in all cases; with women, the anapestic.

5. The irregularity affects the rising meters more than it does the falling in the case of the men; whereas the falling meters are more affected in the case of the women.

Since, as has been seen, the 9 digit series introduces a disturbing factor into the two-part rhythms and the 10 digit series into the three-part rhythms, it will be interesting to consider the two-part rhythm 10 digit series together with the three-part rhythm 9 digit series. The results obtained from this consideration differ from the rest of the results only in this particular, that, with one exception, any rhythm is better than no rhythm at all. This exception occurs in the case of the men and with trochaic rhythm, it being exactly equal to the non-rhythmic series.

This way of regarding the results also raises the feminine recall somewhat above the masculine, but shows that it is a more precarious thing, very easily disturbed.

DIAGNOSTIC VALUES OF SOME PERFORMANCE TESTS[1]

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In preliminary mental examinations of a number of recent admissions to the Ohio Girls' Industrial School, the results obtained from the Binet and Point Scale ratings of intelligence, set apart three groups, which it seemed desirable to investigate further. These groups follow:

1. Twenty-one high-grade morons, whose Binet ratings average 11 years, with a mean variation of 3 years, and whose Point Scale ratings, transformed into years, average 11.6 years, with a mean variation of .4 year.

2. Sixteen, concerning whose intelligence defects we are in doubt, because of the disparity between the Binet and Point Scale findings. Binet ratings of these sixteen average 11.6 years, with a mean variation of .2 year. By Point Scale rating, these are all 12 years or more. One only is flat 12. Four are over 15 years.

3. Twenty-six, who show no defect in intelligence by either of these ratings, being twelve years or more by both scales. Fifteen of them get credits for more than the 82 points for fifteen years.

The actual ages of these sixty-three girls, reckoned by the nearest birthdays at the times of examination, range from 12 to 18 years. The median ages for each subgroup, in the same order as above, are 15.8 years, 16.5 years, and 16.5 years. The average ages for each subgroup, in the same order, are 15.2 years, 16 years, and 15.9 years. The median age of the sixty-three girls is 16.35 years, and the average age of the sixty-three is 15.7 years.

For the further study of these cases, recourse was had

[1] Read before Section L of the American Association for the Advancement of Science, December 31, 1914.

to the following tests. We lack standards in all these tests. For the more delicate diagnosis of intelligence and other mental defects in persons of mentalities of more than ten years, there is no method of procedure in any wise comparable for accuracy of measurement to the Binet and Point Scale for the earlier stages of development. These performances and other tests have been used widely as supplementary aids to psychologists' native intuitions. Methods of testing have been developed, but no standards of the meaning of results.

These subjects having been rated by the two scales (Binet and Point), and classified with respect to the 12-year line, we have a means of preliminary evaluation of these supplementary tests. We may ascertain which do and which do not correlate with the scales. It is not standardization. It is merely evaluation. Standardization is not possible with defectives.

List of the Supplementary Tests

1. Picture Form Board (farm scene, mare, colt, chicken, sheep. Two right-angled triangular pieces to fit into an isosceles triangle). (Healy.)
2. Construction Puzzle (*A*). (Healy.)
3. Construction Puzzle (*B*). (Fernald.)
4. Labyrinth (*B*). (Boston Psychopathic Hospital.)
5. Visual Verbal Memory Test. (Schmidt.)
6. Auditory Verbal Memory Test. (Thorndike.)
7. Learning Test. (10 symbols for 10 figures; 4 lines.)
8. Cross Line Test (*B*). (Healy.)
9. Motor Coördination. (Whipple.)
10. Opposites. (Lists I. and II. of Woodworth and Wells.)
11. Completion Test. (Boston Psychopathic Hospital.)
12. Moral Discrimination Test. (Boston Psychopathic Hospital.) (For girls.)

Table I. presents, for comparison, the summaries of results, from each of the three groups of girls for each of the first eleven tests.

The *first four tests* were given with simple directions, and the time in seconds and the moves or errors, recorded. These are averaged for each of the three groups.

TABLE I.

Test	No Defect		Doubtful		High Grade Defectives		Evaluation
	Time in Seconds	Moves, Errors, Etc.	Time in Seconds	Moves, Errors, Etc.	Time in Seconds	Moves, Errors, Etc.	
1. Pict. Form Board..	102	7.4 errors	123	6.7 errors	151	8.9 errors	By *time* for both differences and for *errors* in defectives.
2. Const. Puz. (*A*) ..	52	11.9 moves	58	14. moves	78	16.6 moves	By *time* and *moves* for both differences.
3. Const. Puz. (*B*) ...	135	23.6 moves	185	23.3 moves	109	19.1 moves	No value.
4. Labyrinth (*B*).....	287	2. errors	321	2.4 errors	345	2.5 errors	*Time* and *errors* set off non-defective.
5. Vis. Verb. Mem. ..	58	11.8 details	56	12 details	59	10.1 details	Details set off defective.
6. Aud. Verb. Mem...	52	9.9 details	44	9.7 details	49	9. details	Details set off defective.
7. Learning.........	114	.6 errors in 36s	107	.6 error in 33s	118	.5 error in 38s	No value.
8. Cross Line (*B*)....	—	.6 in 6	—	2.4 in 6	—	1.9 in 6	Errors set off non-defective.
9. Motor Coordin. ...	30	1.3 errors 79 taps	30	2.4 errors 79 taps	30	.6 errors 75 taps	No value.
10. Forty Opposites ...	324	2.1 errors	301	3.4 errors	396	9. errors	By *errors* both differences. By *time* defectives are set apart.
11. Completion.	346	2.8 errors	414	2.5 errors	420	3.3 errors	Errors possibly differentiated defectives. Time *may* possibly differentiate non-defectives.

In the *Visual Verbal Memory* test, a sheet was handed the subject on which the story of a fire was printed, one line to each detail, and she was directed to read it once aloud, and to do it carefully so she could remember as much as possible.

In the *Auditory Verbal Memory* test, the shipwreck story was read to her four times. Times for recall and numbers of details are averaged for each of the Memory Tests.

In the *Learning* test, times required to fill three lines, and to learn, to the subject's satisfaction, were recorded and averaged, as well as the numbers of errors in filling the 4th line from memory, and the times for the memory work.

In *Cross Line* (*B*) test, the double cross was drawn on a piece of paper and the arabic numerals, 1–9, written in the nine spaces. The explanation of how the symbols could stand for the numbers was accompanied by a drawing of each symbol as the number was named, and an illustration given of writing a symbol under a number. The subject was asked to note carefully the arrangement. The paper was then turned over and six numbers written in a line, and the subject asked to make the corresponding symbol under each.

In *Motor Coördination*, we average numbers of dots made in 30 seconds, and the errors, *i. e.*, dots on lines or outside of squares intended, or a second dot in a given square.

In *Opposites*, we average the time and errors (wrong words and omissions) for 40 words. The observer wrote the opposites and was not hurried.

In the *Completion* test, the same method and data were used.

From an inspection of the table it is at once evident that some of these tests give significantly different averages in each of the three groups; others reveal more or less clear differences as between the *not defective* (mentality of 12 years or more), and the doubtfully defective. Others show differences between the doubtfully and the definitely defective. Others still show no sufficient differences between averages of any two groups to make them of any diagnostic value in this part of the developmental period.

1. In the first class of tests, are (*a*) the Picture Form

Board, (*b*) Construction Puzzle (*A*), and (*c*) Opposites. In the Picture Form Board averages, there appear significant differences in the increasing time throughout, and increased errors in the defective. Construction Puzzle *A* gives marked differences in both time and moves. It seems a valuable means of differentiation in this region. Opposites by number of errors gives significant differences. Defectives are set off strikingly both by time and numbers of errors.

2. In the second class, the Labyrinth averages indicate some diagnostic value in differentiating between the "non-defectives" and the "Doubtfuls," both in time and number of errors. Both the verbal memory tests on the other hand seem to differentiate between the "high-grade defective" and the "doubtful" in the number of details recollected. The norms seem to be ten visual, and nine auditory details for the defective, and twelve visual and nearly ten auditory details for the doubtful. The Cross Line test again separates the normals and doubtfuls strikingly. The Completion test is much less valuable. It separates non-defective from doubtfuls by time, and doubtfuls from defectives by numbers of errors. The latter difference has most weight.

3. In the third class, Construction Puzzle (*B*), the Learning test, and Motor Coördination seem to be of no service for differential diagnosis in this part of the development of mentality from ten and one half years to fifteen.

We compare the *median place of location* of each one of the ten acts, in the Moral Discrimination test, for each of the three groups, and also for a group of fourteen first-year high-school girls of ages from 14 years to 16.5 years, with results as in Table II.[1] The figures give the median positions assigned each act by each of the four groups.

"Flirting with a nice young man" is deemed much worse by the normal girls than by any of the delinquents. Of the delinquents, the high-grade defectives consider it worse than the other subgroups.

"Taking a hair ribbon," is rated about six by all, *i. e.*,

[1] For the data from the reactions of fourteen high-school girls, I am indebted to Dr. R. Pintner, of Ohio State University.

TABLE II.

	Normal		Delinquent	
	14 1st Yr. H. S.	26 Not Defec.	16 Doubt. Intell. Defect.	21 High-grade Defectives
To flirt with a nice young man on the street	5.5	8.5	8.5	7.2
To take a hair ribbon from your employer when she knows nothing of it	5.7	5.8	6.2	5.9
To spend the night in a hotel with some young man	3.5	2.7	1.8	2.5
To take a box of candy from the store where you work	5.5	5.3	5.3	5.7
To tell a wicked lie about some girl	4.5	6.1	5.7	5.7
To get mad and break the dishes when the woman for whom you work finds fault with you	8.5	8.6	8.2	7.5
To spank the baby because you are out of patience	9.3	8.1	8.7	7.3
To put poison in the food of some one whom you dislike.	2.4	1.9	2.4	1.9
Not to go to Sunday school and church, and never to read your bible	1.5	4.5	7.5	7.5
To throw scalding water on the cat	9.1	7.5	7.4	7.5

better than the middle. "Spending the night with a young man in a hotel" is rated a better thing to do by the normals than by any of the delinquents. Perhaps this is due to failure to understand what is meant. Of the delinquents, those of doubtful intelligence defect place it nearest the worst thing to do.

"Taking a box of candy" is placed about 5.5 by all groups. "Telling a wicked lie about a girl" is much worse with the normal girls than the delinquents. The delinquents all place it about the same, but those without intelligence defect consider it least bad. "Breaking the dishes when scolded" is worse for the high-grade defectives than any other group. "Spanking the baby when out of patience" is least bad for the normal girls, and judged worse by the high-grade defectives than by the other delinquents. "Putting poison in the food of one you dislike" is rated about the same by all, but some worse by the high-grade defectives and those of no intelligence defect.

"Not going to Sunday school and church" is the worst of the ten offences according to the normal girls. It is moderately bad for the delinquents with no intelligence defects, and moderately good for the doubtfuls and defectives. "Throwing scalding water on the cat" is the second best act with the normal girls, and is moderately good for all the delinquents.

There are no significant differences between the groups of delinquents in regard to their moral judgments. This test is of no value for differential diagnosis as between these groups. The startling moral judgments of the normal girls suggest the need of psychological inquiries into ethical foundations in the minds of girls.

By our findings then, the values of the tests for differential diagnosis of our three groups are as follows:

1. *Tests of Value for Both Distinctions*

   The Picture Form Board.
   Construction Puzzle (*A*).
   The Opposites.

2. *Tests Good for Differentiation of the Not Defective from the Doubtful*

   The Labyrinth (*B*).
   The Cross Line (*B*).

3. *Tests Differentiating the High-grade Defective from the Doubtful*

   Visual Verbal Memory.
   Auditory Verbal Memory.

4. *Tests of Doubtful Diagnostic Value*

   Completion.

5. *Tests Showing No Definite Diagnostic Value*

   Construction Puzzle (*B*).
   Learning.
   Motor Coördination.
   Moral Discrimination.

PROCESSES REFERRED TO THE ALIMENTARY AND URINARY TRACTS: A QUALITATIVE ANALYSIS[1]

BY E. G. BORING

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Recently the writer promised a descriptive study of certain complex organic processes that have their origin in the alimentary canal and the uro-genital system.[2] The present paper, which fulfills that promise, presents introspective descriptions of thirst, hunger, nausea, the call to defecation, defecation, the call to urination, and urination. Descriptive accounts were obtained, a few in the series of the earlier experiment, some in subsequent series, and others from written reports prepared by the observers outside the laboratory when the particular complex was experienced. These reports are more reliable than the usual answers to a questionary, because they were written down at the time of the experience by trained observers who had consciously adopted the attitude of psychological observation. It is from such accounts that quotations are made in all cases where there is no specific indication of another source.

The procedure was to provide each observer with a booklet which contained space for the description of the various processes mentioned above. At the beginning of the book was the following instruction:

> You are requested to give as careful an introspective description of the various complex organic processes listed herewith as possible. Please select a time when the particular process is intense, and arrange to retire from distractions and write a careful report on these pages.
>
> In the description of the complex organic processes, the observer is advised not to avoid *Kundgabe* or common-sense language, but to deal freely in the meanings of processes as well as in their pure description. It is instructive to be told that a complex feels 'as if . . .' or 'like. . . .' In description, free use should be made of such unsystematic descriptive words as sharp, keen, piercing, lancing, thrusting, insistent, dull, diffuse, definite, pungent, hard, clear, bright, heavy, full, steady, stable, *etc*. It

[1] From the Psychological Laboratory of Cornell University.

[2] 'The Sensations of the Alimentary Canal,' *Am. Jour. Psych.*, 26, 1915, 1.

should be borne in mind, however, that the exact significance of such words is not such that it always leads to a clear and unequivocal interpretation, for their meanings differ from individual to individual. The sensations should therefore, whenever possible, be compared in quality with such better known sensations as those of the cutaneous and, possibly, of the kinesthetic senses. The observer should also be on the watch for such characteristics as are matters of extension or of temporal course, and should, when able to do so, localize the complex as accurately as possible within the body.

There were nine observers,—six men (*A*, *B*, *C*, *D*, *E*, *F*) and three women (*X*, *Y*, *Z*). Four of the observers (*A*, *B*, *X*, *Y*) had received the doctorate in psychology; three of these were instructors in psychology. Two others (*C*, *Z*) were graduate students. The rest were undergraduates in the advanced courses in the laboratory.

Thirst

Observers *A*, *B*, and *X* undertook to go without water or liquid food for long periods, and to keep a running account of the experience. Abstracts from their reports follow.

Observer A.—6:30 P.M. Began experiment.

9 A.M. (14½ hrs.) "Tongue coated. Saliva secreted rather freely. It is rather that I want a drink than that I am thirsty. Pressure and kinesthetic complex in my mouth (not throat) keep reminding me at intervals of the experiment, and I imagine how a drink would feel."

2:30 P.M. (20 hrs.) Warm day. "Have eaten a few figs. Saliva still quite actively secreted, and mouth and throat kept wet by swallowing frequently and licking mouth with tongue. Lips are getting dry. The thirst rather expresses itself in my imagination than in sensation changes. While I am doing other things, I suddenly break off automatically and start for the water faucet, without thinking of what I am doing. Once I got the water turned on before I thought."

4:30 P.M. Ate piece of cheese.

5:30 P.M. (23 hrs.) "More unpleasantness now. A little more intense tactual sensations in mouth. Saliva secretion seems less. Slight headache. Number of times that I start for faucet increased; I should think I have started twenty times in the last hour. The start is like this: I am working and come to a break in the work; automatically I start to get up and move in the direction of the faucet. The tactual sensations in the mouth and the kinesthetic sensations in the tongue are then in the background. Then there is usually a vague visual image of a glass for water or a visual image of the sink or basin toward which I have made my involuntary start. Before I have moved more than a bit with my body, before I have taken a step, I get a kinesthetic shock or check, and remember the experiment. It then takes me some time to get back to work. The images of the basin or faucet persist. I can not keep them out of mind. This, is, for the most part, where the unpleasantness comes in. The unpleasantness does not seem to attach to the mouth sensations at all. The sensations are not strong. They seem like tactual pressure, are superficial, and spread out over the roof of the mouth. They are not in the cheeks, on the tongue, or in the throat back of the root of the tongue. The weakness and paucity of mouth and throat sensations is the striking thing so far."

11 P.M. Ate three cakes.

1 A.M. (30½ hrs.) "Weak in legs. Headache partly gone. Have taken a bath and am less irritable. Starts toward faucet and images of drinking less frequent. Roof of mouth very dry, an intense and thick tactual pressure with a very little livelier sensation almost like cutaneous pain or the prickle that one gets in drinking lemonade. In back of roof of mouth a narrow arch of deeper, duller sensation, more intense, of the quality of muscular pressure. Some time ago there was mixed with this a deep, dull pain, almost like a muscular cramp."

10 A.M. (39½ hrs.) "Slight headache; tactual sensations in roof of mouth; tendency to keep licking roof of mouth and to swallow at saliva, which is scant and seems thick. There are frequent temptations to drink, which differ from the involuntary starts for the faucet in that they arise only when the mouth sensations catch attention. Lips chapped." At this point the experiment was discontinued.

Observer B.—Midnight. Began experiment with a drink of water. 9 A.M. (9 hrs.) "Tongue coated. Feel as if water would be good for me, but would taste unpleasant. Not thirsty. Mouth dry and sort of puckered—drawn."

9:30 A.M. Ate cake of almond chocolate.

11:30 A.M. (11½ hrs.) "Not thirsty. Mouth a trifle dry."

1:30 P.M. (13½ hrs.) "Beginning to get thirsty. Thirst consists of sensations from tongue, roof of mouth, and throat,—principally tongue. Flow of saliva copious. I keep moving my tongue about on roof of mouth. Tongue and roof of mouth feel sort of dead, when brought together. Mouth feels dry, a little bit drawn or puckered. There is nothing in these complexes when I get them clear but pressure; they mean thirst, *i. e.*, they tend to run off into visual imagery of water or kinesthesis of going for water. This tendency for the focus to shift makes it hard to keep the sensations, as such, clear. The pressures are easy to refer below the surface, though they remain on the surface too. I can feel the whole tongue affected, and, at times, this feeling does not seem to be pure pressure, but an ache-like pressure, perhaps muscular pressure. The ache is not explicit; I could not analyse it out. The sensation is more like the forerunner of an ache. I also feel a slight ache in the back of each jaw, almost a cramp, like the ache from eating something too sour." B also fully describes the impulses to go for water that were noted by A.

2 P.M. Ate three slices of bread and a little jam.

5 P.M. (17 hrs.) "Thirst getting insistent, a puckered, swollen, dry feeling in tongue, cheeks, and roof of mouth. Lips very dry. Also a vague cenesthetic ache or weakness. I am a little unsteady upon my feet, and can not articulate with certainty."

5:15 P.M. Ate piece of cheese. 11 P.M. Ate a few crackers and smoked. Midnight: a few more crackers.

Midnight (24 hrs.) "Thirst insistent. Consists merely of dry mouth (pressure). Slight ache in head and limbs. Weak all over. Nothing new." The starts for the faucet are still reported.

10 A.M. (34 hrs.) "Feel pretty good. Mouth dry. Saliva no longer free. General body aches diminished. Impulses to get water less frequent."

10:30 A.M. Ate crackers and cheese.

1 P.M. (37 hrs.) "Very thirsty. Mouth getting actually dry. Lips have to be moistened constantly. Impulse to get water very strong. Chief symptom is general weakness and lassitude. Going upstairs tires me out.

"I tried experiment of putting cracked ice in a thin rubber bag in my mouth. Just as good as a drink! However, pleasant cold in mouth sets up impulse to swallow. If I swallow it as far as my throat, the cold in the throat is even more pleasant than that in the mouth. This surprises me because I have very few thirst sensations in my throat and ordinarily do not notice my throat. It is perhaps barely dry and aches a little, but I have no desire for water there until I put the bag of ice water in my mouth. The thirst disappeared entirely as long as the bag was in my mouth. The bag even felt wet, although it was actually dry. I could not keep it in long, however, because my mouth soon ached from the cold. On removing it the thirst returned immediately. As soon as the ice in the bag had melted, it ceased to be effective."

3 P.M. Ate buttered toast and ham. Rather weak. B found that two drops of acetic acid on the tongue "tasted good" and relieved the thirst for 7 minutes. Thirst had returned in its former intensity after 15 minutes.

4 P.M. (40 hrs.) "Headache. Mouth and face warm. Limbs ache. Mouth very dry, a dull pressure complex, with brighter surface qualities."

7 P.M. Ate toast, ham, and cheese.

10 P.M. (46 hrs.) "Tired, achy, weak. Mouth almost parched. Coated and a bitter taste. Lips have peeled a little; feels as if cheeks might soon. Headache."

2 A.M. (50 hrs.) Same as before. The thirst is a "dry mouth (cutaneous and subcutaneous pressure) and a taste." The rain outside reminded B of water and prevented his sleeping, so he discontinued the experiment at this point with a quart of water and some crackers. The mouth tended to dry quickly after the first drinks, but five minutes later he was quite comfortable. The next morning there were no noticeable effects of the water fast.

Observer X.—8 A.M. Began experiment.

8 A.M. (24 hrs.) "Thirst began to show itself by dryness of the lips. I did not clearly realize that I was thirsty, but found myself at intervals going to the water cooler. All along I was being reminded that I had certain sensations in my mouth that indicated thirst by finding the motor habit of securing a drink set off. Later the feeling of dryness extended beyond my lips to the inside of my mouth, especially the roof, which felt somewhat as if shrivelled. My tongue began to feel changed in shape; it seemed to be smaller, rounder, rather swollen at the back. Later the dry, somewhat irritated, rough feeling increased in the roof of my mouth and extended to my throat as well. I now feel as if I had a 'dry sore throat.' There are little 'painy' sensations in my throat and on the upper surface of my tongue. My dry lips no longer bother me. The sensations on my tongue feel very much like those that I have after I have scalded my tongue by drinking something too hot. What moisture there is in my mouth seems to have got thick and sticky. The different parts of my mouth have a tendency to stick together when I try to spread them out."

4 P.M. (32 hrs.) "Mouth cavity insistently calls attention to itself. Throat seems hot, and inflamed, and sore; mouth somewhat dry and sticky; tongue achy and sore. There are dull pains, with perhaps a dull pressure, localized inside the tongue at its base. Dried-up feeling on top of tongue." Experiment discontinued at this point.

Extracts from reports of thirst experienced under less extreme conditions by other observers follow.

Observer C.—"Dryness expresses the complex as a whole. The qualities seem to be a little warmth, a little pain, which is very mild but seems to be quite important as a component, and a quite distinct pressure. The pressure is like the result of stretching the skin, as if the mucosa had become shrunken and were stretched by a too voluminous submucosa. These sensations come from the sides and back of the throat in the region of the uvula, actually from little more than the soft palate. In the mouth there is a sort of stickiness or dryness."

Observer D.—"Feeling of dryness in throat. Feels cottony."

Observer E.—"A taut feeling of the upper esophagus and back of throat. Tongue feels large. Saliva thick and sticky. Sort of achy feeling in tongue and roots, extending to near-by parts of throat."

Observer F.—"Uncomfortable pressure in mouth, rather 'puckery,' localized in middle and back of tongue and roof of mouth. Also a more vague pressure in upper throat, not 'puckery.'"

Observer Y.—"Light pressure in mouth cavity and soft palate; faint muscular pressure at junction of jaws; perception of wetness under tongue (saliva). Pressure most prominent. Later pressure sensations at junction of jaws and also in cheeks became more intense and clearer. The perception of 'warm-dryness' in mouth was less prominent, but by no means obscure. Finally general muscular sensations took on a weak sort of tired feeling."

Observer Z.—After eating too much candy: "Tongue puffy and furry, rather warm and dry(?). Strong imagery of glass of water, of coolness of glass in hands, of coolness of water in mouth and throat, and of coolness of water in stomach."

From the foregoing reports it is evident that mild thirst consists predominantly in the going to get a drink, or in imagery which anticipates a drink; that is to say, thirst may not be conscious as such, but may be merely the meaning of a complex situation. Mild thirst is accompanied by a pressure-pattern on the tongue and the roof of the mouth, and sometimes by one in the throat. This pattern, which is seldom prominent in weak thirst, becomes marked as thirst gets more intense. Then the pressures increase in intensity and spread more frequently to the throat; the saliva flows freely, and the sensations involved in swallowing it figure in the complex. After a longer period of thirsting (twelve to twenty-four hours) the saliva no longer flows freely, the mouth becomes 'thick' and 'sticky' and 'dry,' the lips are dry and have to be moistened frequently. The pressures of the mouth are referred farther below the surface and, at least in the case of the tongue, take on the ache-like character of intense muscular pressure. The oral sensations touch off the desire to get a drink. In a still more extreme stage, the painful qualities become more marked in the tongue, in the roof of the mouth, and sometimes in the throat. There is general bodily lassitude and weakness. There is no evidence of any qualitative peculiarity other than the pressure-pains of usual organic experience. It is the situation and not the specific quality that makes the experience one of thirst. The oral sensations that are typical of thirst are referred always to the mouth and sometimes to the throat. When they are felt in the throat they are usually much less intense than those in the mouth, and some observers, even in extreme thirst, do not find them in this region at all. Obviously the statement of the textbook of physiology, that 'our sensations of thirst are projected more or less accurately to the pharynx,'[1] needs revision.

It appears that the thirst-perception in the mouth can be adequately neutralized, for short times at least, by a perception of wetness, even though the wetness be illusory. Cold ice-water in a rubber bag, which is dry on the outside, afforded one observer complete relief. The bag felt wet, like a draught of cold water.

Hunger

The recent physiological work which has resulted in the correlation of hunger with stomachic contraction has naturally suggested a tentative psychological definition of hunger. Cannon and Washburn[2] have separated hunger from appetite and characterized the former as an 'ache.'

"Appetite is related to the previous sensations of the taste and smell of food; it has therefore . . . important psychic elements. It may exist separate from hunger as, for example, when we eat delectable dainties merely to please the palate. Sensory associations, delightful or disgusting, determine the appetite for any edible substance, and either memory or present stimulation can thus arouse desire or dislike for food.

"Hunger, on the other hand, is a dull ache or gnawing sensation referred to the lower mid-chest region and the epigastrium. It . . . is likely to grow into a highly uncomfortable pang, less definitely localized as it becomes more intense. It may exist separate from appetite, as, for example, when hunger forces the taking of food not only distasteful but even nauseating. Besides the dull ache, however, lassitude and drowsiness may appear, or faintness, or headache, or irritability and restlessness such that continuous effort in ordinary affairs becomes increasingly difficult. That these states

[1] Howell, W. H., 'A Text-book of Physiology,' 1908, 272.

[2] Cannon, W. B. and Washburn, A. L., 'An Explanation of Hunger,' *Amer. Jour. Physiol.*, 29, 1912, 441. See also Cannon, W. B., 'A Consideration of the Nature of Hunger,' *Pop. Sci. Mo.*, 81, 1912, 291.

differ with individuals—headache in one, faintness in another, for example—indicates that they do not constitute the central fact of hunger, but are more or less inconstant accompaniments, for the present negligible. The dull, pressing sensation is the constant characteristic, the central fact, to be examined in detail."[1]

Carlson[2] also distinguishes between appetite and hunger, although he disagrees with Cannon and Washburn in regard to the nature of appetite. Hunger is pain.

"Pure hunger, not accompanied by 'appetite,' can be experienced, if during hunger attention is fixed on the hunger pangs themselves. . . . When this is done, hunger in its various stages becomes different degrees of pain."[3]

"It seems to me that the pain experienced from contractures or 'cramps' in skeletal muscles and in the intestines is different from hunger pangs, even though pain is inherent in hunger. The difference may be only an apparent one, due to the fact that the latter pains arouse the memories of previous agreeable experiences with food." "It would . . . seem that hunger contains elements of kinesthetic sensation as well as pain, the latter predominating in strong hunger."[4]

The observers in the present experiment were asked to report upon 'hunger' only, under the following instruction:

"The observer is warned to distinguish between 'hunger' and 'apptetite.' Hunger is more nearly 'sensational' and is said to be always experienceable in isolation when the attention is directed toward it. Hunger usually ceases as soon as food is taken. Appetite is more 'ideational' and persists after food is taken. It is the desire for food, the opposite of aversion. Appetite probably constitutes the motive for eating dessert at any meal. We are here interested in the description of hunger only."

In spite of this limitation the observers reported general weakness and faintness, headache, visual and oculomotor disturbances, factors which they recognized, however, as secondary to "hunger proper." The qualitative nature of the more immediately relevant experience may be shown by extracts from the reports.

Observer A.—At noon after eating no breakfast: "Dull pressure of considerable intensity in area above umbilicus. With this also pain, an achy, gnawing pain. Or else a muscular tension, a feeling of muscular contraction in this region, gives the meaning of 'gnawing.' I think I sometimes have mere 'emptiness,' *i. e.*, all this complex except the gnawing, achy pain, but this is only a general impression. It is common for me to say, 'I am empty, but I am not hungry.'"

[1] Pp. 441 f.

[2] Carlson, A. J., 'Contributions to the Physiology of the Stomach. II. The Relation between the Contractions of the Empty Stomach and the Sensations of Hunger,' *Amer. Jour. Physiol.*, 31, 1913, 175.

[3] P. 186.

[4] P. 189. For another discussion of hunger as pain, and a resultant symptomatology, see Jones, A. A., 'Hunger Pain,' *Jour. Amer. Med. Assoc.*, 59, 1912, 1154.

Observer C.—"On the sensory side hunger is composed of temperature and muscular sensations. The temperature is warmth, in the main, but at times there is something resembling cutaneous paradoxical cold. What I have called muscular sensation seems to be a sort of strain or pressure, much like that from the contraction of any skeletal muscle. The localization is in the stomach, and that organ feels as if it were pulling itself into a knot just as the hand does when the fist is quite tightly clenched."

Observer D.—"A dull, yet insistent, ache,—very diffuse. It seems to cover an area of about 20 cm. diameter, fairly deep, and extending upward from the point of the sternum. Sometimes it becomes more intense for an instant at some point. This point changes continually. At times the diffuse ache becomes weak and gives way to a sharper pain, a little higher up. This lasts about a minute and then there is a return to the previous conditions."

Observer E.—"Hunger begins with an unpleasant, uncomfortable feeling below the sternum. This gradually and quickly changes to a raw painful feeling, as if of the rubbing of the two stomach walls together. This feeling of achy, rubby pain increases in intensity until even the esophagus seems to be uncomfortable in its lower parts."

Observer F.—"There seem to be general pressures all through the abdomen, rising up to and above the sternum. They are most definite in the region just below the sternum. They sometimes become a dull ache. (The whole thing is instable and fluctuates, coming and going at almost rhythmical intervals.) They become more definite when the attention is directed toward them. Sometimes there is a 'sharp-hot' pain just below the sternum."

Observer X.—After 20 hours' fasting: "Hunger is such a 'total' experience that it is difficult to pick out what should be labelled specifically 'hunger sensations.' In fact, I doubt if there are any particular sensations that I should label specifically such. I call them hunger sensations, I believe, because I have found regularly that, if I eat, they disappear. Otherwise I should be inclined to label them as: a 'weakness' complex, or a 'feeling of emptiness,' or 'throaty sensations,' according as one or another aspect of the experience became prominent.

"What I feel at present is (1) a general bodily weakness, such that I am inclined to do nothing in the way of work, not even stand, and (2) a particular kinesthetic complex in the region of my diaphragm. Perhaps the latter is also part of the weakness; respiration certainly is not so strong and deep as normally. There are also (3) sensations which I localize in my digestive tract. Some of these I localize in my stomach. They make up the core of the empty, 'gone' feeling. They appear to be vague, dull, quite persistent kinesthetic sensations, as if from contractions of the stomach. They make a definite peculiar complex, but I believe that it is a complex composed of pressures and kinesthesis,—dull, but definite; very strong and insistent at times, but never sharp or bright or clean-cut. Besides the sensations in the stomach, there are others, localized in the back of my throat, at the beginning of my esophagus, which seem like incipient swallowing movements (esophageal kinesthesis). They draw my attention very decidedly to that region; when I attend to them there is a very strong inclination (in visual and kinesthetic terms) to put food in my mouth. The thought of food seems to make the salivary glands more active, so that I occasionally actually swallow. The upper part of my digestive tract seems very ready to react, and is evidently incipiently active,—judging from the sensations. The feeling of 'wanting something' is localized in my throat. The throat sensations are quite steady and persistent."

Observer Y.—"Strong, gnawing pressure (gnawing is elemental, a kind of pain). This complex is unpleasant and means a need of food,—hunger. There is also a complex localized in the pharynx, and muscular sensations in the jaws. Also a perception of wetness, localized under the tongue, meaning much saliva. The complexes localized in the stomach and pharynx together make up the desire for food. The pharyngeal complex is the less prominent."

Observer Z.—This observer reports that she seldom has the intense and vivid 'hunger of childhood' and that she has been unable to induce it for the sake of the experiment. In addition to describing general faintness, however, she gives, after a short fast, and under the caption 'Emptiness,' the following report. "Slight headache and light feeling. No desire for food as food, but knew from experience that it would take away the 'empty' feeling. Experience a slightly unpleasant pressure, localized in bottom of stomach; pressure in throat from about larynx to top of back of mouth; pressure of tongue on roof and sides of mouth (I think this is thirst); tongue felt bigger and softer than usual. The pressure on the bottom of the stomach seemed just like the pressure of a heavy weight on a relaxed muscle, although it was not so intense."

The writer has described elsewhere a case in which hunger was induced in one subject by the introduction of HCl into the stomach.[1] A few sentences will suffice to show that this 'laboratory hunger' does not differ from that occurring under more usual conditions.

Observer B. After 5 c.c. of 5 per cent. HCl: "Hunger, a strong, intense, diffuse ache, varying in intensity and covering an area as shown [*i. e.*, an area extending from the umbilicus to the sternum]. There is an especially intense and achy spot at [a point a little above the umbilicus and to the left]. Hunger goes and then returns; then lasts a long time, getting gradually fainter."

In another trial, a warmth was reported to "die away very slowly, fusing into a general ache in the stomach region. This ache gets more intense and presently without qualitative change turns into hunger."

Again, a 'stinging pain' is supplemented by a 'dull ache' below the sternum. "The ache spreads downwards, and, as the sting disappears, it turns into hunger pains. The hunger pains are marked, but are shot through by a little stinginess, a brighter and more tingling pain."

The hunger-complex is a complex of pressure and pain. Upon a background of dull pressure (*A*, *B*, *C*, *F*, *X*, *Y*), which is sometimes recognized definitely as kinesthesis or the equivalent muscular pressure, there is set a dull ache or gnawing pain which characterizes the hunger (*A*, *B*, *D*, *E*, *F*, *Y*; the intense muscular pressure of *C* is also pain; muscular pressure goes over into ache-like pain, which observers often call pressure).[2] Two observers (*X*, *Z*), who failed to find the pain-quality, also had difficulty in determining just what

[1] *Op. cit.*, 48.

[2] Cf. the confusion of pressure, strain, cramp, and pain in the introspections of esophageal pressure, especially those of G, *op. cit.*, 28 ff.

constituted hunger. Both pain and pressure are referred to the region of the stomach. The pain is noted as fluctuating, as rhythmical, as instable. In more intense hunger the maxima of the fluctuations of the 'dull ache' may involve a sharper pain-quality, which is definitely localized and limited to a very small area (*B*, *D*, *F*). 'Emptiness' appears to consist of the typical pressure pattern of hunger without the algesic components (*A*, *Z*). Three observers (*X*, *Y*, *Z*[1]) describe a complex kinesthesis in the throat and of oral sensations arising from the free flow of saliva, a complex which means for them desire for food, appetite, a literal watering of the mouth. Here we have the true sensory basis of 'appetite.' The ideation of food (mentioned specifically by *X*) is no doubt a usual concomitant, and presumably it often constitutes a desire for food that lacks sensory components entirely. There can be no question that this desire for food—appetite, if one is not disposed to limit the term too closely—may also often be unconsciously carried, just as in thirst the 'appetite' for water may become manifested automatically in the movements of going for a drink (see pp. 307, 310).

Our reports enable us to supplement Cannon's description by many reports of psychologically trained observers. But we have gone farther than confirmation. Hunger is a twofold experience. It is pressure in its weak form, pain and pressure when intense. If one calls the intense experience 'hunger' and the weak 'emptiness,' one has changed the phraseology but not the fact. Moreover, weak hunger appears to be muscular pressure, and intense hunger is the ache of intense muscular pressure. Carlson, we have seen, has also made this point: 'hunger contains elements of kinesthetic sensation as well as pain'[2]; but he thinks that the pain is not 'the pain experienced from contractures or 'cramps' in skeletal muscles.'[3] However, Carlson admits that the difference may be extrinsic rather than intrinsic, and he will doubtless welcome evidence for the muscular quality of the entire hunger-pattern.

It is not easy to follow Carlson in his discussion of appetite.[4] He objects to the view that 'appetite requires a nervous organization capable of associative memory,' because we have "in appetite for food conditions as primitive and essentially fixed by

[1] The three women: but it would be overhasty to discover a sex difference. They found in general more difficulty in deciding just what hunger was—perhaps, after all, their sex is a little less intimate with the inner man—and they gave fuller descriptions. Unable to find a *sine qua non*, they described all possible concomitants. Of course they thus noted complexes which the men overlooked.

[2] *Op. cit.*, 190.

[3] *Ibid.*, 189.

[4] *Ibid.*, 185 ff.

inheritance as in the case of the sexual desires or 'instincts'." Appetite becomes 'the desire for food,' 'the expression of an inherited mechanism.' "The inheritance factor in appetite, the desire to eat, is in some way caused by the hunger pains." When appetite apparently occurs alone, it is due to a concomitant 'subconscious hunger.' Three factors make up the food-taking impulse: hunger (pain), appetite (desire for food; a sensation?), and 'memories of the taste and smell of foods.' There would ap-appear to be seven possible cases: (1) hunger alone (pain), when one attends to the hunger sensations; (2) hunger and desire for food (pain+appetite), when in extreme hunger one eats disgusting food; (3) desire alone (subconscious hunger pains+appetite), the non-rhythmical 'hungry-feeling' of Carlson's subject; (4, 5, 6) any of these states together with 'memories' of food—the usual impulses in food-taking; (7) 'memories' alone, 'the contemplation of a favorite dish after a full and satisfying meal.' So much we are told. But we have no hint as to the nature of appetite. Is it a sensation? is it a group of sensations? if so, what is it like? We do not ask for technical psychology. If it is sensory, what makes it so fundamental that it must be reflexly aroused? And why, in particular, must hunger, already defined as 'different degrees of pain,' be its sole condition? And what is this conditioning hunger, conscious and 'subconscious'? Is it sensation? or is it the nervous substrate of sensation? or is it the physiological cause of the hunger pangs? Until these questions are answered Carlson's distinctions will prove of little service.

If, however, classification and definition in such a simple case are wanted, the writer would suggest the following schema:

Hunger-complex	Hunger	= muscular pain	Sensory
	Emptiness	= muscular pressure	
Desire for food	Appetite	= throat-mouth sensations	
	Imaginal desire	= imagery	Imaginal
	Unconscious desire	= determining tendency	Neural

It seems probable that such an account would be accepted by Meumann,[1] who has laid stress upon the variety of the digestive sensations. Meumann describes three typical digestive experiences. In the first place there is the 'hunger sensation,' which 'is localized not only in the mouth and in parts of the throat, but also quite definitely in the stomach,'[2] a complex equivalent to hunger *plus* appetite as given above. In the second place, there is 'the very characteristic sensation of emptiness of the stomach,' a sensation which "has a very different character from the tension or pressure sensation of the abdominal wall. It can become intensified in a very unpleasant way and is sometimes connected with a vague perception of the peristalsis of the stomach."[3] just such a complex we have also called 'emptiness.' Finally, after eating, there is 'a characteristic sensation of fullness and pressure in the stomach' or, as it is called in another place, 'satisfaction and fullness.'[4] The experience is said to be not one of mere extension, as it is partially independent of the amount of food taken, and to some extent dependent upon the kind of food. Our introspections do not cover this point. Hertz has described the experience and concluded that it is conditioned upon the

[1] Meumann, E., 'Zur Frage der Sensibilität der inneren Organe,' *Arch. f. d. ges. Psychol.*, 9, 1907, 28ff.; 'Weiteres zur Frage der Sensibilität der inneren Organe und der Bedeutung der Organempfindungen,' *ibid.*, 14, 1909, 279 ff.

[2] *Arch.*, 9, 52.

[3] *Arch.*, 14, 293.

[4] *Arch.*, 9, 51 f.; 14,295. The writer does not interpret Meumann as meaning to distinguish between 'fullness' and 'satisfaction,' although it is possible to make such a construction.

tension of the muscular coat of the stomach, a tension of which the effectiveness is independent of muscular tonus.[1] Sternberg, it should be noted, distinguishes between appetite, hunger, and satisfaction.[2]

Nausea

Nausea was experimentally induced in the laboratory by the administration of syrup of ipecac or by a decoction of tobacco and, in one case, by the smell of castor oil.

Numbers below in brackets indicate the time in minutes that has elapsed since the beginning of the experiment.

Observer A.—2 teaspoonfuls syrup of ipecac. [23] "Breathing sensations queer. Feeling like that of respiration in abdomen, but shorter and quicker than breathing. I feel as if I were 'ready to vomit,' which is a meaning. My stomach feels a bit fuller. The tone of my muscles in arms, chest, and abdomen seems to have gone down; I feel weaker." Q. "Is this nausea?" A. (after thinking): "Yes . . . Saliva is forming. Tendency to open my mouth. Sweat comes on. Contractions of stomach, almost painful. Breathing is irregular. I close my eyes. [He vomits five times.] Big muscular wrench. Characteristic muscular weakness. Weeping. Throat and stomach feel full. Achy pains across stomach. I think I feel most nauseated just before I vomit. It feels as if my stomach actually sank. To the best of my knowledge that is like muscular pressure. There was a very, very slight dizziness in my head."

Observer D.—4 teaspoonfuls syrup of ipecac. [1] "Begin to feel something. A sort of sharp ache under sternum." [3] "Dull, heavy ache around stomach region." [15] "Something runs the whole length of my esophagus, up to the back of my throat; it means a desire to vomit. . . . It is taking on a nauseous character. I feel it mostly in the back of my throat; it seems to spread all over from the stomach up; considerable pressure to it. [He vomits violently.] Felt just as if there was a pressure there at bottom of esophagus up to throat. . . . Now I am getting unsettled in stomach. This seems to be a diffused pain, localized at least two inches below sternum. With it there is pressure, which means nausea and which gets more intense, meaning impending vomiting." [21: vomits again.] "Just preceding the vomiting the pressure gets very intense. It seems as if that pressure forced the contents right out. While vomiting I felt violent contractions in my stomach. . . . Nausea is coming again now. The first thing is pain. It is now at the top of my stomach [indicates level of sternum] and now lower down. There is also a band of pressure, below the sternum. Rather suggestive of gripes." Q. "What sort of pain is this pain that comes?" A. "Diffuse, sort of dull. It gets very intense. Not the pain of a prick at all. It seems as if the pain and the pressure constitute nausea. The pressure alone means incipient vomiting. . . . In some ways the pain is more of an ache than a pain; I suppose an ache is a dull pain. It is quite diffuse." [40: vomits.] "Frightful unpleasantness seems to cover up everything. There is a bodily trembling—a general feeling of weakness."

[1] Hertz, A. F., 'The Sensibility of the Alimentary Canal,' 1911, 19 ff.

[2] W. Sternberg's classification is into (1) disgust, (2) appetite, (3) hunger, (4) thirst, (5) feeling of satisfaction; 'Der Hunger,' *Zentralbl. f. Physiol.*, 23, 1909, 105. For his distinction between hunger and appetite, see in particular: 'Physiologische Psychologie des Appetits,' *Zeitschr. f. Sinnesphysiol.*, 44, 1910, 254: 'Das Appetitproblem in der Physiologie und in der Psychologie,' *Zeitschr. f. Psychol.*, 59, 1911, 91.

Observer Y.—5 teaspoonfuls syrup of ipecac. [15] "Dizziness. Pressure sensations in stomach,—a dull pressure, unpleasant, slightly nauseating, a sort of gnawing, a sickish character. Pressure gives a sinking feeling. Occasionally muscular sensations, as if I were about to vomit." [20: vomits.] "I'm not sick or squeamish; it was muscular." [22] "I am sick now. A gnawing, sinking, pressure-like quality in stomach region, extending up a little under sternum. A trifle dizzy, but the stomach-complex is strongest. Pressure-like quality seems to irradiate from the stomach, and I feel generally squeamish. The feeling extends all over me." [24: vomits 5 times.] "Nausea got very strong before vomiting. Muscular sensations are part of it. They were fused with a 'sinking,' which got worse and, as it got worse, gnawing." [25: vomits twice.] "I do not know whether the sinking, gnawing quality is something new in the element-line or not, or whether it is the character of the total complex. It belongs to the pressure family. It may be that there is a dull pain or ache, although it is not what I usually mean by an ache. It might be an approach to a fused non-intensive ache. Certainly its identity as such, if such it is, is so merged in pressure, that it appears more like a coloring, a dull, gnawing, sinking affair. I am rather inclined to think that the 'gnawiness' is in part of the achy character, but that pressure is the clear and stronger component. It is all closely fused—a unique whole." [35] "The gnawing is more prominent than the sinking. Yes, it is something aching; I am quite sure now. It is dull pain, very different from pin-prick, and yet something of the same order,—at least, a pain. . . . The achy character is more prominent than before—a gnawing."

Observer Z.—6 teaspoonfuls syrup of ipecac. [36: vomits several times.] "Feel awfully funny in throat; the muscles feel all tight, and yet the throat feels as if it were bigger than usual. I think that, except when I felt the contents coming up, I did not have any sensations at all below the bottom of my neck." Q. "Would you say you were nauseated?" A. "No, I do not think I was. Generally, as well as I can remember, nausea is decidedly unpleasant in both stomach and throat." Z failed to get any nausea within an hour and went home. There she became quite sick (*i. e.*, vomited) during the night, and recorded at one time the following: "Vague moving pressures, localized in stomach. Slight dizziness and weakness. Spasmodic contraction of muscles over whole trunk, especially in throat. Tears; perspiration." She did not record at the time whether or not the complex was one of nausea, although in the morning, upon being questioned, she was inclined to think that it was.

In order to obtain a nausea which would be more persistent than that induced by the ipecac, two observers took doses of a strong solution of tobacco juice.

Observer B.—2 teaspoonfuls of tobacco juice. [5] "Esophageal sensations, weak, but qualitatively like those in swallowing a hard object. Faint pressure in stomach,—a very vague ache." [15] "Gentle achy pressure in stomach region. Also vague aches from arms, like muscular fatigue. Whole thing makes up 'sick feeling.' Attention to any one part seems to break it up." [19] "When I smell the tobacco juice, a wave of achy pressure travels down the esophagus. It is nauseous." [24] "It seems as if nausea were in this case: general bodily weakness (mostly muscular fatigue)+headache (swimming sensations, eye-pressure-aches, tightness at ear, pressure wave at back of head; eye-aches and swimming most prominent+*intense unclear* pressure-aches around sternum. It seems as if stomach sensations had to be unclear in nausea; attention to them spoils the complex as nausea." [37] "Incipient vomiting sensations, in which stomach-aches get more intense and extend up farther and aches in eyes get intense. This *is* nausea. I also sometimes get stomach-pressures, which mean incipient vomiting, but which are less achy." [80] "The nausea is particularly difficult to localize; it is fleeting, evanescent, by which I think I mean merely that it (or at least the achy complex) is intermittent. Attention always goes naturally to the head sensations. When I voluntarily attend to the stomach-sensation, it always turns out to be an ache; and, since it seems to remain as continuous as any process in changing from obscure to clear, I say that achiness is its normal character. But attention never goes voluntarily to these sensations. I say they are essential because I always find them when I hunt for them while I am feeling nauseated. The principal part of the nausea as regards intensity and clearness is the headache, swimming, and (just now) jaw-aches (crampy character); but I do not think that they could be nausea without being supplemented by the stomach sensations, *i. e.*, I think the stomach-complex gives the meaning nausea."

Observer E.—2 teaspoonfuls of tobacco juice, 50 per cent. dilution. [7] "I begin to feel quite sick now,—a sort of dizziness in head; also pressure in stomach. Feeling of great discomfort all the way up from stomach to throat. More intense at times. Also a little pain, a peculiar ache, a sort of dry achy tenseness. It is very intense every time I smell the tobacco. Also a dull ache in head." [11] "Convulsive movements in the stomach or esophagus." Q. "Would you say you were nauseated?" A. "Yes." [20] "Still a discomfort in stomach. It is almost pain." [165] "Feel sick in my throat. There are aches in my stomach."

Of all the observers, *A* had the greatest difficulty in characterizing nausea. The nausea described above (ipecac) for him was not intense nor was it probably typical. The strong, characteristic experience could invariably be induced, however, by the smell of castor oil. His introspections under these conditions follow.

Observer A.—Nausea induced by smell of castor oil. "With the smell of the oil there was a big shiver, together with a wave of cold, all over my upper chest and abdomen—even in my arms. There was a sensation which has something in it of the sinking sensation that you get down here [umbilicus to sternum] when you drop in an elevator. There is also a start of a vomiting reflex, a muscular, pressury sensation; it seems as if I could feel a contraction. There is a bit of dizziness in head also. I feel myself sweating a bit. . . . Both what is the start of a vomit and the sinking thing are, I think, pressury. . . . The pressure in the stomach region is a pressure down." After another trial: "I do believe that there is something that I haven't mentioned yet, a sensation which forms a part of this whole situation. It is very hard to localize; certainly, however, somewhere in the trunk. I can describe it only as a sickening sensation, a kind of an awfulness and helplessness. It is not intense, if you can talk about its intensity absolutely. The other things were definite and stood there and waited for you; they caught your attention. This sensation is there beside,—a sickening, an awful helplessness." At another time *A* assumed the attitude which stands for this 'awful helplessness:' the body is relaxed, the knees slightly flexed and the arms hanging limp, the body bends slightly forward, the abdomen in, the head is inclined, the mouth is open, the eyes are closed. After another experiment: "I don't know! If it is a meaning, it's the meaning of the smell. And how can the meaning of the smell be down in my chest and abdomen—for that is where I feel sick? This is the case: I do feel sick. And I feel sick down here. Now there are a lot of sensations down here that I can put my finger on and localize."

The confusion of the ache of nausea with the ache of hunger came out in the experiments that were made upon *B* with stimulation by HCl (*cf.* p. 314).

Observer B.—After the introduction of 5 c.c. of 20 per cent. HCl into the stomach. "Ache in stomach region became definite and was recognized as nausea. It was most intense about 3 cm. below sternum. The nausea lasted a long time (I still feel a little bit sick.) It is a pain, very much like hunger." After 5 c.c. of 5 per cent. HCl: "Nausea. I feel pretty sure that 'sickness' is the nausea ache below the sternum *plus* muscular pressure down toward the umbilicus, the latter meaning violent contractions, as if I were going to vomit. I do not believe that the ache is in any way different from the ache of hunger, except that it is a little more diffuse, a little higher up, less likely to be localized and less definitely localized when it is, less intense, and without the rhythmical intensive fluctuations of hunger."

Observer D volunteered a general statement that is relevant: "I can not always tell hunger from nausea. When I am nauseated [he is subject to spells of indigestion] I generally stop eating. At such times I decide to begin with my meals again as soon as I feel hungry, but I can not always tell when to start in, because I can not always tell whether I am hungry or still nauseated."

The reports show that the experience of nausea is very complex indeed. All sorts of factors are mentioned: dizziness or swimming sensations in the head, the sensations aroused by too free a perspiration, aches and pressure-pain complexes in the head, in the eyes, in the jaws, in the arms, general bodily shivers and chills, general weakness. Besides these factors, which sometimes constitute the most prominent part of nausea, there are the sensations which are referred to the alimentary tract proper. Pressure-complexes referred to the stomach, or pressure-waves localized in the esophagus, indicate incipient vomiting and are often present.[1] Other alimentary pressure-sensations, however, appear to be more nearly integral to nausea: the 'sinking feeling' and the dull 'sickishness' are described as purely pressure, the 'gnawing pressure' and even the 'ache' are probably partly pressure. With the exception of *Z*, in whom the occurrence of a true nausea is open to doubt, and of *A* in his series with castor-oil,

[1] It is this complex, apparently, that E. Murray describes as 'revulsion . . . sometimes grading into a feeling of nausea' ('Organic Sensation,' *Amer. Jour. Psychol.*, 20, 1909, 437). To Murray belongs the credit of having obtained introspections upon nausea under experimental conditions. Unpleasant odors were used as stimuli.

all the observers agree that nausea involves a dull ache or pain in the stomachic region. Two observers (*B*, *D*) declare that this ache is indistinguishable in quality from the pain of hunger, and one (*Y*) implies the similarity by describing it at first as a 'gnawing pressure' and coming later to the conclusion that it was an 'ache.'

Which of these factors are essential to nausea, and which are occasional concomitants? Dizziness, headache, bodily weakness, shivers, perspiration, and so forth are by no means invariably present. No one is essential, and all may be absent. They occur more frequently as concomitants, or perhaps indicators, of an intense nausea. The pressures of the vomiting reflex are often absent, and were distinguished by most of the observers as separate from nausea. Their concurrence seems to be only casual; they have not come to mean nausea. Frequently they occur without nausea in vomiting.[1] The pressures of the 'sinking sensation' and the dull ache seem to be the most constant components; but the pressures are lacking in the hunger-like nausea that *B* reports for stimulation by HCl, and the pain is not found by *A* in the intense nausea induced by castor oil. Apparently, then, there is no sensory factor that is invariably present in nausea. The facts become intelligible if we regard nausea as a meaning, a situation. Various organic factors, alimentary or general, may combine in nausea, or (at least after the more complex experience has been had) a few or even one of the more usual constituents may mean the whole situation. Nausea for different persons, or for the same person at different times, may thus be very different. The significance of the nauseous situation is such that one is not likely to adopt even a casually introspective attitude toward it; hence, even if there are variations from time to time in the same person, the qualitative differences are still not likely to be noticed. Nausea involves a condition of the digestive tract, and undoubtedly the alimentary pattern of pressure and ache is the usual result of

[1] In the present experiment both *A* and *Z* vomited without nausea. In the experiments previously reported by the writer (*op. cit.*) nausea was as infrequent as vomiting was common. Observer *F* in that experiment was almost never nauseated; but see the account of vomiting on pp. 6 and 12.

the conditions which produce nausea. In a hypoalgesic individual, nausea may never reach the ache-stage. By association with the pressure-ache complex, or with what may by a given person be regarded as symptoms of nausea (*e. g.*, vomiting, loss of appetite, disgust, weakness, etc.), other sensations may come to stand for the nausea or, in a given case, to constitute the habitual form of nausea.

The foregoing hypothesis not only explains the apparent uniqueness of the nausea experience (a factitious uniqueness, acquired by the failure of the observers to observe sensory quality), but also avoids the implication that a unique experience must have a unique qualitative basis. The positive reduction of nausea to organic pressure and pain is not, however, an easy task. The whole pattern is so complex, the unity of the situation is so insistent, the fusion of the elements appears in consequence so intimate, that analysis is difficult. *B* observed that attention to some parts seemed to destroy the whole, and *Y* declared that the complex as a whole, intimately fused, constituted nausea. *A*, as a matter of fact, was not always able to make the analysis. He could find nothing but organic sensation of pressure-like quality, but he was not sure that there was not something else, 'a sickening sensation, a kind of an awfulness and helplessness.' When asked to describe, he was unable to do more than to assume the 'helpless attitude' (see p. 319). In view of the fact that all the other observers made the analysis, the inability of *A* to reduce the complex does not seem to warrant the assumption that it involved a new element. In a more general context, Titchener has remarked that the impossibility of reduction in a single case need not imply elementariness if the analysis can be made by other individuals or in other instances;[1] and it appears as if we had here chanced upon an attitude so intimately fused that its reduction was, under our conditions, not always attainable. It should be remembered, too, that *A* himself was not sure that the unique residuum was not the 'meaning of the smell.'

[1] Titchener, E. B., 'Experimental Psychology of the Thought Processes,' 1909, 171.

The Call to Defecation

Descriptions of the call to defecation and of the act itself were written down by the observers at the time of occurrence.

Observer A.—"Slight strains and dull pressures of weak intensity in abdomen are all that I can find as sensations."

Observer C.—"The call to defecation is a wave of pressure of quite general distribution and not easily localized. It is somewhere in the lower abdominal cavity and finally reaches the rectum. When this wave has run its course there sets in a general pressure which includes the whole abdominal contents. At high intensities the muscular contractions of the abdominal wall and of the sphincter are added to the former complex. These sensations are just those of normal contracting muscle."

Observer D.—"Insistent pressures in rectum. Faint but slightly achy pressure in front wall of abdomen. Slight achy pressure in temples, which feels as if blood-vessels were distended."

Observer E.—"The call to defecation is a very pleasant experience. It seems to consist of a feeling of fullness, of distension of the bowels. Pressure, which is most prominent, is at first rather indefinite and equal in all directions, but later becomes a downward one. Sometimes aches and pains in the intestine accompany it."

Observer F.—"At first, vague, diffuse pressure in lower abdomen, not particularly unpleasant. Soon, however, it becomes a dull ache and often sharply painful. There also comes an ache about the anus, which often becomes 'hot' and 'burny'; and this is accompanied by violent contractions of the sphincter muscle."

Observer X.—"First noted impulse to go to toilet. The observed vague sensations localized in region of large intestine, a vague perception of movement in that part of the intestinal tract, like a very remote dull pressure that changed its location. Also, sensations of incipient movement in anus; a somewhat rhythmic movement of distension abruptly checked each time by movements of contraction. All this was quite involuntary. The internal movement (intermittent) renewed itself more intensely, but just as vaguely; the localization of it was far from definite. The relaxation-phase of the anal movement tended to increase in duration and intensity. After a time the checking, contracting movement ceased to occur involuntarily. Now there was a definite sensation complex, meaning pressure against the anal opening from within and above."

Observer Y.—"Intense deep pressure, probably muscular, located in region of anus. Soreness (of the pain modality) fused with the pressure. The complex was unpleasant."

Observer Z.—"Dull, diffuse, rather heavy pressure, localized rather vaguely in lower abdomen. Neither pleasant nor unpleasant."

In experiments described elsewhere, the writer has shown that the call to defecation may be induced in all degrees of intensity by the inflation of a rubber bladder within the rectum; that small amounts of warm water (50 c.c., 50–70° C.) produced the call very intensely (although an equal amount of cold water at 0° C. did not); and that HCl (10 c.c., 5 per cent.)

may also constitute an adequate stimulus to the call.[1] The description already printed calls attention to the specific sensation of muscular pressure in the rectum (a muscular 'ache' in intense degrees); to the widespread general abdominal response, apparently secondary and dependent upon muscular contraction; and to the pains, which, when the call is most intense, occur in great variety of quality and reference. A few quotations will render the reference explicit:

Observer B.—After the inflation of a bladder in the rectum 10 cm. above the anus: "First pressure in rectum. Then call to defecation, which differs from the first pressure in that it is more intense, covers a larger area, and has a temporal course of varying intensity (pulsations). Later pain was introduced. From then on intensity increased by jumps. The increase of pain was the most noticeable. The pain was of the achy variety, but got sharper and brighter, more definite and lively, although always diffuse, as the intensity increased. I think the pressure also increased in intensity, although it was largely obscured by the pain. Vague general pressures in abdomen were also noticeable. Still later pain was very intense; it was really very sharp and tended to run off into shoots and stings of pain. The temporal course up to this point shows, I think, all degrees of urgency for defecation. The urgency is not only a matter of intensity, but varies with the area affected and also with the quality of pain. . . . On the release of the pressure there is a tremendous relief, very definite in the region of the umbilicus and in the rectum. It is exactly like the relief of defecation without the sensations of passage. It is kinesthetic pressure."

After the introduction of warm water into the rectum: "Violent call to defecation includes pains and partially initiated movements at rectum. More or less confined to rectal region. General muscular effort in resisting call, even to the circulatory warmth of the face. Call is predominantly a pressury ache overshot with more intense thick pains of the achy variety. Besides this there is a general muscular irradiation." In another trial: "Intense grippy pains about umbilicus. They also shoot down into testes and penis. The abdominal pain has the peculiar character of 'belly ache.' There are also pains in the rectal region, such as occur when it is hard to hold in with an urgent call." At another time 'shivers over whole body' are noted.

After the introduction of dilute HCl: "Sets up the pressure complexes of the call to defecation in rectum at once; very intense. There is in the call also a dull ache; but I should not say that its unpleasantness is dependent upon the intensity of the pain."

The call to defecation is predominantly abdominal pressure. The pressure may mean distension or contraction or movement or effort; it may be weak or intense; it may be dull, diffuse, and vaguely localized or it may be clear-cut and accompanied by a definite visual reference. Of the nine

[1] *Op. cit.*, 50–54. Hertz's observations, *op. cit.*, 28 ff., have shown that the call produced by inflation of a bladder arises in the lower rectum. His descriptions do not indicate, however, the widespread nature of the response nor its painful character in high degrees.

observers five (*B*, *D*, *E*, *F*, *Y*) find that the experience involves aches or pains; two of these (*B*, *F*) also find 'sharp pains.' Besides the general abdominal complexes, definite rectal pressures are mentioned by *B*, *D*, *X*, and *Y*. B notes that the rectal pressures involve the ache of extreme muscular pressure. Two observers (*B*, *Y*) mention that the call is unpleasant, two (*E*, *Z*) that it is indifferent, one (*D*) that it is pleasant.

B's fuller introspections for the intense call indicate that the course for increasing intensity is more or less as follows: (1) muscular pressure in rectum; (2) rectal pressure becomes intense and achy, general abdominal pressures develop; (3) dull pain introduced; (4) sharp, piercing pains, of uncertain and varying reference, appear. There is no indication of the presence of qualities not ordinarily included in the pressure-pain group.

Defecation

Reports relating to the experience of defecation itself are as follows:

Observer A.—"Dull pressures in abdomen; increased strain in abdominal muscles. Pressure in rectum, localized two or three inches above anus. Sensations of cutaneous pressure and of strain as feces pass. Slight sweat."

Observer C.—"At the act of defecation [following the call to defecation] there is no new quality in the abdomen, unless the diminishing pressure due to decreased volume might be considered as such. In the rectum and at the anus, however, the moving contents may be felt as waves of pressure. . . . The only quality that I can find is just pressure, except at times pain, which does not seem to be normal."

Observer D.—"Increase of pressure throughout abdomen and especially in rectum. Bright stingy pressure at anus, which is pleasant and which carries the meaning of expulsion."

Observer E.—"Defecation itself is pleasant. In it a feeling of relaxation and a pressure, bearing down, are mixed."

Observer F.—"First the sensations of relaxation of the sphincter and abdominal muscles; then those of the moving pressure at expulsion."

Observer X.—"Movement of issuing feces somewhat perceived somewhere in the large intestine (as vaguely dull, indefinite, moving pressure, rather rhythmic in its fluctuations of intensity), but chiefly precisely at the anal opening. Here there was a definite clear-cut experience: very strong, bright, 'moving' contact and large pressure sensations; occasional pain elements, sharp and bright. Slightly shivery sensations ran up spine, and to some extent seemed to well out from the anal region. Felt 'goose-fleshy.'" At another time: "Noted that preliminary internal sensations were stronger, more definite and steadier. They included dull, diffuse, deep pressure with a vague, weak subcurrent of dull pain. Occasionally there were sharp, knife-like streaks of pain."

Observer Y.—"Defecation-complex is made up of the following factors: muscular sensations in anus; soreness, which was more distinctly painful in character, and which varied in intensity during defecation; and, I think, another kind of pressure, located at the distal end of the anus and meaning contact of waste-products with that part. There was also present a feeling of general strain."

Observer Z.—"Contraction of many muscles of abdomen with resulting strain sensations. More intense muscular sensations localized in rectum; pressure localized vaguely in the same place. During and after the contraction of the muscles, a pain of a moving pressure."

As defecation follows the call, the abdominal contractions that induce it are sensed as strain (*A*, *Y*, *Z*) or as pressure (*D*, *X*). The dull rectal pressure increases in definiteness and intensity and ordinarily becomes painful (*C*, *D*, *X*, *Y*, *Z*). This pain may be a dull ache or soreness (*X*, *Y*) or it may be bright, stingy, sharp, and knife-like (*D*, *X*, *Z*). *D* and *E* describe the experience as pleasant, *D* specifying that it is the 'stingy pressure' which is pleasant. This introspection agrees with that of the writer. In general, it may be said that the experience of defecation is little more than a heightening of the call to defecation, with a consequent introduction of algesic elements, and with perceptual additions relating to the passage of the feces.

The Call to Urination

The descriptions of urination and of the call to urination were obtained in the same manner as those of defecation.

Observer A.—"Weak sensations, very much like muscular pressure, spread over an area high inside of body with base just above the pubic bone. Later these sensations became slightly stronger in intensity and seemed more strainy in quality, although they were still of weak absolute intensity. In the penis, especially the lower part (urethra), there were very weak sensations like contact and very weak cutaneous pain combined. Also a cool sensation in the glans near the opening. Still later I noticed weak strainy sensations referred to the penis throughout its length and to the body. The sensations were strongest at the opening of the urethra."

Observer B.—"Dull pressure-ache, like that of intense muscular pressure, referred generally to region of penis, scrotum, and pubes. Localization not specific, for complex appears big and round, and attention to any particular organ makes the sensation appear to go elsewhere although it remains in the same general region. The penis is, however, always involved; the dull ache is most intense there. Dull pressure without ache centers in the pubic region. Besides the dull ache there is a sharper ache referred to the penis in a region about half-way between the root and the glans. It is very much like a 'sting' that is spread out. It varies quite regularly in intensity, intense pulses being separated from weak ones or from periods in which there is no ache at all. [I have had an assistant note the time of these fluctuations for 5 min. There were 30 maxima in this time, an average separation of 10.1 ± 3.6 secs. Ten times the pulsations died out entirely. About once a minute there is a long interval, which serves to divide the pulsations into groups.] Ordinarily, I think, attention fluctuates, returning to the call when this sharp ache is most intense. When the call gets strong, there are muscular twitches—pressure sensations—prominent. Also a general restlessness."

Observer C.—"The call in its initial stages is intermittent and lapses with the application of attention. When it really becomes insistent it is very unpleasant. The components seem to be principally strain sensations from muscles and sensations of warmth. The first strain seems to be from the contractions of the bladder, at least it is in the lower abdominal cavity. It is a wave, moving from above downwards. At once this wave is met by a wall of pressure, and the two opposing strains seem to see-saw. The essential thing is *strain*, I think the resisting strain comes from the contraction of the sphincter muscles at the origin of the urethra. From this point lesser waves of pressure occasionally pass outwards to the distal end of the urethra. In addition to this pressure there is a sort of quality much like the pricking of a stiff hair applied to a pain or pressure spot in the skin. The bladder and the sphincter strains spread until the muscles of the abdominal wall are involved."

Observer D.—"The call to urination consists principally of bright stingy pressures in penis, mostly near the base. There is also a slight diffuse pressure higher in abdomen, which feels like the pressure of a filled bladder."

Observer E.—"The call to urination is a very pleasant thing, provided it be not too strong. It consists of a feeling of distension, of outward pressure."

Observer F.—"A vague indefinite pressure in the lower abdomen, rising as high as the umbilicus. Intermittent pain in the glans penis. Whole thing uncomfortable."

Observer X.—"The unintense experience of normal life is as a rule scarcely conscious; there seems to be an automatic reaction before the sensations become at all intense. . . . The experience, when intense, includes (1) a general bodily uneasiness, especially in the lower portion of abdomen; (2) a definite pain-complex, localized slightly below the middle of the abdominal cavity; it is an achy pain with something of the strained feeling to it; it is insistent, definite, persistent; (3) a vague feeling of 'repletion' of the abdominal cavity—a complex made up chiefly of dull pressures with perhaps a slight pain-component; (4) intermittent sensations, localized at the opening of the urethra. The 'general uneasiness' is centered upon this region, and the attention is drawn strongly to very bright and lively kinesthetic sensations of incipient movement there."

Observer Y.—"Warmth; and a sort of ticklish pressure, located in the region of the urethra and the bladder. The 'ticklishness' belongs to the modality of pain. This complex is set in a general muscular feeling."

Observer Z.—"Very slight warmth. Light diffuse pressure, spreading out through a comparatively small space and very poorly localized in the lower pelvic region toward the front of the body. Affective tone was indifferent."

All observers describe sensations of fullness or of pressure or of strain in the region of the bladder. *X* and *Y* refer an algesic quality to this region, and it is just possible that the strain noted by *A* and *C* is incipiently algesic. The most prominent part of the call for the male observers seems to be, however, a pressure-pain complex, in which the pain dominates, and which is referred to the penis (variously to the base, the side in which the urethra lies, a point between the base and the glans, and the glans). All the male observers (except *E*, whose introspection is too scanty to be considered analytically) report pain. *A* finds 'weak cutaneous pain'; *B*, besides the 'dull pressure-ache,' a 'sharp, pulsating, intermittent pain'; *C*, a 'pricking pain'; *D*, a 'stinging pressure'; and *F*, an 'intermittent pain.' *A*, *B*, *C*, and *D* describe the pain as mingled with contact and strain, with pressure and muscular sensations, or with pressure. The reports of the women are quite similar. *X* notes an ache in the region of the bladder, and *Y* a 'ticklish' pain. *X* finds bright and lively kinesthesis at the urethra. For *Z* the experience is quite colorless—merely a diffuse pressure and warmth.

The affective judgments vary considerably. *B* finds the pains pleasant, and *E* reports that the whole experience, when weak, is very pleasant. *Z* records indifference, and *C* and *F* unpleasantness—at least when the call is intense.

Urination

The act of urination is described as follows:

Observer A.—"Voluntary relaxation of the strains referred to the urethral opening, tactual sensations (like those from mucous membrane of mouth) and, at the very first, very weak cutaneous pain sensations, diffused through these tactual sensations, or spotted, peppered, around in them. These sensations are referred to the urethra, almost for the entire length of the penis. The whole experience was pleasant."

Observer B.—"Sharp aches, like the intermittent ones in the call to urination, become very intense just at the initiation of passage. They are referred to the same region as before, *i. e.*, above the glans, about one third of the way to the base. The aches get weak as passage starts and continue so until the end. Then, as the last dribble passes, they become momentarily intense again. After that they weaken and die out slowly during the subsequent minute. There are dull muscular sensations in the region of the bladder, which are not at all prominent. The relief afterwards is represented by the persistent aches in the penis, as described, and a large diffuse ache of the same quality in the region of the bladder. . . . All these sharp aches are very pleasant indeed. Even when one is restraining urination with much effort, the pulsations of pain are very pleasant. They are, I think, similar to, if not identical with, the sensations of the sexual organs at a low degree."[1]

[1] The completion of urination is physiologically similar to ejaculation. The last portions of urine are expelled by rhythmical contractions of the bulbocavernosus muscle. See Howell, *op. cit.*, 785, 898.

Observer C.—"Strain in the muscles of expulsion but relaxation in the sphincter muscles. In the urethra there is a warm pressure, which persists just for a moment after the act is over. General relaxation marks the closing."

Observer D.—"At the beginning the pressure in the penis gives way to a tingling. This changes to a complex which I have not been able to analyse, but which means liquid flowing through the urethra. There is also general relief of pressure in the bladder."

Observer E.—"Pleasant, but I have not been able to reduce the experience to words."

Observer F.—"At first there is a sharp burning pain in the glans penis. Then nothing but the pressures accompanying the flow of urine. At the end there is a repetition of the beginning pain, only it is much stronger."

Observer X.—"Urination is accompanied by almost no sensations. There is a lack of the strain sensations experienced in trying to hold back the reaction,—a general bodily relaxation. There are very weak, bright, contact sensations at the opening. The pain sensations [described in the call in the region of the bladder] do not change in intensity or cease until some time after the operation is completed."

Observer Y.—"Muscular sensations in region of the urethra and bladder. Warm-pressure perception (flowing of liquid) and auditory perception (liquid striking water). This perceptual complex, set in a weak, general tension, present during urination. Afterwards a general feeling of relaxation, slightly pleasant."

Observer Z.—"Almost sensationless. *Very* weak pressure sensations, moving in scarcely perceptible waves,—just like the faintly discriminable changes in pressure you get from floating when in swimming. With these pressures, very weak muscular feeling of relaxed muscles all over and through the abdomen."

There has recently been reported to the writer a case of a woman who was unable on a railroad train to tell whether she was urinating or not. The noise of the train so obscured the usual auditory cues, she said, that she could not make the judgment. No doubt the jar of the train prevented any faint organic sensations, which may have been present, from being distinguished as cues.

Like the call to urination, in the men urination proper involves principally a pressure-pain complex in the penis. Some mention is made of muscular or strain sensations in abdominal regions, but the characteristic experience seems to be referred to the penis, and is caused, no doubt, by the distension of the urethra. Distention here, as elsewhere, may be expected to result in pain. The less experienced observers had difficulty in making the analysis. The others find in some cases tactual sensations, strains, or the less definite 'pressure.' Pain is possibly universal. *A* finds 'cutaneous pain,' *B*, 'sharp aches'; *F*, 'sharp, burning pain.' The 'tingling' of *D* implies pain, and, when one is familiar with the usual stinging response of the penis to warm stimulation, one is tempted to read an algesic quality into *C*'s 'warm pressure.'

In the women the experience seems to be as indefinite and colorless as it is striking in the men. *X* and *Z* call it 'almost sensationless,' and they are supported by the case last cited. Muscular sensations and weak contacts and pressures are noted in the effort to find something to report. A true sex difference seems to exist.

The experience is doubtless affectively indifferent for the women. Of the men, three (*A*, *B*, *E*) mention the affective aspect and all declare that it is definitely pleasant.

Conclusion

We may conclude that thirst, hunger, nausea, the call to defecation, defecation, the call to urination, and urination are all complex experiences reducible, under favorable conditions, to various patterns of pressure and pain.

The experiences may be very complex and may vary from individual to individual. Nausea is, perhaps, the extreme example. Different processes may stand for it at different times; and again, it may become so attitudinal as to defy analysis. Hunger is reduced to a single pain only by isolating it from appetite. Thirst is confined to the mouth and throat, frequently to the mouth only, but is definitely of the perceptual order. The excretory complexes are less frequently recognized as specific, and show a correspondingly wide variation in their many constituents.

The reduction to pressure and pain suggests the possibility of a number of qualities within each of those modalities. Besides occasional pressure of cutaneous quality, dull pressure and muscular pressure were the most frequent forms reported. As is usual, the muscular pressure in intense degrees runs off into painful ache. There were also sharp pains which, it may be, are of more than one kind.

Pain, although less usual than pressure, is by no means uncommon. It may be present in the throat in thirst; hunger is pain; the most constant constituent of nausea is the same pain as that of hunger; the call to defecation, when intense, involves sharp, shooting pains and dull aches; defecation may include stinging sensations at the anus; urination and the call involve aches in the region of the bladder and, in the male, bright pain in the penis. These pains may be accompanied by any affective judgments whatever. The intense bright pain of urination may be very pleasant, while that in the call to defecation may be extremely unpleasant.

The present paper has been mainly analytical. The problem that it attacks must remain unsolved until the complementary synthetic operation has been performed. It is one thing to reduce a large part of organic experience to pressure and pain; it is another thing to say how many pressures and how many pains there are, how they differ from one another, and how they combine to form the typical complex processes of organic life. This second phase of the problem it is the writer's hope to bring into the laboratory.

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THE PSYCHOLOGICAL REVIEW

THE FATHER OF MODERN PSYCHOLOGY

BY PROFESSOR FOSTER WATSON

The 'father' of modern psychology was, I suggest, Juan Luis Vives. It may be objected that if we take 'modern' in a sufficiently literal sense, we must go back to Aristotle. For that great philosopher traced the origin and development of the pre-Socratic psychology, critically examined the Platonic views, embodied and organized from his predecessors whatever would serve as basis; and for the rest, supplied, from his own researches and thought, an organic system of psychology, which has held its own, and still claims close study—from the fourth century B.C. to the present time—continuously for over twenty-two centuries and a half. St. Thomas Aquinas, fifteen hundred years after Aristotle, re-affirmed the main points of Aristotelian psychology, but supplemented them by a rationalized interpretation, in which he, in many cases, anticipated modern psychological thought.[1]

Doubtless the line of continuity in psychological advance is traceable, in this way, to Aristotle as the real founder of the subject. But, our modern division of history, into ancient, mediæval and modern, requires us to consider new starting-points, though, logically, in the light of the principle of continuity, it is misleading to regard even these great divisions as abrupt transitions. Although the Renascence period of the fifteenth and sixteenth centuries brought a steady concentration of attention upon psychological questions, as it did upon all humanistic problems, yet the main

[1] The Rev. Prof. Michael Maher, S. J., has shown in his interesting 'Psychology—Empirical and Rational' the parallels between St. Thomas Aquinas and modern psychology.

setting of psychological theory had clearly been determined by those great thinkers, Aristotle and St. Thomas Aquinas; and any advance was most hopefully to be looked for, which started from them as basis.

It is often stated that 'the father' of modern psychology was René Descartes (1596–1650). Without attempting to withdraw any credit from the actual accomplishments of Descartes, in his day and generation, in the subject of psychology, it would be extraordinary, on *à priori* grounds, if the period of the Renascence (say from 1450 onwards) till the time of the birth of Descartes (1596) had failed to produce a conspicuous thinker on a subject so essentially humanistic, as that of the Mind. Some writers, accordingly, place Francis Bacon (1561–1626) as the pioneer of modern psychology. Bacon was, certainly, the most influential advocate of the empirical scientific method of the seventeenth century. Psychology has made its great advance by the employment of this method. Hence, it is urged, Bacon is the leader to whom modern psychology traces its beginning. But neither Bacon nor Descartes was the first Renascence writer to give his attention to psychological theory, nor even to the advocacy of the empirical inductive method. In a wide though valid sense of the term, every man is a psychologist, and every man employs the inductive method, and the 'fatherhood' of both goes back to an antiquity, not merely as old as Aristotle, but as old as man himself, at least. But the *self-conscious emphasis on induction* as a method of inquiry and discovery in philosophical, and particularly in psychological questions, must be taken back, even in Renascence times, beyond and before Descartes and Bacon, at any rate, to Juan Luis Vives (1492–1540).

Thus, Vives shows explicitly his insight into the significance of the empirical inductive method in his account of the origin of the arts. For the formulation of the arts, says Vives, was due to observation joined with reasoning. "In the beginning, first one, then another experience, through wonder at its novelty, was noted down for use in life; from a number of separate experiments the mind gathered a universal law, which, after support and confirmation by many experiments was considered certain and established. Then this knowledge was handed down to posterity. Others added subject-matter which tended to the same use and end. This collection of knowledge-material by men of great and distinguished intellect, constituted the several branches of knowledge, or the arts. . . . Whatever is in the arts was in nature first, just as pearls are in shells, or gems in the sand."[1] The insight which Vives brought to bear on the place of the empirical, inductive method, in the building up of the arts and sciences, was of the first importance to him, when he came to study the subject of psychology, theoretically.

In 1538, Vives published his book on psychology, entitled, after the work of Aristotle: 'De Anima et Vita.' He addressed it to Francis, duke of Béjar, to whose descendant, another duke of Béjar, Cervantes in 1605 dedicated 'El Ingenioso Hidalgo Don Quijote de la Mancha.'

In this Preface to the 'De Anima,' Vives makes clear that he writes on the mind, in no perfunctory or merely conventional spirit, for he recognizes that the knowledge of the mind has relation to matters 'of the *greatest usefulness*.' To govern oneself one must know onself, not only as so much bones and flesh and nerves and blood (though these come into the calculation) but each should learn to observe the nature, quality, ability, strength, passions, of the mind. Each should 'explore' himself in his varied nooks, and even obscurities of mental life. "On this account, it seemed good to me to ponder deeply on some points of this great subject, all the more because recent philosophers have brought but little industry to the study, *content with what had been left behind by the ancients*. They have added nothing at all except problems, almost impossible of solution, and such that, even if solved, would bring no fruitfulness. Formerly the ancients involved themselves in great absurdities on this subject, for they thought wrongly of the mind, which is not perceived by

[1] For an account of the relation between Francis Bacon and Juan Luis Vives see Rudolf Günther, 'Inwieweit hat Ludwig Vives die Ideen Bacos von Verulam vorbereitet' (1912), and as to the influence of Vives on Descartes see, Roman Pade, 'Die Affectenlehre des Johannes Ludovicus Vives' (1893).

the bodily senses—and even their opinions on the very matters which we do perceive by the senses were most inept."

Vives deliberately passes by the method then,—as ever—so much in vogue of rebutting opinions deemed to be false, which he says are more numerous in this than in any other subject of inquiry. Such refutations would lead to thorns rather than fruit.[1] He will attempt to fit into the structure of his discourse, 'those expressions which have sprung from the people and are in common acceptance with them, and then have become unintelligible as they have passed into the use of the learned.' For in dealing with the recondite problems of the mind, the adaptation of a fitting vocabulary is a serious part of the undertaking if a treatise is to be accommodated to learners. Vives's concern for his choice of language and vocabulary, it will be remembered, was reflected, with such good consequences, in his successors Francis Bacon, Thomas Hobbes, and René Descartes, and very specially in John Locke's 'Essay on the Human Understanding,' Book III., than which it would be difficult to find a more masterly exposition of language as the means of expressing ideas clearly.

In his address to the duke of Béjar, then, Vives claims that he has parted company with the mischievous babblings of the Stoics and their carping sophisms, and no less with the hidden and subtle opinions of Aristotle.

In describing the contents of Vives's 'De Anima' it will be more profitable to emphasize his independent and original views rather than to dwell upon the undoubtedly many points of similarity which could be found in his book with the subject-matter in the works of Aristotle and St. Thomas Aquinas.[2]

Tired of the atmosphere of dialectical displays which inflated the pride of victory rather than stimulated the search for truth, Vives was ready to try new paths. The time-honored treatment of psychology included a discussion of the

[1] Vives, however, to some extent, deals with the 'false opinions' of his predecessors in psychology, in his 'De Causis Corruptarum Artium' and in his 'De Veritate Fidei Christianæ.'

[2] See T. G. A. Kater, 'J. L. Vives und seine Stellung zu Aristoteles;' G. Hoppe, 'Die Psychologie des J. L. Vives;' R. Pade, 'Die Affectenlehre des J. L. Vives.'

question What is the soul? Yet Vives ventures to write a book entitled 'De Anima' in which he says: "What the soul *is*, is of no concern for us to know; what it is like, what its manifestations are, is of very great importance."[1] This calm renunciation of the metaphysical aspect in favor of the descriptive account of the activities of the mind is a natural enough result from Vives's revolt from the older academic disputational methods, which precluded progress, because they could not reach further in their conclusions than they assumed in their premises, and their premises were always ultimately founded on *à priori* abstractions. Thus, disputes on the nature of the soul either began in the unknown, and ended there, or else sought an authoritative basis, which was similarly substantially unknown (because not subjected to enquiry). Vives, therefore, excluded the discussion of what the soul is, in its essence, from his scope. Instead, he asks for careful investigation into the *manifestations* of the soul in all the activities of consciousness. "We cannot rightly declare what the soul is in its essence, and as a bare thing place it, as it were, before the eyes, but we can set it forth, clothed and as if painted in a picture, in its own most apt colors, so that it is seen in *its own actions*. For it has not come under the observation of our senses, but we perceive its works (*opera*) by almost all the senses, internal and external. Surely the goodness of the Lord of Nature to us reveals itself by many proofs, on every side, by thus aiding us, in placing before our eyes and in such abundance, these manifestations of the soul. For there is no sign more clear that things are not to be converted to our use than for them to be remote, rare, difficult to be prepared. Nor did he who bid us know ourselves, refer to the essence, but to the actions of our mind so that they may be ordered for moral life, and by the expulsion of vice, we may follow virtue which will so lead us that we may spend in full wisdom, as immortals, the happiest eternity." It is a central doctrine of Vives that Knowledge is of value, simply when it is 'put to use.' The

[1] Anima quid sit, nihil interest nostra scire, qualis autem, et quae eius opera, permultum. 'Opera,' Vol. III., p. 332.

observational method of studying the manifestations of our minds has a value for application which cannot be gainsaid. If indeed, the processes of cognition were to be regarded as only of intellectual worth, even then the right study of the passions, as they show themselves in ourselves and in the individuals we read of in history, would be of direct ethical significance for the purposes of examples or of warnings. And, accordingly, Vives devotes one of the three books of the 'De Anima' to a critical and constructive study of the passions.

We have seen that Vives places the emphasis in psychological studies upon the observation of the manifestations of the soul, in its outward realized activities. When we thus observe the results of mental activity in the numerous forms of cognition feeling, will, in other persons, or in ourselves, we are at the point of view of empirical psychology. Vives may, therefore, be claimed as a pioneer in the advocacy of this method, prior to Francis Bacon and to René Descartes. It was not a long step to take, to advance from the empirical psychology, which is concerned in tracing the processes of mental activity in others to that of recording, by way of psychological investigation, the results of interrogation as to what has happened in one's own mental experiences. Hence, illustrations can be found in Vives, of the conscious employment of the introspective, empirical method, the method that is especially characteristic of later psychological students, and as these appear to be the first instances of conscious introspective interrogation of consciousness in psychological investigation in modern times, *i. e.*, from the Renascence onwards, they are of more than ordinary interest and significance.

"As often," says Vives, "as I see a house at Brussels, which is opposite to the Royal Palace, Idiaqueus comes into my mind, for he is its occupier. Very often in that house,as far as his business would allow, we used to chat over matters pleasant to both of us. Now, as often as I revolve the idea of Idiaqueus I do not think of the Palace, because the memory of my friend and his house is more noteworthy to me than the idea of the Royal Palace.[1] So with sounds, tastes, smells. When I was a boy at Valencia, I was ill of a fever. Whilst my taste was deranged, I ate cherries. For many years afterwards, whenever I tasted fruit I not only recalled the fever, but also seemed to experience it again."[2]

These illustrations of the introspective method occur in connection with Vives's treatment of two associated ideas [*recordatio gemina*]. Of course, it is not the fact, as some writers seem to suppose, that the doctrine of association of ideas began with David Hartley or with the Mills nor even with Thomas Hobbes, or John Locke. This explanation of mental process, goes back, at any rate, to Aristotle. But it may be claimed for Vives that his development of Aristotle's theory marks the Renascence advance. No other author as early as Vives contributed so strong and comprehensive an exposition of it, as Vives. Indeed, Sir William Hamilton, the most erudite of all British philosophers, in the history of psychology, said: "Vives's observations comprise in brief, nearly all of principal moment that has been said upon this subject (of mental association) *either before or since*."[3]

Before Sir William Hamilton, Samuel Taylor Coleridge[4] in his effort to discountenance Hobbes's claim to be the 'original discoverer' of the law of association, as advocated by Sir James Mackintosh hit upon the 'De Anima' of Vives, and appears to have been the first Englishman to have drawn attention to Vives's enunciation of this law of association of ideas. This was in 1817. The words quoted by Coleridge from Vives are: *Quæ simul sunt a phantasia comprehensa si alterutrum occurrat, solet secum alterum representare.*

[1] Vives is explaining that our minds often travel more readily from the less to the greater than *vice-versa*. Some might think that the house oposite to the Palace would lead to the thought of the Palace. But Vives pays a compliment to his friend, as well as expounds psychology, when he says his friend's house recalls the 'greater' *i. e.*, the more excellent idea of his friend and their former talks together.

[2] Vives argues that, for this reason, in mnemonics, the 'clues' to excite memory should not be themselves of such interest as to detain the attention too much from the suggestion of what they are intended to assist in recalling.

[3] 'The Works of Thomas Reid' (including Hamilton's 'Dissertations'), 1872, 7th ed., Vol. II., p. 896, column i.

[4] In the 'Biographia Literaria,' Chapter V.

Coleridge proceeds to regard Vives as "subordinating all other exciting causes of association to *time.* The soul proceeds '*a causa ad effectum, ab hoc ad instrumentum, a parte ad totum*'; thence to the place, from place to person, and from this to whatever preceded or followed, all as being parts of a total impression, each of which may recall the other." Chains of associated ideas may have the most distant links connected 'by the same thought having been a component part of two or more total impressions.' Vives's example quoted by Coleridge is: "From the idea of Scipio I come to the thought of the Turkish power, on account of his victories in that part of Asia in which Antiochus was reigning."

Hamilton, who shows little mercy to Coleridge, tells us that the whole of the latter's chapter on the history of the law of association was 'conveyed,' to use the old expression, from the German Maass, and is a 'blundering plagiarism.' So, with regard to Coleridge's inference that Vives substantially limits the law of association to that of the sequence of connected ideas *in time, i. e.*, to the phenomena of recollection, Hamilton clearly shows that Vives does not so limit associative ideas to *time*, or to place, but he points out their operation '*in all the connections of thought and feeling.*'

The insight which Vives thus showed in his exposition of the law of association of ideas prepares us for further details in his experiential account of memory. As we have two hands, so memory is twofold, and consists in apprehending and retaining. The differences in memory amongst men are implanted by nature. Some men like Hortensius remember words more readily; others, *e. g.*, Themistocles. Some apprehend quickly and retain better the curious; others, the simple; some, public affairs, others, private matters; some, the old, others, the new; some, their own affairs, others, those of others; some, vices, others, virtues;—each according to the proneness of his disposition. For he attends more willingly and therefore more closely to this or that, and attention strengthens memory. A natural bodily constitution is of the highest importance to memory, and with such were endowed the great men whose magnitude of memory has been handed down, *e. g.*, Themistocles, Cyrus, Cineas, Hortensius. This natural endowment is capable of being strengthened by habits of living. Memory is more tenacious in slow than in quick minds, like an impression driven in for a long time on stone or iron, but the swift minds more readily *bring back* a reminiscence. A deep descent into memory is made by those things to which we give close attention and care when first perceived. If anyone is in a state of emotion or excitement on first hearing of something, and this is mingled with the memory, this reminiscence is easier, quicker, longer, as, *e. g.*, for what has entered the mind when in a state of violent grief the memory is the longest, and for that reason, adds Vives, in determining the boundaries of property it is the custom of some races to bring their boys there, and thrash them with severity, so that the boundaries may be more firmly, and the longer, retained in memory. By practice and frequent meditation the memory gathers great strength. Unlike other gifts of the mind, which do not deteriorate by rest and cessation, memory grows fainter and fainter, day by day, if it is not exercised.

The law of forgetfulness is fourfold. (1) When the image painted in the memory is utterly scratched out; (2) when it is smeared or broken in pieces; (3) when it stealthily evades us in our search for its recall; (4) when it is covered over as with a veil, in disease, or in the excitement of emotion. Vives sketches the method of recall of an idea; by retracing as it were, our steps, we may come in thought to what we are seeking. Thus from the idea of a ring we think of a goldsmith, from him to a queen's collar; thence to a war which her husband waged; from the war to the leaders; from the leaders to their ancestors or children; thence to the studies in which the latter are being exercised. To such series of ideas there is no end. These steps spread themselves widely through all kinds of subjects, from cause to effect, and so on, as we have seen in Coleridge's reference. Vives, however, gives an example, which Coleridge omitted, but which we may note, since we are emphasizing the appeal to experience made by Vives. "In thinking of the name of Cicero, there comes into my memory the name of Lactantius, for he was the imitator of Cicero. From him, I proceed to think of the copper-plate artist, since his book is said to have been either the first or amongst the first printed from copper-plates."

Remembrance is of two kinds. It is either natural, or voluntary, when we pass freely from one idea to another, or it is ordered (jussa) when the mind makes an effort to reach and bring back some idea. Those ideas which occur in a series are more easily remembered, as for instance, in mathematics. Verses are conducive to faithful retention on account of the rhythm of composition, which keeps the mind from straying outside its limits. The art of mnemonics is based on the order of what is committed to memory. It is at this point that Vives introduces his definition of association. For with regard to ideas 'which have been included in the imagination [phantasia] at the same time if one of the two should occur, it is wont to bring back with it, the other.' According to his custom, Vives illustrates this definition. "From the sight of a place there comes into the mind what we know once happened in that place, or what was situated there. When something joyful happened along with a voice or sound, we are delighted when we hear that same voice or sound again. If it was a sad event, we are saddened. This is also to be noted in the lower animals, who if they receive something pleasant, after they have been called by a sound, they will run towards it readily and gladly, on hearing the same call again. But if they were beaten, after being called, they are frightened by the memory of the blows, on hearing the same call again. Returning to memory, another factor in clear memory, Vives points out, is the time-element. A distinction of times is necessary in reminiscence, otherwise images are confused, as if in a picture, other pictures should get painted on the top, after an interval. Those images which we have received with a quiet, leisurely mind imprint their trace for a longer time, and more permanently, if we gave our attention to them. It is for this reason that the things we have seen and heard in early life are recollected by us so clearly. For at that age the mind is free from cares and thoughts. Everything is new. We therefore watched closely these things which won our admiration and they sank deeply into the mind. Older men are preoccupied, and in an 'internal agitation of thoughts' so that they cannot so restfully admit new ideas, or find out readily the old amid their experiences.

In connection with associations by similarity, Vives is impressed by the opportunities for error in memory and in thought, as we pass from like to like. We mistake Georgius for Gregorius; problema for enthymene; Pindarus for Pandarus. Similarity may appear in the beginning, middle, or end, of words. Or the similarity may be in things, from the manner in which we concentrate our attention on them. In philosophy we may think of Xenocrates for Aristotle; in speaking of the Carthaginian Wars we may confuse Scipio with Q. Fabius. In a question of poverty, we may say Irus instead of Codrus. In eloquence, we may ascribe something to Demosthenes instead of to Cicero. In a matter of beauty, we may speak of Narcissus for Adonis. Or in a question of smell, mistakes may be made of garlic for onion. So, in matters of times, actions, qualities. Similarity, therefore, disturbs the memory, as it does the eyes of the body, so that judgment is confused. Vives sees that errors can creep in at the 'first attention' when an idea is received or at the 'second consideration' when reminiscence wrongly draws forth those ideas which were received as wholes. "Yesterday," illustrates Vives, "in the market-place, Peter of Toledo saluted me. I did not notice the fact sufficiently, nor remember it accurately. If anyone asks me: 'Who saluted you yesterday in the market-place?' and adds nothing further, I shall answer more readily than if he were to say: 'Was it John Manricus or Ludovicus Abylensis?' The labor thus becomes two-fold, first to reject what does not fit, secondly to replace it by what is right."

We can thus see that Vives is permeated with the idea of an empirical and introspective method of the modern type in his 'De Anima,' and that the application of this method has taken him far along the path of development of the doctrine of association of ideas, and in the exposition of observational aspects of memory. There are many other views of Vives which have special interest. Thus his emphasis on the necessity of observing and distinguishing the great variety of differences in men's minds became the basis of the treatise of his fellow countryman, Juan Huarte, written in 1557, and translated into English[1] by Richard Carew in 1594, under the title of the 'Examination of Men's Wits.' This book following on explicit suggestions in Vives's 'De Anima,' demands that children's impulses and tendencies, both in play and at work, should be studied so as to afford a psychological basis for their studies and after-occupations. Vives distinguishes carefully between the *ratio speculativa*, whose 'end' is the truth and the *ratio practica* whose end is the 'good.' The Spanish biographer of Vives, Señor Professor D. Adolfo Bonilla y San Martín, points out the parallel between Vives's view and that afterwards developed by Kant. Vives introduces *à priori* subjective forms of reason which he terms *anticipationes seu informationes naturales*, and as Bonilla remarks, *anticipationes naturales* is also the term used by Francis Bacon in 'Novum Organum,' Book I. Nor will theologians and philosophers, if they turn to Vives's 'De Anima,' pass by without notice his treatment of the problems of free-will, and of immortality, for they present an interesting individual statement of these problems—of this Renascence period.

Educationists ought to realize that Vives writes in the 'De Anima,' a section which he terms 'de Discendi Ratione.' He attempts the well-trodden path of an evaluation of the order of intellectual resources and discipline afforded by the several senses. He remarks: "The course of learning is from the senses to the imagination, and from that to the mind of which it is the life and nature, and so progress is made from singulars to combinations, from singulars to the universal; which is to be noted in boys. . . . And so *the senses are our first teachers*[2] *in whose home the mind is enclosed.*" It is in this section of the 'De Anima' that Vives refers to the ad-

[1] Indirectly, from the Italian of Camillo Camilli.

[2] Cf. Rousseau's often-quoted "Our first teachers of philosophy are our feet, our hands, and our eyes." 'Emile' (Payne's ed.), p. 90.

miration, almost bordering on incredulity, with which he heard that a deaf-mute had become taught, and he points out that the method of learning necessarily implied a large measure of self-teaching.

Whilst the psychological views of Vives, sketched above, are of this most significant modern cast, yet it must be stated clearly that the general setting from which they are taken is that of the old Aristotelian psychology, and if there are modifications in statement, these are usually clearly based on scholastic writers, for as a Spaniard, Vives was thoroughly unwilling consciously to borrow or adopt from Moorish sources. He treats of 'souls' as distinguished from 'torpid things' (or the inorganic world), the distinction being founded on the power of self-movement. He is thus provided by the Aristotelian-Scholastic psychologists with (1) the *anima alens* of the plants, (2) the *anima sentiens* of zoöphytes, (3) the *anima cogitans* of birds and four-footed beasts and (4) the *anima rationalis* of man. The soul of man is the 'form' of which the body is the 'matter.' The human body therefore is a potentiality, which is actualized by the union with it of the soul. But this principle of the form giving actuality to the potentiality of suitable matter is characteristic of all life, *e. g.*, in plants, zoöphytes, birds, beasts—up to and including man. The lowest kinds of life, *e. g.*, plants, have the motions of nutrition, growth and decay. Animals, in addition, have sensation, and developments from sensations. Man combines all these stages, including the appetency which springs from sensation, cognition and reasoning. The soul of man is therefore an epitome of all lower life and also possesses psychical ingredients of its own. Hence, an investigation is necessary into the vegetative 'soul,' and the 'animal' soul, as well as the human soul. Thus the physical phenomena of nutrition, growth, decay, generation, sensation (in general) and the special senses (and the hierarchy amongst them), are passed in review, then 'interior cognition' including imagination, phantasia (into which angels, good and bad, insinuate themselves), the *sensus communis*, the cognitive judgment and reason. Vives then offers his definition of the soul[1] and makes the usual inquiry as to the seat of the soul, deciding, with Aristotle, that it 'informs' the whole body; though certain functions are localized, *e. g.*, the front part of the brain is the seat of *phantasia* and in the back part of the brain memory is localized, and so on.

The above topics comprise the contents of Book I. of Vives's 'De Anima.' Book II. is devoted to the rational soul and its faculties. Man has been created for eternal felicity and provided with the means for accomplishing it. This implicitly demands the intelligence to know the good, the memory to retain that knowledge, and the will to act it out in life. We thus have the trinity of the soul. Vives then describes in detail the functions of the *simplex intelligentia* (simple apprehension), memory and reminiscence, composite ideas, reason, judgment, mental ability and its individual varieties, speech, the method of learning (in which Vives inquires why there are so few learned people), knowledge, contemplation, will (in which occurs the discussion on its freedom),[2] on the mind in general, on sleep and dreaming, 'habit,' old age, length of life, death, and the immortality of the human soul.

In the third book of the 'De Anima,' the subject of which is the emotions or passions (affectus), Vives undoubtedly followed[3] St. Thomas Aquinas. We have seen that Vives regarded the intellect as supplying knowledge as to the good, which the will was to carry into effect. We are, therefore, prepared to find that 'passions' are looked upon in their relation to the supreme end to be achieved in volition. They are defined by Vives as 'the natural faculties of our soul by which we are carried towards the good and endeavor to avoid

[1] This follows substantially the teaching of Aristotle. Animam esse agens praecipium, habitans in corpore apto ad vitam.

[2] It is worth noting that Vives strongly protests against the doctrine current long after his age, that the will is controlled or influenced by the motions of the stars. The significance of the protest is only realized when we remember that Tycho Brahé, Kepler and even Galileo 'cast nativities.'

[3] Aristotle is out the question. As Mr. Hicks says Aristotle 'exalted the cognitive element, while his treatment of the emotions and the will is wholly inadequate even if the ethics and the rhetoric be called in to redress the balance.' *Aristotle de Anima*, p. LXXII. On the parallels between Vives and St. Thomas Aquinas see Roman Pade, 'Die Affectenlehre des J. L. Vives,' Münster i. W., 1893.

the evil.' The close relation, therefore, between psychology and ethics is evident. Vives's treatment of the passions, though based on St. Thomas Aquinas, is yet largely supplemented by his own introspection and observation.

Whilst thus he tends to emphasize the interest in the description and analysis of the separate emotions, and to give the result of his wide intercourse with men of very varied kinds, he has not been so thoroughgoing in his psychology of emotion in general. He does not offer an elaborated theory of the passions like Descartes and Spinoza, leading in the one case to a discussion of emotion in the abstract, and in the other to an *à priori* mathematically-based theory, but he is quite as comprehensive and at points shows depth of empirical interest, which place him in the direct line of continuity between the Scholastics and the modern descriptive school of psychology, in the treatment of the passions. Vives reduces all the passions to two, love and hate. All that stirs and stimulates towards the good, comes from the incitement of love, and all that stirs to evil, from the passion, in some form, of hate. Yet Vives describes fully and separately the passions: good-will, respect, sympathy, gaiety, hope, laughter, annoyance, scorn, anger, hatred, envy, jealousy, indignation, vengeance, cruelty, sadness, mourning, fear, shame, pride. Vives is strongly attracted to the Platonic treatises, especially the 'Phædrus' and 'Symposium.' The value of the third book, as with the other two, is in its empirical method, for it also contains numerous observations, personal illustrations based on Vives's own introspective experiences. The distinguished psychologist Harold Höffding[1] in the section on the psychology of the feelings, whilst discussing the topic of laughter, quotes Vives, who described himself as unable to refrain from laughter as he first tastes food after a long fast ('De Anima,' Bk. III[2]). And, again, Höffding recalls the observation of Vives, that what man expresses by laughter may be expressed by animals in other ways[3] (*e. g.*, by wagging the tail).

[1] 'Outlines of Psychology' translated by Mary E. Lowndes, 1891, pp. 291–2.
[2] 'Opera,' III., p. 469.
[3] 'Opera,' III., p. 470.

In his professedly psychological work—throughout the three books of the 'De Anima'—Vives discloses himself as the pioneer of the modern empirical method in psychology. But we only realize the full sense of conviction, which animated Vives in his use of the method of introspection and observation, when we further note in his other writings, his constant application of the same method in the ordinary affairs of life. He applied psychological principles to professional practice, to individual conduct, and particularly to the function of teaching. In other words, in practical affairs, he sought to introduce psychological precepts and methods, to create a habit of introspection which might be turned to use—to create an atmosphere of psychology, to think psychologically.

No writer of the Renascence period was so distinguished by his application of psychology to education, as Vives. In his 'Transmission of Knowledge' ('De Tradendis Disciplinis,' 1531) he is permeated with the desire to bring education to a psychological basis. We have seen that his account of the memory was an outstanding feature in his *de Anima*. But in dealing with the subject from an educational point of view, he is in accordance with the most modern of writers in pointing out that both quick comprehension and faithful retention in memory are helped by a right *arrangement of facts*. This, he adds, is just that art of memory 'which beasts are said to lack.' His rules for the cultivation of memory have not lost their suggestiveness. For instance, he says: "What we want to remember must be impressed on our memory *while others are silent*. We need not be silent ourselves, for those things which we have read aloud are often more deeply retained. . . . It is a useful practice to write down what we want to remember, for it is not less impressed on the mind than on the paper, by the pen. The attention is kept fixed longer by the fact that we are writing it down." "Great is the help to memory if reasons are associated with the matter taught." We have seen that Vives recognizes *interest* as a strong stimulus to the attainment of knowledge. He goes far in the direction of Herbart in the recognition of interest not merely as a means in the acquisition of knowledge but also in the advocacy of a wider scope of interests as the outcome of our studies. Vives in his story of Charles Virulus the schoolmaster of Louvain is as modern in significance as Herbart himself. When a pupil's parent came to visit, especially to dine with him, he made a point of finding out what his visitor's work and interests were and *prepared himself carefully* to converse on matters familiar to his guest, and lead him to speak freely on what was best known to him. "He would thus hear in the briefest time details which he could scarcely have gleaned from the study of many years."

Education with Vives is not the preparation for a career, but the increase in practical wisdom of life and the preparation for moral excellence. In each school masters should meet four times a year and discuss the 'nature' of each boy and then apply the boy to those studies for which he seems most fit. The fruit of studies is not honor or money, but the culture of the mind—'a thing of exceeding great and incomparable value—that the youth may become more learned and more virtuous through sound teaching.' Boys should only at first be taken on trial in the school. The teachers are to determine who are fit and who unfit for learning. As Ascham maintained, Vives previously urged that the slow wit is usually the surest. The wonderful variety of dispositions in boys requires the closest attention of teachers, in 'choosing' scholars. Yet there is scarcely anyone who will not profit by being taught, if the right sort of teaching is given him. Probably no Renascence writer has taken so much notice of the problem of the feeble-minded, the deaf and dumb, the blind, as Vives, though naturally he has not been able to suggest the most effective lines of training in each case. But his firm grip of the principle of suiting instruction to the individual capacity puts him in the direction of perceiving the problem involved. Vives, again, sees clearly that the problem of education is essentially that of self-activity. He requires the pupil to keep paper notebooks, in which he gathers for himself the main materials of his own instruction. His notebooks must have divisions and heads, and be provided by himself with indexes. In these he must enter, under proper heads, all he learns from teachers and books. In other words, he must largely *make his own text-books.* He presents a well thought out psychology of school punishment, and indeed his psychology of examinations, if we may so call it, is in advance of present-day methods, for, instead of pitting boy against boy (when he has emphasized the great variety of original mental capacity) he logically requires the *comparison of the boy with himelf at an earlier stage.* "Let scholars keep what they have written in earlier months, in order to compare it with that written at a later month, so that they may perceive the progress made, and persevere in the way of improvement."

Thus, whilst Vives sees the overwhelming importance of building up the art of pedagogy upon a sound psychological basis, he by no means limits the value of the application of psychology to the work of the schoolmaster. The knowledge of psychology is essential to all who have to deal with spiritual affairs. "The study of man's soul exercises a most helpful influence on all kinds of knowledge, because our knowledge is determined by the intelligence and grasp of our minds, not by the things themselves." The text-books for psychology recommended by Vives are the sacred writers of the Bible, and the three books of Aristotle's 'De Anima' (especially Books II. and III.). Other writers to be read are Alexander, Themistius, Timæus of Locris, and Plato's 'Timæus,' Proclus, Chalcidius, and of the Renascence writer, Marsilius Ficinus, who will act as guide for Plotinus. The physician, to Vives, is on the very borderland of nature-study and soul-study, and must be at home in both. But the psychological aspect must be present not only in his studies, but in his professional habits. He must himself "not be in infirm health, not pallid in countenance, so raising the suggestion put in the sacred Gospel: 'Physician heal thyself.' Further let the doctor be clothed neatly rather than sumptuously. At the first sight of his patient he will immediately take in his appearance and constitution, age and vitality. All necessary information he will gather in an urbane and affable fashion. He will listen with patience. . . . He will neither exchange views nor discuss with other physicians in the presence of patients, or of lay-people, who know not which side to take. To do so, easily raises a 'certain despair' in them and a hatred against knowledge, 'which comes to be regarded as a matter of uncertainty.' "

In the case of the historian, wars and battles are to be regarded as 'cases of theft.' The historian should study peaceful affairs, trace the glory and wisdom of virtuous acts and note the disgrace of evil-doers. The wisdom of great statesmen, and those who have excelled in 'good arts,' philosophers, saints of the faith, and all that has been said in practical affairs should be studied with the full force of weighty intellect and judgment. For "it is unworthy to hand over to our memories historical actions due to our passions, and not also to study what took place as the outcome of the rational judgment." History discloses the essential nature of human beings, and discloses the manifestations of the affections and judgment of the human mind, in short, the subject has a distinctly psychological basis.

Again, the politician and economist have to study the dispositions and minds of the people. Herein is the predominant value of experience. "Sometimes old men converse with one another in an experienced way and allow youth to listen." This is a privileged method of study to youth, if they kept free from the company of cavillers and obstinate dialecticians. "For a man to be more anxious about achieving a dialectic victory than of discovering truth leads to the ruin of practical wisdom, as indeed Cicero has said." Vives points out the mental characteristics desirable in the administrator, and for the study of political philosophy recommends not only Plato and Aristotle and other classical writers, but also the 'Utopia' of Sir Thomas More and Erasmus's 'Christiani Principis Institutio.' Equally clearly the lawyer must be a psychologist. He must understand "the common nature of mankind, the views and customs of many kinds of people, especially of his own country. This is brought about by wide experience in seeing, hearing, observing things; through reading of the deeds of ancestors and varieties of changes which have befallen the state. Such men need *alert minds and keen judgments, so as to observe and to estimate circumstances, one by one.*"

In all these instances Vives's introduction of psychological observation bears a modern aspect, and affords illustration of the attraction which he felt towards the empirical standpoint and self-exercised thought on the environment rather than the older type of abstract, metaphysical explanation or discussion of the more ultimate foundation of psychological phenomena. The most marked characteristic of the early Renascence writers is the backward-looking concentration on the golden age of Roman and Greek culture and knowledge. In a notable passage[1] however we find Vives exclaiming: "The student should not be ashamed to enter into shops and factories, and to ask questions from craftsmen, and to get to know about the details of their work. Formerly, learned men disdained to inquire into those things which it is of such great import to life to know and remember. This ignorance grew in succeeding centuries up to the present . . . so that we know far more of the age of Cicero or of Pliny than of that of our grandfathers." His keen interest in the experiential side of psychology, therefore, infused itself into his whole outlook, in wishing to get an intellectual grip of the problems of the human mind, and its manifestations, in its relations to its own growth and development, and also in its actions and reactions, in connection with its environment. No doubt he believed that the glorious achievements and experiences of the past threw light on those questions, perhaps to a degree which has been lost in modern times, but Vives's psychological *attitude* towards life, in its present environment is clearly modern, rather than ancient or mediæval.

In England, Vives's name has fallen into undeserved oblivion. For the Spaniard of Valencia made his home in this country for portions of the year from 1523 to 1528, the year in which he had to withdraw from England, on account of his known adhesion to the cause of Queen Catharine of Aragon, who had such a belief in his abilities that she desired him to act as her advocate in the court, so adroitly constituted by Henry VIII to try her case. During his visits in England, Vives lectured on rhetoric at Oxford, where

[1] 'De Tradendis Disciplinis,' Book IV., Chapter 6. 'Opera,' VI., p. 374.

he was associated with Corpus Christi College. He was one of the friends of Cardinal Wolsey, and of Sir Thomas More. Yet, curiously, as we have seen, it is to a representative of Scotland, Sir William Hamilton, that Vives particularly owes his acknowledgment, in the last century, though another Scot, Dugald Stewart, perhaps even more fittingly brings out the modern aspect of Vives, in the striking passage:

"Of all the writers of the sixteenth century, Ludovicus Vives seems to have had the liveliest and the most assured foresight of the new career on which the human mind was about to enter. The following passage from one of his works[1] would have done no discredit to Francis Bacon's 'Novum Organum': 'The similitude which many have fancied between the superiority of the moderns to the ancients, and the elevation of a dwarf on the back of a giant is altogether false and puerile. Neither were *they* giants, nor are we dwarfs, but all of us men of the same standard,—and *we* the taller of the two, by adding their height to our own: Provided always, that we do not yield to them in study, attention, vigilance and love of truth; for, if these qualities be wanting, so far from mounting on the giants' shoulders, we throw away the advantages of our own just stature, by remaining prostrate on the ground.'"

[1] 'De Causis Corruptarum Artium,' Bk. I., Chap. 5. 'Opera,' VI., p. 39.

AN INVESTIGATION OF THE LAW OF EYE-MOVEMENTS

BY MILDRED WEST LORING[1]

The first investigation of eye-movements was made in 1826 by Johannes Müller (1). He stated that the eyes in their movements do not rotate about their long, *i. e.*, sagittal axes. He said: "I have convinced myself, while observing various points on the white of the moving eye, which were marked beforehand with ink, that the eye, through action of the oblique muscles, does not rotate about its long axis." In 1838, however, Hueck (2) demonstrated the compensatory rotation of the eyes around the line of sight, by observing that a given horizontal blood-vessel on the conjunctiva remained horizontal even when the head was inclined to the right or left. Burow (3), 1841, reached the same conclusion using his own paralyzed iris for the demonstration. But Ruete (5), in 1846, like Müller, got negative results, using after-images for a criterion. Donders, (7) 1848, showed his work to have been careless, and demonstrated the well-known principle, that the after-image of a vertical strip remains parallel to itself, with vertical and horizontal movements of the eyes, but with oblique movements, becomes itself oblique. He however formulated no law.

It remained for Listing to put the law concisely in the form now known as Listing's Law, although he did not prove it, or publish it himself. It first appeared in this form in an article by Ruete (11) in 1855, in which he says: "The principle of the mechanism of the eye can be expressed according to Listing in the following simple way; 'From the above-mentioned normal position of the eye which may be called the primary, the eye will be moved into any other secondary position by the coöperation of the six muscles, in such a way

[1] The results of this paper were obtained in the psychological laboratory of the University of Washington, 1912–13, under the direction of Dr. H. C. Stevens.

that this displacement can be represented as the result of a rotation about a definite axis different from the above three, which always passes through the center of the eye, and is perpendicular to the primary and secondary position of the optic axis, so that each secondary position of the eye stands in such a relation to the primary, that the rotation projected on the optic axis will equal zero.'"

Meissner (9), in 1854, was the first to use the method of double images, which he observed are not parallel for a given object with near or far fixation. The inclination of the two images he took as representing the torsion of the two eyes about the line of sight. Both Meissner (13), (1860) and Fick (12), (1858), investigated the subject by means of the change of orientation of the blind spot with eye-movements. Fick's results are irregular, and he says that the movement of the eyes is not a simple geometrical one, about a definite axis, as Meissner held, but that it is a physiological change of position, whose beginning and end only, we know. Meissner found his results to be similar to those of Listing.

The next important work on eye-movements was done by Wundt (14) in 1862. He used the method of after-images, and presented the theory that the eye rotates to such a degree as to allow it to take the desired position of the line of sight with the least muscular effort. He found a slight torsion inward for movements of the eyes vertically above, and torsion outwards for movements vertically below. Helmholtz (15), in 1863, denied the reliability of Wundt's hypothesis, owing to individual differences in muscle strength, and the general unreliability of the muscle sense. He preferred to state the facts in the form of Donder's law, which says: When the position of the line of sight in relation to the head is given, we have a definite and unchangeable amount of rotation.

Volkmann (16), in 1864, attacked the problem somewhat after the fashion of Meissner, using two rotating discs, one for each eye, on each of which was marked a diameter. These were rotated till judged parallel, for any given convergence, and the real torsion thus determined. The torsion as expressed by its effect on the two diameters placed vertical, was as follows:

Primary position	2°.21
30° above to the right	2°.74
30° above to the left	2°.92
30° below to the left	1°.31
30° below to the right	1°.40

This small amount of variation accounts, he says, for the difficulty of using after-images.

Helmholtz (17), 1866, using after-images, got the following results:

A. Turning the eyes to the right above or left below

1. After-image of horizontal line is turned to the left.
2. After-image of vertical line is turned to the right.

B. Turning the eyes to the left above or right below

1. After-image of horizontal line is turned to the right.
2. After-image of vertical line is turned to the left.

In 1868, Hering (18) published his results on binocular vision. In the main he agrees with Helmholtz. He worked with (*a*) after-images, using parallel lines of sight, observing the customary phenomena of Listing's figure, (*b*) double images, using convergent lines of sight. His general conclusion was that Listing's Law holds for parallel lines of sight, but not for convergence lines of sight. According to him, then, Donder's law is invalid, inasmuch as it maintains a constant torsional value for a fixed position of the line of regard in relation to the head.

Le Conte (20), in 1881, published a severe criticism of Helmholtz's version of Listing's Law, and framed it in terms exactly opposite to those of Helmholtz. The whole occasion of the difference of conclusion rests, as Le Conte shows, on the fact that he has used the normal spherical surface for projection of the after-images, while Helmholtz stated the law for projection on a plane surface. Le Conte denies all real rotation about the sagittal axis. It is only apparent, he says, and attributes it to a rotation about some axis perpendicular to the sagittal axis, which, because we observe all the movements of a spherical surface like the eyeball from one point of view.

we interpret as rotations about the line of sight. So Le Conte concluded there was no torsion with parallel lines of sight; but for convergence he finds not only an apparent but a real torsion about the line of sight.

One of the most recent studies on eye-movements has been made by Bernice Barnes (23) in 1905, at the University of Michigan. Her apparatus which she calls a torsio-meter, consisted of 'an iron arc of 180°, one meter in diameter, mounted on a standard, so that its center may be directly in front of the eye of the subject, who is seated before it.' The arc could be swung into horizontal, vertical and oblique positions. It was fitted with a telescope which was directed at the observer's eye. The latter seated himself in front of the arc, with head firm; a thread stretched between the extremities of the arc served to determine the center of the arc, at which point the observer kept the pupil of his eye. The cross-hair in the telescope was set in coincidence with a line on the iris, which reading was taken as zero. Then the telescope was changed in position, and the eyes were again directed at the telescope. The cross-hair in the telescope was set again on the same line on the iris. In this way any real torsion of the eye about its sagittal axis was determined. Miss Barnes found real torsion for every position different from the primary position, even with parallel lines of sight, a conclusion very different from Listing and his successors'. Her chief conclusions are these:

1. There is a contradiction between Listing's and Donder's Laws for torsion in eye-movement.

2. Experiments by the after-image method seem to confirm Listing's Law. But there are two sources of error—inaccuracies in measurement, and false torsion, which it is difficult to make allowances for.

3. More accurate direct measurements show that there is always torsion with rotation, and the amount of torsion is proportional to the amount of rotation.

4. Donder's law holds.

Miss Barnes's work is interesting for the reason that she found, even with parallel lines of sight, an invariable real torsion with movements away from the primary position in any direction whatever. Yet a closer analysis of her results shows certain ambiguities.

1. There is nothing in her article to show which eye of the observer was used in the experiment, or whether both were used indifferently.[1] This is an important point, for it is possible that the two eyes undergo torsion symmetrically, but in opposite senses, when the lines of sight are either parallel or converged. An adequate interpretation of her results is impossible without a knowledge of this point.

2. Her results show that there are some regions, for instance between 0° and 30° to the right above, where the sense of the torsion changes, for at 0° it is to the right and at 30° it is to the left. This must indicate one of two things; (*a*) that somewhere between 0° and 30° there is no torsion at all, this position being at the point where the direction of the line of sight changes from right to left. This point, mathematically speaking, could be called the point of inflection. (*b*) Otherwise the phenomenon must indicate a region in the field of vision such that, if one directs the line of sight there, the sense of the torsion depends on chance, that is, it is as likely to be to the right as to the left and cannot be predicted. Since Miss Barnes denies any direction in which there is no torsion, the first alternative is for her out of the question. She would of necessity have to accept the second. But this, however, violates Donder's Law, which Miss Barnes upholds, inasmuch as the law mentioned demands a constant torsional value for every given position of the line of regard.

The only conclusion that can be drawn from Miss Barnes's work is that further investigation is necessary in the above-mentioned regions of the fields of vision, using each eye, and comparing the results for the two eyes.

Wichodzew (24), 1912, has recently contributed to the subject. He has used the size of the field of vision (both monocular and binocular) to determine the influence on eye-movements of the inclinations of the head to the shoulder. His main conclusions are:

[1] I have since learned that only one eye of the subject was used, but which, is not recalled by the author.

1. There is a compensatory torsion (Raddrehung) of the eyes about the sagittal axis, which causes a change in the mutual relation of the fixation points of the eye muscles and so hinders the eye movements.

2. The capacity of the eyes for symmetrical torsion about a sagittal axis increases with inclination of the head to the right or left shoulder. This increase might be explained as a stimulation by the compensatory torsion to the innervation of the muscles, which turn the eye about the sagittal axis.

The work of Wichodzew, like that of his predecessors, still leaves the question of the sense of the torsion for each individual eye still undecided. Besides there is no reason for accepting a priori that the movements of the eyes are the same when the fixation point is kept constant and the head rotated, as when the head is kept constant and the fixation shifted.

Repetition of Previous Experiments by Hering and Helmholtz

Hering.—When two parallel strips, at interocular distance, are looked at with the eyes in the primary position, the two strips are seen as three, all parallel. But as soon as the fixation point approaches nearer than infinity, the three break into four, the inner one making two images not parallel but converging at the top. This is due to torsion of the eyeball, which increases as the fixation point is brought nearer the observer. Likewise, when the eyes are shifted horizontally to right or left, the convergence of the two middle strips increases, the greater the shift of the eyes. The purpose of this experiment was to measure the amount of torsion and to measure individual differences in the amount of torsion.

Essentially the same apparatus and procedure was used as that described by Hering (18). Likewise the results obtained agree with Hering.

1. The amount of torsion of the eyes, as measured by the convergence of double images of two straight lines, increases directly as the amount of convergence of the eyes increases, from an apparent minimum of zero to the maximum with maximum convergence.

2. The torsion of the eye increases as the point of fixation is directed to a greater distance horizontally either left or right of the primary position.

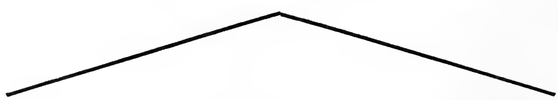
3. Keeping the fixation point constant, a bending of the head forward increases the amount of torsion; raising the head decreases the amount of torsion.

Helmholtz.—(*a*) It is an established fact that we do not see all actual straight lines as straight. In fact, if we look at a line which is really straight, thus ———————— with parallel lines of sight and a card held in front of the nose in the median plane of the body so that each eye sees but half the line, it will not seem straight but broken, the two arms converging thus (allowing for much exaggeration):

If now by some device we compensate for this apparent brokenness, the line must actually be made thus:

Now if the eyes are converged to the middle point of the line, where the card meets the line, the line which had been made straight apparently, seems broken again thus:

And to compensate for this additional apparent slant of the arms of the line, the two arms of the actual line must be bent toward one another downward, still more, thus:



The apparent convergence of the two arms of the line therefore increases as we pass from an infinite fixation point to one very near the eyes. This is due to actual torsion of the eyeball about its sagittal axis, the right eye turning to the left and the left eye turning to the right, from the point of view of the observer.

The apparatus, based on Helmholtz's idea (17), consisted of a strip of cardboard 10 × 1.5 cm., fastened horizontally at one end by a screw to a vertical screen of cardboard. Along the length of the strip, which was at the level of the eyes, was drawn a black line which was extended through the point of rotation to an equal horizontal distance on the screen. This gave a horizontal line whose apparent brokenness when fixated under certain conditions, with a vertical screen perpendicular to the face in the median plane, could be compensated for by rotating the strip, and so give a measure of the rotation of the eyes.

The results indicate that with any given fixation point, there is a torsion of the eyes, the left eye to the right, and the right eye to the left, from the point of view of the observer himself. This result agrees with that obtained in Hering's double image experiment above, and with the results of our original experiment below.

Helmholtz.—(*b*) It has been shown that if the head is held in the primary position, and a colored cross, with vertical and horizontal arms (Listing's figure), be located on a vertical screen with the intersection point at the middle point of the line joining the points of intersection of the lines of sight of the eyes with the screen, the shifting of the eyes to any oblique position when an after-image has been developed with head kept fixed, results in the following distortions of the arms of the cross (17):

1. Turning the eyes to the right above or left below
   After-image of horizontal arm is turned to the left.
   After-image of vertical arm is turned to the right.
2. Turning the eyes to left above or right below
   After-image of horizontal arm is turned to the right.
   After-image of vertical arm is turned to the left.

According to Listing and corroborated by Helmholtz, if the eyes are turned straight above or below, or directly to right or left, the after-images of horizontal and vertical arms remain horizontal and vertical.

A much modified form of Helmholtz's apparatus was employed. It consisted essentially of a black strip rotating at its middle point against a white screen. The after-image of the black strip could be directed to any angle of any quadrant of the field of vision at a fixed distance from the point of rotation. The torsion of the eye as determined by a torsion of the after-image was measured by making the strip parallel to the after-image, and reading the angle of torsion directly.

The results show that there is a torsion for all angles of oblique fixation, for the right eye to the left and the left eye to the right, from the point of view of the observer. This is contrary to Listing, Helmholtz and Sanford in that they deny torsion for the horizontal and vertical, and Sanford also for 45°. The results show too that the method of after-images is capable of showing the vertical and horizontal torsion, which Miss Barnes denied.

Experiment

Problem.—It was the purpose of this investigation to see whether or not the long-debated question of torsion of the eyeball around the sagittal axis could be settled by means of a very carefully made apparatus which would detect any rotation ordinarily unobserved by other methods of investigation. To that end, recourse was had to the principle of the method of Johannes Müller, namely the observation of a fixed line upon the eye when that eye is placed in different positions in the field of regard. The fixed line of the eye was one of the radial striæ of the iris. A description of the apparatus, by which the observation was made, follows.

Apparatus.—The apparatus consists of a large, vertical, semicirculcar iron arc, one meter in radius, attached solidly to a heavy brick chimney in the dark room of the psychological laboratory. By means of a galvanometer, it was determined that the vibration of the chimney is so slight as not to overcome the inertia of the apparatus. The iron arc, which was turned in a lathe to insure smooth surfaces, is 5 cm. wide and 2.5 cm. thick, and is graduated into arcs of 10°. The whole arc rotates in a horizontal direction about its extremities, but its points of rotation are shifted 7.5 cm.

to the right by means of iron plates, to accommodate for the eccentric position of the telescope which is attached to the arc. In this way, the vertical cross-hair of the telescope coincides with the axis of the arc. The upper center of rotation has on it a horizontal, circular protractor, 7.5 cm. in radius, graduated in degrees, to measure the amount of rotation of the arc. Attached to the right side of the arc is a transit telescope, with a magnifying power of 34 times, at the distance from the telescope to the middle point of the arc. The magnifying power was determined as follows:

At a distance of one meter from the telescope a meter stick was placed in a vertical position. A rider, made of a narrow band of paper, 1.5 cm. wide, was freely movable along the length of the meter stick. The experimenter then looked at the rider through the telescope with his left eye, and directly at it with his right eye, and adjusted the rider, so that the upper edge of it, magnified in the telescope, coincided with the upper edge of it, unmagnified, as seen by the right eye. With the images of the left and right eyes thus superimposed, the experimenter observed the length of the magnified rider on the unmagnified scale of the meter stick. The left eye image of the magnified meter stick and right eye image of the rider, were inhibited. The quotient then of the magnified length of the rider and its unmagnified length represents the magnifying power of the telescope.

Magnified length of the rider................	51.5 cm.
Unmagnified length of rider................	1.5 cm.
Magnifying power................	51.5/1.5 = 34.3 times

This method was checked by the method of focal lengths. The magnifying power $= F/f$, where F is the focal length of the objective and f the focal length of the eye-piece. Measurement of the focal lengths of the telescope showed:

Focal length objective, F..................	26.5 cm.
Focal length eye-piece, f....................	.8 cm.
Magnifying power................	26.5/.8 = 33.1 times

The telescope of the apparatus is detachable and can be slid along the arc to any desired position and fastened there. It is directed inward, perpendicular to the arc.

Exactly at the middle point, between the two points of rotation of the arc, suspended by means of an iron support, extending out from the brick chimney, is a small brass ring, 3 cm. in diameter, permanent in position, at which the observer places his eye. Also from this same support is attached a head rest. It is made of an iron band 27 cm. in diameter surrounding the observer's head at the temples, and supported by four vertical iron strips adjusted in length vertically, which at a distance of approximately 12 cm. above their attachment to the iron band, bend inward to a common point, which is the center of support. Screws, with flat discs at their extremities, point inward through the iron band, which are adjustable to accommodate differences in size of head of observers. The chin is supported by a U-shaped piece of iron, swinging from the iron band so that it hangs about in the plane of the brass ring. It too is adjustable vertically by means of screws.

The head rest can be set for either right or left eye position. The observer places his head in the iron band, with the eye exactly in the center of the ring; the screws are tightened to fit the head, the band is lowered or raised to accommodate his height, the chin rest is adjusted likewise and then the head is so firmly fixed that there is no error from head movements.

The telescope is attached at about its middle point to the right side of the arc, and is adjustable in three planes, vertically, horizontally, and circularly about its long axis. It is equipped with vertical and horizontal cross hairs to obtain accurate settings. A vernier, which reads to $0'.5$ is attached to read the circular rotation. It is to be noticed that since it is necessary practically to have the telescope fastened to one side of the arc, the points of rotation of the arc have been shifted an equal amount in the same direction. By dropping a plumb line from the upper point of rotation, the center of rotation of the arc was obtained, at which point the center of the brass ring was placed and marked by two perpendicularly intersecting threads, the vertical one being determined by the plumb line. The telescope was then set in a horizontal position at the zero point on the arc, which is half way between its extremities, and the intersection of the cross-hairs of the telescope coincides with the intersection of the threads on the brass ring. In this way the points of rotation of the arc, the center of the brass ring, and the intersection point of the cross-hairs of the telescope all lie in a plane, parallel to the plane of the arc. With a new position of the telescope on the arc, or a rotation of the arc, the horizontal and vertical cross-hairs of the telescope still coincided with the horizontal and cross-threads passing through the center of the brass ring. This statement must be modified. Since the apparatus is to measure rotation in a vertical plane, perpendicular to the plane of the arc, the aim was so to construct it that, with the arc and telescope in a given position, a change in position of the arc, or of the telescope on the arc, the vertical and horizontal cross-hairs would still be horizontal and vertical. By making the axis of rotation of the arc absolutely vertical, placing the center of the brass ring precisely at the center of the diameter of this axis, and since the arc was constructed as true as possible, it was found that the vertical and horizontal cross-hairs remained so with various positions of the apparatus. There was but one variation in the absolute trueness of the instrument. When, for instance, the arc and the telescope were both placed in the zero position and the vertical cross-hairs set coincident with the vertical cross-thread of the brass ring, and then the arc was rotated to right, the thread and cross-hair, though both remained vertical, were no longer coincident but were laterally displaced. Since they were parallel, by adjusting the screw controlling the horizontal shift of the telescope, they could be brought into concidence again. In this operation, the cross-hair was not rotated from its vertical position, so that whenever a rotation of the cross-hair was necessary in the actual experimentation, to cause coincidence, it indicated, not imperfection in the apparatus, but a rotation of the line which the cross-hair of the telescope was being set upon. This was the intended function of the apparatus. This parallel shift was due to the size of the apparatus and the high magnifying power of the telescope, but did not amount at a maximum to more than two or three turns of the small screw, controlling the horizontal adjustment. The same displacement occurred in a vertical direction but to a much less degree. Two electric lights served for illumination. One light was attached to the arc by a jointed arm and could be slid along the length of the arc. It was used to illuminate the vernier; the other was suspended from a horizontal arm pivoted to the top of the head rest, and illuminated the eye of the observer. A frosted globe was used in this lamp, to lessen the fatigue of the eye, and to give a more diffuse illumination.

Procedure.—When the observer was seated with his head in the head rest as indicated above, the arc and telescope were placed in the primary or zero position; that is, the arc was swung to a zero position on the dial, and the telescope set horizontally on the arc at its middle or zero point. The observer sat with his left eye at the brass ring and the plane of his face perpendicular to the plane of the arc, so that looking straight ahead of him, his eye was fixed on the telescope. A small spot of white paint was placed at the middle point of the objective to facilitate fixation. Next, the operator examined the eye through the telescope, and selected on the iris a line or set of two points upon which to set a cross-hair of the telescope. The same line was not used through the whole experiment, inasmuch as it might have an unfavorable location for observation with certain oblique positions of the eye. This initial reading on the vernier was taken as the zero reading. Whenever a new line was taken on the iris, and with each sitting of the observer, a new zero was taken.

With the telescope at zero position still, the arc was swung 15° to the right. The head of the observer was not moved; in fact, its position was kept constant throughout a sitting. The eye, however, was turned horizontally in its orbit to the new position of the telescope and fixed again on the white spot. The operator reëxamined the eye, set the cross-hair on the experimental line of the iris, and read the vernier. The difference between the first and second readings represented the angle of torsion of the eyeball. Similarly angles of torsion were obtained for 15° to the left and 30° to both right and left. The arc was then returned to the zero position of the dial, and the telescope was then moved along the arc to a position 15° above the zero, or middle point of the arc. The reading was taken here, and again the arc was swung 15° to the left and to the right and also 30° to the left and to the right, and readings taken. The difference of the readings in any position from the reading of the zero, represented the torsion for that position. Observations were made upon both right and left eyes. As summary of the positions of the eye in which readings were taken is as follows:

Telescope.	Arc.
I. *30° above*	*30° left—15° left—zero—15° right—30° right*
II. *15° above*	*30° left—15° left—zero—15° right—30° right*
III. *Primary position*	*30° left—15° left—zero—15° right—30° right*
IV. *15° below*	*30° left—15° left—zero—15° right—30° right*
V. *30° below*	*30° left—15° left—zero—15° right—30° right*

In spite of the accuracy of the instrument, certain unavoidable errors arose in taking data. These were:

1. Certain reflex pupillary expansions and contractions even with a constant intensity of illumination. This changed the size of the iris and therefore the direction of the line set upon, in spite of every care to have the line a true radius of the iris.

2. Nystagmoid movements of the eyeball, entirely involuntary, due to reflex contractions of the eye-muscles, as a result of continued fixation.

3. A marked change in the pattern of the iris, owing to torsion. Sometimes it was difficult, in a new position of the telescope, to recognize the line to be set upon, owing to certain twistings in the pattern, which, while not very extended, yet interfered markedly with an accurate setting of the cross-hair.

Results.—1. Each number in Table I. (*a*) and (*b*) represents the average of five observations in that position.

2. In Table I., the column headed "Position," the sign + before a parallel indicates a position of the telescope above the zero parallel on the arc, the sign −, a position below the zero parallel on the arc; the sign + before an angle in the subgroup (+30°, +15°, 0, −15°, −30°) underneath a parallel, indicates a position of the arc to the right of the zero on the

Table I

Position	(a) Left Eye						(b) Right Eye					
	Torsion			Mean Variation			Torsion			Mean Variation		
	Observer (1)	Observer (2)	Observer (3)	Observer (1)	Observer (2)	Observer (3)	Observer (1)	Observer (2)	Observer (3)	Observer (1)	Observer (2)	Observer (3)
+30° parallel												
+30°	− 0° 0′.8	+12° 4′.0	+ 5° 44′.4	0° 10′.4	0° 12′.9	0° 17′.6	+4° 43′.6	+ 6° 31′.8	−2° 21′.4	0° 16′.2	0° 12′.8	0° 11′.1
+15°	− 0° 5′.8	+ 8° 16′.4	+ 7° 7′.0	0° 42′.8	0° 13′.4	0° 14′.1	+5° 9′.4	+ 1° 40′.8	+4° 16′.2	0° 18′.8	0° 12′.9	0° 16′.7
0°	− 4° 0′.0	+ 2° 42′.6	− 1° 45′.6	0° 53′.2	0° 13′.1	0° 10′.8	+2° 41′.4	− 2° 20′.6	+6° 4′.6	0° 14′.0	0° 20′.5	0° 25′.8
−15°	−12° 3′.6	+ 2° 3′.6	− 9° 14′.8	0° 17′.3	0° 14′.5	0° 19′.8	−3° 36′.4	− 6° 46′.4	−2° 56′.2	0° 20′.6	0° 24′.0	0° 17′.6
−30°	−17° 18′.2	− 6° 1′.6	−15° 4′.8	0° 20′.6	0° 10′.8	0° 22′.9	−7° 27′.8	− 7° 18′.0	−4° 1′.8	0° 20′.7	0° 8′.6	0° 6′.6
+15° parallel												
+30°	+ 2° 50′.0	− 2° 10′.2	− 2° 16′.4	0° 17′.6	0° 14′.6	0° 10′.2	+6° 2′.8	+ 1° 42′.2	+2° 45′.6	0° 13′.7	0° 13′.6	0° 12′.6
+15°	+ 3° 27′.9	− 4° 34′.6	− 1° 49′.4	0° 42′.7	0° 16′.1	0° 3′.1	+4° 14′.0	− 0° 21′.0	+3° 24′.4	0° 10′.3	0° 14′.0	0° 19′.1
0°	+ 2° 29′.6	− 3° 42′.2	− 0° 41′.2	0° 18′.1	0° 17′.0	0° 22′.3	+2° 55′.0	− 6° 33′.2	+1° 24′.0	0° 12′.1	0° 11′.4	0° 30′.4
−15°	− 3° 44′.8	− 5° 48′.6	− 4° 5′.4	0° 31′.7	0° 19′.3	0° 16′.0	+0° 34′.0	− 3° 15′.6	−1° 5′.8	0° 15′.5	0° 22′.2	0° 31′.7
−30°	− 5° 41′.4	−10° 56′.6	− 8° 3′.8	0° 55′.6	0° 15′.7	0° 6′.5	+0° 58′.2	− 2° 24′.0	−1° 48′.6	0° 21′.9	0° 11′.2	0° 68′.4
0° parallel												
+30°	− 0° 58′.8	− 4° 10′.6	− 2° 19′.2	0° 16′.5	0° 12′.4	0° 17′.5	−2° 58′.2	− 3° 12′.8	+0° 52′.4	0° 20′.0	0° 11′.7	0° 15′.0
+15°	+ 0° 20′.2	− 2° 18′.6	− 0° 41′.4	0° 17′.7	0° 20′.0	0° 9′.7	−1° 8′.8	− 1° 12′.6	−3° 4′.4	0° 14′.5	0° 22′.3	0° 13′.6
0°	0	0	0	0	0	0	0	0	0	0	0	0
−15°	− 5° 4′.6	− 2° 16′.0	− 1° 11′.8	0° 26′.6	0° 13°.8	0° 9′.2	+0° 31′.0	− 2° 16′.0	+0° 10′.0	0° 15′.5	0° 14′.4	0° 27′.5
−30°	− 3° 47′.4	− 3° 16′.2	− 2° 7′.4	0° 23′.4	0° 8′.2	0° 15′.5	+0° 59′.0	− 5° 21′.0	+0° 9′.2	0° 15′.2	0° 7′.6	0° 10′.9
−15° parallel												
+30°	− 8° 37′.4	−15° 54′.4	− 3° 42′.6	0° 12′.9	0° 21′.4	0° 14′.4	−6° 36′.6	−11° 19′.3	−3° 8′.0	0° 17′.2	0° 16′.4	0° 7′.6
+15°	− 2° 32′.8	−10° 18′.7	− 2° 28′.4	0° 18′.9	c° 20′.7	0° 17′.4	−0° 2′.6	− 8° 55′.7	+4° 21′.6	0° 6′.8	0° 12′.3	0° 17′.0
0°	− 1° 53′.0	− 3° 14′.4	− 2° 25′.0	0° 23′.9	0° 12′.9	0° 20′.0	+0° 41′.0	− 5° 5′.5	−0° 12′.0	0° 17′.6	0° 9′.8	0° 10′.0
−15°	− 1° 46′.0	− 1° 53′.2	− 4° 10′.2	0° 27′.7	0° 18′.4	0° 6′.5	+0° 41′.0	− 4° 57′.2	−1° 17′.2	0° 22′.3	0° 16′.8	0° 29′.7
−30°	+ 0° 26′.0	+ 0° 10′.4	− 4° 29.2′	0° 8′.3	0° 13′.2	0° 21′.1	+1° 16′.4	− 2° 57′.6	−4° 6′.8	0° 26′.4	0° 11′.9	0° 19′.0
−30° parallel												
+30°	−17° 38′.0	−16° 58′.6	+ 9° 32′.6	0° 54′.1	0° 16′.0	0° 46′.9	−7° 19′.4	− 8° 14′.4	+9° 55′.4	0° 25′.4	0° 20′.7	0° 26′.2
+15°	−10° 11′.8	−10° 48′.6	+ 0° 12′.2	0° 37′.8	0° 12′.0	0° 15′.4	−1° 25′.4	− 4° 11′.8	+5° 55′.6	0° 22′.2	0° 22′.4	0° 27′.2
0°	− 6° 15′.6	− 6° 20′.4	− 0° 49′.0	0° 30′.0	0° 14′.6	0° 33′.4	−3° 20′.0	− 4° 21′.0	+2° 5′.0	0° 18′.6	0° 10′.8	0° 16′.1
−15°	− 4° 16′.0	+ 1° 41′.8	− 8° 20′.6	0° 26′.9	0° 21′.1	0° 19′.3	+4° 54′.6	− 1° 42′.8	+2° 20′.4	0° 16′.6	0° 22′.9	0° 18′.8
−30°	− 0° 33′.6	+ 2° 15′.2	− 5° 1′.4	0° 28′.2	0° 25′.9	0° 20′.6	+9° 40′.4	+ 4° 56′.8	+9° 33′.4	0° 29′.1	0° 35′.0	0° 25′.6

dial, the sign $-$ before an angle indicates a position to the left. For instance $\frac{+15^\circ \text{ parallel}}{-30^\circ}$ indicates a position where the telescope has been placed 15° above the zero or middle point of the arc, and the arc rotated 30° to the left of the zero on the dial. These signs are taken from the point of view of the experimenter, who looks through the telescope.

3. In the column headed "Torsion," the sign $+$ indicates torsion of the eye to the right from the point of view of the experimenter; the sign $-$ indicates torsion to the left.

Conclusion.—I. The results are to some extent irregular, both as to the amount of torsion and direction.

(*a*) As the eye travels from the primary position to successively more oblique positions, there seems to be a tendency for the angle of torsion to increase, but some exceptions occur. Likewise a movement of one eye to a given position does not give the same amount of torsion as the movement of the other eye through the same angle. Similarly, the sense of the torsion does not remain constant for one eye in all cases, and the torsions for the two eyes for movements through similar angles is not always opposite or always the same.

(*b*) There is a predominance of torsion to the right of the right eye, and to the left of the left eye, from the standpoint of the experimenter. Out of 24 positions for each eye away from the primary position, for each observer we find the following:

	Torsion of Right Eye		Torsion of Left Eye	
	To the Left	To the Right	To the Left	To the Right
1	9 cases	15 cases	19 cases	5 cases
2	20 cases	4 cases	17 cases	7 cases
3	10 cases	14 cases	20 cases	4 cases

It will be seen that for the left eye, the great predominance of torsion is to the left for all three observers, and that for the right eye, the predominance is to the right for two of the three observers.

II. The most definite conclusions consist then mainly in the two following observations:

(*a*) A tendency for the torsion to increase as the eye passes from the primary position to successively more oblique positions, including horizontal, vertical and 45° meridian.

(*b*) A predominance of torsion of the left eye to the left, and of the right eye to the right, from the point of view of the experimenter.

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VARIABILITY IN PERFORMANCE DURING BRIEF PERIODS OF WORK

BY A. T. POFFENBERGER, JR., and GLADYS G. TALLMAN

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Numerous researches have been carried on to show the effects of working for long periods of time or with extremely difficult tasks. The effects, known as fatigue, may show themselves in a decrease in the quantity or quality of the product. Analogous changes in the character of the performance, which have scarcely warranted the name of fatigue as the term is ordinarily used, appear in extremely brief periods of mental work. For instance, Professor Woodworth in a recent paper,[1] showed some changes in speed of performance which take place during the naming of only ten simple colors. The present report contains a brief series of records showing a variation in performance in tasks, none of which lasted as long as one minute.

In some of the previous work on fatigue one finds references to the effects of brief work periods, although in few of them did the matter receive much attention. Voss[2] made detailed studies of an hour's work in addition, taking time for each separate problem. He concludes that practice tends to increase the number of rapid additions and fatigue the number of slow additions. The procedure followed made it impossible to separate the practice and fatigue effects for the short periods measured. Hylan and Kraepelin[3] measured the variation in adding one-place numbers in five-minute periods. They concluded that mental work lasting for only

[1] An unpublished paper read before the New York Branch of the American Psychological Association at Princeton, N. J., Feb. 23, 1914, entitled 'The Work Curve for Short Periods of Intense Application.'

[2] George von Voss, 'Ueber die Schwankungen der geistigen Arbeitsleitung,' *Psychol. Arbeit.*, Vol. 2, pp. 399–449.

[3] Hylan, J. P., and Kraepelin, E., 'Ueber die Wirkung kurzer Arbeitszeiten,' *Psychol. Arbeit.*, Vol. 4, pp. 454–495.

five minutes produced an appreciable amount of practice and fatigue. The ratio of the two factors differed for different individuals. In the work of Arai[1] on the multiplication of a series of two four-place numbers the time for each problem was measured. In the author's conclusions (p. 93) we note that "the difference between the time taken for one example and that taken for another is greater in the second half than in the first half of the curve. This fact together with the evidence of introspection of the subject suggests that fatigue not only causes decrease of efficiency, but also loss of the subject's control over herself. For this reason the subject tends to occasionally relax her original standard of effort." In this experiment the subject had passed the stage where practice was an important factor.

In the experiment to be reported two time records were taken for each task, one at the completion of the first half and the other at the end of the task. The time was taken with a stop watch in units of one fifth of a second. From these two records one may compare the performance during the first and second halves of the work. Speed was the only variable factor, for the experimenter announced all errors and these were corrected by the subject. It is to be noted further that the subjects were trained in all of the tests, and that practice effect was thus practically eliminated. If this were not the case it is quite possible that the improvement by practice within a single test might have clouded the results. An intensive study was made upon two subjects, each test being repeated from 60 to 70 times after the preliminary practice.

Four tests were used, two of them being taken from the Woodworth and Wells[2] monograph on association tests. These were the color naming test and the number checking test, called the cancellation test in this report. Only half of the regular number checking blank was used for one test, so that the halves measured were really fourths of the whole

[1] Arai, Tsuru, 'Mental Fatigue,' Teachers College, Columbia University Contributions to Education, No. 54, 1912.

[2] Woodworth, R. S. and Wells, F. L., 'Association Tests,' Psychological Monograph, 1911, No. 57.

blank. The numbers 3 and 5 were used alternately for cancellation. The third test was the opposites test and consisted of 50 words, the opposites of which were to be named. They were about equal in difficulty to the moderately difficult series of Woodworth and Wells. The fourth test was the addition test and consisted in adding 17 to each of 50 two-place numbers, ranging from 20 to 80 with all of the combinations containing zero being omitted. In every case the test material was divided into two parts by a line, so that the subject could see the limits of the first and second half. The subject's work, however, was continuous from the beginning to the end of the test.

The tests were repeated approximately five times each day, with an interval of several hours between any two, thus avoiding cumulative fatigue. All of these tests involve the process of association and the connection between situation and response was in each case partially established in the preliminary tests. It is to be remembered that the test material was always the same, the only difference being the order in which the stimuli were presented. There were four possible changes of order in the color naming test, by rotating the color blank 90 degrees at each trial; four in the number checking test, by using different halves of the test and either one of the numbers 3 or 5 for cancellation; ten in the opposites and addition tests by means of ten cards containing the same numbers in a random position on each.

The data of the experiment are presented in the accompanying table. The third column of the table shows the number of cases from which the calculations are made for each test. The fourth column gives the average speed for the first half of the test in terms of seconds. The fifth column gives the gross differences between the first and second halves. In every case the first half was subtracted from the second half, so that the absence of minus signs indicates greater speed in the first half. The sixth column shows the reliability of these differences in terms of the probable error of the difference. The seventh column shows the difference in terms of per cent. of the time of the first half. The column of gross differences shows that in every test and for both subjects the first half is quicker than the second half, while the following

RELATION BETWEEN SPEED OF PERFORMANCE IN THE FIRST AND SECOND HALF OF THE TESTS.

(In every case the first half is subtracted from the second half, so that absence of minus signs indicates that the first half is faster.)

Test	Subject	No. Cases	1st Half	Gross Diff.	P. E.	Per Cent. Diff.
Opposites..........	*T*	59	16.2	1.9	0.1	11.7
	P	67	12.9	1.7	0.1	13.2
Addition..........	*T*	70	28.5	3.3	0.3	11.6
	P	67	18.4	1.1	0.2	6.0
Color Naming......	*T*	63	20.0	1.9	0.2	9.5
	P	67	17.0	1.0	0.2	5.9
Cancellation.......	*T*	61	23.8	2.2	0.2	9.2
	P	60	20.3	0.7	0.2	3.4

column shows that the differences are reliable. The data when examined in detail and when treated in several other ways, show only one interesting exception. When the tests are grouped according to the time of day they were performed, namely 9.30 A. M., 1.30, 3.30 and 5.30 P. M., it appears that in the case of the cancellation test and at the 9.30 period both subjects were on the average slower in the first half than in the second half of the test. No explanation for this peculiar difference is suggested. The data treated in this fashion show only one other case where the first half is slower and here the negative quantity is less than its probable error.

The last column in the table shows the per cent. of difference in favor of the first half. It should be considered merely as indicating differences between *halves* in the tests, for only two subjects are not sufficient to establish differences of this type among the tests used. If the tests are grouped, one can say roughly that in speed of performance subject *T* was about 10 per cent. less efficient, and subject *P* about 7 per cent. less efficient in the second thirty seconds of the work than in the first thirty seconds.

A series of ten questions was given to each subject at the end of the series of tests to determine if introspection could throw any light upon the results. For instance, the subjects were asked if they noticed any difference in speed, ease of performance, number of errors, etc., in different parts of the tests, and if so whether they could account for such changes. A separate series of questions was given for each of the four tests. Both subjects felt that they made more errors, and hesitated more in the second half than in the first. In both subjects there was consciousness of being slow with a feeling of inability to make the mind work any faster. The speed was probably judged by the number of errors and hesitations that occurred, according to the answers to some of the questions. The mistakes were attributed not so much to lapses of attention as to just getting tired. Neither subject reached the stage, during the course of the experiment, where the task became automatic, *i. e.*, where he no longer felt as though he were putting forth a great deal of energy.

In drawing conclusions from these records it is necessary to keep the specific conditions in mind: (1) The test material was always the same, except for the difference in arrangement. (2) The process involved in the reaction to the situations was mainly the recall of previously formed associations. (Formed or at least strengthened during the preliminary tests.) (3) There was practically no practice effect within any test, on account of the preliminary tests, and the use of the same material throughout. This is a condition not found in mental fatigue tests where the records are made at frequent intervals. (4) The subjects worked at their best speed, with no rest period between the first and second halves of the test. (5) There was no rest interval between the performance of separate items of the tests, such as the relaxation which may take place at the end of each addition or multiplication problem usually employed for fatigue work, especially where the time required for each problem is the unit of measure.

This falling off in performance within such a short period in the case of two subjects in the mental tests described, suggests the importance of devising simple mental tests, which shall approximate the classical ergograph test as performed upon a trained subject, in its simplicity, forced regularity of response (*e. g.*, metronome beat in the ergograph test), and in the complete record obtained. For it is probable that mental fatigue is not so rare as is sometimes supposed, but that the repair process is so rapid compared with muscle repair, that as work is usually done, the loss may be compensated for during brief intervals of relaxation.

THE STANDARDIZATION OF KNOX'S CUBE TEST

BY RUDOLF PINTNER

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This paper deals with the results obtained with Knox's[1] Cube Test, one of the performance tests used by him for the mental classification of immigrants at Ellis Island. The test has been given to 867 normal children and a few adults, and to 463 feeble-minded individuals. An attempt has been made to enlarge the scope of the test and to standardize it a little more adequately. The test appeared to me, after first seeing it applied, to be an excellent one in many ways. Without attempting to enter into a useless discussion as to the actual mental processes involved, we may say in a general way that it depends largely upon imitation, at the same time affording every opportunity for other factors involving intelligence to assert themselves.

The Method of Procedure.—Five blocks are required. The Binet black cubes were used, but any other cubes of about the same size would be satisfactory, provided they are all of the same color. Four of these are placed on the table in front of the subject at a distance of about two inches apart. The examiner holds the fifth cube in his hand. He says to the subject: "Watch carefully, and then do as I do." He then taps the blocks with the fifth cube in a certain definite order and at a certain definite rate (about one tap per second), always beginning with the cube at the child's left or the examiner's right if he is facing the child. He then lays the fifth cube down in front of the child equidistant between the third and fourth cube, but nearer to the child, and says: "Do that."

These oral directions, "Watch carefully and then do as I do," and "Do that," were given to almost all of our subjects.

[1] Knox, Howard A., *The Journal of the American Medical Association,* March 7, 1914, Vol. LXII., pp. 741–747.

I do not believe this is necessary and I am sure exactly the same results would have been obtained by saying nothing. This is borne out by some of the subjects who did not understand English and by deaf children and by some children who seemed too young to understand such verbal directions. In these cases all that was necessary was to make some gesture indicating that the fifth block was to be picked up and the others to be touched.

The first line of the test, as will be noted presently, is so simple that even although the subject does not know while watching the examiner that he will be required to do the same thing, he can easily remember and imitate what has been done. There are of course innumerable combinations in which one can tap four blocks, if one is not limited to touching each cube only once. If we number the blocks the different combinations will be readily understood, and the following diagram should make absolutely clear their position with regard to the subject and the examiner (if he is facing the subject).

Subject

4	3	2	1

Examiner

The following twelve combinations or lines have been used in this experiment. Number 1 always refers to the block on the left-hand side of the child.

A line..................	1 2 3 4
X "	1 2 3 4 3
Y "	1 2 3 4 2
B "	1 3 2 4
C "	1 4 3 2
D "	1 4 2 3
E "	1 3 2 4 3
F "	1 4 3 2 4
G "	1 3 1 2 4
H "	1 4 3 1 2 4
I "	1 3 2 4 1 3
J "	1 4 2 3 4 1

Knox uses only five lines. The advantages of my extension to twelve will, I think, be apparent in the light of the results. The lines are taken up in the order of sequence in which they appear above. It will be noted that this is roughly an order of increasing difficulty. A line is never repeated, not even if the subject begins any line, the *A* line included, at the wrong end. This is an error that we have marked W.E. (wrong end) and will discuss later. The subject is never corrected, but is allowed to do a line exactly as he chooses. No hint must be given the subject as to whether he is doing a line right or wrong. Some children pause and look up at the examiner waiting for a cue as to the next move. The subject is encouraged to do his best and is told that he is doing well, regardless of his actual accomplishment. Each line is marked plus or minus according as the subject does it correctly or incorrectly. If the subject corrects a wrong move, thereby making the whole line correct, he is credited with a plus.

The numbers of the moves must not be shown to the child and the examiner must not count aloud or indicate in any way that he is counting. This precaution is very necessary with intelligent older children and with adults. Nothing in fact is said about counting and the subject is left free to pursue that method if he has the intelligence to think of it. This will of course show in a greater number of lines passed correctly and therefore the subject receives credit for his intelligent adaptation. We asked every subject who did very well, *i. e.*, succeeded in almost all of the lines, how he did it. Some replied at once that they were counting. Others were uncertain. We suggested to the latter that they were counting and if they assented, we asked them and also those who told us they were counting, to count aloud while we repeated one of the longer lines or a new combination of moves. The examiner can then tell whether it is the kind of counting that will help in remembering the moves. Some children who said they remembered the moves by counting were found simply to be saying to themselves, one, two, three, four, etc., every time. This, to be sure, would help them to remember the number of moves but would not help them to remember the position of the blocks touched. The other kind of counting which assigns constant numbers or letters to each block was found to be very rare. We found it only in about twenty cases out of our 867 individuals. This kind of counting did not seem to occur to the majority of adults who were tested by us.

In many respects this method of giving the test differs radically from that of Knox, as far as can be determined from the very brief description of the test in the article cited above. Knox allows a repetition of a line on the part of the examiner, in contrast to my procedure where a line is never repeated. For the first five of his lines he gives three trials 'if necessary,' and for his last and most difficult line involving six moves he allows five trials. If the subject fails he is evidently shown the moves over again to the extent of three or five times if necessary. The drawback of this method seems to lie in the fact that a varying number of repetitions of any line will cause unequal practice effect. For example, the subject that fails twice on the second line and passes on the third trial will have made up to that point four responses to the test, whereas the subject that passes the second line at the first trial will have made only two responses. Each will then start the third line with different degrees of familiarity with the situation, and it is possible that the first subject may have gained an unfair advantage over the second, even although the first subject is not doing as well as the second as shown by his failures. A fair comparison of their performances in the succeeding lines will nevertheless be impossible, however much the one may be superior to the other. I think that any difficulty in this respect that may exist can be adequately overcome by extending the number of lines, as has been done, by following always the same sequence and by rigidly adhering o the rule never to repeat a line.

The Subjects.—The subjects included 867 presumably normal individuals. These were in the main pupils in the ordinary grade schools of about four or five different schools in Columbus, and some from a junior high school. Most of the five-year-olds were kindergarten children and those below that age were examined in day nurseries and settlement houses. About half of the adults tested were university students. Four hundred and sixty-three feeble-minded individuals were also tested. The vast majority of these were inmates of an institution for the feeble-minded, but there are also included in this number several feeble-minded children that were met with in the juvenile court or in school. No systematic attempt was made to exclude all cases of suspected feeble-mindedness from the data for normal children. This would have involved giving lengthy tests to some hundreds of children in the public schools. Any child who was obviously feeble-minded was excluded from the normal group and if his Binet age had been determined he was included among the feeble-minded.

The tests were not all given by the writer himself,[1] but the technic of this test is so simple that uniformity in giving it is very easily attained. I do not think that any possible error from this source would materially affect the results.

The number of normal children tested at each age is given in Table I. below, and the feeble-minded in Table IV. The normal children are grouped according to chronological age and the feeble-minded according to Binet age. It will be seen that the number for each age is not uniform, but a sufficiently large number between the ages of five and sixteen were obtained for each age. The usual difficulties were encountered in getting children below five and above sixteen years of age. The feeble-minded differed chronologically a great deal, some of them were adults and others merely children.

Tabulation of the Data.—The actual recording of the results while giving the tests is perhaps best done by making out some such blank as is shown below, which is a copy of some of the actual data.

[1] The writer wishes here to acknowledge the generous help given him in this work by Mr. Donald G. Paterson, graduate assistant in the department of psychology. He is also glad to acknowledge the assistance rendered by Miss M. Anderson and Miss A. Beekman, advanced students in the same department.

Name	Age	Grade	*A*	*X*	*Y*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
Catherine M...	12	5B	+	—	+	—	+	+	+	—	—	+	—	—
Paul C........	6	1A	+	+	+	—	+	—	—	—	—	—	—	—
Matilda S.	14	7A	+	+	+	+	+	+	+	+	—	—	—	—
Rosie S.	5	Kindg.	W.E.	+	+	—	—	—	—	—	—	—	—	—

During the collection of results covering in all about thirteen hundred cases the following device was employed to keep an oversight of the results as they came to hand and also to afford some clue as to when sufficient results had been collected. As the results came in they were recorded on squared paper and a curve was slowly built up. This was done for each separate line and for certain groups of lines. Fig. 1 shows the results for two lines correct out of the *BCD* lines.

The numbers along the abscissa represent the ages. A mark or dot above the line represents a correct response and a similar mark below the line represents a failure. The figure shows the building up process at the stage of completion. The figures to the left of each column represent the number of cases, the figures to the right are the percentages of correct responses for each age at different stages in the growth of the curve. When each fresh group of results was added, the percentage of correct responses was calculated and noted on the growing curve. This device for recording the results was found to add a much greater interest in the work than could have been attained by waiting until all the data had been collected, and it also shows the worker how his results are developing. If the percentages are calculated every now and then, he can see whether new results that are coming in are adding anything new to his work or are merely confirming the results that have already been obtained. If the percentage fluctuates a great deal, it is a sign that more results are required; if it remains more or less stationary for some time, it may be assumed that additional results are not likely to affect the shape of the curve. From Fig. 1 it will be seen that the percentages for most ages remained more or less constant. Some slight fluctuation is seen at age eight, where the percentage drops from 68 to 65 and then rises steadily to 74.

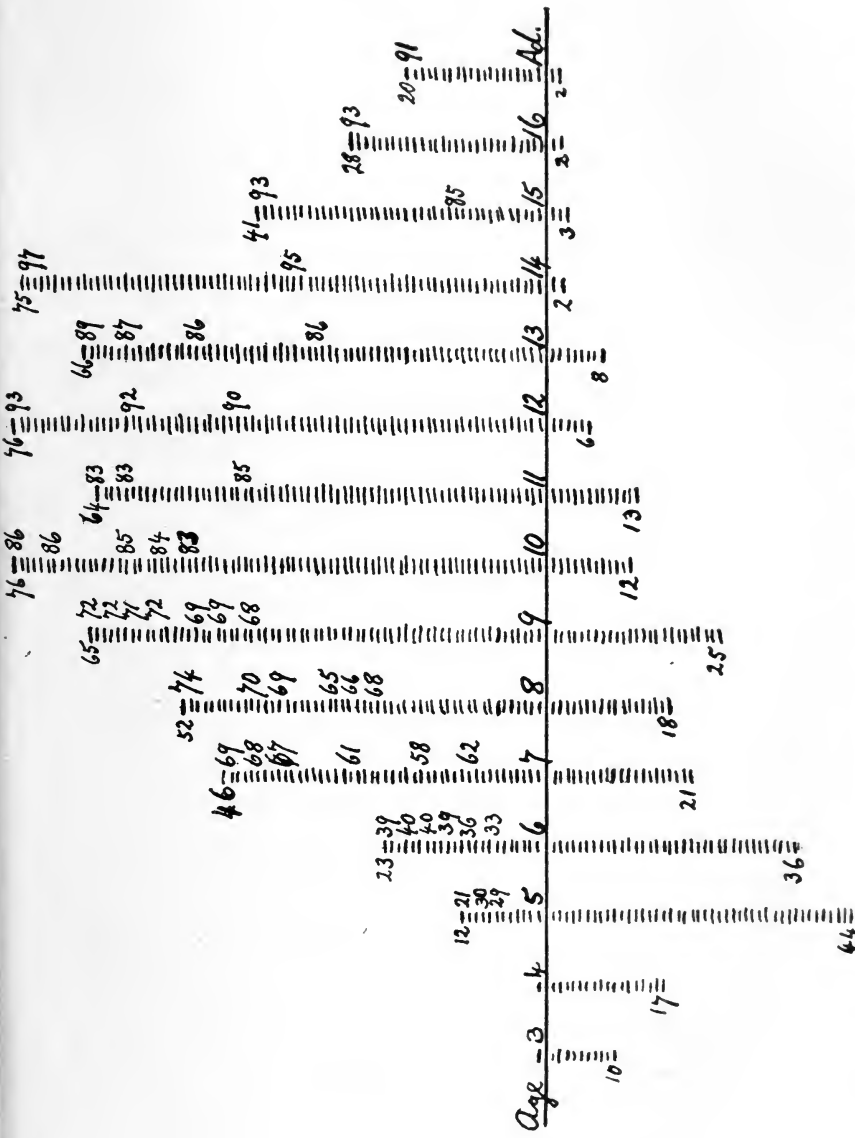

FIG. 1. Tabulation of Results for Two Correct of the *BCD* Lines. Figures to the right of the columns show percentages correct.

Still more fluctuation is seen at age seven, where the percentage at first drops from 62 to 58 and then rises slowly to 69. From the point of view of standarization this fluctuation of the percentage becomes important. We obviously cannot say that a test is a seven- or eight-year-old test, as the case may be, until we are certain that the addition of subsequent data is not likely to affect our percentages. At one stage of our curve it might have seemed that this particular combination of lines would have made a good eight-year-old test, since 68 per cent. of the eight-year-olds passed it and only 57 per cent. of the seven-year-olds. As the other results were added, however, it became obvious that a larger and larger majority of the seven-year-olds were able to accomplish it, practically as large a majority as with the eight-year-olds, and since this percentage remained more or less constant for some time, it may be safe to assume that this group of lines (to do any two correct out of the *BCD* lines) is a fair seven-year-old test.

In a similar manner curves were built up for all lines of the test and for various combinations of lines, and the comparative lack of fluctuation of the percentages seemed to show that additional data would not radically alter the results already obtained, at least with ages five to sixteen inclusive.

Standardization.—After the results had all been collected in this manner, curves for each line were drawn to show the percentage of correct responses for each age. These curves are shown in Fig. 2. The curves show how the lines compare with each other in difficulty. There are four groups of three lines each which are, as we mentioned before, about equal in difficulty. It will be seen also that most of the curves are more or less irregular and do not show any very decided increase from one age to the next, and therefore are not very satisfactory from the point of view of standardization. The actual percentages from which these curves have been drawn, together with the total number of children tested at each age, are shown in Table I.

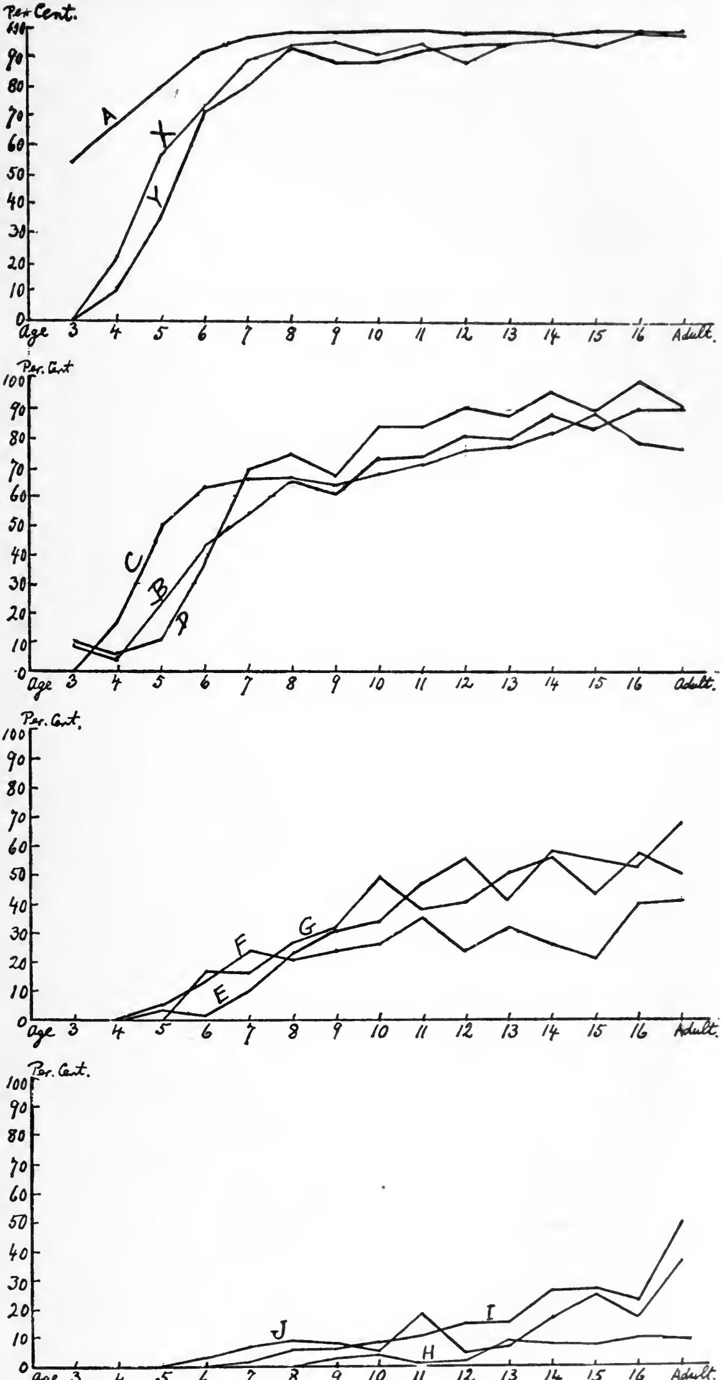

FIG. 2. Percentage of Passes at Each Age for the Twelve Different Lines of the Test.

TABLE I.

NORMAL CHILDREN. PERCENTAGE CORRECT FOR EACH LINE OF THE TEST

Chronological Age	Number Tested	Lines of the Test											
		A	*X*	*Y*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
3	11	54	0	0	9	0	10	0	0	0	0	0	0
4	18	67	22	11	5	17	6	0	0	0	0	0	0
5	56	80	57	36	23	50	11	4	5	0	0	0	0
6	59	92	74	72	44	63	37	2	14	17	2	0	2
7	67	97	90	81	54	66	69	10	24	16	0	1	7
8	70	99	95	95	66	66	74	23	21	27	0	6	9
9	90	99	96	89	61	64	67	31	24	32	3	6	8
10	88	100	92	90	73	68	84	34	26	49	4	8	4
11	77	100	95	93	73	71	84	47	35	38	2	11	18
12	82	99	89	96	81	76	91	56	24	40	2	15	5
13	76	100	96	96	80	77	88	41	32	51	9	15	7
14	77	99	97	97	88	82	96	58	26	56	8	26	17
15	44	100	95	95	84	89	89	55	21	43	7	27	25
16	30	100	100	100	90	80	100	53	40	57	10	23	17
Adult......	22	100	98	100	91	77	91	68	41	50	9	50	38

This brings us to the question of standardization. If we wish to include these tests in any scale of mental measurement, it is necessary to decide what line or lines of the test we should be justified in demanding that a child should pass at certain definite years in his chronological development. It has been arbitrarily assumed by some writers that a percentage of 75 or more correct responses is necessary before we are warranted in placing a test at a certain age. This seems to be taken for granted by Bobertag, Kuhlmann, Goddard and others. Stern[1] emphasises the 75 per cent. standard, without however disregarding the amount of advance at each age. Rogers and McIntyre[2] hold to the 75 per cent. basis. They say, "The standard for a pass at a given age should be determined on the basis of actual experience; our own results agree with those of Bobertag and Goddard, as about 70 or 75 per cent.," and further on, "A test was considered properly assigned to a given age when at least 70 per cent. of the children of that age were able to pass it," which shows a little weakening from the rigid 75 per cent. standard.

[1] Stern, 'The Psychological Methods of Testing Intelligence,' Trans. by Whipple. Educational Psychology Monographs, No. 13.

[2] Rogers and McIntyre, 'The Binet Simon Tests,' *The British Journal of Psychology*, Vol. VII., No. 3, October, 1914.

Bobertag,[1] discusses the matter at length and decides for the 75 per cent. standard. His discussion is undoubtedly the most exhaustive that has come within the notice of the writer. He shows the relation of this question to the normal distribution of any ability and makes a comparison with pupils' grades as given by teachers.

On the other hand Binet himself nowhere seems to have been very dogmatic on this point. As far as one can discover he seems to have considered a test standardized if passed by from 60 to 90 per cent. of the individuals tested. Similarly Terman and Childs[2] feel it impossible to adhere rigidly to the 75 per cent. standard. They say that two thirds ought to pass a given test, but they lay more stress upon "a sharp rise in ability from the year before." It may be interesting to note that the latter workers cited, Binet and Terman and Childs, have all been actively engaged in adding new tests, whereas the former are more especially those who have merely worked with the tests. It would seem to suggest that in actual practice there is some difficulty in arriving at the 75 per cent. standard.

If we now turn to actual results as shown in various standardizations of tests, we see this practical difficulty. In Goddard's[3] results we find two or three tests that have been retained in a given age where less than 75 per cent. have passed. In Bobertag's[4] results we have numerous instances where the number of passes is not up to the 75 per cent. standard, although it must be remembered that he is merely trying out the tests and not attempting to standardize them.

[1] Bobertag, 'Ueber Intelligenzprüfungen,' *Zeitschr. f. angewandte Psychologie*, Vol. 6, 1912, p. 495.

[2] Terman and Childs, 'A Tentative Revision and Extension of the Binet-Simon Measuring Scale of Intelligence,' *Journal of Educational Psychology*, Vol. 3, Nos. 2 to 5, 1912; and Terman, 'Suggestions for Revising, Extending and Supplementing the Binet Intelligence Tests,' *Journal of Psycho-Asthenics*, Vol. XVIII., No. 1, September, 1914, p. 20.

[3] Goddard, 'Two Thousand Children measured by the Binet Measuring Scale of Intelligence,' *The Pedagogical Seminary*, Vol. 18, June, 1911, p. 232.

[4] Bobertag, 'Ueber Intelligenzprüfungen,' *Zeitschr. f. angew. Psychologie*, Vol. 5, 1911, p. 105.

In Winch's[1] recent re-standardization of the Binet Scale for English children a very high percentage of passes is required before the test is admitted to a given age. But the actual numbers tested are not shown clearly and it is difficult to believe that in so many cases 100 per cent. passed the tests. Tests are shifted from one year to another rather too freely in the opinion of the present writer. If 42 per cent. pass a given test at six years, and 52 per cent. at seven, we are not warranted in assuming that the test is an eight-year-old test without testing any eight-year-old children. This seems to have been done.

From the foregoing facts and from my own experience, I am inclined to believe that it is impossible to lay down a definite percentage for the standardization of a test. It may be that theoretically about 75 per cent. should pass a given test, and probably the greater number of children we test the nearer to this theoretical standard we may attain. In actual practice, however, with large groups of unselected individuals this 75 per cent. standard is difficult to obtain, and for the practical placing of a test at a given age the crucial point seems to be the more or less sudden rise in ability from one age to another. We must require about 60 per cent. passes, but beyond that the best age for placing a test will depend upon the shape of the curve showing the percentage of passes at each age. It is significant for us to know at what age the ability of the child for a special response arises. If we were to adhere strictly to the 75 per cent. standard, we might place a test at a given age where 76 per cent. of the children pass, and where 71 per cent. of the lower age also pass. We would then be giving 71 per cent. of a lower age credit for a test of a higher age. For example, on curve *C* Fig. 2, we have 76 per cent. of the twelve-year-olds passing the *C* line, and 71 per cent. of the eleven-year-olds. It would seem to me to be obviously wrong to give the 71 per cent. eleven-year-olds credit for a twelve-year-old test standardized by this 75 per cent. procedure.

[1] Winch, 'Binet's Mental Tests; What They Are and What We Can Do with Them,' *Child Study*, Vol. VII., Nos. 1 to 8, 1914.

It will now be clear what I meant by saying above that the curves in Fig. 2 are not satisfactory from the point of view of standardization. The curves rise gradually and many of them do not show any sudden rise that might warrant us in placing the test at any special age. From my point of view we must look for some other way to make our results useful as actual tests. If we group our lines and mass the results for various combinations of lines, we get the curves shown in Figs. 3 and 4.

Curve 1 *XY* shows the percentage of those who passed one correct of the *X* or *Y* lines; curve 1 *BCD* shows those who accomplished one line correctly out of the *B*, *C* or *D* lines; curve 2 *BCD* those who did two of the *B*, *C* or *D* lines; curve 1 *EFG* those who did one of the *E*, *F* or *G* lines; curve 3 *BCD* those who did three of the *B*, *C* and *D* lines, *i. e.*, all three lines correctly; curve 2 *EFG* those who did two of the *E*, *F* or *G* lines; and curve 2 *EFGHIJ* those who did two out of the six lines from *E* to *J*. All these curves are much more satisfactory from the point of view of standardization with the exception of the curve 2 *EFG*. From this group of lines (2 *EFG*) it is impossible to get a test, at least under the age of sixteen, because the percentage nowhere rises above 50, and this is too low for the standardization of a test. Other combinations of the *E*, *F* and *G* lines were not drawn, since it is obvious that three correct out of the *EFG* lines would show still smaller percentages at all ages. Similarly the *H*, *I* and *J* lines proved themselves too difficult. The highest percentage of passes for the *H* line is 10 per cent. at sixteen; for the *I* line 27 per cent. at fifteen; and for the *J* line 25 per cent. at fifteen. Combinations of these lines would obviously not rise above 50 per cent. and so they must be discarded at least as tests suitable for ages below sixteen.

Discarding the 2 *EFG* lines, we have six curves that seem to show all the characteristics essential for the standardization of a test. Curve 1 *XY* shows a marked rise between ages four and five, from 22 per cent. to 66 per cent. and from that place onwards to 84 per cent., 95 per cent. and then re-

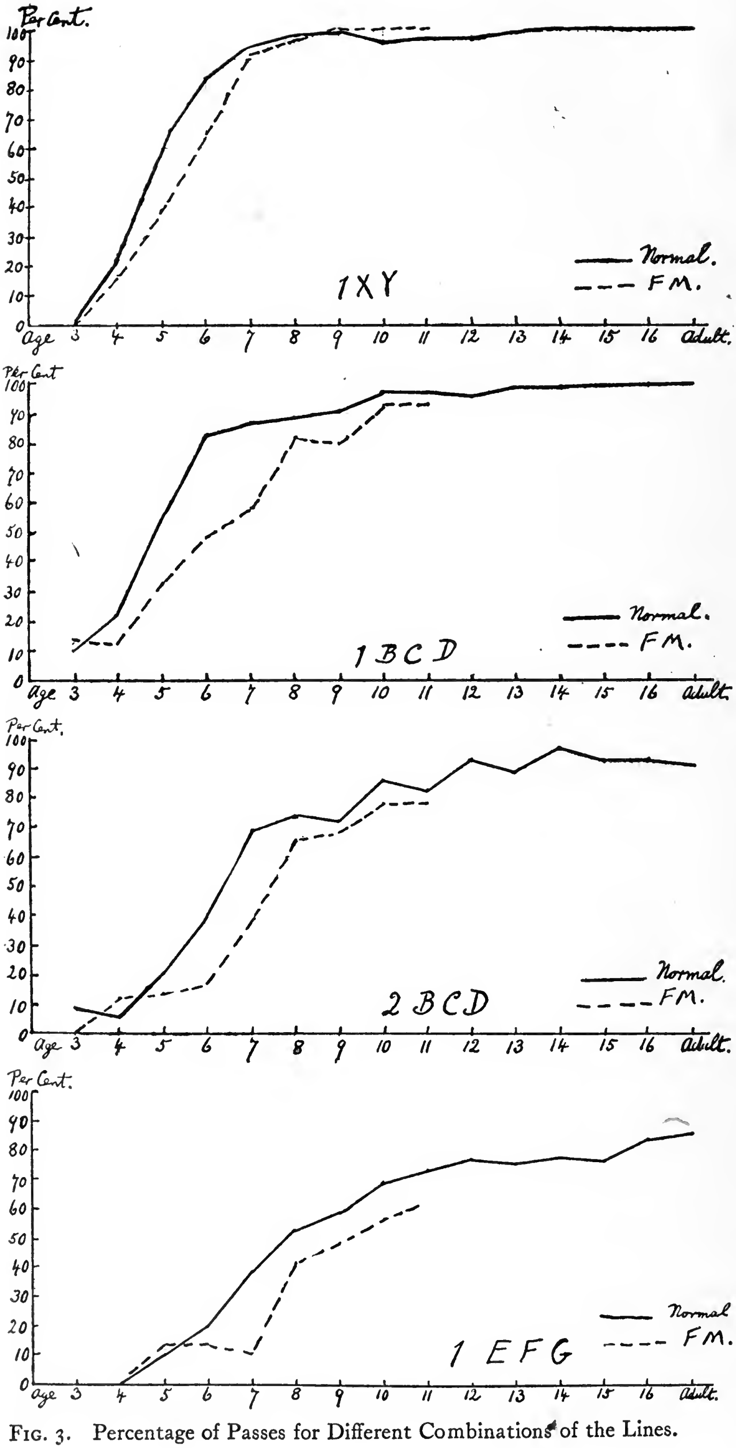

FIG. 3. Percentage of Passes for Different Combinations of the Lines.

mains in the nineties until 100 per cent. is reached at age fourteen. This means that the vast majority of children of five years and over are able to do correctly any one line of the

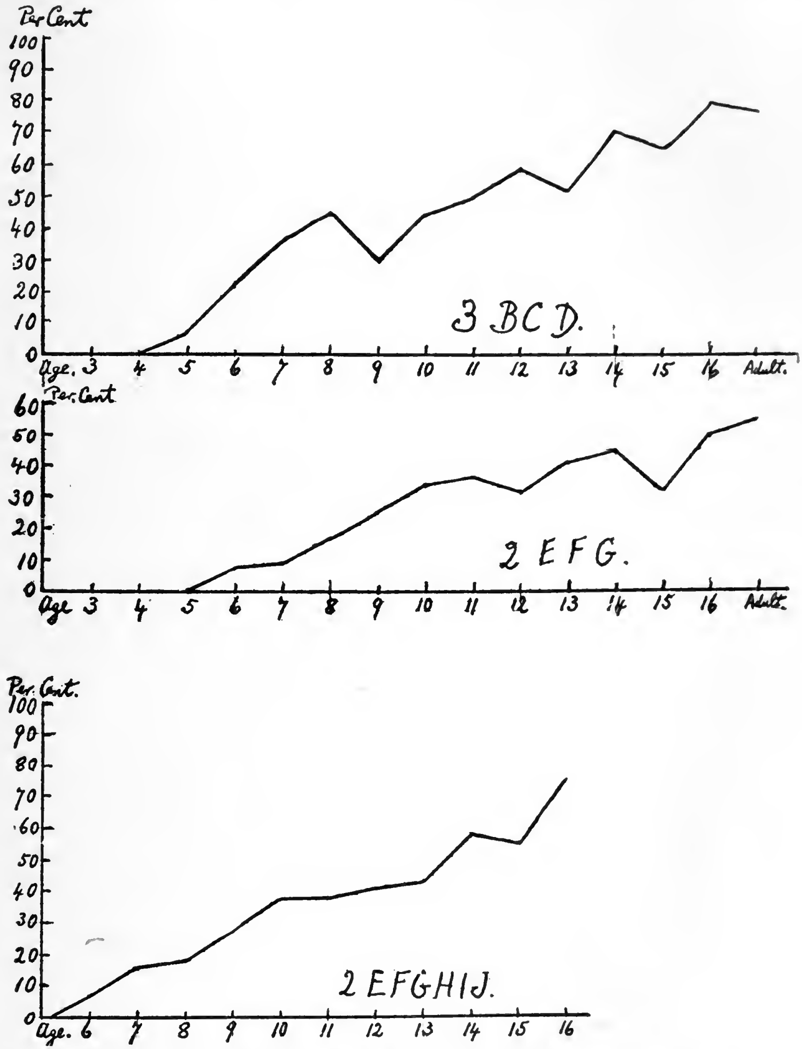

FIG. 4. Percentage of Passes for Different Combinations of the Lines.

X and *Y* lines, whereas very few of those below five years can do this. Curve 1 *BCD* shows a marked rise between five and six years, from 56 to 83 per cent. Only about half of the five-year-olds can do one line of the *BCD* lines correctly and so it is obviously not a five-year-old test, whereas it is well within the ability of the six-year-olds. Curve 2 *BCD* shows a sudden rise between six and seven, from 39 to 69 per cent. This is obviously a seven-year-old test. Curve 1 *EFG* shows its most significant rise between nine and ten, from 58 to 69 per cent. This is not such a marked rise as in the other cases, but is probably sufficient to warrant a placing of the test at ten years. It would seem in general that the higher the ages the less marked are the rises in the curves, owing to the well-known fact that one year in the lower ages means a greater advance in the development of intelligence than in the higher ages. It would seem to me that this test is correctly placed at ten years, since the difference between the percentages at nine and ten, 58 and 69 per cent., is greater than the differences between the percentages at ten and eleven, 69 and 73 per cent., or between those at eleven and twelve, 73 and 77 per cent. This curve would also illustrate well the fallacy of adhering rigidly to the 75 per cent. standard. If we were to do this we would have to consider it a twelve-year-old test and give such credit to 73 per cent. of the eleven-year-olds and to 69 per cent. of the ten-year-olds. Curve 3 *BCD* shows a marked rise between thirteen and fourteen years, from 52 to 71 per cent. It is to be noted, however, that the twelve-year-olds with 59 per cent. passes do better than the thirteen-year-olds with only 52 per cent. passes. But the difference between 59 and 71 per cent. seems sufficient to place the test at age fourteen. Again fifteen-year-old children do more poorly on this combination than do the fourteen-year-olds, only 65 per cent. passing. This may be explained, perhaps, by the fact that the majority of these were pupils in the grades and very probably slightly below normal. The sixteen-year-olds do well. A good many of them were high school pupils.

It was surprising to me to find that a correct passing of all the three lines, *BCD*, is delayed until about the fourteenth year. Six-year-olds can accomplish one and the one passed by most of them is the *C* line. With children between the ages of six to thirteen we find a large number of them passing each line regarded singly, (see curves *B*, *C* and *D*, Fig. 2), but it seems extremely difficult for the same child to pass all three without a mistake. Perhaps the close attention demanded is beyond the powers of the younger child. Or again the similarity of the moves may be confusing to the child, when line follows line with only a slight difference between them.

Curve 2 *EFGHIJ* shows a marked rise between fifteen and sixteen years, from 55 to 75 per cent., although we have 59 per cent. of the fourteen-year-olds passing this combination. The rise from 59 to 75 per cent. is no doubt sufficient to standardize this test and yet I feel some doubt in regard to the results in this case. The fifteen-year-old children were not on the whole as typical of their age as the children of the other ages. They were mainly children from the grades with a sprinkling of high school students. Almost every curve shows their deficiency as contrasted with the fourteen-year-olds. It may be therefore that two out of the *E* to *J* lines is a fifteen-year-old test. This is possible, although in view of the results obtained not very probable.

TABLE II

NORMAL CHILDREN. PERCENTAGE CORRECT FOR VARIOUS COMBINATIONS OF THE LINES OF THE TEST

Chronological Age	Number Tested	Combinations of Lines					
		1*XY*	1*BCD*	2*BCD*	1*EFG*	3*BCD*	2*EFGHIJ*
3	11	0	10	9	0	0	0
4	18	22	22	6	0	0	0
5	56	66	56	21	11	7	0
6	59	84	83	39	20	22	8
7	67	95	87	69	39	36	16
8	70	98	89	74	53	45	18
9	90	99	91	72	58	30	28
10	88	96	97	86	69	44	38
11	77	97	97	83	73	50	38
12	82	97	96	93	77	59	41
13	76	99	99	89	76	52	43
14	77	100	99	97	78	71	59
15	44	100	100	93	77	65	55
16	30	100	100	93	84	80	75
Adult....	22	100	100	91	86	77	—

Table II. shows the results for the six combinations, which we believe can be satisfactorily used as tests. In each case the number tested at each age and the percentage of correct responses is given. It will be seen that an attempt was made to test large numbers of children at each age. Even although the experimenter often felt that a given line was far too difficult for a child, yet it was given in order to get the negative results, without which no real standardization of a test is possible. It is just as important to know that the children below a given age cannot pass the test as to know that those of a given age can pass it, and the comparison between the two ages is not a just one unless equal numbers of both ages in question have been tested. Much of the work in standardization done up to the present time is open to just this criticism. In Goddard's figures for the revision of the Binet Scale we find tests placed at a given year because of a high percentage of passes obtained from about 100 children, whereas only between thirty to forty children and sometimes even fewer of the next lower age had been tested. From my experience with the results from this test and with others upon which work is now being done, it seems to me to be dangerous to take for granted that a few results in the age below the test age are sufficient negative evidence to exclude the possibility of the test falling at that age. It is generally conceded that the tests at the lower end of the Binet Scale are too easy and an inspection of Goddard's figures will show that in the six-year-old tests, for example, only about half and in some cases less than half of the children at the test age were tested in the age below the test age. In Table II. it will be seen that the first test of the Knox Cubes—1 *XY*—is open to just the criticism that I have been urging. There are 56 five-year-olds and only 18 four-year-olds. This is owing to the fact that I experienced great difficulty in getting four-year-olds. It may be that if fifty four-year-olds were tested, this combination of lines might prove a four-year-old test, and yet I do not believe this would be the case and so I have considered it a five-year-old test. To none of the other tests can this criticism apply. Large numbers above and below the test age have been tested and there are sufficient negative results below the test age in each case.

Total Number of All Lines Passed.—The results of the test were also tabulated in another manner to show the number of lines passed correctly at each age. The total number of lines passed correctly by each child was added together and the average and average deviation for each age found. These figures are shown in Table III.

TABLE III

AVERAGE NUMBER OF LINES PASSED AT EACH AGE

Chronological Age	Av. Number of Lines Passed	A. D.	
3	0.57	0.49	
4	1.43	0.76	
5	2.41	1.17	77 % pass 2 or more lines.
6	4.22	1.25	71% pass 4 or more lines.
7	5.12	1.28	70% pass 5 or more lines.
8	5.60	1.18	
9	5.59	1.34	
10	6.29	1.32	72% pass 6 or more lines.
11	6.68	1.76	
12	6.66	1.10	
13	6.66	1.41	
14	7.55	1.24	75% pass 7 or more lines.
15	7.45	1.66	
16	8.05	1.11	62% pass 8 or more lines.

It will be seen that there is an almost steady increase in the number of lines accomplished from age four up to age eleven, where it becomes almost stationary at 6.6, to rise again at age 14 up to age 16. In no case is the average deviation larger than 1.7.

The results in this form may not be so suitable for the placing of a test at a definite age, but they could of course be incorporated in a scale. They show a very close correlation with the lines which I have considered standardized. Age four can do one line, that is the *A* line, and they are not able to do one out of *XY*. Age five can do two lines, that is the *A* line and either *X* or *Y*. Age six can do four lines, that is the *AXY* lines and one out of *BCD*. Age seven can do five lines, that in *AXY* and two out of *BCD*. Ages eight and nine cannot do more than five lines, that is *AXY* and two out of *BCD* and not one out of *EFG*. Age ten can do six lines, that is *AXY* and two out of *BCD* and one out of *EFG*.

Ages eleven, twelve and thirteen all show an accomplishment of a little more than six but not seven lines, that is they can do *AXY*, two out of *BCD* and one out of *EFG*, but they cannot do all *BCD* nor two out of *EFG*, as our curves have already shown us. Age fourteen and above can do seven lines, that is *AXYBCD* and one out of *EFG*. Age sixteen can do eight lines, that is *AXYBCD* and two out of the remainder. All this serves to corroborate our placing of the lines at particular ages. There is of course no reason why this method should not be used in crediting for different ages. In that case we should expect at five years two lines to be passed correctly; at six years four lines; at seven years five lines; at ten years six lines; at fourteen years seven lines and at sixteen years eight lines.

There does not seem to be very much difference in marking between the two methods. In 33 cases examined nineteen gave the same results for both methods; the other cases showed a slightly lower age estimate when the system of marking by total number accomplished correctly was used.

In twenty cases where the Binet ages were available for comparison the estimate of age from performance on the Knox Cubes agreed with the Binet age in seven cases by the group method and in eight cases by the total number method. The average amount of difference between the Binet age and the age as estimated by the Knox Cubes was 2.17 years for the group method and 2.36 for the total number method of using the cubes. Apart from the slight differences in results obtained by these two methods of marking for the test, it is also interesting to note how well on the whole the estimate of mental age agrees with that arrived at by the Binet Scale. It is surprising that any one test should come so near the result arrived at by a whole series of tests. I do not mean by this to suggest that the Knox Cube Test alone should ever be used to compute mental age. It must, of course, take its place in a series of mental tests.

Perseveration and Reverse Order.—Two definite types of errors occur in this test to warrant a few words. The first I have called perseveration. It consists in repeating the *A* line after the examiner has continued with other lines of the test. The child does not seem to notice that the *X* or *B* or *C* line, as the case may be, is different from the *A* line with which he started. There is a perseveration of the tendency to tap the blocks in the order given first, which also happens to be a very easy, natural order. The child taps one, two, three, four, again and again, sometimes adding some other moves to these first four. This error of perseveration is not common among the older children. Only one of our fourteen-year-olds did this, and from his performance in other tests it is very probable that this child is defective. Of the other children we found one case at 12 years, one at 9, one at 8, five at 7, three at 6, six at 5, one at 4, and two at 3, and it is to be remembered that relatively few three- and four-year-olds were tested. It would seem then that this error is due to the lower stage of intelligence of the child, and perhaps one that may be a clue to possible feeble-mindedness. This idea is strengthened by the frequency of this error among our feeble-minded cases. This occurred as follows:

At mental age 2	3 times
3	2 "
4	10 "
5	16 "
6	22 "
7	10 "
8	3 "
10	2 "
11	1 "

This makes a total of 72 cases among the feeble-minded as contrasted with 21 among the normal children. With the feebleminded as with the normal, we notice a larger percentage of cases among the children of lower mental age.

The second error is that of reverse order, *i. e.*, beginning with the block at the child's right instead of with the block at his left, which is the one touched first by the examiner. This was noted as "wrong end." It occurs most frequently with the *A* line and rarely is peristed in for more than three or four lines. It is again an error found more often among the younger children, but it cannot be said to be particularly characteristic of the feebleminded. The following number of cases were noted:

Normal		Feeble-minded	
Chronological Age		*Mental Age*	
Age 3	1 time.	Age 2	2 times.
4	4 times	4	2 "
5	7 "	5	1 "
6	4 "	6	2 "
8	1 "	7	1 "
		8	1 "

No credit was allowed for this error. It was treated always as a mistake, even although the line, whether the *A* line or a more complicated line, was in other respects correctly performed. In a system of mental classification giving a number of points for each test or computing by whole or half credits, it might be justifiable to given this error a half credit or a fewer number of points.

The Feeble-minded.—Four hundred and sixty-three feeble-minded individuals were given the same test. These were inmates of an institution[1] and all of them had been graded by the Binet Scale. They have been classified according to mental age and the percentage correct for the different lines is shown in Table IV. and for the various combinations of lines in Table V. Their performance in some of the combinations of lines is shown by curves on Fig. 3. On the whole

Table IV

Feeble-minded. Percentage Correct for Each Line of the Test

Mental Age	Number Tested	Lines of the Test											
		A	*X*	*Y*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
2	6	50	0	0	0	0	0	0	0	0	0	0	0
3	7	57	14	0	0	0	14	0	0	0	0	0	0
4	17	76	17	8	6	12	12	0	0	0	0	0	0
5	37	92	32	21	19	27	8	5	8	13	0	0	0
6	67	97	61	38	19	40	13	7	6	3	0	0	0
7	85	99	87	54	31	41	36	4	2	8	0	0	2.5
8	73	99	98	82	55	62	71	20	26	19	1.5	1.5	4
9	75	100	97	79	45	71	67	25	24	20	2.5	5	7
10	69	100	89	92	69	67	87	35	33	30	0	10	9
11	27	100	100	100	66	70	89	41	29	29	7	15	11

[1] The writer wishes here to acknowledge the kindness and courtesy of Dr. Emerick, superintendent of the Ohio Institute for the Feeble-minded.

TABLE V

FEEBLE-MINDED. PERCENTAGE CORRECT FOR VARIOUS COMBINATIONS OF THE LINES OF THE TEST

Mental Age.	Number Tested	Combinations of Lines				
		1 *XY*	1 *BCD*	2 *BCD*	1 *EFG*	2 *EFG*
2	6	0	0	0	0	0
3	7	14	14	0	0	0
4	17	17	12	12	0	0
5	37	39	32	14	14	9
6	67	65	49	17	14	3
7	85	92	59	38	11	3.5
8	73	97	82	66	42	22
9	75	100	80	68	49	16
10	69	100	93	78	57	30
11	27	100	93	78	63	22

the curves follow those of the normal children pretty closely, generally remaining a little below. This means that the normal children as a whole do slightly better on this test than the feeble-minded of corresponding mental age. In the lower ages—three and four—the curve for the feeble-minded is very close to and sometimes even rises above the curve for the normals. This corresponds to the well-known fact that the Binet Scale is too easy at the lower end. Only in one curve, 1 *XY*, do we see the feeble-minded curve rising above the normal curve. In all the other curves it remains below the normal, although sometimes it is very near the normal. The feeble-minded individuals of the higher mental ages, ten to eleven, do not surpass normal children of corresponding chronological age, so that in this test we find no corroboration of the fact that the Binet Scale is too difficult at the upper end. This is, of course, not to be taken as a denial of the difficulty of the higher tests of the Binet Scale, although none of the tests have been subjected to the rigid standardization as has been undertaken for this test. It may be that this cube test is testing something that is not tested by the tests at the upper end of the Binet Scale. It demands concentration of attention to a continuously varying task and it requires the subject to work at a pace that is set for him. The feeble-minded are not by any means lacking in attention and perseverance. Many will work for long stretches of time at a task with the greatest concentration of attention, but if their attention is required for a certain definite period to a changing stimulus, it seems difficult for them to adjust their attention to the continuously varying aspects of the problem. It is probable that many of the feeble-minded would do much better if the blocks were tapped not at a given constant rate, but at a rate varying with their ability to shift their attention from the one block to the other. In terms of rhythm, we might say that the feeble-minded individual is too dependent upon his own individual rhythm, and that he lacks the capacity of adjusting himself readily to external rhythms.

Summary.—The Knox Cube Test given to normal children and standardized for the different ages gives the following tests:

1 out of *XY* lines	5 year test.	
1 out of *BCD* lines	6 " "	
2 out of *BCD* lines	7 " "	
1 out of *EFG* lines	10 " "	
3 out of *BCD* lines	14 " "	(probably).
2 out of *EFGHIJ* lines	16 " "	(probably).

In actually using this method it would seem well to credit the child with the age at which the most difficult combination is passed. The child who passes 2 *BCD* is credited with a five-, six- and seven-year-old test, even although he may have failed in one of the easier combinations, the presumption being that the failure was not due to an inability to pass these easier lines, but to some disturbing factor foreign to the test.

The actual number of lines passed may also be used as an index to mental age, and if this method is followed we must give credit in this manner:

For 2 or 3 lines	5-year credit.
" 4 lines	6 " "
" 5 "	7 " "
" 6 "	10 " "
" 7 "	14 " "
" 8 "	16 " "

In using this test it would be well to follow strictly the directions given at the beginning of this article, since any deviation from this method is likely to give different results. Touch the blocks at a uniform rate, beginning with the cube at the child's left, never repeat a line and do not give the child any suggestion of counting.

In conclusion, the justification for a long article such as this one that deals solely with a single test may be found in the fact that up to now we have been more or less satisfied with a very indifferent standardization of the mental tests that are being widely used in computing mental age. We have, in fact, been avoiding the hard work and an inadequate solution of the problem of standardization. Only by a thoroughgoing treatment of each and every test, such as has been attempted here, will we ever arrive at tests that will give us something more than a mere approximation to a child's mental age. A scale made up of different tests standardized in this fashion might lay claim to some exactitude. I think emphasis must be laid on the number tested before we can rest satisfied with the standardization of any test, and in this connection it seems most important to me to have practically the same number of cases in the ages immediately above and below the test age, as we have in the test age itself. We dare not assume that the age below the test age cannot accomplish a given test unless we have sufficient negative results in that age. Our knowledge of general intelligence and the development of intelligence is so limited that it is very dangerous to take anything for granted on an a priori basis.

THE ADEQUACY OF THE LABORATORY TEST IN ADVERTISING

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That it is possible, by means of a simple experiment, to tell even roughly the relative amount of business which each of a series of advertisements will bring in, is a revolutionary idea. The feasibility of such a prediction, however, has been indicated by the writings of Strong and Hollingworth. Granting this assumption to be true, it opens up an entirely new field of experimentation for the practical psychologist. Not only that, but it should result in the development of a series of principles which would be of the greatest benefit to the advertising man. The historical summary will show the evidence upon which the assumption is based.

Historical Summary

Chronologically, the first experiment which I have found comparing the results obtained by laboratory and business methods was performed with a set of five Bullard Lathe Advertisements.[1] Ten subjects, mechanics and engineering students, were used. They were told to arrange the advertisements 'in the order in which you would buy the machine.' From the results, the relative position of each advertisement in the series was obtained. The order as determined in this way was then compared 'with the actual number of replies for catalogues received by the Bullard Co. from each advertisement.' It was found that the two orders agreed perfectly.

Another similar experiment was performed with a set of Packer's Tar Soap advertisements.[2] Fifty advertisements

[1] Strong, 'The Relative Merit of Advertisements,' 10–11. Hollingworth, 'Advertising and Selling,' 8–10.

[2] Strong, 'The Relative Merit of Advertisements,' 11–15: 63–81; *Jour. of Phil., Psy.*, etc., VIII., 600–606. Hollingworth, 'Advertising and Selling,' 11–14.

were arranged by twenty-five subjects 'in the order in which *you* would *buy* the soap.'

"When the order was compared with the order submitted by Mr. Edward A. Olds, Jr., of the Packer Manufacturing Company, and with the one from the Blackman-Ross Advertising Agency, we found a high degree of similarity between the three orders. The resemblance between the experimental order and either of the other two is equal to a coefficient of correlation of plus .52. The resemblance between the order of the Packer Manufacturing Co. and the Blackman-Ross Agency is equal to plus .64. There is then nearly as great agreement between the experimental order and that of the Packer Manufacturing Co. as between the latter and the agency, which is now handling their advertising business."

Eight advertisements out of the 50 were then selected for a more detailed study. These advertisements were arranged by 100 subjects, 60 men and 40 women. The following coefficients of correlation were found to exist:

Between	100 subjects and 25 subjects	.947
"	100 subjects and Packer Co.	.893
"	100 subjects and B.-R. Agency	.866
"	25 subjects and Packer Co.	.840
"	25 subjects and B.-R. Agency	.920
"	Packer Co. and B.-R. Agency	.866

A third set of experiments was performed with Electric Light Advertisements.[1] There were originally in the set five advertisements, but only three were used. For in two of them there was a difference in one respect which affected the company's data so much as to render them unsuitable for the experiment. Thirty-six subjects were used. Working the results of the three advertisements out by the order of merit method, Strong found a coefficient of correlation of plus 1.00 between the order as determined by the laboratory test and by the business returns.

Hollingworth,[2] who gives what are apparently the returns from all five advertisements in the set, finds a coefficient of

[1] Strong, *Jour. of Ed. Psy.*, IV., 393–404.

[2] Hollingworth, 'Advertising and Selling,' 14–15.

correlation of plus .60 between the laboratory returns and the business returns as measured by the cost per inquiry.

There have been made, then, three tests, the reports of which are accessible in the psychological literature, determining the correlation between the laboratory test and the business test. The lowest coefficient is plus .52; the highest is plus 1.00. The average is in the neighborhood of plus .82. The general conclusion drawn is that the laboratory test is a satisfactory preliminary for any set of advertisements which is to be used in business. For by it the poorer advertisements can be eliminated and only the best kept. The business returns and the laboratory returns agree so closely that each can, in general, be used as a measure of the other.

Strong[2] also worked out the coefficient of correlation between the results of groups of persons coming from different walks of life. His general conclusion is: "A group of 50 college students will represent very closely the judgment of groups of educated business men and women, of young business men, such as attend evening schools, etc., and of women of the middle class regardless of age. They will not represent at all the judgment of groups from small towns and farming sections such as the regions around Garrison, N. Y., from which the data were obtained.

"It is fair to extend the results as set forth in previous chapters regarding the judgment of college students to groups of educated men and women in general. But as the data of this report are mainly concerned with cheap articles in common use, very little can be postulated concerning the relation of various groups of individuals with regard to more expensive commodities."

Experimental Results

In view of the last sentence of the quotation given above, it was thought that some profitable data might be disclosed by the study of an entirely different kind of advertising material, such as that of a mail order business.

The experimental work was done by John S. Deuble, in the

[2] Strong, 'The Relative Merit of Advertisements,' 62.

psychological laboratory at the University of Michigan. The results were put in their final form by the writer.

Three sets of advertisements were tested with a total of 161 subjects, 69 men and 92 women. The sets of advertisements were as follows: 4 half-page advertisements of the American Collection Service, Detroit, Mich., procured from Mr. William A. Shryer; 10 full-page advertisements of the American Collection Service; 9 quarter-page advertisements, *Saturday Evening Post* size, of the Burroughs Adding Machine Co., obtained from Mr. William A. Hart of the advertising department of the Burroughs Company.

The following data concerning the American Collection Service advertisements were furnished: the number of insertions, the total number of inquiries, the advertising cost, the cost per inquiry and the profit or loss for each advertisement.

From the Burroughs Adding Machine Co. the following data were received concerning each advertisement: the number of inquiries, the number of trials, the number of sales, the total amount received from the sales.

From these data, it should be possible to determine with some accuracy which of the advertisements was of the greatest value from the business standpoint. The settling of this question is, however, more complicated than appears on the surface. Any one of the points mentioned may be considered as a test of efficiency. The trouble is that they may not agree to any remarkable extent. To indicate this, the various measurements are put in the form of tables which will show the order of merit according to the different standards. In the tables throughout the paper, number 1 indicates that the advertisement so designated was the best from the standpoint of the standard used; number 2, that it was in second place, etc.

American Collection Service

Half-Page Advertisements

Ad.	Average Number of Inquiries	Cost per Inquiry	Profit
A.........	1	1	1
B.........	2	3	2
C.........	3	2	4
D.........	4	4	3

Full-Page Advertisements

E	3	3	6
F	10	9	5
G	5	6	9
H	6	5	7
I	4	2	2
J	8	7	3
L	7	8	10
M	9	10	8
N	1	1	1
O	2	4	4

Burroughs Adding Machine Co.

Ad.	Inquiries	Trials	Sales	Amount
1872 *A*	5	4	2	2
1792 *A*	3	2	3	3
1803 *A*	4	7	5	6
1804 *A*	6	6	8	9
1820 *A*	2	3	4	5
1832 *A*	8	5	7	7
1844 *A*	7	9	6	4
1870 *A*	9	8	9	8
1883 *A*	1	1	1	1

It is evident from these tables that the measurement of efficiency which is used is of some importance. The coefficient of correlation[1] between the orders as established by the various possible measurements will show their resemblance.

American Collection Service

Correlation between	Half Page	Full Page
Inquiries and cost per inquiry	.80	.915
Inquiries and profit	.80	.430
Cost per inquiry and profit	.40	.624

Burroughs Adding Machine Co.

Correlation between	Quarter Page
Inquiries and trials	.790
Inquiries and sales	.834
Inquiries and amount	.650
Trials and sales	.773
Trials and amount	.617
Sales and amount	.933

These figures indicate that the busines test which is used has a considerable influence on the coefficient of correlation

[1] The coefficients of correlation are worked out by the formula given by Myers, 'Text Book of Experimental Psychology,' 1909, page 131. The formula is $r = 1 - [6 \text{ sigma } (d)^2/n(n^2 - 1)]$.

between it and the laboratory test. For reasons which will be explained more fully in a later part of the paper, it appears that the average number of inquiries per insertion of the advertisment is the fairest measure of the pulling power of the advertisement under actual business conditions. This measure was taken because it seems to be the fairest test of the actual pulling power, undisturbed by such things as a follow-up system, the arguments of salesmen, etc.

The experiment was carried on as follows. The subject was handed a series of advertisements and told to look them over carefully. Having become familiar with them, he was instructed to pick out the one which was to him the most persuasive. By persuasive was meant the one which would be most likely to make him answer the advertisement. This done, he was asked to pick out the second best, the third, and so on, until he had the series arranged in a descending order from the most persuasive to the least persuasive. A first choice was given a credit of 1, the next best a credit of 2 and so on down throughout the entire series. When the 161 subjects had arranged the set of advertisements, the credits which each advertisement had received were added and divided by 161, thus giving the average place which the advertisement occupied in the opinion of the 161 subjects. The one which received the smallest average was considered to have the greatest pulling power; the one which obtained the largest average was credited with the least pulling power.

In the tables below are given the averages for each advertisement as worked out in this way, together with the average deviation (A. D.). The reactions of the men and of the women are not given separately, for no significant differences were found.

American Collection Service

Half Page

Ad.	Average	A. D.	Position
A................	1.84	0.74	1
B................	1.93	0.84	2
C................	2.81	0.81	3
D................	3.31	0.75	4

Full Page

E................	6.13	2.31	7
F................	4.47	2.69	3
G................	6.00	2.00	6
H................	4.83	2.19	4
I................	6.78	2.14	9
J................	5.38	2.13	5
L................	6.60	1.97	8
M................	4.12	2.29	2
N................	7.71	1.80	10
O................	2.98	1.67	1

Burroughs Adding Machine Co.

Ad.	Average	A. D.	Position
1	3.95	1.93	2
2	6.87	1.88	9
3	4.54	2.34	5
4	4.52	2.02	4
5	6.70	1.92	8
6	4.00	1.89	3
7	5.32	2.42	6
8	6.37	1.77	7
9	3.71	1.72	1

The tables show that a more or less definite order of the relative persuasiveness of the advertisements in these three sets has been worked out. The average deviations indicate that some changes in order might occur if more subjects were tested, but the extremes at least are fairly well defined.

As a check upon the probability of the final order of the series as determined by the experiment, the following method was used. The order was determined by averaging the results of the first 10 subjects, then of the first 20 and these orders compared. Then the order of the first 20 and the first 30 was compared, and so on for the 161 subjects. If the final order was established relatively early in the series and persisted without actual change throughout, it was thought that the final order had a high degree of probability. The question of the number of subjects necessary in an experiment of this sort is always an important one and one which it is difficult to settle off-hand. Such a test as the one described is an entirely practical measurement which can be applied at any time in the experiment.

It was found that the final order of the half-page advertisements of the American Collection Service was determined with 30 subjects. The addition of 130 more did not change the relative order of merit, though, obviously, there were changes in the degree of merit. Advertisements *A* and *B*, even at the end of the 160 trials, might have been changed by the addition of the results of 10 more subjects if they had all given *A* fourth place and *B* first place. In the entire experiment, however, *A* was put in fourth place only 10 times, so the chances of its appearing in last place are 1 to 16. In the same way, the chances of *B* appearing in first place are 1 to 2.4. We are justified in concluding, then, that the final order of this series is determined beyond reasonable doubt for the class of subjects used.

With the full-page advertisements of the American Collection Service, the final order was determined by the 120th trial. The addition of the next 40 subjects did not affect the relative order. The probable accuracy of this series, while not so great as that of the half-page series, is sufficient for all practical purposes. Some of the advertisements in the middle of the series might have shifted one place by the addition of 10 more subjects, but this is unlikely, for in order to do so, the ten would have to give averages which were not even approached in the course of the experiment. So we may conclude that we have obtained here an order which would be very closely approximated by any laboratory test conducted upon average undergraduates.

With the Burroughs Adding Machine advertisements, it may be said a satisfactory final order never was obtained. An order was established with the 120th trial which lasted through the 150th, but the difference between the credits for two of the advertisements was so slight that they were inverted with the addition of the next ten subjects. Two other advertisements in the series might well have been inverted by the addition of another 10 subjects. So we may say that an entirely satisfactory final order never was determined for this set of advertisements. The extremes are clearly defined, but the intermediate members of the series are somewhat variable. It seems probable, in view of these facts, that this series of advertisements was of a more even, homogeneous sort than was either of the series of the American Collection Service. This does not necessarily mean that the Burroughs Adding Machine advertisements are either better or worse than the American Collection Service series. It simply means that it was more difficult to make adequate judgments between them.

This general condition emphasizes one fact which must be plainly apparent to any student of the psychology of advertising,—namely, that our laboratory tests have been unable to tell us whether an advertisement is absolutely a good advertisement or not. The only thing such a test can do is to place it relatively in a series. The limits of goodness and badness are to be found inside of that particular series. Because one advertisement is the first of one series and another the last in a second is no reason for asserting that the former is absolutely a better advertisement than the latter.

American Collection Service.
Half Page.

Ad.	Order as Determined by			
	Lab. Test	Inquiries	Cost per Inq.	Profit
A..........	1	1	1	1
B..........	2	2	3	2
C..........	3	3	2	4
D..........	4	4	4	3

The following coefficients of correlation are found to exist:

Between the laboratory test and the number of inquiries.......................1.00
" " " " " the cost per inquiry.......................... .80
" " " " " the profit.................................. .80

American Collection Service
Full Page

Ad.	Order as Determined by			
	Lab. Test	Inquiries	Cost per Inq.	Profit
E..........	7	3	3	6
F..........	3	10	9	5
G..........	6	5	6	9
H..........	4	6	5	7
I..........	9	4	2	2
J..........	5	8	7	3
L..........	8	7	8	10
M..........	2	9	10	8
N..........	10	1	1	1
O..........	1	2	4	4

The following coefficients of correlation are found to exist:

Between the laboratory test and the number of inquiries .... —0.43
" " " " " the cost per inquiry .... —0.58
" " " " " the profit .... —0.01

BURROUGHS ADDING MACHINE CO.

Ad.	Order as Determined by		
	Lab. Test	Inquiries	Amount Received
1	2	3	3
2	9	4	6
3	5	5	2
4	4	6	9
5	8	2	5
6	3	7	4
7	6	1	1
8	7	8	7
9	1	9	8

The following coefficients of correlation were found to exist:

Between the laboratory and test the number of inquiries .... —0.43
" " " " " the amount received .... —0.06

The second step in the experiment is to compare the order as determined by the laboratory experiment with the order as determined by the different measures of business efficiency. This comparison is given in the preceding tables.

Taking these three sets of results, we find the following correlations between the order as determined by the laboratory test and the order as determined by the average number of inquiries.

Half-page advertisements, A. C. S. .... = 1.00
Full-page advertisements, A. C. S. .... = —0.43
One-fourth-page advertisements, B. A. M. .... = —0.43
Average .... = 0.047

We also find the following correlations between the order of merit as determined by the laboratory test and the order determined by the profit or the amount received.

Half-page advertisements, A. C. S. .... = 0.80
Full-page advertisements, A. C. S. .... = —0.01
One-fourth-page advertisements, B. A. M. .... = —0.06
Average .... = +0.243

These figures show simply chance resemblance between the results of the laboratory test and the average number of inquiries per insertion and very little better than chance resemblance between the laboratory test and the business test where profits are used as the measure. The indication is that the order of merit method has at least no universal application to advertising problems. Of course, not enough series of advertisements have been tested to settle the question definitely. But since two of the three tests made have shown a significant negative correlation, a fairly large number of tests which result in equally strong positive correlations will be necessary to offset the negative results of these experiments.

It is possible, too, that college students are not satisfactory subjects for the kinds of advertisements which were used. Since a fairly large percentage of the men go into business upon leaving college, they are possibly more satisfactory than would appear at first.

As will be seen below, advertisement *N* of the American Collection Service series present some queer anomalies. Even if this advertisement is left entirely out of consideration, the coefficient of correlation between the laboratory test and the business test as measured by the average number of inquiries is -0.12.

The general conclusion seems to be that the mail order business appeals to a special and limited class of individuals. College students, on the average, are not fair representatives of such a class. With a mail-order business, it is possible to get returns which are extremely accurate, so such advertisements would make the best material for laboratory tests, if such tests would only work. On the other hand, it does seem probable that the order of merit method might be applied to those commodities which are in more general demand, such as soaps, foods, and the like. It is, however, impossible to get accurate returns as to the exact amount of business which each advertisement has brought in. This renders it impossible to be sure that the laboratory test actually has a high coefficient of correlation with the business returns.

There are several interesting things which crop out in connection with the advertisements of the American Collection Service. It will be recalled that Mr. Shryer furnished two sets of advertisements, one consisting of four half-page, the other of ten full-page, advertisements. Half-page advertisement *D* was, as far as possible, an exact copy of full-page advertisement *N*. In *N* there were five pictures in connection with testimonials, and a return coupon. In the half-page advertisement *D* there were but three pictures accompanying testimonials, and no return coupon. The wording of the argument in the two advertisements was as nearly identical as possible considering the change in size. In fact, only 6 words were changed. The half-page advertisement, however, was printed in smaller type, as necessarily must be the case, considering the difference in size.

In spite of the great similarity of the two advertisements, the results of the business returns, taking the average number of inquiries as the basis, show that the full-page advertisement *N* was the best of the set of ten. The half page advertisement *D* was the poorest one of its set. With such discrepancies as this in the business returns, it would be truly remarkable if the laboratory test did show a high coefficient of correlation with the business test. The laboratory test is at least fairly consistent, for it ranked each of the advertisements, *D* and *N*, as last of the set to which it belonged.

In the business test, the presence of the return coupon in one advertisement and its absence in the other may have been a determining factor. Shryer[1] ran two half-page advertisements at different times which were just alike except that one had a return coupon, the other did not. The one with the return coupon brought 83 replies, the other 41, showing that in this particular case, the use of the return coupon more than doubled the number of inquiries. If *N* had brought only half as many replies, it would have ranked sixth in the series, while if *D* had brought in double the number of replies, it would have ranked second. However, if there is so great an increase in efficiency coming from the return

[1] W. A. Shryer, *System*, December, 1913, p. 579.

coupon, the laboratory returns, if they are adequate, should have indicated it.

It so happened that there was a great similarity between two other members of the two sets. Half-page advertisement *B* was very much like full-page advertisement *O*. The wording was practically the same, though different pictures were used. On the basis of the average number of inquiries per insertion, *B* was in second place. According to the laboratory test, it was second in the set of half-page advertisements. From the standpoint of business returns, full-page advertisement *O* was in second place; in the laboratory test, it was first. This shows a fair consistency for both the laboratory test and the business test.

The similarity between the returns from *B* and *O* is an interesting check upon the dissimilarity found to exist between *D* and *N*. For it might have been thought that the half-page advertisements were much superior in general make-up and appeal to the full-page advertisements. Logically, *D* might easily have been the worst of the half-pages, while its duplicate *N* might equally well have been the best of the full-page displays. This, however, is rendered extremely doubtful by the results obtained from *B* and *O*. The indication is that the peculiar results obtained with *D* and *N* are due to extraneous conditions which could not possibly be controlled in the laboratory.

Because of the results of this experiment as well as on account of theoretical considerations, the writer has been led to question the application of the order of merit method to advertising problems. He does not question the true usefulness of the method, but does deplore the uses to which it has been put. In his opinion, the experiments which have been performed by this method on advertising problems can be attacked on several sides.

In the first place, the experimenters have in but a very few instances compared the laboratory results with the business results. The idea of this comparison is, of course, to show the dependability of the method as a laboratory technique for investigating advertising problems. One thing which has been done is to compare the order as determined by the laboratory experiment with the order as determined by the opinion of certain selected advertising experts, who were practically put through the same experiment. Since Dr. E. K. Strong, Jr., was one of the first to apply this method to the psychology of advertising, a quotation from one of his articles will be appropriate in bringing out the point. "It is scarcely necessary to repeat that the results of the Packer Manufacturing Company are not based upon carefully compiled data, but only upon the judgment of the firm based on their business experience. Any one familiar with advertising knows that such data have not been compiled for any extensive set of advertisements, let alone a series of fifty extending over twenty years of service. If such data did exist, it could not be used at its full face value, as an advertisement of twenty years ago might have been very effective then and be out of date to-day.

"The order of the twenty-five subjects correlates plus .52 with the order of either of the two advertising experts. The correlations between the orders of the two advertising experts is plus .64. These relationships are lower than those which have been obtained with other sets of advertisements. . . .

"It is evident, then, that the 'order of merit method' does give results that correlate high with results obtained in business."[1]

Since by results obtained in business, Strong must evidently mean, in the above connection, the opinion of advertising experts, another quotation taken from the same writer, but in a different article, will be especially interesting. "At the present time there is no way of estimating which are the good and which are the poor advertisements except on the basis of personal judgment; and when the reviews and criticisms of different advertising men are compared, it is apparent that this personal judgment is today a very variable factor."[2]

The second quotation robs the first of whatever force it might originally have had.

[1] Strong, *Jour. of Phil., Psy., etc.*, VIII., 603, 604.

[2] Strong, *Jour. Ed. Psy.*, IV., 393.

In order to be of any particular value, the correlation between the business test and the laboratory test must be worked out with actual business returns. These are obtainable for but few kinds of commodity, since they depend upon elaborate systems of keying. In order to have the keying satisfactory, all orders must eventually come to a head office, labeled in such a way that each advertisement may receive full credit for its work. Such a thing is an obvious impossibility with such products as soaps, foods, and in general those things which are procurable at stores.

The advertisements which can be accurately keyed are ordinarily mail order propositions. With any adequate system of checking returns, it is possible to figure out from keyed advertisements the following things: the average number of inquiries per insertion, the average cost per inquiry, the total number of sales, the profit or loss. Some of these returns obviously depend upon other things than the advertisement itself, but it was the advertisement which started the whole process going.

Which of these is the fairest measure of the pulling power of the advertisement? The number of inquiries indicates the number of persons who were influenced sufficiently by the appeal to be incited to action. The weakness of this method is that the position of the advertisement on the page[1] or the position of the page[2] in the advertising section of the magazine may be detrimental. The same advertisement in some other position might have pulled many more inquiries. Again, the time of year is a very important matter. There are good seasons and bad seasons.[3] General economic conditions, national or sectional eras of prosperity are also modifying factors.

[1] Hollingworth, 'Advertising and Selling,' 80–90.

[2] Starch, 'Advertising,' 106–116.

[3] Shryer, 'Analytical Advertising,' 167–170. Shryer says, on p. 169: "As a whole, however, it may be said that the three *largest* months of practically every year are January, February and March."

See also Starch, 'Advertising,' p. 50. The table at the bottom of the page shows that, for the commodity mentioned, more advertising was run and more sales were made during the first half of the year than during the last half. Another table, given by Starch on p. 93, indicates that the most advertising is carried in May and December; the least in January and August.

It seems obvious that the natural procedure in such cases would be to repeat the advertisement enough times in different parts of the magazine and at different times selected to take account of seasonal differences and so on. The objection is that with successive appearances of the advertisement there is a fairly constant and regular decrease in the number of inquiries.[1] However, if enough advertisements were used in this way, either the total or the average number of inquiries would be a sufficiently satisfactory measure of the pulling power of the advertisement. It is, in fact, the only obtainable measure of the pulling power uncomplicated by other factors.

A second possibility is the average cost per inquiry. This method is open to all of the objections noted above, and to a still further one. The actual cost of the space occupied by the advertisement does not in any way directly effect the excellence of the advertisement itself. Even in the same medium, the charge per page is liable to sudden shifts. It is unfair to the advertisement to make it suffer the handicap of the increased rate. The amount charged per page is not an accurate measurement of the circulation of the medium and so an approximation of the number of persons who may read the advertisement.

The number of sales is obviously unfair, for we have to do there not only with the advertisement itself, but with the goodness or badness of the follow-up system, the efficiency of salesmen, etc. Some of the blame may be laid to the advertisement for it may have been constructed in such a way as to have interested many who could not possibly have bought that line of goods. Or they have been misled by the advertisement and when they found out what the product was from the follow-up system, they lost interest.

The question of profit or loss resulting from the use of a certain advertisement, while of considerable interest to the business man, is still not a test of the pulling power of the advertisement, but is a measure of the pulling power as modi-

[1] Shryer, 'Analytical Advertising,' 81 ff., 220–223. Starch, 'Advertising,' 170–179. Hollingworth, 'Advertising and Selling,' 235. Strong, Psy. Rev., XXI., 147.

fied by the cost of the advertisement and the adequacy of the follow-up system.

Taking it all in all, the average number of inquiries per insertion seems to be the fairest test of the actual pulling power of the advertisement. It is, then, the measurement which should be used in endeavoring to obtain the correlation between the orders of the business test and the laboratory test.

Another criticism of the order of merit method as it has often been used is on the ground of the number of subjects employed or the number of tests made. Obviously, if relatively few additional tests will change the order of the advertisements in the series, the experiment is unfinished.

From the experiments discussed above, it appears that the number of tests necessary depends upon at least two factors. In the first place, the actual amount of difference in terms of judgment steps between the contiguous advertisements in the series is an important consideration. With advertisements far apart, where the judgment is easy to make, the order will be established with relatively few subjects. But, as the judgments become more and more difficult, an increasing number of tests will be necessary. Secondly, the number of advertisements in the series will be a determining factor. For as we increase the number of advertisements in the series, we ordinarily must necessarily decrease the judgment steps, thus rendering a satisfactory arrangement more difficult.

Shryer[1] who was the first to use any considerable number of persons in an advertising experiment, employed a total of 508 in his efforts to reach practical certainty. In the most complex of his experiments, in which the method of paired comparisons was used, the final order was obtained at the 300th trial. The addition of 200 more subjects left the relative order of the advertisements in the series unchanged. In this experiment he used but five different advertisements. Had he used more than five, ten for example, he probably would have had to employ a great many more individuals

[1] Shryer, *System*, XXV., 146.

before obtaining a satisfactory final order. To be sure, his material was such that there was a great chance for variability of response, but this is true of practically all experiments carried on in the field of advertising.

Since Shryer, in his experiment, used the method of paired comparisons, his results are not strictly applicable here. The experiments which have been described in this paper indicate something about the number of tests necessary. With four advertisements in the series, the final order was determined with 30 trials; with ten advertisements, the final order was determined with 120 trials. With three other sets of ten advertisements each, which were used to test a different point, a satisfactory final order was not obtained with 100 trials. Until enough data have been obtained to work out a satisfactory mathematical law, it seems that one of the tests of an adequate number of subjects is the purely practical one which was given in the first part of this paper. Enough has been said, however, to indicate that in the majority of tests, an insufficient number of subjects has been used.

The next point to be considered is whether the order of merit method can be used to determine the relative pulling power of a series of advertisements. Before considering this point theoretically, we may repeat that the experiments which have been designed and carried on to test the correlation between the laboratory and the business test have sometimes shown correlations as high as plus 1.00 and sometimes as low as − 0.60. It would seem, then, that sometimes the method will work and sometimes it will not.

The instructions usually given in the experiment are, "Sort these advertisements according to the order in which *you* would *buy* the . . . ." That means that every individual who performs the experiment makes a definite arrangement of the advertisements, the order showing the persuasiveness as far as he is concerned. The assumption is that from his arrangement of the advertisements, it is possible to tell which one made him buy the article, for each one experimented upon is evidently regarded as a purchaser. There is, unfortunately, no way of telling which of the persons experimented upon would, in actual life, be sufficiently interested in any of the advertisements in the series to make him purchase the commodity.

In business, the situation is quite different. The following figures, taken from Shryer,[1] will point out what is likely to happen in a mail-order business. "Let us assume a circulation of 100,000 at $100 a page—an honest rate. Let us use a page of the strongest copy, yielding inquiries at 10 cents. Let us assume a selling average of 20 per cent., just double the ordinary. We therefore secure 1,000 inquiries. We therefore sell 200 of the 100,000 or one-fifth of 1 per cent. . . . These figures are assumed figures, but they represent the outside limits of actual average results." These figures indicate that on the average the inquiries are 1 per cent. of the circulation of the magazine. It has been estimated that five persons read each magazine. There is, then, a possibility that the advertisement will be seen by 500,000 persons. The estimate of the number of persons who see the advertisements varies from 10 per cent. to 50 per cent. of the readers of the magazine. If we take the lower limit, 10 per cent., that means that 50,000 will see some of the advertisements. The proportion which will see a particular advertisement is pure guess work. As a working basis, we will take 20 per cent. That means that 10,000 will see the advertisement, and a thousand will be sufficiently interested in it to reply, or 10 per cent. A great many of the other 90 per cent. who do not inquire are almost interested enough to do so, still more are slightly interested, others are indifferent, while still others get a negative reaction. Therefore, the results obtained from the mail-order business test are got from a very small percentage of the total number of readers. The results obtained from the laboratory test are arrived at by using the results of the whole 100 per cent. of readers, instead of the 10 per cent. who would on the average be interested enough to answer the advertisement. The using of the other 90 per cent. of the persons introduces factors into the experiment which would quite certainly modify the results so that they

[1] Shryer, 'Advertising and Selling,' XXII., 24.

would not adequately express the normal results for the 10 per cent. If we only had some way of determining, in our laboratory experiments, the individuals who make up the 10 per cent. who are sufficiently interested, we probably could arrive at fairly dependable results.

It must be kept in mind that a mail-order business appeals to a very small number of persons at best. The same general situation exists, also, with regard to the more expensive commodities, such as pianos and vacuum cleaners. Such advertisements certainly appeal to a very small and select class. Consequently, it is doubtful if adequate experiments could be performed upon advertisements of these commodities in the laboratory. The cheaper, more frequently used goods, such as foods, soaps, etc., very probably could be tested adequately if there were any way of determining accurately the actual business returns.

Lastly, it is extremely doubtful if the great majority of individuals can tell which of a series of advertisements would be most likely to make them buy the advertised product. It is very much like asking a man what he would do if his house burned up in the night. The measurement of impressions in relative terms offers considerably less difficulty, as has been demonstrated in the experimental work upon sensation, esthetic judgments, and so on. Predicting probable conduct is a much more hazardous matter. It is extremely improbable that we can really tell what we will do under a hypothetical condition unless we have developed a very definite habit for meeting that situation. Then the chances are that we will have two or more habits which are about equally serviceable. Unfortunately for the advertiser, a considerable percentage of the readers of advertisements have formed the habit of appreciating advertisements and seldom if ever responding.

The reading of advertisements has become a fixed habit with many persons, not because they expect to buy anything, but because the advertisements are an essential part of the enjoyable features of the magazine. They are looked at for esthetic appreciation, they are looked at for news value, for they give information concerning the industrial activities of the country which could never be found in the body of the magazine.

The general conclusion which we seem forced to accept is that the order of merit test is not a very adequate laboratory method for testing the business value of advertisements. Where it is possible to obtain accurate business measurements, the laboratory test, using students as subjects, appears to be quite inadequate. Where it is impossible to secure accurate business measurements, the laboratory test may be adequate. There is no way of telling.

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REACTIONS TO THE CESSATION OF STIMULI AND THEIR NERVOUS MECHANISM

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The study of reactions to the cessation of stimuli, although hitherto largely neglected, nevertheless presents a number of points of genuine scientific interest. Reactions to the cessation of stimuli, or cessation reactions, are the same as regards the reaction movement as ordinary reactions to the beginning of stimuli, or beginning reactions. While, in the case of beginning reactions, we have energy acting upon some receptor of the individual's nervous system, and producing a motor response, in the case of cessation reactions, we have merely the discontinuance of energy, which, on the reflex theory, would readily account for the discontinuance of the motor response. But reference to more complex processes than mere reflex conduction is necessary in order to understand how the mere discontinuance of energy can produce a positive reaction similar to any produced by the application of energy; and yet, if cessation reactions are found to be as quick as beginning reactions, the same complexity of mechanism must be presumed for both.

Another point of interest, in connection with cessation reactions, is the question whether the relation of intensity of stimulus to reaction time is the same as in beginning reactions, where the times are longer with weak stimuli than with strong. This relation might be reversed if the reason for the long reaction times with the weak stimuli was that in such cases a weak nervous current was meeting with great resistance which it took a long time to overcome: for then the cessation of the weak nervous current could not also meet with great resistance, but, on the other hand, the weaker the current, the more its cessation would be favored.

That cessation reactions may throw some light upon the explanation of the differences in reaction time which occur with variation in the mode of stimulus has already been recognized.[1] The fact, that with moderate intensities, the reaction time to sound is ordinarily shorter than that to light is commonly explained on the ground that the stimulation of the ear by sound, a mechanical process, takes less time than the stimulation of the eye by light, a chemical process. That this explanation is as yet a matter of speculation must be admitted, as has been pointed out by Dunlap and Wells, who instigated a series of experiments designed to afford a more satisfactory explanation.[2] Wells, with this same problem in mind, conducted some experiments in which the reactions occurred upon the disappearance or occlusion of the stimulus. That is, the stimulus consisted in darkness, preceded and followed by illumination, with the subject reacting at the moment the darkness appeared. From his experiments, he concluded that the lag in the sensory process in the case of vision cannot account for as much as 10 σ of the lengthening of the visual reaction time beyond that of the auditory. The argument, by which he arrives at this conclusion, need not here be analyzed, as it is not based so much upon the reaction times to the disappearance of the stimulus, as upon the fact that an interruption of the light for only 10 σ was found to be plainly perceivable.[3] As regards the reaction time to the disappearance of the light stimulus, Wells concludes that it differs little from that to the appearance of the light. This conclusion is supported by a large number of sensory reactions, and is in conformity with that indicated by the data presented below.

Another question concerning cessation reactions is whether the after-image has anything to do with determining the

[1] Wells, G. R., 'The Influence of Stimulus Duration on Reaction Time,' Psychol. Monog., 5, 1913.

[2] 'Some Experiments with Reactions to Visual and Auditory Stimuli,' Psychol. Rev., 1910, 319.

[3] *Op. cit.*, 65.

reaction time. Many other questions also suggest themselves, but the problems that have been referred to are sufficient to indicate the nature of the scientific interest of the present investigation.[1]

In the case of all the reactions here reported, the instructions called for a reaction which would ordinarily be regarded as a form of "motor" reaction. Reactions are usually divided into two main groups, those in which, during the preparatory interval, the attention is mainly directed to the stimulus, called sensory, and those in which it is focused primarily upon the reaction movement, called motor. A more important distinction, however, at least from the point of view of reaction *time*, is that between reactions with instruction to react as quickly as possible, and reactions without such instruction.[2] In the present instance, the subjects were always instructed to make every reaction as quickly as they possibly could. Nothing was said about the direction of attention, but the subjects' introspections indicated that their attention was, primarily, neither upon the stimulus or its image, nor upon the reaction movement or its image, but upon the idea of reacting as quickly as possible. Just how this idea was carried in the subjects' minds, that is, in what imagery, or whether in any imagery, I am unable to conclude from the introspective data. No attempt was made at elaborate systematic introspection.

Apparatus.—The reaction times were measured by means of a Hipp's chronoscope. The chronoscope circuit and the stimulus circuit were separate, but by means of a double switch both circuits could be closed simultaneously. The stimulus, for light reactions, consisted in the illumination of a Geissler's tube, while for sound reactions, the stimulus was the vibration of a telephone receiver. For beginning reactions, both the chronoscope and stimulus circuits were closed at exactly the same instant by means of the double switch. Each side of this switch was provided with a platinum wire which sank into an adjustable cup of mercury when the experimenter tapped upon the switch handle. For cessation reactions, the stimulus circuit was arranged so that it was closed when the handle of the switch was raised, and broken when the handle was tapped down. Thus, in this case, the chronoscope circuit was closed as the stimulus circuit was broken, though there was an exceedingly slight time between the breaking of the latter and the closing of the former. This time, when measured, was found to

[1] The general subject of cessation reactions was first suggested to me by Professor H. C. Warren of Princeton, in 1907. The results here reported on sound were presented in full in a paper read in April, 1912, before the Minnesota Psychological Conference.

[2] See Woodrow, 'The Measurement of Attention,' Psychol. Monog., 1915, Chap. 11.

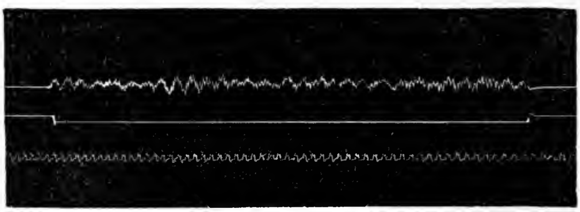
vary from 0 to 3 σ, but is regarded as a constant error of 2 σ, and added to all averages of cessation reaction times.

The chronoscope circuit included besides the chronoscope and half of the double switch already mentioned, the subject's reaction key, a battery of Edison primary cells of 12 volts, and a Wundt's fall-hammer. The chronoscope was controlled before and after each 50 reactions, by means of the fall-hammer, and the fall-hammer itself was tested each day with a 250 d.v. fork.

The stimulus circuit, when arranged for light reactions, included, as already stated, a Geissler's tube. This tube was suspended before an oblong aperture in a dark box, a few feet in front of and slightly below the subject's eyes, as he sat in the dark-room at his reaction key. The Geissler's tube was actuated by an inductorium placed in the experimenter's room. The buzzer of the inductorium was kept going continuously during the experiment, but only the secondary circuit, which included the Geissler's tube, passed through the operator's double switch, and so the tube was luminous only when the secondary circuit was closed at this switch. In order to weaken the intensity of the stimulus, no change was made in the electrical circuits, but a ground glass covered with white paper was placed over the aperture of the dark box containing the Geissler's tube.

In the case of sound reactions, the sound used as a stimulus was that made by a telephone receiver, through which there passed a current which was interrupted both 50 and 250 times per second, by two electro-magnetic tuning forks. All the current passed through the forks, but the telephone was in a shunt circuit. Consequently, the forks ran continuously, whereas the telephone ran only while its circuit was closed at the experimenter's double switch. By decreasing the resistance in parallel with the telephone, the loudness of the telephone could be decreased.

The reason for running the telephone current through both a 50 and a 250 fork was that only in this way could the click which followed the break of the current be eliminated. This method is noted by Pillsbury in his discussion of 'Methods for the Determination of the Intensity of Sound,'[1] and is one that I have long employed.[2] Records of the vibration of the telephone plate were obtained by attaching a light glass pointer to the plate and having this pointer mark upon a smoked drum alongside a Pfeil time marker placed in the chronoscope circuit. A sample of these records is here reproduced.



The vibrations of the 250 fork are seen superimposed upon those of the 50 fork. The record shows that the sound started in at its maximum intensity with the first

[1] 'Report of the Committee of the American Psychological Association on the Standardizing of the Procedure in Experimental Tests,' Psychol. Monog., **1**, 1910. Pillsbury writes as follows: "Dr. Shepard, working in my laboratory, found that the click could be lessened to a point of not being noticed if two tones were superimposed upon the telephone. He used the commercial current of 60 cycles and a 250 d.v. fork. The physical basis for the effect is obscure, but the empirical effect is obvious."

[2] See 'A Quantitative Study of Rhythm,' *Archives of Psychol.*, **12**, 1909.

vibration of the telephone plate, and that it stopped as suddenly as it began. It will be noted that no large vibrations follow the breaking of the circuit as they would if there were a click. A number of careful observers agreed that there was no noticeable click at either the closing or the opening of the circuit, thus confirming the objective record.

Sound reactions were taken with three different intensities of stimulus, called medium, weak and liminal. Physical measurements of the intensity were not made, but care was taken to keep the current the same throughout the work with any one intensity. The reaction times themselves offer a sufficient index of the intensity. The intensity given in Table I. as liminal, in reality exceeded the true limen by an extremely small amount. It was determined as follows: Four series of minimal changes in intensity were used, two of which passed from an audible to an inaudible sound and two from an inaudible to an audible one. This procedure was just what one might employ to determine the sensation threshold. The sound was then placed not at the average liminal intensity obtained, but at the lowest intensity which did not fail to produce a sensation in any of the four series.

Since an object of the present study is a comparison of cessation reactions with beginning reactions, both kinds of reactions were always taken at each sitting of one hundred reactions. On alternate days the middle fifty reactions were cessation reactions and the first and last twenty-five were beginning reactions; while on the remaining days the middle fifty were beginning reactions and the first and last twenty-five were cessation reactions. In the tables below, averages for beginning and cessation reactions given on the same line of the table were obtained at the same sitting. Each average is the average of fifty reactions. No reactions were thrown out on account of their deviation from the average, except in the case of the liminal sound. However, all reactions which, by a prearranged signal, the subject indicated were mistakes, as well as all cases of error of manipulation on the part of the experimenter, were rejected. In the case of the liminal sound there were a number of cases where no reaction at all occurred and some that were very long, from 1,500 to 5,000 σ. These long reactions occurred both in the case of beginning and cessation reactions, but all reactions over 1,500 σ were not counted. The total number of reactions to sound and light stimuli reported in the following tables is 10,000.

As a warning signal, in the case of beginning reactions the click of an electric sound-hammer was used, and given always two seconds before the reaction stimulus, the interval being indicated to the experimenter by means of a pendulum. In the case of cessation reactions the beginning of the stimulus itself acted as the warning signal. The cessation of the stimulus always occurred two seconds after its beginning.

Five subjects were used, three of which, subjects *Ww*, *Ht*, and *St* were quite practiced in beginning reactions before the work began, while the other two, subjects *Vs* and *Sz*, were altogether unfamiliar with work in reaction time.

The results obtained for reactions to sound and to light are presented in Tables I., II. and III. These tables show the results of each sitting in the order in which they were obtained. The results are summarized in Table IV. In this latter table, the mean variation given is the average mean variation for a series of fifty measurements.

The conclusion to be drawn is clear and simple. In the case of sound and light reactions, there is no appreciable difference between reaction time to the beginning of a stimulus and reaction time to its cessation, no matter what the intensity of the stimulus, and no matter what its mode. In view of the variability in reaction times, such a close correspondence between the cessation and the beginning reaction times as shown by Tables I. to IV. could not be regarded as a reasonable expectation unless the true values of both were in all cases substantially the same.

It is to be regretted that results with other senses than light and sound were not obtained. Difficulties of technique were largely responsible for limitation of the work to these modalities. Some reactions to touch were taken, however, using an intermittent current passing through two fingers of the left hand, each finger being placed in a salt-water electrode; but so much difficulty was experienced as a result of sensory adaptation and the after-tingling of the fingers, that

TABLE I

SOUND REACTIONS

N, for each av. = 50. Total No. of reactions = 4,000.

Intensity	Subj.	Series	Beginning Reactions		Cessation Reactions	
			Av.	*M. V.*	Av.	*M. V.*
Medium..........	*Ht*	I.	115	*11*	121	*15*
"	"	II.	113	*19*	125	*15*
"	"	III.	121	*20*	128	*17*
"	"	IV.	136	*20*	127	*15*
"	"	V.	123	*13*	115	*15*
"	"	VI.	116	*14*	121	*11*
"	"	VII.	106	*12*	113	*15*
"	"	VIII.	122	*15*	120	*13*
"	"	Av.	**119**	**16**	**121**	**15**
"	*Vs*	I.	143	*17*	144	*16*
"	"	II.	123	*18*	142	*20*
"	"	III.	141	*18*	155	*18*
"	"	IV.	135	*15*	147	*21*
"	"	V.	140	*20*	136	*14*
"	"	VI.	145	*12*	144	*17*
"	"	VII.	132	*15*	138	*16*
"	"	VIII.	137	*17*	140	*14*
"	"	Av.	**137**	**17**	**143**	**17**
"	*Sz*	I.	146	*20*	157	*24*
"	"	II.	143	*17*	155	*12*
"	"	III.	162	*20*	143	*22*
"	"	IV.	137	*23*	151	*18*
"	"	V.	140	*14*	152	*23*
"	"	VI.	154	*22*	136	*17*
"	"	VII.	147	*26*	147	*25*
"	"	VIII.	157	*14*	144	*25*
"	"	Av.	**148**	**19**	**148**	**21**
Weak............	*Ht*	I.	172	*34*	170	*44*
"	"	II.	202	*27*	190	*26*
"	"	III.	175	*18*	170	*27*
"	"	IV.	185	*24*	201	*36*
"	"	Av.	**184**	**26**	**183**	**33**
"	*Vs*	I.	188	*18*	186	*20*
"	"	II.	196	*28*	155	*32*
"	"	III.	160	*17*	155	*20*
"	"	IV.	150	*18*	171	*17*
"	"	Av.	**174**	**20**	**167**	**22**
"	*Sz*	I.	215	*31*	226	*35*
"	"	II.	198	*34*	204	*27*
"	"	III.	222	*31*	223	*40*
"	"	IV.	201	*36*	218	*29*
"	"	Av.	**209**	**33**	**218**	**33**
Liminal..........	*Ht*	I.	829	*146*	682	*119*
"	"	II.	728	*114*	807	*147*
"	"	Av.	**779**	**130**	**745**	**133**
"	*Vs*	I.	755	*137*	735	*177*
"	"	II.	995	*315*	909	*359*
"	"	Av.	**875**	**226**	**822**	**268**

Table II

Reactions to Bright Light

N, for each av. = 50. Total No. of reactions = 3,600.

Subj.	Series	Beginning Reactions		Cessation Reactions	
		Av.	*M. V.*	Av.	*M. V.*
Ww	I.	160	15	152	17
"	II.	142	11	147	16
"	III.	154	13	160	16
"	IV.	140	17	152	21
"	V.	169	19	153	13
"	VI.	168	24	144	13
"	VII.	159	17	145	16
"	VIII.	140	15	157	14
"	Av.	**154**	**16**	**151**	**16**
Ht	I.	156	18	169	18
"	II.	163	25	158	25
"	III.	168	19	171	12
"	IV.	160	21	171	15
"	Av.	**162**	**21**	**167**	**18**
St	I.	190	17	187	20
"	II.	192	22	190	13
"	III.	187	15	180	20
"	IV.	178	21	192	16
"	V.	187	22	176	26
"	VI.	178	19	187	16
"	VII.	176	11	170	22
"	VIII.	178	28	193	15
"	Av.	**183**	**21**	**184**	**19**
Vs	I.	200	24	196	21
"	II.	207	22	177	12
"	III.	190	18	180	13
"	IV.	193	14	193	18
"	V.	194	17	192	19
"	VI.	179	20	178	16
"	VII.	186	18	180	16
"	VIII.	186	21	184	20
"	Av.	**192**	**19**	**185**	**17**
Sz	I.	196	13	200	23
"	II.	200	23	193	27
"	III.	207	21	193	18
"	IV.	177	23	201	14
"	V.	191	25	200	22
"	VI.	199	21	200	23
"	VII.	216	28	205	23
"	VIII.	221	24	212	26
"	Av.	**201**	**22**	**201**	**22**

the results cannot be regarded as reliable. The few that were obtained showed a marked prolongation in reaction time with decrease in the intensity of the stimulus, both with beginning and cessation reactions. The cessation reactions, however, were uniformly about 30 σ longer than the beginning reactions, which fact may be attributed to the after-tingling of the fingers set up by the irritation of the electric current. Some experiments were also tried, in which the stimulus consisted in the fall and rise of an electric hammer arranged to strike the back of the finger. In this case, while the stimulus was adequate, one could not be certain that the

TABLE III

REACTIONS TO WEAK LIGHT

N, for each av. = 50. Total No. of reactions = 2,400.

Subj.	Series	Beginning Reactions		Cessation Reactions	
		Av.	*M. V.*	Av.	*M. V.*
Ht	I.	179	*35*	192	*28*
"	II.	196	*44*	178	*30*
"	III.	211	*37*	205	*26*
"	IV.	216	*34*	214	*22*
"	V.	204	*20*	194	*18*
"	VI.	221	*32*	217	*24*
"	VII.	211	*28*	217	*32*
"	VIII.	201	*33*	209	*28*
"	Av.	**205**	**33**	**203**	**24**
Vs	I.	221	*25*	230	*21*
"	II.	217	*23*	222	*15*
"	III.	252	*43*	214	*21*
"	IV.	240	*18*	234	*30*
"	V.	244	*22*	255	*21*
"	VI.	258	*29*	229	*18*
"	VII.	269	*19*	241	*22*
"	VIII.	241	*28*	243	*17*
"	Av.	**243**	**26**	**234**	**21**
Sz	I.	254	*27*	244	*15*
"	II.	239	*33*	250	*22*
"	III.	269	*30*	252	*31*
"	IV.	296	*22*	269	*29*
"	V.	284	*32*	268	*30*
"	VI.	258	*26*	255	*34*
"	VII.	264	*24*	228	*21*
"	VIII.	280	*27*	230	*23*
"	Av.	**268**	**28**	**250**	**26**

removal of the pressure was a change equivalent in intensity to its occurrence; but the results obtained in this case were to the effect that the beginning and cessation reaction times are equal. Thus, with one subject, the average reaction time for 100 reactions to the beginning of the stimulus, was 119 σ and for 100 reactions to its removal, 120 σ. With another subject, the average for 100 beginning reactions was 128 σ and for 100 cessation reactions with the same stimulus, 132 σ.

The simplicity of the conclusions drawn above from the results presented in Tables I. to IV. should not blind us to the important bearing that they have on current theories of the action of the nervous system. The explanation of the results here presented, by means of the theories of nervous action that are at present most in vogue, is extremely difficult,—so difficult, in fact, as to suggest that these theories are themselves incorrect, and that they must either be given

Table IV

A Summary of Tables I., II. and III

Mode	Intensity	Subj.	Beginning Reactions		Cessation Reactions	
			Av.	*Av. M. V.*	Av.	*Av. M. V.*
Sound	Medium	*Ht*	119	*16*	121	*15*
"	"	*Vs*	137	*17*	143	*17*
"	"	*Sz*	148	*19*	148	*21*
"	Weak	*Ht*	184	*26*	183	*33*
"	"	*Vs*	174	*20*	167	*22*
"	"	*Sz*	209	*33*	218	*33*
"	Liminal	*Ht*	779	*130*	745	*133*
"	"	*Vs*	875	*226*	822	*168*
Light	Bright	*Ww*	154	*16*	151	*16*
"	"	*Ht*	162	*21*	167	*18*
"	"	*St*	183	*21*	184	*19*
"	"	*Vs*	192	*19*	185	*17*
"	"	*Sz*	201	*22*	201	*22*
"	Weak	*Ht*	205	*33*	203	*24*
"	"	*Vs*	243	*26*	234	*21*
"	"	*Sz*	268	*28*	250	*26*

up or seriously modified. In particular, it can be shown that certain phenomena which it is customary to explain by reference to the latent period of sensory stimulation or by reference to certain hypothetical effects of the synapse, are not adequately explained thereby. Their explanation requires either a theory which is incompatible with the statement that the reflex is the type of all nervous activity, or else a theory which involves some modification of the ordinary concept of the reflex.

In the first place, let us consider the explanation of the increase in reaction time with decrease in the intensity of stimulus. Piéron,[1] in a recent able discussion of this matter, concludes that the main factor, in determining the increase in reaction time, is the increase in the latent period of excitation of the first sensory neurone with decrease in intensity of stimulation. His discussion of other possible explanatory factors, however, is unprejudiced. He correctly regards as very doubtful the proposition that the rate of transmission of nervous energy along a neurone varies with the intensity of stimulation.[2] Likewise, and with equal correctness, he rejects the idea that the effect can be explained by reference to phenomena on the motor or centrifugal side of the process.[3] He does not deny, though, that when the excitation is weakened there may be an increase in the time of transmission from one neurone to another. He thinks, however, that the variations in time, due to this last-mentioned factor, are not very considerable, and that they do not suffice to explain the variations obtained experimentally. Piéron also raises the question whether the central or brain phase is not the seat of the principal variations. "La partie la plus longue, dans cette phase, correspond à la circulation de l'influx associatif dirigé, et sa brièveté dépend surtout de l'état d'attention, c'est-à-dire de l'aiguillage préalable qui assure la conduction de cet influx par les voies les plus rapides."[4] He argues, however, that since the reactions are always accompanied by a state of intense attention, the fact that a more intense

[1] 'Recherches sur les lois de variation des temps de latence sensorielle en fonction des intensités excitatrices,' *Année psychol.*, 1914, 17–96.

[2] In confirmation, see the following: Gotch, *Journ. of Physiol.*, 1902, 395. Koike, *Ztsch. f. Biol.*, 1910, 310. Lucas, *Journ. of Physiol.*, 1911, 46. Adrian, *Ibid.*, 1912, 389; 1913, 384.

[3] In this connection, one may well cite the work of Moore, 'A Study of Reaction Time and Movement,' Psychol. Monog. Sup., 1, 1904. Moore writes, "In one and the same series the reaction time undergoes considerable change, but the movement time is fairly constant. If you introduce factors which increase the difficulty of attention, the reaction time is lengthened and rendered still more variable, but the time of movement remains about the same," p. 58. "That the efficient path from cortex to muscle is not affected by the disturbance of the attention and that reaction time is not lengthened by any changed conditions along the path, seem to be conclusions warranted by the fact of constancy in the time of the movement by which the reaction was executed," p. 59.

[4] *Op. cit.*, 73–74.

excitation better calls out attention cannot be of very great importance. Concerning his final hypothesis, that the explanation of the relation between reaction time and intensity of stimulus lies chiefly in the latent period of the sense-organ, Piéron himself admits that it is an hypothesis, the truth of which he cannot demonstrate. One of the principal arguments which he suggests in favor of his conception is the fact that the data at hand indicate that the relation between intensity of stimulus and reaction time differs in the case of different senses.

In spite of the lack of evidence in support of the conclusions of Piéron concerning the rôle of the sensory latent period, they have a certain plausibility which would no doubt lead many to agree with him. Others, however, like Sherrington, would give more stress to the rôle of the synapse. Sherrington calls attention to the fact that the latent period of reflexes, that is, the time between application of stimulus and appearance of end-effect, increases with decrease in the intensity of the stimulus.[1] Thus, he finds that the latent time of the scratch-reflex varies from 140 σ with intense stimulation to 500 σ, or even several thousand σ, with weak stimulation. He writes: "This slackening of propagation speed under weak stimuli is, I would urge, a more significant difference between reflex-conduction and nerve-trunk conduction than is the mere greater slowness of the former than the latter."[2] This difference between conduction in reflex-arcs and nerve-trunks is referable to that part of the arc which lies in the gray matter. The slower conduction in the gray matter as well as the increase in reflex time with weak intensity of stimulus is supposed to be due to phenomena of transmission occuring at the synapse, where a surface of separation acts as a resistance or barrier to the passage of the nervous current from one neurone to the next.

The weaker stimulus, then, is to be thought of as resulting in a longer reflex time because it is delayed longer in transmission from one neurone to the next, than is the more intense

[1] 'The Integrative Action of the Nervous System,' 1906, 21.

[2] *Loc. cit.*

stimulus. Sherrington, apparently, does not regard the effect of intensity on reflex time as due in any appreciable degree to the latent period of the sense-organ.

Now, while the prolongation in reflex time with decrease in the intensity of the stimulus is a different phenomenon than the prolongation in human reaction time with decrease in intensity, yet it seems highly probable that the essentials of the explanation are the same in both cases. At any rate, there is just as much evidence that the effect of intensity upon reaction time is to be explained by reference to the impeding action of the synapse, as that the effect of intensity upon reflex time is to be so explained. Fully as many synapses are involved in the voluntary reactions of human beings as in the reflexes of the dog, and any effect which results from the nature of the synapse must be present in both cases and must be explained in both cases by the action of the synapse.

The inadequacy of both the sensory latent period and the synapse theories may be shown, to a large extent, by the same argument: for after all, both theories offer much the same explanation of the prolongation in reaction time with decrease in intensity of stimulus. In both cases, the concept of resistance to be overcome is the fundamental one; in one theory, the resistance is thought of as occurring at the first sensory neurone met with by the stimulus and in the other, as occurring at every synapse. It is the same concept that is constantly used in attempts at explaining the physiological side of all manner of psychological phenomena.[1] The excitation produced by the stimulus has to exceed some minimal limit, or threshold, before it is sufficient to bring about a reaction. A small force has to act longer to produce a given effect than a large force: consequently, it is argued, the weaker the stimulus, the longer the time to produce the required degree of excitation.

Now is such an explanation as the above tenable? We

[1] Ladd and Woodworth write as follows: "In fine, it seems possible to conceive the action of the nerve-centers as a process of the transmission of nerve-impulses that is subject to the peculiarities of central conduction. Most of these peculiarities can be stated in terms of resistance—resistance in general high, but variable with many conditions." 'Elements of Physiological Psychology,' 1911, 287.

must conclude that it is not, if for no other reason than that it does not hold for cessation reactions; for, if we argue that a large resistance increases the time required for any stimulus to produce its effect, we cannot also argue that this same large resistance also increases the time required for the disappearance or the cessation of the effect. On the contrary, the disappearance of the effect should be hastened. A few analogies will make this clear. If we make a slight impression on the skin, it will disappear, or fall below a prescribed threshold, quicker than if it is deep. The after-image of a bright light lasts longer than that of a faint light. A string vibrating with large amplitude requires a longer time to come to rest, or to fall below any given threshold, than one vibrating with small amplitude. The plasma membrane of a nerve cell would recover from a slight increase in permeability more quickly than from a great increase in permeability.[1] Such illustrations, which could be multiplied indefinitely, all show how improbable it is that any resistance offered by the first sensory neurone, or even by the synapse, would act in such a way that it would not only impede the action of a weak stimulus more than that of a strong, but would also impede the cessation of the action of the weak more than that of the strong.

One might object, and not without some justification, that this argument leaves out of consideration the fact that the resistance offered by the synapse may not be resistance to the rise or fall of the excitation but merely to its conduction. This resistance, it might be alleged, merely results in the weak current traveling along the nerve fibre more slowly than the strong. In all cases, the awareness of the cessation of the stimulus would be delayed by the time required for the very rearmost part of the stream of excitations produced by the stimulus to pass from the sense-organ to the cortex. Now,

[1] For an excellent discussion of the electric phenomena in nerve and muscle cells, see Höber, 'Physikalische Chemie der Zelle und der Gewöbe,' 4th ed., 1914, especially Chap. XII., 'Elektrische Vorgänge an physiologischen Membranen.' Cf. also Lillie, 'The Relation of Stimulation and Conduction in Irritable Tissues to Changes in Permeability of the Limiting Membranes,' *Amer. J. of Physiol.*, Vol. XXVIII., 1911, 197–222.

one might argue that this time would be greater for weak than for strong nervous excitations. Against this view, however, we have the results of recent physiological investigations, which show that the rate of transmission of nervous impulses is independent of their magnitude.[1] More decisive is the fact, apparently well established,[2] that the 'all-or-none' law applies to the normal nerve-fiber. This law is to the effect that any stimulus which excites a nerve-fiber at all will produce a maximal excitation. It follows that differences in intensity of excitation of sensory nerves is due to difference in the number of nerve-fibers (or conducting elements) stimulated, or else, as indicated by the results of Fröhlich,[3] to variation in the frequency of secondary excitation waves. Now, if the 'all-or-none' law be true, it is impossible for any number of synapses to delay the excitation produced by a weak stimulus more than that produced by a strong one. Consequently, the assumption of resistance offered by the synapse cannot account for the marked prolongation in either reaction time or reflex time with decrease in intensity of stimulus.

It is true that fatigued nerve-fibers do not follow the 'all-or-none' law. It might be held that, in a similar way, a sensory nerve-fiber subjected to stimuli such as light or sound would not follow this law. The high frequency of the secondary oscillations, in the case of the excitations produced in an optic nerve by a light stimulus, might result in the nerve taking on the properties of what Verworn calls an 'heterbolic' system,[4] and consequently in its showing variation in magnitude of excitation with variation in intensity of stimulus. However this may be, it is very improbable that such differences in intensity of excitation of single nerve-fibers (assuming them to exist) could account for the increase

[1] In confirmation, see the following: Gotch, *Journ. of Physiol.*, 1902, 395. Koike, *Ztsch. f. Biol.*, 1910, 310. Lucas, *Journ. of Physiol.*, 1911, 46. Adrian, *Ibid.*, 1912, 389; 1913, 384.

[2] Gotch, *Journ. of Physiol.*, 1902, 392. Symes and Veley, *Proc. Roy. Soc.*, B. lxxxiii., 1910, 431. Verworn, *Ztsch. f. allgm. Physiol.*, 1912, 277. Veszi, *Ztsch. f. allgm. Physiol.*, 1912, 321. Adrian, *Journ. of Physiol.*, 1913, 389. Lodholtz, *Ztsch. f. allgm. Physiol.*, 1913, 269. Lillie, *Amer. Journ. of Physiol.*, Vol. XXXIV., 1914, 410.

[3] *Ztsch. f. Sinnesphysiol.*, Vol. XLVIII., 1914, 28–165.

[4] *Ztsch. f. allgm. Physiol.*, 1912, 289.

in time of cessation reactions with decrease in intensity of stimulus. This becomes very evident from a consideration of reactions to a decrease in intensity, of which, after all, cessation reactions are merely a limiting case.

The assumption of greater slowness of conduction of weak excitations than of strong, even were it warranted, would not account for the fact[1] that a slight decrease in intensity of a strong stimulus results in a longer reaction time than a larger decrease in the same stimulus: for it cannot be held that a slight decrease in excitation is conducted more slowly than a large decrease. The decrease itself is not conducted at all, but merely the excitation as it exists before and after decrease. It may be urged that this argument is not pertinent, but I think there is little doubt that just as the reaction time to the beginning of a stimulus equals that to its cessation, so does the reaction time to an increase in intensity equal that to a decrease of the same size. We know, at least, that the reaction time, whether the reaction is to an increase or a decrease in intensity, is lengthened as the size of the change in intensity is decreased. Reactions to a decrease in intensity should, then, be considered along with cessation reactions.

Another point to be considered, in addition to the effect of intensity of stimulus upon reaction time, is the difference in reaction time between sound and light. Reactions to a moderately bright light are longer than those to a moderately loud sound. This fact is not uncommonly explained by assuming that the latent period of stimulation of the sense-organ is longer in the case of light.[2] Against this assumption, we may urge the following considerations:

First, since cessation reaction times are the same as beginning reaction times in both sound and light, we would have to assume that in both cases the latent period for the

[1] See Woodrow, 'The Measurement of Attention,' Psychol. Monog., 1915, Chap. IV.

[2] Ladd and Woodworth write as follows: ". . . there seems good reason to suppose that the reaction time of sight is necessarily longer than that of hearing or touch, on account of the photochemical nature of its more immediate stimulus." "On the whole, the suggestion which probably is most generally entertained . . . is that already adopted by us,—namely, that the inertia or latent time of different sense-organs differs." 'Elements of Physiological Psychology,' 1911, 472.

dying out of the excitation just equalled that for its rise, that is, not only assume that the photochemical process was longer in getting started but also equally longer in dying down. Second, if we assumed any considerable latent period of stimulation, it would seem reasonable to suppose that such latent period would increase markedly with weakened intensity. But as has been pointed out in preceding sections, such an increase in latent period is incompatible with the fact that reactions to the cessation of a very weak stimulus are longer than to the cessation of a strong one. At any rate, the effect of intensity, which cannot be explained by reference to the latent period, is sufficient to completely overwhelm the effect of the latent period, since a very weak sound may give much longer reaction times than a very bright light. Third, in the case of reflexes, we know that different reflexes may vary greatly in time even when they are elicited by direct stimulation of the sensory nerve, so that the latent period of stimulation of the sense-organ is not involved. Similarly, a great variation in the time of different reflexes occurs where the sense-organ stimulated remains the same. For example, the latent time of the scratch-reflex is, on the average, very much longer than that of the flexion-reflex of the same limb, although the distance of nerve fiber conduction is not greater.[1] Fourth, and lastly, it can be shown, as will be pointed out below, that the degree of attention is less with light reactions than with sound, and that this fact offers an intelligible explanation of the variation in reaction time with mode of stimulus. It is true that reflexes as a rule do not involve attention, but since the work of Sherrington, showing the important rôle of the central nervous system in determining the characteristics of reflexes, it may very well be assumed that a nervous mechanism analogous to the nervous mechanism of attention, though less complex and without conscious accompaniments, is involved in every reflex.[2]

One other point may be mentioned as one which should be

[1] Sherrington, *op. cit.*, p. 21.

[2] "The interference of unlike reflexes and the alliance of like reflexes in their action upon their common paths seems to lie at the very root of the great psychical process of 'attention.'" Sherrington, *op. cit.*, 234.

illuminated by knowledge of cessation reactions. I refer to the long time required by a weak intensity of stimulus to produce its maximal intensity of sensation. The data here presented do not deal exactly with this point, but the experiment with the liminal sound as stimulus is of interest in this connection. It is known that a sound of constant physical intensity increases in apparent intensity up to a duration, in the case of weak sounds, of 1.5 secs.[1] It ought, then, to be possible, by using a sound so weak as to be inaudible until nearly its maximum effect is produced, to obtain a sound so weak that it could not be heard at all for a large fraction of a second. The reaction time I actually obtained for such a sound was about three fourths of a second,[2] and the reaction time to its cessation was fully as long. In the case of a liminal sound such as here used, there can be no doubt that the reaction does not occur until the subject is aware, in the case of beginning reactions, of the presence of the sound, and in the case of cessation reactions, of its absence. It is well known, apart from the present data, that it takes longer to become aware of a weak stimulus than of a strong.[3] It may be said, then, that the very long reaction time in the case of beginning reactions shows how very long it takes to become aware of a liminal stimulus. Moreover, in view of the approximate equality of the beginning and cessation reaction times, it may be concluded that just as it takes longer to become aware of a weak stimulus than of a strong, so does it take longer, and about equally longer, to become aware of the cessation of a weak stimulus than of a strong.

Here again, we may raise the question of explanation by resistance. And there can be little doubt that the explanation of the period required for the rise of the excitation up to the awareness threshold is closely connected, if not essentially

[1] Kafka, 'Ueber das Ansteigen der Tonerregung,' *Psychol. Stud.*, 1907, 256–292. See also Sander, 'Das Ansteigen der Schallerregung bei Tönen verschiedener Höhe,' *Psychol. Stud.*, 1910, 1–38.

[2] The great discrepancy between this value and the corresponding values obtained by Wundt (337 σ) and by Piéron (361 σ) is probably due to the stricter insistence upon liminal intensity of stimulus in the present instance.

[3] See Minneman, 'Untersuchungen über die Differenz der Wahrnehmungsgeschwindigkeit von Licht und Schallreizen,' *Psychol. Stud.*, 1911, 1–82.

identical, with the explanation of the period required for the further rise to a maximum. In the explanation by resistance, in the present instance, the resistance would probably be thought of as at the synapse; but no mode of behavior on the part of the synapse has yet been suggested which would explain how it can offer resistance both to the rise and to the fall of the excitatory process. The cessation of the excitation is not a new excitation as we know from the introspection that the sound sensation merely ceases,—no new sensation is set up, when the stimulus is cut off. So it is evident that neither the long period required for the rise of the excitation up to the sensation or awareness threshold, nor, in all probability, the further rise to a maximum, can be explained by reference to the concept of a surface of separation at the synapse or to any other form of resistance to conduction along nervous arcs.

A consideration of the facts of cessation reactions, then, leads to certain negative conclusions concerning the explanatory value of the concept of resistance, whether this resistance is placed at the point of stimulation, at the synapse, or in the whole neurone. Such resistance cannot explain the fact that cessation reactions to a weak stimulus are longer than to a strong one, but on the other hand would tend to cause us to expect the reverse. Moreover, since both cessation and beginning reactions follow the same law as regards variation in time with variation in intensity of stimulus, we should expect the same explanation in both cases; and so, if resistance to conduction does not explain the one, it probably does not explain the other. Again, as regards the difference in reaction time between sound and light, this, likewise, does not seem explicable by reference to the greater resistance of the retina to stimulation. Further, since it takes as long to become aware of the cessation as of the beginning of a very weak sound, the long time required for the awareness of either cannot be due to resistance to conduction. And, moreover, this latter fact renders it probable that such resistance has nothing to do with the explanation of the long time required for a weak stimulus to produce its maximal sensory effect.

While the facts of cessation reactions thus serve to call attention to the shortcomings in the explanation of the behavior of the nervous system by means of the concept of resistance, it must be admitted that there exists a large accumulation of other facts which likewise greatly impair the plausibility of such explanation. Among this mass of evidence, we may cite the great disproportion and lack of correlation between energy of stimulus and energy of response. Even more conclusive is the temporal discrepancy. This discrepancy is especially striking in cases where psychocerebral processes are involved, but it is very evident even in reflexes, in the phenomenon of the after-discharge. Concerning this phenomenon, Sherrington writes as follows: "Tetanic contraction of the knee-flexor muscles of the dog induced by brief faradization of the motor-nerve usually ceases within 150 σ of the cessation of the stimulation of the nerve, if crude condition of fatigue, etc., be avoided. The contraction of these same muscles, when induced reflexly by a similar brief stimulation, often persists for 5,000 σ after cessation of the stimulus.[1] Now this great duration of the after-discharge certainly cannot be explained by resistance offered at the synapse; and it is equally certain, that resistance at the synapse cannot explain the relation between intensity of stimulus and *both* duration of after-discharge and reflex latent time, since the duration of the after-discharge *increases* with intensity of stimulus, while the length of reflex time *decreases*. It would be easy to continue, indefinitely, these instances of the unsatisfactory nature of the theory that the action of the nervous system is explicable merely by the assumption of a network of reflex paths, which offers more or less resistance to the passage of currents from the sense-organs to the muscle. However, the following two citations will suffice to indicate the dissatisfaction with such a theory.

Titchener writes as follows: "The assumption that the reflex arc is the unit of nerve function evidently makes the brain nothing more, in principle, than a mass of superposed reflex arcs; the central is assimilated to the peripheral mechanism; the office of the brain is to perceive, to couple up, and

[1] *Op. cit.*, 26.

to send out. But this view that the nervous system is a system of conduction, a sort of glorified telephone exchange, is in the author's opinion wholly inadequate to explain the phenomena of mind. The theory of conduction, with obstacles or easements between cell and cell, must, he believes, be replaced by a theory of intracellular change, or change within the cell-body; and if this is the case, the cortex must be regarded rather as a disjunction of the reflex arc than as a switchboard for the manifold connection of afferent with efferent process."[1]

Reference may be made also to the views of the physiologist, T. Graham Brown. From experiments which demonstrated that the phenomenon of "narcosis progression" in the cat may occur at a depth of narcosis at which the spinal reflexes are abolished, and from a general consideration of the facts of rhythmic motor phenomena, Brown concludes: "The fundamental unit of activity in the nervous system is not that which we term the spinal reflex." He says that his experiments "show the independence of the efferent neurone, and suggest that the functional unit is the activity of the independent efferent neurone; or rather, that it is the mutually conditioned activity of the linked antagonistic efferent neurones ('half-centers') which together form the 'center,' and they also suggest that the primitive activity of the nervous system is seen in such rhythmic acts as progression and respiration."[2]

We may now take up the specific and difficult question of how the results obtained with cessation reactions, discussed above, are to be explained. We have seen that the concept of resistance is inadequate. Is it possible to offer a more satisfactory explanation? While in the present state of nerve physiology it would require an unprofitable degree of speculation to attempt a detailed explanation, it does seem feasible to indicate at least the general direction which such an explanation must take.

[1] 'A Text-Book of Psychology,' 1911, 489.

[2] T. Graham Brown, 'On the Nature of the Fundamental Activity of the Nervous Centers, Together with an Analysis of the Conditioning of Rhythmic Activity in Progression, and a Theory of the Evolution of Function in the Nervous System,' *Journ. of Physiol.*, Vol. XVIII., 1914.

In the first place, it seems clear that a greater and more distinctive rôle must be given to the central nervous system, a rôle such that the activity of the central nervous system cannot be said to be assimilated to the peripheral. To substantiate this proposition I shall refer to some results I have already published elsewhere, on the effect of the preparatory interval on reaction time.[1] These results show that a much longer reaction time is obtained with a set of totally irregular preparatory intervals of varying length, or with very long preparatory intervals, than with a regularly repeated preparatory interval of two seconds. We may speak, then, of the prolongation in reaction time produced by the unfavorable preparatory intervals. Now I have shown that with weak intensity of stimulus this prolongation is very much greater than with strong or moderate intensity; and I have pointed out at length,[2] that this result can be explained only on the assumption that in the case of the weaker intensity we are dealing with a lower degree of attention. Moreover, it is known that with weakened attention we have lengthened reaction time. We have, then, experimental proof that a large part, at least, of the increase in reaction time with decrease in intensity is due to a decrease in the degree of attention. This conclusion is in general agreement with the explanation long since advanced by Wundt[3] and by Martius.[4] The basis of the explanation is the fact that intensity of stimulus is a condition determining the degree of attention; and I shall attempt to show how, on this basis, we can explain the laws of cessation reactions as well as those of ordinary reactions.

Another investigation which I desire to mention, is one on the measurement of attention as it occurs in reactions to sound, light, and touch, using as the measure of attention the reciprocal of the prolongation in reaction time produced by unfavorable preparatory intervals. While these results may not be published for some months yet, it is perhaps permissible

[1] 'The Measurement of Attention,' Psychol. Monog., 1915.

[2] *Op. cit.*, Chap. III.

[3] 'Physiologische Psychologie,' 5th ed., 1903, Vol. III., p. 430.

[4] 'Ueber den Einfluss der Intensität der Reize auf die Reactionszeit der Klänge,' *Philos. Stud.*, 1892, 469–486.

to state that, in the case of nearly all of the twelve subjects so far measured, with whom over 15,000 reaction times have been taken, the greatest prolongation occurred with light, the next greatest with sound, and the smallest with touch; from which I conclude that the degree of attention is in general lowest in the case of light reactions, and highest in the case of touch reactions. I am convinced from an analysis of the data, that the difference in degree of attention in these cases is sufficient to account for the difference in simple reaction time that occurs with difference in mode of stimulus.

Befor making use of these results in the explanation of the phenomena here under discussion, I wish to emphasize the fact that the reactions in human reaction time experiments are not at all simple reflexes. Of course, cessation reactions can never be what we ordinarily think of as reflexes, for in this case there is no stimulus, merely the cessation of a stimulus. One cannot mince words in such a fashion as to say that the cessation of stimulation is stimulation. The reflex theory would seem entirely inadequate to account even for the existence of cessation reactions. A similar point could be made in the case of reactions to a mere *decrease* in intensity. Moreover, the reaction is only in a very secondary manner a reaction to the change in stimulus. It is primarily brought about by the subject's intention to react, which in turn is the result of acceptance of instructions given the subject a considerable time before the reactions. It is further evident, at least in the case of weak intensities, that the reaction does not occur until the subject becomes aware of the change in stimulus. The process of inhibition, so far as can be judged either from introspection or from the objective reaction times, is present equally in cessation and beginning regctions. In both cases the subject is prepared to react when the right time comes, but this preparation remains merely a preparation until then. In one case the 'right time' means when the subject becomes aware of the stimulus, and in the other case, when he becomes aware that the stimulus has ceased. No satisfactory account of the reaction process can be given without taking into consideration this intention and preparation to react.

In the present state of our knowledge of the physiology of the nervous system, it is impossible to say definitely in what the preparation to react consists. We know that, in practiced subjects, it is often unaccompanied by any appreciable tension on the part of the muscles of the fingers used in the reaction. It appears, therefore, not to consist in any actual innervation of the specific reaction movements. We know, too, that this preparation is constantly undergoing certain irregular variations in degree, that it takes about two seconds to reach its maximum, and that thereafter, in addition to irregular oscillations, it shows a definite general tendency to weaken. It should be emphasized, however, that a certain degree of adaptation may be maintained throughout a considerable period of time. We know, further, that this preparation can vary in degree or intensity, and that the greater the intensity of such preparation, the quicker the reaction. Without attempting to go into details, it is evident that the preparation to react means, on the nervous side, the presence of certain nervous energies which are held in check, so far as a reaction movement is concerned.

There is energy, the intimate nature of which we must admit is as yet unknown, present in the central nervous system and temporarily inhibited in some way from producing the reaction. It is probable that this energy is of an electrical nature, and that it always involves many, if not all, of the neurones of the central nervous system. The condition within any one neurone is to be thought of as interrelated with the conditions within all the others, so that there is always involved an immensely complicated and widespread system of energies, including, perhaps, both potential chemical energies and electrical activities. So long as the subject's state is merely one of *preparation* for the reaction, it must be supposed that the interrelated cortical energies, while by no means entirely suppressing each other (for the subject would then be unconscious), yet, interact in such a way that some sort of equilibrium occurs at the point of exit of the motor pathway for the reaction movement. The existence of this equilibrium, as respects the reaction movement, is evident merely because the reaction movement does not occur, ordinarily, until after the stimulus which comes at the end of the preparatory interval.

The mere fact that both beginning and cessation reactions are possible, shows that this equilibrium may be upset either by the beginning of some new disturbance, or by the cessation of one. There is no theoretical difficulty here such as we encountered in attempting to explain both the laws of cessation and beginning reactions by reference to the concept of resistance. Different instructions set up different central adjustments, and while these adjustments in both cases consist in a system of activities which are in equilibrium so far as the reaction movement is concerned, the pattern of the adjustment must be different in the two cases; so that, in one case, the occurrence of a new excitation destroys the existing equilibrium, while, in the other, the new excitation merely serves to increase the intensity of activity of a different system, the equilibrium of which, however, is not upset until this excitation ceases. The widespread system of cortical energies takes on such a form, as the result of the instructions, that upon the occurrence of the proper sensory disturbance, some of this cerebral energy is released for the work of innervation of the reaction movement.

The energy used in the innervation of the reaction movement is not the energy of the excitation set up in the sensory nerve by the stimulus; it is energy already present in the central nervous system before the stimulus acts. In a reflex, as commonly described, the outgoing energy is merely the redirected incoming energy. This remains true, even though we further add that reflexes may inhibit or reinforce one another. The present view, however, regards the central nervous system as not merely a network of paths, but also as a vast system of interrelated energies, potential and otherwise. A disturbance produced in this system is not conducted through it; it causes a readjustment of the system, which readjustment may (or may not, so far as is known) result in the release of energy for the work of motor innervation. It is difficult to see how, on any other theory, we can account even for the fact of cessation reactions. It has already been pointed out that it is impossible to think of the cessation reaction as a simple reflex, for in this case there is no stimulus, but merely the cessation of a stimulus. On the other hand it is not difficult to understand that a change in any part of a system of energies may upset the balance of the system as a whole, whether the change in question be an increase or decrease of activity.

The process by which the change in sensory excitation disturbs the preëxisting cerebral system is in part, no doubt, the physiological process corresponding to becoming aware of the change. Even if actual awareness were not a necessary condition of the reaction, it seems altogether probable that a nervous process identical in nature to that which underlies awareness, though less in degree, must precede the reaction movement. The process which underlies awareness of the cessation of a stimulus must consist in an effect produced by this sensory change upon more or less widely distributed parts of the cerebral cortex. It could not consist in the local sensory effect, for this is merely a cessation. It is probably no less true that the awareness of the occurrence of the stimulus involves changes in the entire cerebral activity. We know from introspection that any new perception coming into the field of consciousness ordinarily modifies the pattern of consciousness as a whole.

It is clear that these central nervous processes in which alone is to be found the explanation of the phenomena discussed in the preceding pages, are in large part identical with those of attention or awareness; but that these central processes are all such that they may be subsumed under this heading is improbable. They are quite likely very complex. It may be, for instance, that in addition to the physiological process underlying awareness, there occurs another process which consists in the release of so-called determining tendencies.

We may now ask why a small change in the sensory excitation, whatever this may mean in terms of nerve physiology, should bring about the required disturbance in the preexisting system of cerebral energies so much more slowly than a large one, regardless of the direction of change. This is satisfactorily explained as due to the inertia of the preexisting cortical adjustment. Of course we do not know the intimate nature of the cortical changes brought about by the sensory change; but that there is work to be done in bringing about these charges, and that this work would be done more slowly by a small local disturbance than by a big one, seem to me unquestionable propositions. I have already argued that the cortical processes in question are largely those underlying the phenomenon of attention. It is well known that these processes require considerable time; *e. g.*, it is well established that maximal adaptation of attention requires about two seconds. There is, moreover, no difficulty in understanding why the reaction time to the cessation of the excitation equals the reaction time to its occurrence, no matter what the intensity of the stimulus. The size of change in sensory excitation is of course the same, whether the change be due to the occurrence or the cessation of the stimulus. The direction of change of the excitation need not affect either the size or rate of change. The rate of change in the sensory cortical excitation, then, we assume to be practically the same at both the occurrence and cessation of the excitation; and, further, we assume that so long as the intensity of the preparatory adjustment remains constant, the time required for the disturbance or release of the preadjusted system of cortical energies varies with the size and rate of change in excitation, which brings about this release. The difference in direction of the change in excitation is counterbalanced by the difference in the nature of the preadjustment.

The next question to be answered is why light reactions, at moderate intensities of stimuli, are longer than sound reactions. This is easily accounted for on the hypothesis that the cortical preadjustment is less effective in the case of light than in the case of sound. The lesser effectiveness of preadjustment, in the case of light reactions as compared with sound or touch reactions, may possibly be accounted for by the fact that the adjustment of the sense-organ in the case of vision—including the control of convergence, accommodation, winking, proper direction of head, etc.—is more elaborate than in the case of hearing or touch, where no sensory adjustments of any great consequence are required. The expenditure of energy on the sensory adjustment may detract from the adequacy of the central cerebral adjustment as a preparation for the reaction movement. Whatever be the cause of the better cerebral adjustment in the case of sound and touch reactions, the fact of such better adjustment is evident; for upon no other assumption can we explain the result already mentioned, that light reactions are prolonged more by the use of unfavorable preparatory intervals than are sound or touch reactions. I have shown elsewhere, that the prolongation produced in reaction time by unfavorable preparatory intervals is due, solely, to the effect of such as a detractor of attention. This detraction effect occurs in accordance with the law that the prolongation produced varies inversely with the degree of attention acted upon. Accordingly, since the prolongation in reaction time produced by unfavorable preparatory intervals is greater in the case of light reactions than in touch or sound, it follows that the degree of attention in the former is less than in the latter. On the cerebral side we may substitute effectiveness of cerebral adjustment for degree of adaptation of attention.

Conclusions

1. The experiments here reported show that the reaction time to the cessation of a sound or light stimulus is in all cases the same as the reaction time to the beginning of that stimulus. This statement includes the following two corollaries. (1) The same difference between reaction times to sound and light exists in the case of reactions to their cessation as in the case of reactions to their beginning. (2) The lengthening in reaction time due to a decrease in intensity of stimulus is equal in amount, and follows the same law, in the case of both beginning and cessation reactions.

2. The above mentioned similarity, in the variation of both beginning and cessation reactions with mode and with

intensity, cannot be explained either by the hypothesis of variation in the latent period of stimulation of the sense-organ, or by the hypothesis of variation in the resistance offered by the synapses. Likewise, this similarity cannot be adequately explained on the commonly accepted hypothesis that all nervous action consists essentially in conduction of nervous impulses from the sense-organ to the muscle along arcs which offer varying amounts of resistance. On the other hand, the facts on beginning and cessation reactions are incompatible with such an hypothesis.

3. The explanation of the experimental data presented in the preceding pages, while at present impossible in detail, seems to require us to regard the central nervous system as not merely a network of paths, but also as the seat of a complex system of interrelated activities and potential energies which is disturbed throughout by any change in any part of the system. The fact of cessation reactions cannot be adequately explained without postulating such a central system of energies, the balance of which may be upset by either an increase or decrease of activity in any part of the system. The pattern of the preëxisting system differs in beginning reactions from that in cessation reactions in such a manner that, in beginning reactions, the same effect is produced by the rise of the excitation as is produced in cessation reactions by its fall. A small change in excitation disturbs the preëxisting cortical system so as to bring about the reaction movement more slowly than does a large one—not because of resistance to its conduction, but because of the inertia of the preëxisting central system. The reaction time to light, both for cessation and beginning reactions, is longer than for sound because of an inferior preadjustment of the cerebral mechanism. In all cases the reaction results from the release of energy already within the nervous system before the occurrence of the stimulus; and is not due to the mere redirection or modification of the incoming sensory excitation.

4. The physiological disturbance of the central nervous system here involved is in large part, though not entirely, that which underlies the process of becoming aware of a

stimulus or of attending to it. This is shown by the fact that the degree of attention, in the case of reactions to a weak stimulus, is less than that in the case of reactions to a strong stimulus, and also, less in the case of reactions to light than in the case of reactions to sound or touch.

FACILITATION AND INHIBITION OF MOTOR IMPULSES

A Study in Simultaneous and Alternating Finger Movements

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Description of Experimentation

The purpose of the investigation[1] was to ascertain the facilitating and the inhibitory effect of successive and simultaneous muscular impulses in the movements of the several fingers of both the right and left hands. The data have also been arranged to show the effect of practise and fatigue and the relation of the movements of the fingers of the right hand to those of the left hand.

The instrument used was similar to that of the Whipple tapping-board[2] as will be seen from the cut, p. 476. In place of a metal stylus, a ring was worn on the finger. This was insulated by rubber tubing except on the under part where a metal peg protruded and made an electric contact with the metal base of the board. A light flexible wire ran from the ring to an electric marker which registered the contact on a kymograph.[3] In order to obtain a free and natural movement of the fingers, the hand rested on a curved block of wood, and was thus slightly vaulted. The tips of the fingers and cushions of the palm of the hand rested on the board. The arm rested on the table, several of the subjects using a cushion for greater comfort.

It was deemed of importance that the height of the finger

[1] The experiments were conducted in the Harvard Psychological Laboratory during the academic year, 1913–14. Trial experiments, not here reported, were made the previous year.

[2] 'Manual of Mental and Physical Tests, Simpler Processes,' p. 131.

[3] It was under similar conditions that one of the first tapping tests—that of von Kries—was performed, 'Zur Kenntniss der willkürlicher Muskelthätigkeit,' *Arch. f. Anatomie u. Physiologie*, 1886, Sup. Band, pp. 1–16.

movement should be constant so that there could be no doubt that differences in the rate of tapping were not due to changes in the length of stroke. A bar was therefore placed 4 cm. above the board and the subject was instructed to hit the bar at each stroke. The bar met the finger slightly below the second joint, and care was taken that it should always strike at approximately the same spot throughout the tests. This arrangement worked very well, the subject soon becoming used to the task. The movement thus remained more nearly voluntary than would probably have been the case if the finger had not had to touch the bar.[1] The hand was not strapped to the board as it was soon evident that the subject could keep his hand and arm still, and if there did happen to be a slight movement, his attention was called to it at once; nor was the tapping continued to that period of fatigue when the subject in seeking relief begins to use other muscle groups of the arm. Any slight error that might have crept in could not be as great as would have been the danger to the result caused by the binding of the muscles. The only muscles used, therefore, were the extensors and the flexors of the phalanges. These muscles pass from their origin in the forearm over the wrist joint to the phalanges and at the end of the grasping movement there is a tendency for them to flex the hand, which is inhibited by the synergic muscle of the wrist.

The time in two-second periods was marked on the drum at the beginning of each set of trials by a marker actuated

[1] In regard to the length of stroke, von Kries writes, *loc. cit.*, page 4, "Der Umfang der Bewegungen ist auf die Dauer von nur geringem Einfluss; doch scheint es, dass die Bewegungen von einem gewissen mittleren Umfamg am schnellsten ausgeführt werden können und sowohl die sehr kleinen als die sehr grossen ein wening länger dauern." Bryan ('On the Development of Voluntary Motor Ability,' *Amer. Jour. of Psychol.*, 5, 1892, p. 150) is in agreement with von Kries as to the slight effect of change of amplitude. Max Isserlin states that ". . . die Tendenz besteht, trotz abnehmender Geschwindigkeit die Bewegungszahl konstant zu erhalten. Diese wird zuletzt herabgesetzt" ('Ueber den Ablauf einfacher willkürlicher Bewegungen,' *Psych. Arbeiten*, 6 1910, p. 186). From this we may conclude that the change in amplitude conceals the fatigue as measured by the rate of tapping alone. It was also found in the preliminary tests before the control bar was used that there was a strong tendency for several subjects to execute a series of quick reflex movements similar to a tremble which greatly increased the tapping rate and seemed difficult at times to avoid.

by the laboratory clock. When both hands were used, a board was employed for each hand.

There were four subjects who will be referred to as *A*, *B*, *C*, *D*. *A* and *B* were advanced graduate students; *C* and *D* undergraduates. *A* and *D* were very athletic, *B* less so and *C* the least strong of the four. The experiments were made in the morning, the subjects coming always at the same hour. As a rule, there was a week's intermission between each set of trials. The period of tapping for all fingers and all combinations of movements was 30 seconds with a two seconds' pause between the members of a series and a five minutes' pause between the series. The finger movements examined were as follows: During the first half year, the subject began by tapping with his right index finger (*R*1). This was followed by the second finger of the same hand (*R*2). Then these two fingers were tapped alternately (*A*), a movement similar to the walking movement, that is, *R*1 was raised as *R*2 was lowered, the two fingers passing each other in the middle of the stroke. There then followed what may be termed complete alternation (*CA*). *R*1 made a complete stroke up and down before *R*2 began. The signal for a finger to begin was the return of the other finger to the starting point in the manner of a relay race. *R*1 went up and down then *R*2 went up and down, etc. Finally *R*1 and *R*2 tapped simultaneously [*S*(*R*1 *R*2)]. This completed the series. After a five minutes' pause, the series was repeated in the same order. On alternate weeks, the order of the series was reversed, beginning with *S* and ending with *R*1. At the beginning of the year the left-hand fingers were tapped in the same manner as the right and again for one series about six weeks later, in order to ascertain if there was any transfer of practise. During the second half of the year, both hands were used. The series began with the right index finger alone. Then followed the left index finger alone (*L*1) and then both simultaneously [*S*(*R*1 *L*1)]; and then the second finger of the right hand (*R*2) alone for thirty seconds, followed by the second finger of the left hand (*L*2) alone and then both simultaneously [*S*(*R*2 *L*2)]. This series was repeated and the order reversed on alternate weeks. A few series were also made with $R1$ and $L2$ and [$S(R1\ L2)$] and $R1$ and $L4$ and [$S\ (R1\ L4)$].

Simultaneous Movement

The final averages have been gathered of the simultaneous tapping of the pairs of fingers of both hands and have been placed together in Table IV.[1] for convenience of comparison. In the second and fourth columns for each subject are the rates of tappings for 30″ for the fingers separately, and in the sixth column, the rates when tapped together. In the seventh column is the difference in rate of the two fingers, and in the eighth, the difference between the rate of the simultaneous tapping and the rate of the slower finger when tapping alone. Examining first the two fingers of the same hand, $R1$ and $R2$, we find the interesting fact that with all the subjects the two fingers are moved more rapidly together than the slower finger alone and in the case of *A* faster even than the faster finger by a considerable amount. With *B*, *S* is one stroke faster than the faster finger. A similar relation holds with the symmetrical fingers of the left hand (second horizontal column). Here in fact in the case of all but *B*, who shows little change, the two fingers are tapped faster together than either of the separate fingers. In Tables I., II. and III. the maximal rates of tapping are in heavy type and may be readily compared. For the right hand the highest *S* is greater than the highest $R1$ or $R2$ for all subjects but *C*. For the left hand the highest *S* is greater than the highest $L1$ or $L2$ for all the subjects. The explanation which suggests itself is that the two fingers being very closely related, an impulse to one tends to influence the other, since a strong coördination has probably been induced by the grasping movement. In the single movement the other finger must be voluntarily held down and this slows up the action of the moving finger. When both fingers are moved together this inhibition is removed and they both move faster, unless one is much slower than the other, when it acts as an inhibition. It is possible

[1] These averages have been taken from Tables I., II. and III.

that with the left hand, the inhibition of the idle finger is more difficult than with the right hand due to less practise. This would account for *S* being greater than the single tapping

TABLE I

No.	Subject *A*					Subject *B*					Subject *C*					Subject *D*				
	R_1	R_2	*A*	*CA*	*S*	R_1	R_2	*A*	*CA*	*S*	R_1	R_2	*A*	*CA*	*S*	R_1	R_2	*A*	*CA*	*S*
1	154	165	120	37	172		137	116	36	163	178	148	44	23	160	139	161	106	25	172
2	155	170	121	39	192	168	141	131	34	170	161	162		24	174	**149**	158	112	34	**174**
3	153	165	117	34	187	143	138	131	41	161	119	121	**68**	27	114	137	152	108	32	151
4	**184**	**190**	126	40	196	172	161	136	40	155	115	117		**31**	132	147	162	112	36	157
5	145	153	118	40	178	173	152	140	35	170	174	162	57	29	175	141	148	104	34	154
6	146	163	115	46	187	170	164	143	42	188	193	151	67	27	**190**	136	146	102	36	138
7	159	171	119	43	167	178	160	133	39	166	191	**168**	56	26	168	133	152	109	35	135
8	170	166	136	47	**197**	166	170	141	42	164	**194**	165	66	26	174	136	153	103	40	134
9	137	145	120	43	163	160	151	127	40	181	178	161	54	24	167	121	146	103	35	148
10	154	143	130	44	179	189	171	**168**	**59**		187	159	59	25	186		145	105	36	148
11	155	159	131	37	150	178	178	133	37	173						142	**168**	113	40	146
12	160	150	137	40	168	**192**	**186**	154	45	181						141	160	109	40	146
13	141	136	**139**	41	176	169	161	142	33	169						145	156	105	**42**	148
14	159	150	137	46	169	170	170	138	40	177						129	158	**114**	34	148
15	157	161	133	45	161	178	178	155	44	188						129	151	106	36	138
16	154	150	134	**52**	182	180	175	153	53	178						135	148	101	32	149
17						186	177	151	41	187						139	162	103	38	158
18						185	185	144	40	**198**						143	153	110	35	155
Av.	155	158	127	42	176	173	164	141	41	174	169	151	59	26	164	138	154	107	36	150
R1 to S	149	153	125	42	177	174	160	140	40	178	178	157	56	25	175	135	153	106	35	154
S to R1	161	164	129	42	176	173	168	142	42	170	159	142	63	27	147	138	156	108	36	144
m.v.	7	10	8	4	11	9	13	9	4	9	22	14	6	2	17	6	5	3	3	8

of either finger for the two subjects who did not show this with the right hand.

Is, however, the release of inhibition due to the close relationship of the fingers the only factor which causes the

TABLE II

No.	Subject *A*					Subject *B*					Subject *C*					Subject *D*				
	L_1	L_2	*A*	*CA*	*S*	L_1	L_2	*A*	*CA*	*S*	L_1	L_2	*A*	*CA*	*S*	L_1	L_2	*A*	*CA*	*S*
1	110	128	46	27	135	134	131	111	32	131	**115**	113	44	24	116	134	120	90	30	**156**
2	**125**	**140**	55	36	**164**	139	143	**119**	35	140	111	117	58	27	**128**	137	132	105	34	143
3	107	128	56	30	133	143	142	119	34	131	114	117	**81**	22	125	137	135	104	**38**	127
4	113	125	**68**	**37**	141	**154**	146	114	36	142	113	113	71	25	120	**151**	**139**	**106**	35	141
5	107	122	59	32	133	149	148	115	44	155	110	109	53	**28**	118	137	125	97	34	134
6	113	119	68	36	140	150	**149**	113	**49**	**164**	110	**120**	59	28	121	136	133	98	38	142
Av.	112	127	59	33	141	145	143	115	38	144	112	115	61	26	121	139	131	100	35	140
m.v.	5	5	6	3	7	6	4	2	5	10	2	3	10	2	3	4	5	5	2	7

increase especially in the slower finger? Does not one impulse directly influence the other when discharged simultaneously, not only exciting an inhibitory effect in the case of the slower movement, but a facilitating effect in the case of the faster? To answer this question symmetrical fingers of the two hands, *R*1 and *L*1 and *R*2 and *L*2 were tapped simultaneously. Here there cannot be the same strong natural tendency to move the two fingers simultaneously as is prob-

TABLE III

No.	Subject *A*						Subject *B*					
	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2	*S*
1	146	117	137	147	124	155	172	146	155	170	143	163
2	157	119	150	151	123	164	184	153	167	177	151	174
3	164	128	140	145	117	156	173	144	163	174	148	151
4	159	115	149	168	140	158	193	146	169	180	148	154
5	135	129	147	156	131	151	174	150	165	177	148	165
6	160	131	146	173	**141**	152	180	158	162	174	154	161
7	169	117	152	170	131	155	192	**159**	175	185	154	159
8	**177**	130	144	160	132	150	**203**	153	167	**186**	**160**	170
9	167	133	**163**	163	125	150	180	151	157	186	149	156
10	162	**138**	160	158	132	**166**	187	148	157	172	146	153
11	158	119	143	167	123	151	193	153	170	180	148	162
12	162	125	150	**174**	131	155	200	148	**178**	185	152	**182**
13							175	145	162	173	152	165
14							186	152	162	172	158	160
Av.	160	125	143	161	129	155	185	150	164	178	151	162
R1 to S	155	129	140	158	131	156	179	150	161	175	150	162
S to R1	166	122	146	164	129	154	192	150	170	180	152	163
m.v.	7	6	7	8	5	4	8	4	5	5	4	6

No.	Subject *C*						Subject *D*				
	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2
1	179	107	118	168	114	133	130	124	128	151	128
2	191	114	127	167	116	122	135	125	128	151	132
3	190	113	122	171	124	139	131	128	127	152	130
4	200	116	123	180	114	126	**147**	140	133	**164**	134
5	173	123	131	169	117	135	133	132	131	153	135
6	192	111	124	**184**	124	**141**	138	136	130	158	**140**
7	180	115	123	169	114	120	142	132	132	153	127
8	172	116	123	120	101	134	142	**144**	136	153	135
9	182	123	**134**	169	125	134	138	132	128	157	132
10	**204**	119	122	162	111	118	138	129	130	153	131
11	176	121	120	156	118	124	135	140	133	154	131
12	179	**124**	132	164	**135**	133	143	133	**141**	163	137
13											
14											
Av.	185	117	125	165	118	130	138	133	131	155	133
R1 to S	187	116	126	170	118	130	135	129	129	154	133
S to R1	181	118	124	160	118	130	140	136	134	156	132
m.v.	9	4	4	10	6	7	4	5	3	3	4

ably the case with the fingers just examined. In Table IV. we find that with three of the subjects for both sets of fingers there is an increase in the tapping of the slow and a decrease in that of the fast finger. In the case of both *A* and *B* the simultaneous tapping approaches the average of the two fingers. For *C* the effect of the faster finger is not so great and the increase of speed of the slower finger is below that of *A* and *B*. A further peculiarity of this subject to be discussed later offers an explanation for this. For *D* the simultaneous tapping for both pairs is about the same as the slower finger. The difference between *R*1 and *L*1, however, is very slight and between *R*2 and *L*2 less than with any of the other subjects. This difference is an important factor as will be seen below (p. 460). It is evident, however, that with three of the subjects the rapidity of the movement is increased by the simultaneous exercise of a more rapid movement taking place in a symmetrical part of the opposite side of the body. The more rapid movement, on the other hand, is to an extent inhibited.

Will this phenomenon occur if the fingers moved are not symmetrical? To investigate this point *R*1 and *L*2, *R*2 and *L*1, *R*1 and *L*4 and *L*1 and *R*4 were tapped simultaneously.[1]

TABLE IV

Subject *A*								Subject *B*							
*R*1	155	*R*2	158	*S*	176	3	21	*R*1	173	*R*2	164	*S*	174	9	10
*L*1	112	*L*2	127	*S*	141	15	29	*L*1	145	*L*2	143	*S*	144	2	1
*R*1	160	*L*1	125	*S*	148	35	23	*R*1	185	*L*1	150	*S*	164	35	14
*R*2	161	*L*2	129	*S*	155	32	26	*R*2	178	*L*2	151	*S*	162	27	11
*R*1	161	*L*2	140	*S*	132	21	— 8	*R*1	181	*L*2	150	*S*	160	31	10
*R*2	157	*L*1	132	*S*	132	25	0	*R*2	174	*L*1	149	*S*	158	25	9
*R*1	160	*L*4	89	*S*	93	71	4	*R*1	177	*L*4	133	*S*	144	44	11
*R*4	107	*L*1	140	*S*	109	33	2	*R*4	145	*L*1	152	*S*	150	7	5
Subject *C*								Subject *D*							
*R*1	169	*R*2	151	*S*	164	18	13	*R*1	138	*R*2	154	*S*	150	16	12
*L*1	112	*L*2	115	*S*	121	3	9	*L*1	139	*L*2	131	*S*	140	8	9
*R*1	185	*L*1	117	*S*	125	68	8	*R*1	138	*L*1	133	*S*	131	5	—2
*R*2	165	*L*2	118	*S*	130	47	12	*R*2	155	*L*2	133	*S*	133	22	0
*R*1	193	*L*2	125	*S*	129	64	4	*R*1	135	*L*2	139	*S*	127	4	—8
*R*2	167	*L*1	126	*S*	120	41	— 6	*R*2	152	*L*1	139	*S*	138	13	—1
*R*1	195	*L*4	98	*S*	95	97	— 3	*R*1	141	*L*4	84	*S*	111	57	27
*R*4	113	*L*1	119	*S*	112	6	— 1	*R*4	98	*L*1	139	*S*	106	41	8

In the combination *R*1 *L*2 both *B* and *C* and in *R*2 *L*1, *B* still show an increase in the rate of the slower finger. *A* has now dropped below the single tapping for *R*1 *L*2 and *D* is below for both. With *R*1 *L*4 and *R*4 *L*1 all the subjects but *C* show an increase in the slower movement. *C*'s simultaneous movement is slightly below that of the slower finger.

In order more readily to examine and analyze these results the difference between the tapping rates of the two fingers

[1] It has not been thought necessary to give a complete table of these tests. The averages were taken from fewer series than the previous ones, but as the general relationship does not vary materially from series to series they can be safely used.

has been placed in the seventh column of Table IV. and the increase in the rate of the slower finger during the *S* movement in the eighth column. A minus sign, of course, indicates a decrease.

Examining first *B*'s result we find that in the asymmetrical pair *R*1 *L*2 there is less of an increase of the slower movement than with the symmetrical pair *R*1 *L*1, an increase of ten as compared to fourteen, but the pair *R*1 *L*4 which is more asymmetrical than *R*1 *L*2 shows a slightly greater increase, *i. e.*, eleven compared to ten. In this latter case, however, the difference between the rates of the two fingers *R*1 and *L*4 is greater than between the former pair *R*1 *L*2. The former is raised one third of the difference, the latter only one fourth of the difference. The result suggests that there are two factors influencing the rapidity of the simultaneous movement, the degree of symmetry and the difference in the rapidity of the two members of the pair. These two factors should act in opposite directions; the difference between the rate of the two fingers increases as a rule with the decrease in symmetry, and the greater this difference in rate the more should the slower finger be aided by the faster in simultaneous tapping, but the greater the assymmetry the less is the advantage of simultaneous tapping. The relation of these two factors very likely differs in individuals. When the coördination is not good asymmetry probably plays an important rôle in slowing the movement. When the coördination is good the rate differences have more effect. Let us examine the data further with this suggestion in mind. Take for example *B*'s *R*1 *L*1 and *R*2 *L*2. The asymmetry is the same but the difference in rate of the *R*2 *L*2 pair is less than that of the *R*1 *L*1 and consequently the increase of the slower movement is less. With *R*1 *L*4 the asymetry is increased but the difference rate is also, so that the actual increase in rate remains the same as the other pair. In the case of *A* with the same two pairs it is true that the results are in the opposite direction, but in the next pair there is a drop in both symmetry and difference and there is in consequence, a falling off in the rate of simultaneous tapping in the one case even below the slower of the pair.

In the most asymmetrical pair, *R*1 *L*4, the difference is very large and there is again an increase of the slower movement notwithstanding the great asymmetry. In *R*4 *L*1, a pair of like asymmetry, there is less difference and less increase. Turning to *D*'s record, we find that although, like the other subject, he showed an increase when the two fingers were of the same hand whether the right or the left, as soon as the movements are on opposite sides there is often evidence of an inhibition. In the *R*1 *L*1 pair we should not expect much change for there is little difference between the rates, but with the *R*2 *L*2 pair, although the difference is twenty-two taps there is no increase of the slower finger, and in the *R*1 *L*2 pair the lack of coördination actually causes an inhibition of the slower movement amounting to eight taps. The results for *R*1 *L*4 and *R*4 *L*1 taken in connection with the foregoing results of this subject, speak strongly for the assumption of the above mentioned opposing factors. The pairs are the most asymmetrical but the differences in rate are very large, being for one almost three times as great as the largest previous difference. Examining the rate for simultaneous tapping we find that the slower movement increases by twenty-seven taps for *R*1 *L*4, that is, the facilitation is 31 per cent. of the rate when the finger is moved alone, and for *R*4 *L*1 there is an increase of only eight but the difference is less, forty-one compared to fifty-seven. The inhibitory effect of asymmetry which, judging from the previous results, is most probably present, has been more than overcome by the facilitating effect of the rate difference. This explanation also fits *C*'s result although he differs in type from the other subjects. It will be found when we come to the further test performed that *C* showed much more pronounced lack of coördination of different muscle groups than the other subjects. There should therefore be less facilitation and probably even inhibition. But he also showed the largest rate difference. Therefore, although the facilitation is less than in the case of *A* and *B*, nevertheless it is present in some instances. In the pair *R*1 *L*4 the large difference of ninety-seven was not sufficient to overcome the inhibition of asymmetry and when we notice that with the much more symmetrical pair *R*1 *L*2 the large difference of sixty-four was only able to cause a facilitation of four taps, and with *R*2 *L*1 there was a difference of forty-one and yet a decrease of six taps, this result is rather to be expected. The only figures which do not readily offer themselves to the explanation here attempted are those of the pair *R*2 L2. The symmetry of *R*2 *L*2 is the same as *R*1 *L*1 but a difference of forty-seven in the former pair causes a facilitation of twelve taps while a difference of sixty-eight in the latter pair only causes a facilitation of eight. It is true that with none of the subjects is the relation of facilitation to rate difference constant. This ratio also varied with the different subjects. Two points might be mentioned in this regard. First, equality in symmetry does not necessarily mean the same amount of coördination of different muscle impulses. Thus in the results of *C*, although *R*1 *L*1 and *R*2 *L*2 are both symmetrical pairs the coördination between *R*2 *L*2 is better than between *R*1 *L*1, and perhaps for that reason the smaller rate difference has a greater facilitating effect. This explanation could also be offered in regard to the similar results of *A*. Secondly, although the change in symmetry between any two pairs is naturally the same for all subjects, yet one subject will probably have a different change in coördination in going from one pair to another, than a second subject, and this will readily explain individual differences in the above mentioned ratio. It should also be mentioned that investigations on other subjects revealed a difficulty to synchronize, which retarded the simultaneous movement.

Alternating Tapping

As was stated above the alternating tapping was performed in the same series as the simultaneous and the figures for *R*1 and *R*2 may again be used. *A* is the alternation in which one finger ascends while the other descends and *CA* the complete alternation in which one finger does not begin to move until the other has returned. The figures express only the rate of one finger, the number of actual taps made being twice that number. In Table V. these alternations are expressed in per cent.. of the average of the two fingers tapping singly. Let us first discuss the *A* results. If the fingers were alternating levers of a mechanical machine there would, of course, be twice as much work done in the same time as one lever working alone would perform. In the human machine when two different movements, and in this case opposite movements, are carried on simultaneously we look for some

TABLE V

	Right Hand		Left Hand	
	$\frac{A}{\frac{R1+R2}{2}}$	$\frac{CA}{\frac{R1+R2}{2}}$	$\frac{A}{\frac{L1+L2}{2}}$	$\frac{CA}{\frac{L1+L2}{2}}$
Subject *A*....	.81	.27	.50	.28
Subject *B*....	.83	.24	.80	.26
Subject *C*....	.37	.16	.54	.23
Subject *D*....	.73	.25	.74	.26

inhibition. The amount of this inhibition is expressed in the per cent. It will be seen that for three of the subjects *A*, *B*, and *D*, with the right hand there is only a loss of about one quarter of the speed of one finger when working alone. In other words by carrying out simultaneously two movements, though opposite in nature, there is a gain of fifty per cent. as compared to the amount of work done if only one movement was performed in the same time. The other subject, *C*, shows a much lower figure. It is only thirty-seven per cent., which means that there is an actual loss in work accomplished by alternating simultaneous movements of thirteen per cent. In his present state of muscular coördination he would accomplish more with one finger moving alone than he would by moving two fingers. Fifty per cent. would mean that the work accomplished by the two fingers is the same as if one had moved alone. The actual figures make the above perhaps clearer. Alternating he only taps fifty-nine times for each finger, or one hundred and eighteen taps in all, while the mean of the rate of the two fingers tapping alone is 160. The loss is forty-two taps or twenty-six per cent. for the two fingers. This subject is the one referred to on page 461 as having poor coördination between different muscle groups.

The above figures make this evident. In this connection it is very interesting to note that *C*'s rate of tapping with one one finger is faster than any of the other subjects with the exception of *B*. From this it would seem that the inhibition is a variable independent of the rate of movement of the separate muscle groups. This assumption is strengthened by the results of the left hand. The separate tapping rates are much lower but the *A* rate is about the same as before, consequently the per cent. is higher. Subject *A* shows this same independence. The *A* rate for the left hand has dropped relatively much lower than the separate rates and the per cent. is consequently lower. In fact it is now about the same as *C*'s rate. That is, his coördination in the left hand is worse than in the right. Subjects *B* and *D* show almost identically the same amount of coördination for both hands. To repeat, the above results substantiate a fact which from what we already know is rather obvious, that the degree of coördination between several muscle groups does not bear any definite relation to the degree of efficiency of the separate muscle groups concerned.

Particularly in regard to the coördination of these movements it seems of interest to inquire into the musical training of the subjects. *A* is proficient with the violin, *B* has played the organ since boyhood, *C* has just begun to take piano lessons, and *D* has played the piano for years for his own amusement. *B*'s ratios of 83 per cent. and 80 per cent. are the highest of any subjects and one is disposed to say that this is due to greater practice and that *C*'s low ratio is due to lack of training. *A*'s figures are what one would expect. Being a violinist the fingers of the left hand are trained to a different set of movements. In playing the fingers are bent and one is held down while the other taps. There would therefore be an inhibition when the fingers were forced to tap alternatingly. This would account for the 50 per cent. which is even lower than that of the untrained *C*. These results, then, seem to indicate degrees of practice but they are too few to be more than a suggestion.[1]

[1] O. Raif argues that the fastest rate required by any piece of music is slower than the average rate of tapping, and therefore piano practice does not increase the rate

There remain to be examined the results of complete alternation (*CA*). Again using the illustration of a mechanical machine the one lever begins to move after the other has stopped. The work done in a given time is the same as if there were only one lever which moved continuously instead of alternating with the second lever. Each finger carrying out such a movement should do fifty per cent. of that which it would do if working continuously. Instead it will be seen that all the subjects with the exception of *C* do only about twenty-five per cent. whether with the right or left hand. *C*'s loss is again greater than that of the other subjects for the right hand. This difference is less than in the *A* rate and with the left hand he shows almost the same per cent. of loss as they do. These figures mean that in this alternation there is a fifty per cent. loss in muscular work done. This amount of loss seems to be relatively the same for both the right and left sides.

Index of Righthandedness

The indices of righthandedness of the first and second finger in the performance of the different combinations here investigated are given in Table VI. As has been done both by Woodworth and by Wells[1] the index is obtained by dividing the rate of tapping of the left hand by that of the right, thus giving the ratio of the efficiency of the two sides. As has been found in the tapping with the whole hand there are great individual differences. The range is even greater than that found by Wells. It is significant that subject *C*, who showed poor coördination in the more complex movements has also the lowest $L1/R1$ index which means that he is also relatively inferior to the others in this simpler coördination for the left

but rather the proper timing of the movements. He says: "Nicht in der Bewegung an sich, sondern in der Rechtzeitigkeit der Bewegung, d. h. in dem Zeitverhaltniss von einer Bewegung zur anderen liegt die Schwierigkeit. Diese Rechtzeitikgeit kann zweifellos nur ein Product unseres Willens sein, wir haben also den Ausganspunkt für die Fingerfertigkeit in den Centraltheilen unseres Nervensystems zu suchen, etc." ('Ueber Fingerfertigkeit beim Clavierspiel,' *Zeitschrift für Psychol.*, 24, 1900, p. 354.) While upon the subject of characterization it is worth mentioning that *B* has the fastest tapping rate and *D* the slowest, and both are very athletic, as was mentioned above, which means that here there is zero correlation between strength and rate of tapping.

[1] 'Normal Performance in the Tapping Test,' *Am. Jour. of Psychol.*, 19, 1908, p. 446.

hand. The indices for the efficiency during the first and second half year's work have been given separately under each subject, and we notice that the change is not great. Of these indices five are slightly lower, two are the same, and one higher. That is, there is an indication that practice has had somewhat more effect upon the right hand than the left.[2] The individual characteristics, however, remain unaltered, the ranking of the subjects according to the size of index being the same. With three of the subjects the $L1/R1$ index is somewhat lower than the $L2/R2$ index due to the superiority of the efficiency of the index finger of the right hand. This is not the case with subject D, whose $R2$ is throughout decidedly the most efficient finger.[1]

Table VI

Subject	$\frac{L1}{R1}$	$\frac{L2}{R2}$	$\frac{(L)A}{(R)A}$	$\frac{(L)CA}{(R)CA}$	Subject	$\frac{L1}{R1}$	$\frac{L2}{R2}$	$\frac{(L)A}{(R)A}$	$\frac{(L)CA}{(R)CA}$
A...	.72 .78	.80 .80	.46	.78	C.....	.66 .63	.76 .71	1.03	1.04
B...	.83 .81	.87 .85	.82	.92	D....	1.00 .96	.85 − .86+	.93	.97

In the A and CA movements subject B, who had the best coördination, shows an index similar to his index for simpler movements. Subject A has the same for CA but as shown above his coördinated wth the A movement on the left side was poor and his A index is therefore much lower than his other indices. Subject C has the same great difficulty with both hands and consequently has practically no index. Subject D has a higher index for the CA movements. From

[2] Wells's results are similar to these. He writes: "Again, in neither subject does the left hand show an improvement relative to the right. In Subject *I* the index of right-handedness remains practically the same. In Subject II. the right hand may even improve more than the left." *Loc. cit.*, p. 454. Our subject *A* with one of the fingers of his left hand showed an improvement relative to the right-hand finger. Whipple remarks, *loc. cit.*, p. 143, that "practice affects the left hand no more than the right; consequently the index of right-handedness is unaffected by repetition of the test." This generalization is not borne out by all of the subjects in this experiment, nor by all of Wells's subjects.

[1] The subject could give no reason for this. He had never to his recollection exercised this finger more than the others, and believes it must be an inate characteristic.

these results we again see the low correlation of these complex coördinations with the simpler coördinations of the single movement as was shown before.

Practice Effect

The experiments were not arranged with the idea of investigating practice and fatigue, but it does not seem amiss to discover what evidence there is of their effect under the conditions described. An examination of the tables I., II. and III. shows as was to be expected from the results of previous work that the improvement is not a steady one. If curves were plotted they would reveal the characteristic fluctuations. As stated, the maximum rate for each series is in heavy type. See Tables I., II. and III. It will be seen that it may occur at almost any point of the series, nor is the *R*1 maximum necessarily obtained on the day of the *R*2 maximum or the *S* maximum on the day of the maximum of either finger concerned, nor does the *A* maximum always occur on the day of the *AC* maximum. In short there is a low correlation as regards the days of the maximum results for the different fingers of the different movements.

Table VII. has been arranged to show the general change between the first and second half-year's work, and between the first and second part of the first half year. The figures in the first and second horizontal columns are the averages for the first and second half of the series given in Tables I. and II. The third, fourth, and fifth horizontal columns contain the result of the second half year's work.

It will be seen that with the separate tapping of the fingers of the right hand in the case of two of the subjects, *B* and *C*, there is decided evidence of the effect of practice, not so much with *B* in the difference between the first and second half year as in the difference of the halves of the first half year. The other two subjects do not show this difference, in fact *A* in the first half year shows a falling off. With the left hand fingers *B* and *C* again show the effect of practice, but it is not so marked as with the right hand. Subject *A* shows a practice improvement in the index finger of the left hand,

and subject *D*'s results again show no evidence of practice effect. In the alternating movements *B* shows decided improvement as does also *A*, while *C*, who had great difficulty, shows a considerable loss. *D* remains about the same. In the complete alternation there is no significant changes. The change in the simultaneous movement follows the change in the individual movement.

The most evident fact in these results is the wide individual differences which preclude any general statement. It is

Table VII

Subject	*R*1	*R*2	*A*	*CA*	*S*	*L*1	*L*2
A......	158	168	121	41	184	112	127
	152	149	133	43	168	125	129
	160	161					
	160						
	161	157					
B......	169	154	136	41	168	145	143
	180	176	148	41	181	150	151
	185	178					
	177						
	181	174					
C......	157	143	68	27	158	112	115
	187	163	59	25	174	117	118
	185	165					
	193						
	195	167					
D......	140	159	106	34	152	139	131
	136	155	107	37	148	133	133
	138	155					
	141						
	135	152					

interesting to note, however, that practice does affect the result in some cases even though the daily amount of work of each finger is slight and there is a week's interval, if not more, between each series.[1] Another fact to be noticed is that the practice gain in the more complex coördination, as in the case of the *A* movement, may be independent of the progress of a less complicated movement. In the case of subject *A* both *R*1 and *R*2 show a loss in rate in the second half of the series, *R*2's loss being considerable, and yet the *A* movement,

[1] The difference of practice gain between the two hands appeared in the difference of indices for right-handedness, p. 466.

which is a coördination of these two, shows a decided gain. Subject *C* shows a gain by practice in *R*1 and *R*2, and a loss in the *A* movement. Finally the fact that the more voluntary movement, *CA*, shows practically no change in rate, should be emphasized.

The two horizontal columns next to the last column in Tables I. and III. are arranged to compare the averages of the two orders of procedure. The first one of these horizontal columns for each subject gives the averages when a single finger movement precedes the double, simultaneous movement, the second when the reverse order is used. In both series the rate for the *R* finger for all subjects except *C* and one figure for *B* is more rapid when the *R* succeeds than when it precedes the *S* movement. In the majority of cases the difference is as large or larger than the m.v. Only in two instances is the rate for the *L* appreciably affected by the order and the results are of opposite nature. The simultaneous movement in three cases is decidedly more rapid when it starts the series, in two instances the difference is greater than the m. v. The only results, then, that seem at all significant are those that show the *R* movements more rapid when they succeed the other movements. The possible explanation is that with this finger the warming up effect was greater than the fatigue. It must be remembered that there was a pause of two minutes between the 30-second tests which could very well be sufficient for recovery of this finger but not for the others. In the wrist-tapping test Wells used two and a half minute pauses and his results show an increase in rate as the series progressed.

Fatigue

The amount of fatigue in each 30-second series is calculated by finding the relation of the difference in rate of the first and second 15 seconds to the entire 30 seconds. These were found for the results in Tables I. and III. and are given in decimal form in table VIII. The absolute differences are also given. For instance, the first figure in column 3, *i. e.*, .047, was found by dividing the rate of *R*1 which is 155 into 7.3 which is the difference of the two 15-second halves.[1] The averages for all the subjects are given in column 10.

Most striking is the fact that fatigue is greatest for the *A* movement and that there is no fatigue in the *CA* movement. It must be remembered that the *A* movement is the rapid antagonistic movement, the *CA* movement is very slow and one set of muscles rests while the other reacts. This readily explains

TABLE VIII

	Subject *A*		Subject *B*		Subject *C*		Subject *D*		Av.
*R*1	7.3	.047	6	.034	12	.071	6	.044	.049
*R*2	6.5	.041	4.5	.028	8	.053	9	.059	.045
A	9	.071	7	.05	4	.07	6	.056	.062
CA	—1/8		—1/18		—2/5		—1/3		
S	7.5	.042	5	.028	5.5	.033	6	.04	.036
*R*1	4	.025	5	.027	11	.059	6	.043	.039
*L*1	5.5	.044	4	.027	5	.043	7	.053	.042
S	2.5	.017	5.5	.034	4.5	.036	6	.046	.033
*R*2	5	.031	5	.028	7	.042	10	.065	.041
*L*2	5	.039	5	.033	2	.017	6	.045	.033
S	6	.039	5	.031	6	.046	3.5	.026	.035

the absence of fatigue in 30 seconds. This is not only true for the averages but with few exceptions for all the subjects. For subject *C* the *A* movement fatigue is about the same as the *R*1 fatigue. The *S* movement for *R*1 and *R*2 for two subjects is the least fatigueable, for the other two it is the same as the lowest index of the single finger. In the *S* movement for *R*1 and *L*1 and *R*2 and *L*2 the index is the lowest in three instances and only twice is it higher than both of the single movement indices.

It may be said, therefore, that in general the simultaneous movement of two fingers for 30 seconds does not show more, and very often less fatigue than one finger. A comparison of *R*1 and *R*2 in the two halves of the table shows as other experiments have before, that practice has the tendency to reduce the fatigue. Only in one instance out of eight is it

[1] In view of the manner in which the results were recorded, it was thought better to divide the periods in halves rather than to compare the first five seconds with the averages of the 1st, 2d, 3d, 4th, 5th and 6th five second periods as Wells did. *Loc. cit.*, p. 469. His index is higher probably because the initial spurt has thus more influence on the result, it being reduced in the above index by the results of the 2d and 3d five-second periods.

greater with practice. As has frequently been found the decrease of fatigue is the important factor in practice gain. No correlation can be found between the fatigability of the two hands. Nor can a general statement be made as to the fatigue index of the right as compared to the left hand. In three subjects the relation between *R2* and *L2* and *R1* and *L1* in regard to fatigue is in the same direction, except that subject *B* shows the same fatigue for *R1* and *L1* while *R2* is less than *L2*, but subject *D* shows opposite results. For him the fatigue for *L1* is greater than for *R1* and for *L2* less than *R2*. One cannot say, therefore, that if the *R1* finger is more easily fatigued than the *L1* finger, that the *R2* finger will be more easily fatigued than the *L2* finger.

Variations.

There is less variation in the left-hand movements than in those of the right hand. This is what both Bryan[1] and Wells[2] found for wrist movements. The small m. v.'s accompany the slower reactions but there are even indications of a less relative variation on the left side. The simultaneous movement shows about the same variations as the single movements. The *CA* movement shows the least variation of all the movements. This movement being very slow (fewer taps per 30″) the relative variability is higher than with the other movements. No general statement can be made in regard to the *A*-movement. There is a tendency for it to be relatively larger than that of the single movement. Absolutely it is sometimes larger and sometimes smaller.

Summary and Conclusion

If the index and second finger of the right hand are tapped simultaneously as rapidly as possible the resulting rate according to the results of four subjects of varying degrees of motor ability, is faster than the rate of the slower finger and may even be more rapid than the faster finger when tapping singly. There is doubtless a more or less innate coördination

[1] *Loc. cit.*, p. 163.
[2] *Loc. cit.*, p. 480.

between the movements of the fingers of the same hand caused by the biologically important grasping reflex. When the extensor of the index finger is innervated a tendency for the symmetrical extensor of the next finger to move is also observed.[1] Ths impulse must be inhibited and it is probable that this inhibition also extended to the motor half-center of the first finger causing a loss in rate of movement. An inhibition somewhat similar in nature has been demonstrated by Sherrington in his experiments on the stepping reflex when he simultaneously stimulated two afferents which are antagonistic in their effect. He says: "Of the two afferents concurrently stimulated, that one which when stimulated alone causes flexion of the joint excites the flexor half-center and inhibits the extensor half-center; and the other afferent, which when stimulated alone causes extension of the joint, excites the extensor half-center and inhibits that of the flexor. When both afferents are stimulated simultaneously with appropriate intensity, the discharge from the flexor half-center represents the algebraic sum of the opposed excitation and inhibition which the two afferents individually exert on it, etc."[2] In our experiment it is the inhibition of one flexor half-center which is communicated to the other flexor half-center and this inhibition is then compounded algebraically to the excitation of the extensor half-center. When both fingers move simultaneously this inhibition is removed. This at least seems to be a plausible explanation of the fact that the simultaneous movements were faster than either single movement. Whether it was the only factor could readily be tested by tapping simultaneously symmetrical fingers on two hands. It was found that under these conditions the movement of the slower finger was facilitated, the degree varying with different individuals. The two fingers together, however, never tapped faster than the faster finger.

[1] L. Huismans states "Homolaterale M–B. (Mit-Bewegungen) sind in den weitaus meisten Fällen auf eine Irradiation des Bewegungsimpulses in der Hirnrinde Zuruckzuführen" ('Über Mitbewegungen,' *Deutsche Zeitschrift für Nervenheilkunde*, 40, 1910, p. 233).

[2] 'Reflex Inhibition as a Factor in the Coördination of Movement and Postures,' *Quarterly Journal of Experimental Physiology*, 6, 1913, page 269.

Numerous examples are to be found in the literature relative to the influence of the movement of one side of the body upon that of the other. In the simple reaction experiment Paul Salow found that the reaction time for simultaneous movements of two hands was longer for each hand respectively than when they reacted separately.[1] M. L. Patrizi found in his ergograph tests that in simultaneous action both hands did less, but when they worked alternately the right hand action reinforced that of the left hand. He believes that one cannot give maximal attention to the two simultaneous acts.[2] W. W. Davis found that in general the right hand tapped more rapidly alone than in connection with either the other members. Two of his subjects were able to tap more rapidly when all four members were tapping. He remarks that "with a longer practice the right hand, in multiple tapping, would undoubtedy excel in rapidity its record while tapping alone." He also concludes from his results that "there is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or position." In this work Davis was not interested in the influence of the faster on the slower member.[3] Mention should also be made of the fact that a paralyzed member may be moved by moving a healthy member.[4]

[1] 'Untersuchungen zur uni- und bilateralen Reaktion. II. Einige Versuche am Chronographen,' Psychol. Stud., 8, 1913, pp. 506–540).

[2] Patrizi writes: "Les recherches que j'ai rapportées dans ce mémoire nous font admettre une uncompatibilité d'états psychiques, même quand il s'agit de la coïncidence, dans le même instant, d'impulsions maximales destinées à des mouvements symétriques et homogènes et qui sont habituellement accouplés." In regard to Féré's results, which indicate that when the left hand is almost fatigued its capacity can be increased by the movement of the right hand, he says that an increase will not occur if the two movements are exactly simultaneous ('La simultanéité et la succession des impulsions volontaires symétriques,' *Arch. Italiennes des Biol.*, 19, 1893, p. 138).

[3] 'Researches in Cross-Education,' Yale Studies, 6, 1900, pp. 6–50.

[4] See J. Grasset, 'L'action motrice bilatérale de chaque hémisphere cérébral,' *L'annee Psychol.*, II., 1904, pp. 434–445. It is interesting to note that W. P. Lombard said some years ago: "Not enough work has been done to admit definite statements concerning the strengthening or weakening effect of the action of the one hand upon the other. . . . The few observations which have been made with reference to this question favor the idea that if one hand acts simultaneously with the other it tends to weaken rather than strengthen its movements. This effect is not a constant one, however, as

In searching for an explanation for this contralateral facilitation of the slower movements by the faster, the fact of sympathetic movement seems the most significant. Everyone has had the experience when the member which one desires to move is held or when it has become fatigued, of moving the opposite member. In this regard Ch. Féré remarks that the examples drawn from his research "indiquent que, lorsqu'il existe un obstacle à un mouvement volontaire unilatéral, l'influx nerveux a une grande tendance à prendre la voie symétrique du côté opposé." This tendency is greater in children, being later more or less suppressed.[1] In the simultaneous movement of the two fingers the faster finger must be held back to synchronize with the slower. According to the above if the one finger were not already moving while the other finger was being held back from its full movement the sympathetic movement of the former would probably be evident. When it is moving at the same time as the finger which is being somewhat retarded, its movement is facilitated by the surplus energy of the faster finger. There is also another explanation or, perhaps, a second factor in conjunction with the above and that is the increased amount of peripheral stimulation, *i. e.*, the contact with the board and bar. We know from the work of Sherrington, Alexander Forbes, T. Graham Brown, and others that the afferent impulse on one side of the body may cause a contralateral reflex. Now the afferent path on the ipsolateral side may be fatigued and the stimulus on the contralateral side may become more effective. Alexander Forbes says: "The fact that central fatigue induced through one afferent nerve usually does not impair the reflex involving the same muscles induced through another afferent nerve supports the conclusions of Lee and Everingham that this fatigue does not involve the moto-neurones, and accords with the view of Sherrington that its seat is the synapse."[2] This explanation

many exceptions occur" ('Alterations in the Strength which occur During Fatiguing Voluntary Muscular Work,' *Jour. of Physiol.*, 14, 1893, p. 116).

[1] L'alternance de l'activité des deux hémisphères cérébraux,' *L'Année Psychol.*, 8, 1901, p. 148.

[2] 'The Place of Incidence of Reflex Fatigue,' *American Journal of Physiology*, 31, 1912–13, p. 122.

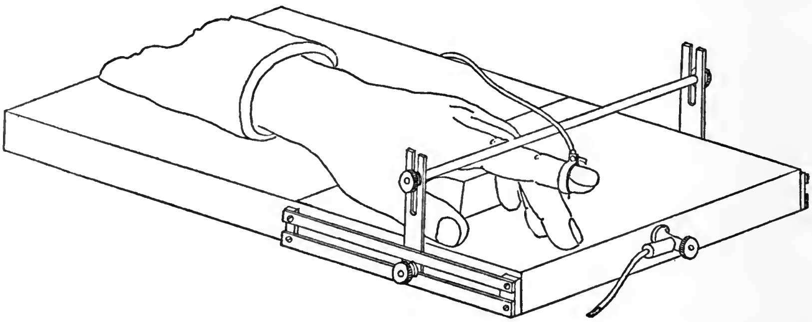
can also be applied to simultaneous tapping on the same hand. Owing to the fact, however, that these movements were never carried on sufficiently long for any great fatigue this factor must play, if at all, a very small rôle.

Thus far we have been discussing simultaneous movements of symmetrical members. This facilitation was also observed between asymmetrical members but it was not so great. There can be little doubt that the coördination is less perfect the more asymmetrical the members are. Even the transfer of practice is greater to symmetrical parts.[1] There is not, however, a very high degree of correlation between increasing symmetry and increasing facilitation with all the subjects. In order to explain the results an antagonistic factor has been suggested, *i. e.*, that, ceteris paribus, the greater the difference within limits between the rates of the two fingers the greater the facilitation of the slower. This difference increases with the asymmetry and at times causes an asymmetrical pair to show greater facilitation than a more symmetrical pair. An explanation which includes two opposing factors can explain anything and should not be used without strong proof. There are, in this series of experiments, instances where the degree of symmetry is a constant and we see here the effect of difference in rate in the direction just mentioned. We have also cases of similar degrees of difference in rate and here the effect of symmetry can be seen. The results are not sufficiently consistent to be conclusive. They are, however, suggestive and the explanation founded upon them must necessarily also bear that adjective.

2. An examination was also made of the simultaneous movement of the index and first finger of the right and left hand when these movements were in opposite directions, a combination of movements similar to the stepping reflex and spoken of in this paper as alternating movements. Here there is double reciprocal innovation according to Sherrington. The four subjects were divided into two groups in respect to the amount of coördination. With three of the subjects each finger was able to do about three quarters as much in this

[1] See W. W. Davis, *loc. cit.*, page 49.

combination as when tapping alone, except for one of the subjects with the left hand. The loss for each finger due to this combination of impulses was only 25 per cent., so that in the same time the pair was able to do one and a half times as much as a single finger. The fourth subject showed a much poorer state of coördination. Two of the three subjects with good coördination were piano players and the other who had a low efficiency with the left hand had his fingers of that hand trained for another system of coördination, for violin practice. Judging, however, from the great differences be-



tween these subjects and from observations on others, it seems probable that this test shows some fundamental differences in motor coördination. It would appear profitable to gather further data with special reference to the effect of practice, the tests here described being too few to draw any conclusion from them on this point.[1]

Another fact which appears clearly in the results is that the amount of coördination between the two reflexes does not run parallel with the development of these reflexes. The subject who has the worst coördination has one of the highest

[1] Sherrington, in speaking of grace in walking, says that "the proper execution of the act ensures a moment of complete rest to each of the opposed motor centers engaged." *Loc. cit.*, p. 271. T. Graham Brown found very decided differences in the reflexes of the cat " . . . these individual variations are probably due in great part to more fundamental differences in the constitution of the nervous centers. Some cats are 'walkers.' They exhibit in a marked degree the phenomena of 'narcosis progression.' Other cats are 'scratchers.' In them the scratch-reflex is peculiarly excitable" ('Studies in the Physiology of the Nervous System, XIV. Immediate and Successive Effects of Compound Stimulation in Spinal Preparations,' *Quarterly Journal of Experimental Physiology*, 7, 1914, p. 200).

rates in single tapping. He is a subject with little piano practice, so that this ability in single tapping substantiates Raif's assumption (vide supra, p. 464) that piano players do not show any special ability in rate of tapping.

3. The term complete alternation has been applied here to successive movements of the two fingers; one finger completes both the extensor and flexor movements before the other begins. With all the subjects and with both right and left hand the two fingers in this combination tap only one quarter the amount that they do in the same time in tapping continuously and alone. The loss in this form of alternating movement is about 50 per cent. This combination is a system of successive reflexes. The loss in efficiency is probably in great part due to the necessary inhibition of the previous reflex. As Sherrington says ". . . there will persist during the new reflex activities belonging to the old with, in result, confusion of the two. Rarely, indeed, can it happen normally that the reflex machinery in executing a train of different reflexes is actuated by a train of different stimuli, each one of which abruptly ceases just as the next one begins."[1] And further: "For orderly and unconfused sequence of reflex acts—also of willed acts—central inhibition is a necessary element of coördination in the transition from one muscular act to another."[2] Another reason for the loss is that in the single tapping movement inhibition of one of the antagonistic movements increases the tendency for that reflex to discharge, causing what might be termed a rebound. If, however, the finger must rest after the completion of each flexor movement until the other finger has completed its movement, most of this post-inhibitory effect is lost.[3]

4. Even with an interval of one week between the tests a practice gain in most of the movements is noticeable. An exception must be made with the most voluntary movement, namely, the complete alternation. The subject who had the worst coördination in the alternating movement even showed a loss. The practice effect in the alternating movement does

[1] *Loc. cit.*, p. 275.

[2] *Loc. cit.*, p. 276.

[3] See Sherrington, *loc. cit.*, p. 278.

not run parallel to the practice effect of the fingers tapping separately. A gain in the former may be accompanied by a loss in the latter. Practice in the simultaneous movement, on the other hand, does follow that of each finger when working separately. There is some evidence that practice affects the right hand more than the left.

5. Fatigue is noticeable for all the movements except the complete alternation.[1] It is greatest for the alternating movement and generally least for the simultaneous movement. That is, during thirty seconds there is less fatigue when two fingers are working simultaneously than when one is working alone.

6. The index of righthandedness is not necessarily the same for all the movements, nor is it always the same between the different pairs of symmetrical fingers.

7. The variations in tapping rate are less for the left hand.

[1] Wells says: "To sum up, the maximum rate of repeated voluntary movements is a function that practically every investigator working with sufficiently accurate methods has found to be subject to fatigue effects, though the degree of this subjection has differed considerably" ('A Neglected Measure of Fatigue,' *Am. Jour. of Psychol.*, 19, 1908, pp. 352–3.)

RETINAL FACTORS IN VISUAL AFTER-MOVEMENT

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The present paper is a continuation of a previous study made by myself on the after-effects of visual motion.[1] On pages 255–257 of that article, comments are to be found bearing upon retinal factors effective in the production of the illusory motion which occurs after gazing for some time at a series of moving bands. Again on pages 275 and 276 an experiment is described in which a moving area was used of a size sufficient to cover most or all of the visual field. In this test an after-movement was observed which went in the same direction as the real or stimulus movement. Ordinarily the after-effects seen are secured with small stimulus areas and move in a direction contrary to the real movement. In the earlier study, it was held that the normal after-effect was a result of some or all of the following factors: eye-muscle strain, association factors and retinal processes which were probably fading after-images. The present work is primarily concerned with a determination of the nature of the effective retinal factors. The conclusion reached is that this factor is a *streaming phenomenon* which moves through external space in a direction opposite to that taken by the stimulus area. The evidence for this is necessarily of an introspective and theoretical nature.

The data presented were obtained largely from three subjects, two of whom had served in the earlier tests. A number of observations were made on untrained subjects. The apparatus used was the large striped curtain (six feet long by four feet high) described in the earlier paper. This curtain could be made to move either up or down. A small square of white paper was held close in front of the curtain by a thread and served as a fixation point. A number of

[1] Hunter, Walter S. 'The After-effect of Visual Motion,' Psych. Rev., Vol. 21, pp. 245–277, 1914.

newspapers were tacked on the wall above the curtain to serve as a projection field for the after-effects. Other fields will be mentioned later. Tests were also made with rotating black spirals and with a striped Scripture drum.

Experimental Data

The immediate problem from which the present tests took rise was that of the occurrence of an after-movement (abbreviated as a.–m.) in the same direction (an s.–a.–m.) as the stimulus movement. It will probably conduce to clarity, if the observed facts are grouped accordingly as this type of after-movement is or is not seen. The facts concerning streaming can then be presented.

Observations on S.–a.–m.—If an observer is seated about one meter from a moving curtain of the type used here and fixates a point in front of the curtain for twenty seconds, upon turning his eyes to a series of newspapers tacked upon the wall above the curtain, he will usually see the following phenomenon: the fixation point of the projection field and probably about one or two square feet of the immediately adjacent paper will be seen to move in the same direction as the real or stimulus movement. This movement is most rapid during the first second and is always a drifting bodily movement of the projection field. It is identical in quality or kind with the usual after-movement seen when a small stimulus area (parallel stripes on the Scripture drum or the Archimedian spiral) is involved and when the projection field is the stopped stimulus area. The difference between the two phenomena is one of direction only.

I have never secured an s.-a.-m. using the stopped curtain as a projection field. (The same is true for spirals and for striped fields of small extent.) It is all but impossible to secure it if the observer sits within eight inches of the moving curtain even though he projects the after-effect above the curtain as before. When the observer sits some two and a half meters from the moving curtain, he can secure the s.-a.-m.; but in the tests reported here, it has not been so easy as from the distance of one meter.

The following test has also been made. A white cardboard 11 in. × 14 in. with an aperture 4 in. × $7\frac{1}{2}$ in. was placed in front of the moving curtain. An observer seated at from one to three meters fixated a point on the edge of the aperture for twenty seconds and then fixated a dot on the papers above the curtain. A stationary black negative after-image of the cardboard was seen. In this a.-i., there was a rushing movement either upward or downward in the same direction as the real or stimulus movement. Outside the black a.-i. and in the area corresponding to the aperture of the card, a movement is usually to be seen which goes in a direction opposite to the real movement. (This we shall term an op.-a.-m.) Some observers reported the whole projection area to be involved in an s.-a.-m. When the after-effect was projected upon either a black or a white cardboard, no movement was seen in the negative after-image of the cardboard which had been in front of the curtain.

Observations on Op.-a.-m.—An after-movement in the opposite direction to that of the real movement is the usual and "normal" after-effect save under the circumstances just described. When the moving area of large extent is used and the after-effect is projected upon newspapers on the wall above the curtain, an op.-a.-m. is seen particularly below and to the sides of the fixation point. Simultaneously the s.-a.-m. described above is seen around the fixation point. At times the op.-a.-m. appears later than the s.-a.-m. Some observers see a drifting op.-a.-m. around and over the fixation point also. In this case the movement seems to be between the observer and the paper. These central and peripheral op.-a.-ms. are nearly always described as a film moving over the projection field and not as a movement of the field itself.[1] It is a radically different type of movement from the s.-a.-m. seen about the fixation point. The op.-a.-m. is a film which is either described as a series of shadows or as a 'rainfall' or 'sleet' or 'dust film.' Frequently this film will drag the projection field along with it; but even in this case there is no drifting bodily movement as is seen in the s.-a.-m.

[1] See Hunter, *op. cit.*, pp. 247–8 for results of earlier tests.

If the projection field is the stopped curtain, an op.-a.-m. is seen which is a slow drifting movement of the field itself. A film has never been seen by my observers under these conditions in the present tests. This is also true when stimulus areas of small extent are used, whether they are spirals or systems of parallel lines. In the earlier tests just referred to in the footnote, such a film was suggested by the observers. The absence of this in the present tests is probably due to the fact that the subjects were mainly familiar with intense films.

Observation on Streaming.—It will be well to preface these observations with a few historical comments. Our interest lies chiefly in the observations of Pierce,[1] Szily[2] and Ferree[3] who wrote in that chronological order. The present writer does not care to examine any claims as to originality. It is quite probable to his mind that the phenomenon here under consideration has long been known. Schilder,[4] *e. g.*, quotes from Purkinje's 'Beobachtungen und Versuche' passages on the streaming phenomenon that parallel Szily's. Ferree accepts Pierce's explanation as a basis for distinction between their respective observations.

Pierce's observations were made with a stationary black and white striped area. The projection field was a plain black ground of cloth or card. After some twenty seconds' fixation, upon turning to the projection field, "The appearance is that of *a thin cloud of fine white dust moving across the field of vision.*" The direction of moving is always perpendicular to the striped lines. The same observations were verified with concentric circles. Again, "if the usual field of fixation be divided by a vertical strip of some uniform color, no 'drift' will be seen in that portion of the field corresponding to the strip." In explanation Pierce says "it seems probable on the whole that the ultimate explanation of this as of all after-

[1] Pierce, A. H., 'Studies in Space Perception,' pp. 331–8, N. Y., 1901.

[2] Szily, A. v., 'Bewegungsnachbild und Bewegungskontrast,' *Ztsch. f. Psych. u. Physiol. d. Sinn.*, 1905, Bd. 38, S. 124.

[3] Ferree, C. E., 'The Streaming Phenomenon,' *Amer. Jr. Psych.*, 1908, Vol. 19.

[4] Schilder, Paul, 'Über auto-kinetische Empfindungen,' *Arch. f. ges. Psych.*, 1912, Bd. 25, S. 71–3.

images of motion, will be somehow formulated in terms of impulses to movement aroused by the particular stimulation that precedes. Perhaps the experiments here recorded may contribute their mite towards this final explanation, if that ever comes."

Szily's essential statement is made in connection with his discussion of the s.-a.-m. and is as follows: "Die hier geschilderten Nebenerscheinung begleitet, die dem minder unsichtigen Beobachter allerdings erst dann auffällt, wenn sie unter gewissen Versuchsbedingungen in erhöhtem Masse zur Geltung gelant. Ich selbst sehe unter allen umständen in der Peripherie das Regelrechte [op.-a.-m.] Bewegungsnachbild, zumeist in der Hülle eines strahligen Nebels, in entgegengestzter Richtung ablaufen. Bedient man sich eines noch dichteren Streifenmusters, verlängert man die Dauer des objektiven Eindruckes, setzt man die Beleuchtung des Projektionsgrundes herab, so verbreitet sich diese Zone des regelrechten Bewegungsnachbildes mit gleichzeitig erhöhter Intensität immer mehr zentrumwärts, so dass sie selbst dem Ungeübten auffallen muss. An den Konturen im Bereich des direkten Sehens aber, solange sie als wahrgenommen werden, vollzeiht sich auch dann noch stets die paradoxe [s.-a.-m.] Scheinbewegung."

From Ferree we may take the following: "When one sits with lightly closed lids, which must be kept from quivering, before a bright diffuse light such as that of a partly clouded sky, and looks deep into the field of vision thus presented, beyond the background as usually observed, one sees about the point of regard, after the field of vision has steadied, slowly moving swirls. These swirls have the appearance of *streams of granules*[1] moving in broad curves now this way, now that, seemingly without order, unless a noticeable eye-movement occurs, or is made voluntarily, when the direction of streaming changes to that of the eye-movement." Ferree is inclined to identify the phenomenon with a diffusion of lymph over the retina. Aside from the above general description, our interest centers in the drawings which his subjects made. These

[1] Italics mine.

represent the qualitative character of the phenomenon and are identical in everything save direction with those made by my subjects and represented in Fig. 1.

The quality of the streaming observed by the subjects of the present tests was either of two kinds: (1) 'a shadow-like succession of clouds' of rather medium velocity; or (2) a 'sleeting,' 'snow-fall' or 'dust streaks' which went usually at a high velocity. In any case, *this streaming is always in a direction opposite to that of the real or stimulus movement.* Neither type appeared in these tests when the projection field was the stopped stimulus area. The first type has never appeared when the projection field is a plain black or white cardboard. It has been observed in the periphery where the after-effects have been projected upon the paper above the large curtain, and all over the visual field when the after-effect has been projected upon floors and upon plain gray walls.

The greater theoretical significance attaches to the 'sleet' film. This will appear as the discussion advances. I have assumed that the best method of detecting what takes place in or on the retina is to project the phenomena upon black or white surfaces (cardboard, shadows or the black of the retinal field). Under these conditions the phenomena appear unmodified by variations of the external world.

A 'dust' or 'sleet' film—which is best represented by *A* in Fig. 1—is always seen if the after-effect of the moving curtain is projected upon plain surfaces as just described. In these cases it is pure, *i. e.*, unmixed with the first type. Pierce's test was carried out with the present subjects. The stimulus areas were the large striped cloth, the striped Scripture drum and a black spiral on a white disc. In each case after a fixation of twenty seconds, the observer turned his eyes to a black or white ground and described the phenomena seen. Drawings of the after-effects were requested. When the stimulus area was one of parallel stripes, the after-effect was a 'dust film' as described by Pierce. It differed from that secured with a moving stimulus area *only in its fainter intensity and in its less certain direction.* By the last phrase, I mean this: the Pierce dust film was perpendicular to the lines, but the film could be interpreted as moving either up or down. With the dust film produced by a moving stimulus area, there was no doubt but that the film moved in a direction opposite to that of the stimulus area. The drawing *B* in Fig. 1 represents the phenomenon seen on a plain card after having fixated either a stationary or a rotating spiral for twenty seconds. The difference just described for parallel

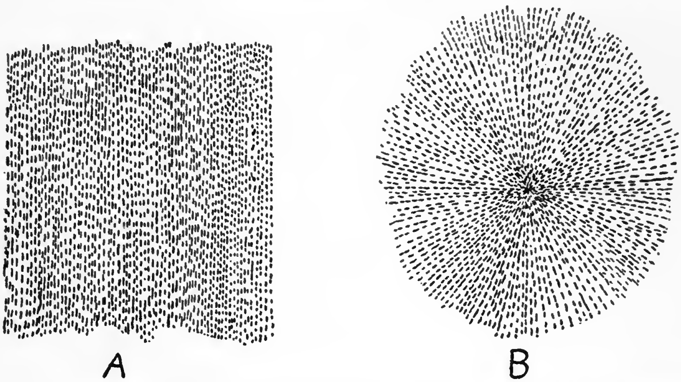

FIG. 1. Patterns of Streaming Observed in the Present Tests. *A*. Seen with either a moving or stationary area of parallel stripes when the projection field is a plain surface. *B*. Seen with either a moving or a stationary spiral when the projection field is a plain surface.

lines is the only one that holds here also. In each case the film has appeared before the negative after-image of the stimulus area.[1]

Earlier in this paper, tests were described where a white cardboard with an aperture was placed in front of the large moving curtain. In order to analyze the nature of the retinal factors producing the peculiar after-effects already described, the phenomenon was projected upon a black card. In the black negative after-image of the white card (where the s.-a.-m. had been under other conditions), there was no movement; but in the remainder of the card which corresponded to a stimulated retinal area, the dust film was seen (moving of course in a direction opposed to that of the stimulus movement). There can be little doubt, then, but that the

[1] See Hunter, *op. cit.*, p. 258.

s.-a.-m. seen in the black after-image when this was projected upon the newspapers was due to association factors. The movement of the film produced the impression of a movement of the objects within the after-image in an opposite direction. Szily[1] describes a similar experiment under the caption 'Kontrast im Bewegungsnachbilde.' I am uncertain how he would explain it. However other cases of 'contrast' are accounted for on the basis of a higher threshold for the perception of movement in central than in peripheral vision. This, I think, in all of the cases described, is a needless hypothesis.

The author has repeated some of Ferree's tests upon himself and one of his subjects. It has been possible to confirm the existence of a normal streaming activity in the eye. This when represented by drawing is exactly similar to the pictures of dust films found in the present experiments upon stationary and moving striped areas.

Theoretical Considerations

To keep theory close to observed fact, it seems to the present writer that the choice of explanatory retinal processes lies between a "streaming phenomenon" and fading after-images. The latter is the more conventional and therefore the more apt to be favored. The evidence in its favor, however, is purely hypothetical and non-observational. Hence as an hypothesis it may well be faulty. What is actually seen when the eyes are either closed or turned toward a uniform field is a dust like film in constant movement. To pass from Ferree's streaming to Pierce's streaming, it is necessary to assume: (1) that the after-image effects of the striped field make the dust film more vivid; and (2) that in some manner the striped field gives definite direction to the streaming. I can offer no solution for the last statement, although it is by no means absurd and impossible. Concerning the first assumption, there can be no difficulty for experiment shows that the film can be more readily seen upon certain backgrounds than upon others. This will also account for the fact that the after-effect is largely confined to (*i. e.*,

[1] Szily, *op. cit.*, S. 126.

visible on) an area corresponding to that stimulated by the real movement.

Let us examine each proposed retinal factor with the aid of Fig. 2. Of the phenomena described above, it is necessary to bear in mind particularly the s.-a.-m. about the fixation point and the dust-like film which is seen to pass over the projection field in a direction opposite to that taken by the stimulus area. Our discussion will be further aided by a quotation from Wundt giving the conventional statement of the after-image theory. "Indem ein schwaches Nachbild der gesehenen Bewegung im Auge zuruckbleibt, scheint ein fixiertes Objekt infolge der Relativität der Bewegungsvorstellung in entgegengesetzten Sinne bewegt zu sein. Das Nachbild, in der Regel zu Schwach um selbst gesehen zu werden, genügt doch um auf das Objekt die zu seiner eigenen entgegengesetzte Richtung zu überträgen."[1]

In Fig. 2, *O* represents the curtain moving up. i. is its

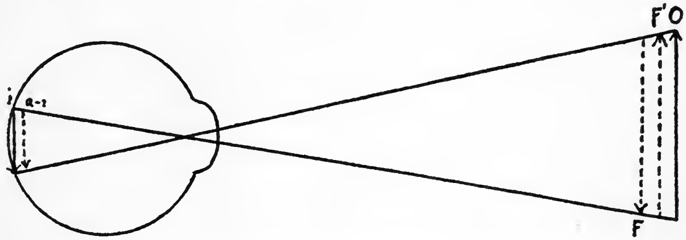

Fig. 2. Relations between External Movements and Retinal Processes.

image on the retina where the movement passes down. a.-i. stands for the fading after-images which move in the same direction as i. and not up with the curtain, *O*. Let us assume that twenty seconds have passed and the striped curtain or drum is stopped. Upon whatever objects the eye is turned the stationary images of these objects will be cast upon the area i. over which the fading after-images (a.-i.) are passing downward. We have two possibilities now: (1) The fading after-images do not enter consciousness. In this case a.-i. passing down over i. gives the impression of i. moving up, *i. e.*, is the same as though a.-i. were stationary

[1] 'Physiol. Psych.,' Bd. 2, S. 622, 6. Aufl., Leipzig, 1910.

and i. were moving up. i. projected, then, into the external world would be seen as *O* in movement downward. (Here I assume that a sub-liminal retinal process is not projected into space. If it were it would be discussed as though the a.-i. came into consciousness and hence would be treated under (2) below.) The present case can explain an s.-a.-m. about the fixation point by claiming that for some reason the movement of a.-i. on the retina is, relatively, non-effective in central vision. Szily does this by appealing to the high central threshold for the perception of movement. The truth in this point lies only in the observable fact that the direction of apparent movement differs in the periphery and in the center. The explanation of this lies in the fact that the clear bold contours in central vision dominate over the moving process as compared with the peripheral contours. When the after-effect is projected upon plain cardboard the dust film is seen all over the field. The effectiveness of contours in inhibiting the perception of after-movements is shown by Szily,[1] and is continually verified in tests on after-movements. The validity of that investigator's threshold hypothesis is further impugned in the following case. (2) *If the fading after-images do enter consciousness*, they do so as projected into external space. a.-i. is then seen as F′ moving up. *O* seen through this appears to move down. The damning fact, however, is that the film or 'sleet' is actually seen not as *F′* but as *F* and moves down. *It cannot therefore be the projection into space of fading after-images.* The retinal equivalent of the film which is seen to move down over the external objects must be the passage of a stimulation upward. However before ruling fading after-images out entirely, it is well to consider the following possibility: May it not be that a.-i. passing down over i. is the equivalent of a wavering or filmy passing of i. upwards on the retina? This when projected would give *F*. Such an hypothesis appears fairly plausible when *F* is projected upon print or other equally diversified objects. In that case a cloudy or wavy film is often seen to pass down. The plausibility of the theory,

[1] *Op. cit.*, S. 110.

however, vanishes when one bears in mind the cases where a dust film is seen even when the after-effects are projected upon the printed sheets and particularly upon dark shadows or cardboards. This film has been sufficiently described already. It is exactly like fine particles of dust moving rapidly through space. One would not expect fading after-images to be of this nature.

A final objection to the fading after-image theory is as follows: The continued passage of a series of stimuli across the retina fatigues the retinal elements. If the elements can recover between stimulations, a movement will be seen. This is what actually occurs. Now after the stimulation has ceased, it is conceivable that one wave of recovery (fading a.-i.) would sweep across the retinal area. I see no reason why successive waves should do so. And yet this would be necessary in order to secure such temporally extended after-effects as are actually observed.

In the light of what has gone before in this paper, we are therefore led to interpret visual after-movement in terms (so far as the retina is concerned) of a streaming phenomenon which passes across the retina in a direction opposite to the image of the objective movement. I confess my ignorance of what this streaming may be. It may be lymph currents, as Ferree supposes. It may be an electrical phenomenon. The uncertain status of its exact nature cannot, however, overcome the necessity of its postulation.

The theory readily explains the phenomena described: The dust films are the projections into space of the streaming. The s.-a.-m. is due to the invisibility of the film because of the clear bold contours of central vision as compared with peripheral vision. This results in the interpretation of the external objects as moving in a direction opposite to the film and occurs under special circumstances only. In the regular after-movement with the large areas, the film is itself visible and drags the external objects along with it. With small areas the same thing occurs with a minimum of film visibility.

EXPERIMENTAL DATA ON ERRORS OF JUDGMENT IN THE ESTIMATION OF THE NUMBER OF OBJECTS IN MODERATELY LARGE SAMPLES, WITH SPECIAL REFERENCE TO PERSONAL EQUATION

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I. Introductory Remarks

In attempting to estimate the number of a considerable group of objects of the same kind, the observer can seldom state the true number but generally gives one which is either too high or too low. If a series of such estimates by the same observer be considered it may be found that there is no tendency for the errors in excess of the true value to be more numerous or greater in amount than those in defect. In such a case the average of the deviations of the estimates from the true number of objects will be sensibly zero.[1] The individual making the estimates may then be said to have no personal equation. Other individuals, however, may have a definite tendency to err on one side of verity in their evaluations. Such may be said to have a positive or a negative personal equation, as the case may be.

Personal equation is not the only factor which should be taken into account in determining the rank of an individual among a number passing judgment upon the value of any magnitude. An observer with no consistent bias towards over or under valuation may be characterized by very erratic judgment—assigning sometimes a value far in excess, at other times a value far in defect of the actual. Thus consistency or steadiness of judgment is also a characteristic of importance which should be taken into account in the comparison of individuals.

In this paper, I have presented several series of experi-

[1] The mean actually determined by experiment would be 0 plus or minus a smal amount due to the errors of sampling.

mental data bearing on these questions. Such materials, properly analyzed and interpreted, should be of value to the psychologist. The data, which are a by-product of long routine processes in experimental breeding, are presented solely for their intrinsic value as experimentally determined facts. The arrangement of the material is that suggested by the view point of a quantitative biologist, a biometrician. Comparison, criticism and interpretation are left to those having the necessary training. I trust, therefore, that the professional psychologist will overlook crudities in terminology, and accept the experimental data for what they may be worth when interpreted from his own point of view.

The circumstances leading to the collection of these data were the following:

At various times, I have found it necessary to obtain very large series of countings of bean seeds, either for germination tests or for determining the mean weights for different series. Various considerations (which need not be reviewed here) led to the conclusion that the counting could most easily, accurately and advantageously be carried out in units of 25, 50 and 100 seeds. By the slightest addition to our labor we could determine the accuracy of judgment in the estimation of these lots of 25, 50, 100 or 200 seeds. The advantages of so doing were threefold: (*a*) It gave a means of testing the personal equation and the steadiness of judgment of the three assistants who are responsible for a large part of the routine work in my laboratory. (*b*) Competition (for first place in accuracy of estimation) added a little spice to a long task which would otherwise have been the most monotonous drudgery. (*c*) It gave an extensive series of data on errors of judgment.

II. Experimental Methods

The method of the experiment was as simple as can be imagined.

The observer took from a container a handful of beans and poured as nearly as possible a specified number of seeds (25, 50, 100 or 200 according to the experiment) on the table.[1]

[1] The seeds were poured upon a dark gray felt paper mat. This was chosen

If there appeared to be too few, more were added, if too many, a portion were put back. The work was carried out so rapidly that there was no possibility of counting assisting in the estimate.[1] The number was then at once determined by counting and the deviation of the sample from the desired value was noted and recorded by the observer who made the estimate. A persistent effort was made by each observer to improve in succeeding estimates. The influence of this constant checking upon personal equation and upon steadiness of judgment seems to me the most interesting phase of the present study. It will be considered in a subsequent paper.

Except in one special case fifty estimates were made by each observer in the morning and another fifty in the afternoon. In general, this was attended to early in each half day's work. Each lot of fifty may be designated as a period. The periods were, with a single exception, consecutive except for a break over Sunday. Ample details concerning the individual experiments are given below. Four individuals took part in the work.[2]

The following series of experiments were made:

A. Our first series of experiments was made in May, 1912. The attempt was in every instance to lay out a sample of fifty seeds. Observer *A* made only 141 estimates and these with

because the color selected was easy for the eye although affording sufficient contrast with the (generally) white seeds and because the seeds do not roll about as badly on the felt surface. These conditions count for rapidity and comfort of work. Each of the observers occupied her own table, so that light and other conditions were perfectly familiar through long experience.

[1] The validity of this statement will be apparent from the fact that in the first experiment the average time required for pouring out the samples of fifty seeds, counting them twice, recording the deviation of the guess from the true number, and replacing slightly wrinkled or weevil eaten seeds by perfect ones was 86 minutes for Observer *B*, 83 minutes for Observer *C* and 80 minutes for Observer *D*.

[2] My own observations were too few and too continually broken into by extraneous matters to be of particular value. The other three are Miss Edna K. Lockwood, Miss Margaret G. Gavin and Miss Lily J. Gavin. Each of them has been with me for six years or more. It would be difficult to express adequately my obligation to them for their patient, conscientious and highly efficient assistance in the onerous routine, observational, clerical and arithmetical work of a biometric laboratory. For convenience the observers are designated hereafter by letters: Mr. Harris, Observer *A* or merely *A*; Miss Gavin, Observer *C*; Miss Lockwood, Observer *B*; Miss Lily Gavin, Observer *D*.

numerous interruptions. Observers *B–D* made each 700 attempts at laying out the required number. These are grouped in 14 periods of 50 trials each. In the case of *B* and *C* these were made in the mornings and afternoons of seven days which were consecutive except for Sunday. Observer *D* worked on the same schedule but was necessarily absent one afternoon, hence the 14 periods were distributed over 8 days. The data of this series will be designated as I*A*, I*B*, I*C*, and I*D*, the Roman numeral referring to the series and the letter to the observer.

B. The second series of experiments was made in November, 1912. Again the White Navy bean seeds were used and the attempt was to lay out samples of fifty seeds each. The work covered a period of two weeks, in which there was a morning and an afternoon period of 50 estimates each. Thus there were altogether 1,200 estimates by each of three observers, *B*, *C*, *D*. The estimates logically fall into two major periods of six days each, separated by Sunday. They are therefore designated as series II. and III. The appended letters designate the subjects, *B–D*.

C. The experiments were again taken up with the White Navy beans in February, 1913. Again two weeks were devoted to the work and 1,200 trials, in daily morning and afternoon periods of 50 each, were made by the three individuals *B–D*. This time 100 instead of 50 seeds was the number aimed at in the laying out of the samples. The first of the two weeks may be designated by IV., the second by V.

D. The last four days in July, 1913, trials at estimating 200 seeds were made by *B*, *C*, and *D*. Four days' work of 50 trials per day completed the countings that were necessary for the masses of Navy seeds then to be weighed. Because of the time required for the counting and recounting of samples of 200 seeds it was not feasible to do more than 50 estimates in a day. These were made consecutively and usually required a full half day's concentrated work. Doubling the number would have meant an abnormal mental and physical effort for those making the estimates. The experiment is designated as VI.

E. The Tuesday following the preceding set of estimates, (Aug. 5) which were closed on Thursday, a set of trials at laying out 25 beans, of a larger-seeded brown bean—"Ne Plus Ultra"—was begun. Sets of 50 estimates in the morning and afternoon were made daily except for Saturday afternoon (Aug. 9). Thus there were 9 periods each by *C* and *D*. These will be referred to as VII.

F. The week immediately following the preceding trials (Aug. 11–16) Observer *D* did one full week (12 periods of 50 estimates each) on the "Ne Plus Ultra" seeds; again the attempt was to lay out 25 seeds. Series VIII.

G. For the two weeks beginning August 25, 1913, Observers *B* and *C* made trials at the estimation of lots of 25 seeds, using a White bean somewhat smaller than the Navy on which the main bulk of these experiments was based. The first week embraced 11 periods, Saturday afternoon being out. The second week included only 9 periods, Sunday and Monday (Labor Day) separated the two lots. The estimates were closed with Saturday morning of the second week, when the supply of seeds which required weighing was exhausted. Note that Observer *B* had made no estimates since the end of July when she was estimating at lots of 200 seeds. Observer *C* had made no estimates since August 9, when she was working with large seeded brown beans, and estimating at fifties. The first week forms series IX. and the second X.

H. During the period of May 4 to May 9, 1914, inclusive, Observers *B*–*D* made estimates twice daily at 50 seeds of a large brown bean, Burpee's Stringless. Thus these estimates were made after a lapse of several months (August, 1913–May, 1914) since the last trials. For convenience these will be designated as Experiment XI.

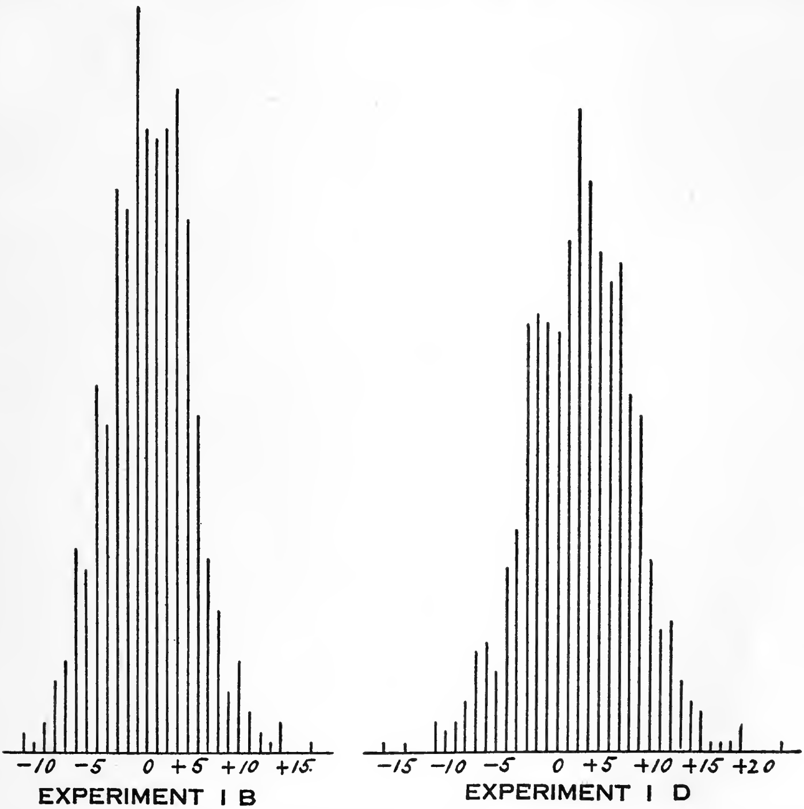
Thus there are altogether 28 sets of experiments, carried out by three observers, distributed over a period of two years, and comprising a total of 15,200 estimates.

The analysis of the data is carried out by the modern higher statistics, the notation of which is very generally familiar or easily accessible.

III. Presentation and Analysis of Data

1. *Personal Equation*

Data Tables *A–E* give the deviations of the number of seeds actually laid out from the desired number (25, 50, 100 or 200), *i. e.*, seeds actually drawn *less* seeds intended to be drawn. Note that the attempt was in each case to lay out a definite number of seeds, say 50. +15 indicates,



Diagrams 1–2. Distribution of Errors of Estimation in Experiment I. The heights of the ordinates indicate the frequency of deviations of different grades.

therefore, that 65 instead of 50, and −6 indicates that 44 instead of 50 were actually drawn.

Certain of these series are also represented graphically in Diagrams 1–2.

Two distinct problems are presented by these distributions and graphs. The first is that of personal equation properly so called; the second is that of steadiness of judgment.

By personal equation, we understand a bias in a given direction—a tendency to estimate too high or too low. If there be a personal equation, one observer will tend regularly to pour out too many seeds, just as another will tend to make the sample too small. By steadiness of judgment, we mean consistency in estimation. One observer may be more erratic than another, estimating now far too high, now far too low.

These points may to some extent be illustrated by the first two diagrams. We note that for Observer *B* the frequencies (represented by the heights of the bars) of the grades above and below 0 are about equal, while in the case of the Observer *D* the frequencies above 0 are distinctly greater than those below . Indeed in the cases of *D*, there are six groups of errors of observation above 0 which contain more cases than any class below 0. Apparently Observer *B* has little personal equation, while *D* has a pronounced tendency to lay out too many seeds—that is to underestimate the number of objects in a group. The diagrams also show somewhat the relative steadiness of judgment of the two observers. The deviations appear to be less widely scattered about the mean in the case of *B* than in that of *D*—judgment is apparently steadier, less erratic.

Numerically the existence of personal equation may be most simply tested for by determining the relative numbers of estimates in excess and in defect of the true value. For convenience the tables have been broken up into three compartments. At the head the frequency of cases in which the error was 0 (*i. e.*, in which the experimenter actually succeeded in laying out the number of seeds desired) is indicated. The frequencies of + and − deviations of various magnitudes are shown side by side in the two parallel columns. The totals of these columns give the data needed in answering in the most rough and ready manner the question as to the existence of a personal equation.

Of the 28 experiments made, the totals of the tables show that in only 3 cases is the frequency of minus deviations greater than that of plus deviations. Or in other words, in 25 cases out of 28 the experimenters *in the long run* made the error of laying out too many seeds.[1]

In comparing the frequencies of + and − deviations of the same magnitude (which have been placed in the tables in parallel columns to facilitate such comparison) it is also clear that in almost every grade of deviation represented by a material frequency the results in excess of the attempted number are more numerous than these in defect.

With results indicating in so striking a manner the existence of a pronounced personal equation on the part of the observers, the calculation of any probable error seems superfluous, especially since probable errors are given for a subsequent test.

A measure of the magnitude of personal equation as well as a demonstration of its existence and direction must be sought. This is most simply expressed in terms of the mean deviation of the estimates from the desired value.

Since in our experiments the attempt was made to lay out a sample of a given size, an excess of plus deviations either

TABLE I

PERSONAL EQUATION FOR INDIVIDUAL EXPERIMENTS

Experiment	Trials	Number Sought	Observer *B*	A/E_A	Observer *C*	A/E_A	Observer *D*	A/E_A
I.	700	50	$+.171\pm.107$	$+1.60$	$+.926\pm.106$	$+8.74$	$+2.170\pm.133$	$+16.32$
II.	600	50	$+.510\pm.098$	$+5.20$	$+1.023\pm.104$	$+9.84$	$+.592\pm.121$	$+4.89$
III.	600	50	$+.510\pm.080$	$+6.38$	$+.560\pm.089$	$+6.29$	$-.368\pm.114$	-3.23
II.+III.	1,200	50	$+.510\pm.063$	$+8.10$	$+.792\pm.069$	$+11.48$	$+.112\pm.084$	$+1.33$
IV.	600	100	$+.785\pm.188$	$+4.18$	$+1.043\pm.197$	$+5.28$	$-.212\pm.210$	-1.01
V.	600	100	$+.280\pm.151$	$+1.85$	$+.893\pm.156$	$+5.72$	$+.890\pm.172$	$+5.17$
IV.+V.	1,200	100	$+.532\pm.121$	$+4.40$	$+.968\pm.126$	$+7.68$	$+.339\pm.136$	$+2.49$
VI.	200	200	$+.805\pm.057$	$+14.12$	$+1.960\pm.058$	$+33.78$	$+3.055\pm.077$	$+39.68$
VII.	450	25	....	......	$+.280\pm.062$	$+4.52$	$+.751\pm.098$	$+7.66$
VIII.	600	25	....	......	....	......	$+.205\pm.064$	$+3.20$
IX.	550	25	$+.805\pm.067$	$+12.01$	$-.060\pm.058$	-1.03	....	......
X.	450	25	$+.433\pm.064$	$+6.77$	$+.167\pm.055$	$+3.04$	....	......
IX.+X.	1,000	25	$+.638\pm.047$	$+13.57$	$+.042\pm.040$	$+1.06$	....	......
XI.	600	50	$+.180\pm.090$	$+2.00$	$+.221\pm.087$	$+2.54$	$+.342\pm.121$	$+2.83$

[1] This is also conspicuously the case in the short series of trials by Observer *A*, whose estimates are not to be discussed in detail.

in number or magnitude, or both, is indicated by a mean error with the positive sign. This will indicate a tendency to underestimate any given quantity, since an actually larger number than that desired was laid out.

Table I. gives the results. Of the 28 means for the individual experiments 25 have the positive sign, *i. e.*, in 25 out of 28 experiments the observers had a tendency to lay out too many seeds.

The results are shown graphically in Diagram 3. Here the signs, frequencies and amounts of the personal equations are shown by the direction and length of the lines. The dotted

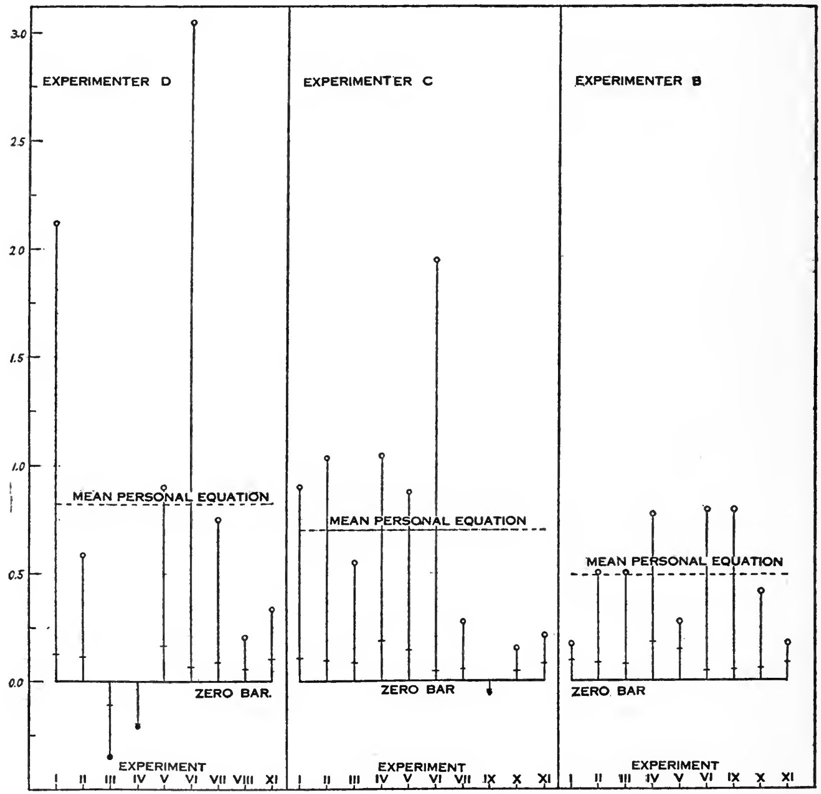


Diagram 3. Sign and Magnitude of (Absolute) Personal Equation in All the Experiments. The mean deviation from zero for the several experiments is indicated by the lengths of the lines. The dotted lines indicate the mean of all the experiments by the individual observers.

transverse lines show the mean values of all the personal equations for the individual observers.

The magnitudes of these means are, however, very low. For Observer *B* there is not a single case in which it amounts to one seed, although in every case it is positive in sign. For Observers *C* and *D* the values are somewhat higher. The largest single value is that of Observer *D*, experiment VI., where the estimates were on an average three seeds off. In this case however there were only 200 trials and the attempt was being made to estimate in lots of 200 seeds.

To the question of the actual magnitude of these deviations from zero personal equation I shall return presently. For the moment, the feature of these results which impresses me is the fact that so slight a personal bias should be so persistent among the three observers throughout the two years during which these observations were carried on.

With regard to the significance of the deviation of these means from 0 little need be said. The probable errors of the mean have been calculated from the usual.

$$E_A = .67449 \frac{\sigma}{\sqrt{N}}.$$

Where σ is the standard deviation of the series of errors (to be discussed shortly) and N the number of observations. In Diagram 3 the amount of the probable error is indicated in each case by a cross on the ordinate indicating the amount of personal equation. The ratio of the deviation of the mean from 0 to its probable error has been tabled in each case. Of the 36 constants (including these in which two consecutive experiments have been combined) 28 are over 2.5 times as large as their probable errors. Of these, 27 are positive and one negative in sign. There can be no reasonable question, therefore, of the statistical trustworthiness of these individual constants.

One may test most critically the existence of personal equation by splitting these masses of observations up into sub-classes, say into the groups of 50 estimates known as periods. Constants must then be determined for these in the same manner as for the "general population" as the statisticians call it.

The detailed analyses of one series of data in this way is sufficient, since the others will be treated in a slightly different manner, giving nearly comparable end results, in a subsequent paper. Table II. for the first experiment furnishes data for determining whether the personal equation demonstrated in the massed estimates is persistent throughout the course of the experiment in the individual periods. In Diagram 4 the solid line in each panel, the zero bar, shows the 0 average of errors of observation which would be secured if there were no

TABLE II

Period	Observer *B*		Observer *C*		Observer *D*	
	Personal Equation	Steadiness of Judgment	Personal Equation	Steadiness of Judgment	Personal Equation	Steadiness of Judgment
1	+1.40	6.24	+ .44	5.34	+1.92	6.85
2	+ .82	5.46	+1.84	4.93	+2.30	7.03
3	− .80	4.10	+1.02	4.13	+3.88	4.89
4	− .56	3.95	+1.02	3.83	+3.02	6.22
5	− .44	3.69	+1.72	4.30	+3.78	5.20
6	+ .10	3.95	+2.04	3.86	+1.36	4.96
7	+ .32	4.23	+1.24	4.05	+2.58	4.75
8	− .72	3.87	+ .50	3.75	+1.22	3.95
9	+ .90	3.79	+1.40	4.22	+1.50	4.34
10	+2.04	3.88	+ .18	4.50	+1.74	4.13
11	+ .20	3.75	+ .50	3.35	+1.04	4.07
12	+ .32	4.11	− .36	3.66	+1.68	3.95
13	− .44	3.46	+1.06	3.70	+ .80	5.42
14	− .58	3.23	+ .36	2.96	+3.56	4.45

personal equation—*i. e.*, no bias towards too high or too low estimates. The circles show the actual means for the 14 individual periods of 50 estimates each. The light line shows the mean deviation for the whole 14 periods, the amount of which is forcibly brought out by the shaded area. The sloping lines show the rate of change. These will be discussed later.

For the whole series of observations, Observer *B* had within the limits of the probable error, no personal equation, *i. e.*, her mean deviation from the true value was only $+ .171 \pm .107$. Here it appears that in 8 of the periods her means fall above and in 6 of the periods below the 0 bar. For Observer C, who appeared from the massed observations to have a distinct personal equation of about one seed, the diagram shows that in 13 out of 14 cases the period means fall on the positive side of the line. Finally, for Observer *D*, who in this series has the greatest bias of the three towards underestimating the number of seeds in a sample—that is, towards laying out too many seeds in the attempt to get 50—all 14 period means are positive.

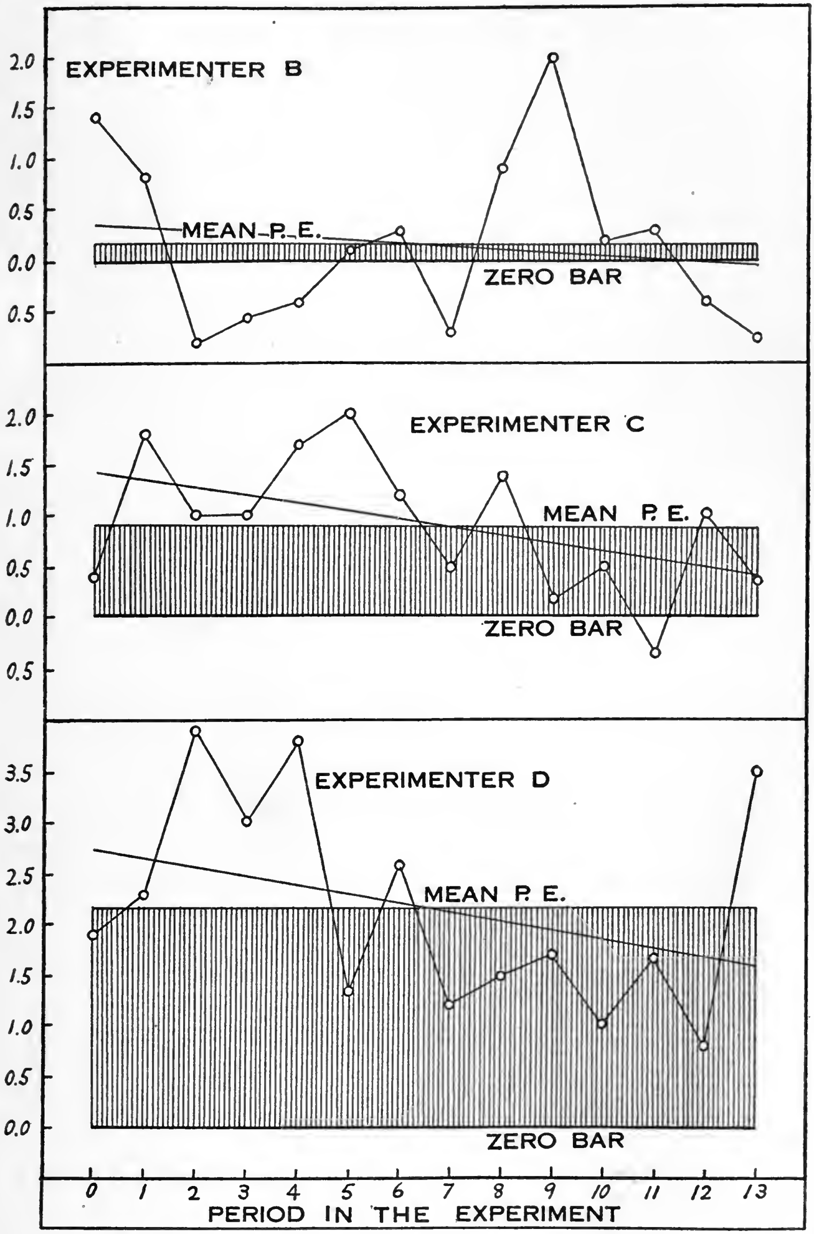


Diagram 4. Personal Equation in the Fourteen Individual Periods, Each of Fifty Trials, of Experiment I. The circles indicate the personal equation for the individual periods. The shaded area shows the amount of personal equation for the whole experiment.

With regard to the actual magnitude of the personal equation, it seems reasonable to assume that if there be a tendency to err from the true value in any definite direction, the actual mean deviation observed will be to some degree dependent upon the number of objects with which the experimenter is dealing.

The simplest assumption is that the actual amount of the personal equation in any given case should be approximately proportional to the number which the experimenter is attempting to estimate. On such an assumption (which on more extensive investigation may or may not be found to be borne out by the experimental facts) one may take the ratio of the actually observed mean deviation to the ideal number. Concretely, one divides the measures of personal equation given in Table I. by 25, 50, 100 or 200 as the case may be. Expressed in this way we have an 'error per object,' or a

Table III

Relative Personal Equation and Differences in Relative Personal Equation for Three Observers in the Individual Experiment

Series	Observer B	Observer C	Observer D	$D-C$	$D-B$	$C-B$
I.	+.0034	+.0185	+.0434	+.0248	+.0399	+.0151
II.	+.0102	+.0204	+.0118	−.0086	+.0016	+.0102
III.	+.0102	+.0112	−.0073	−.0185	−.0175	+.0010
II.+III.	+.0102	+.0158	+.0022	−.0136	−.0079	+.0056
IV.	+.0078	+.0104	−.0021	−.0125	−.0099	+.0025
V.	+.0028	+.0089	+.0089	−.0000	+.0061	+.0061
IV.+V.	+.0053	+.0096	+.0033	−.0062	−.0019	+.0043
VI.	+.0040	+.0098	+.0152	+.0254	+.0112	+.0057
VII.	......	+.0112	+.0300	+.0188	......	......
VIII.	......	......	+.0082	......	......	......
IX.	+.0322	−.0024	......	......	......	−.0346
X.	+.0173	+.0066	......	......	......	−.0106
IX.+X.	+.0255	+.0016	......	......	......	−.0238
XI.	+.0036	+.0044	+.0068	+.0024	+.0032	+.0008

'relative personal equation.' The resulting values are given in Table III.

The individual entries and the averages show that the relative personal equation is low. The observers tend to lay out about 1 per cent. too many seeds.

Comparisons between the different workers are possible on the basis of these relative values which may be averaged.

From the differences for the individual experiments (leaving the combined series out of account) the following facts are to be noted.

Observer B has a higher personal equation than Observer C in 2 cases and a lower personal equation in 7 cases. The average difference between them in terms of relative personal equation is only .0004. Observer B has a higher personal equation than D in 2 cases and a lower deviation in 5 cases. The average differences, regarding signs as before, is only .0050. A comparison of the records of Observers C and D shows that in 4 experiments Observer D has a greater personal equation than C whereas in the other 4 experiments precisely the opposite conditions prevailed. The average difference is only .0040.

From these experimental data taken as a whole one cannot conclude that there is any demonstrated difference between the personal equation of the three observers. All have a bias in the direction of laying out more than the intended number of seeds, but that one is worse than another cannot be asserted.

If now one considers these differences between the personal equations of the three observers in their relation to their probable errors, as shown for the differences in the absolute values given in Table IV., it appears that in a high proportion of the cases they are statistically significant. This is true in

TABLE IV

DIFFERENCES IN PERSONAL EQUATION FOR INDIVIDUAL OBSERVERS

Experiment	$D-C$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	$D-B$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	$C-B$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$
I.	+1.244±.170	+ 7.32	+1.999±.171	+11.69	+ .755±.151	+ 5.00
II.	− .431±.160	− 2.69	+ .082±.156	+ .53	+ .513±.143	+ 3.59
III.	− .928±.145	− 6.40	− .878±.139	− 6.32	+ .050±.120	+ .42
II.+III.	− .680±.109	− 6.24	− .398±.105	− 3.79	+ .282±.093	+ 3.03
IV.	−1.255±.288	− 4.36	− .997±.282	− 3.54	+ .258±.272	+ .95
V.	− .003±.232	− .01	+ .610±.229	+ 2.66	+ .613±.217	+ 2.82
IV.+V.	− .629±.185	− 3.40	− .193±.182	− 1.06	+ .436±.175	+ 2.49
VI.	+1.095±.096	+11.41	+2.250±.096	+23.44	+1.155±.081	+14.26
VII.	+ .471±.116	+ 4.06	....	.....	....	.....
VIII.	....	.....	....	.....	....	.....
IX.	....	.....	....	.....	− .865±.088	− 9.83
X.	....	.....	....	.....	− .266±.084	− 3.17
IX.+X.	....	.....	....	.....	− .596±.062	− 9.61
XI.	+ .121±.149	+ .81	+ .162±.151	+ 1.07	+ .041±.125	+ .33

cases in which (for example) B has a greater personal equation than C as well as in these in which she has a smaller personal equation. The reader may compare the entries in Table IV. for details.

The statement that there are statistically significant differences between two observers for individual experiments, and that these differences are sometimes positive and sometimes negative may be taken at once by that still considerable body of students who are hostile to the newer statistical tools of research to discredit entirely the methods employed and to cast doubt upon the conclusions just drawn. Such an attitude seems to me quite unjustified.

The true interpretation of the results seems to me to be rather that the observers vary somewhat in their personal equation from experiment to experiment, just as they vary from time to time in general health, physiological tone, and mental vigor, alertness, or whatever one may care to call it. As a result of this variation from time to time one observer may show an abnormally high personal equation in a particular experiment in which a second observer shows an unusually low one. On an other occasion the condition may be exactly reversed.

Thus *in an individual experiment* one observer may seem to be decidedly better than another. *In the long run* there is no *fully demonstrated* difference between them.

2. *Steadiness of Judgment*

Steadiness of judgment will best be measured by some expression showing the scatter of estimates around their mean. The best constant for this is the standard deviation, σ.

$$\text{S.D.} = \sqrt{\frac{\text{Sum of (deviations from mean)}^2}{\text{Total estimates}}},$$

which here is most easily calculated from the formula[1]

$$\text{S.D.} = \sqrt{\Sigma(d)^2/N - [\Sigma(d)/N]^2},$$

where Σ is the conventional summation sign, N is the number of estimates and d indicates the deviation of the estimate from

[1] Sheppard's modification has not been applied to the second moment.

the true number of objects, *i. e.*, the actual number laid out less the required number.

The constants with their probable errors are given in the first three constant columns of Table V.

TABLE V

Experiment	Standard Deviation			Coefficient of Variation		
	Observer *B*	Observer *C*	Observer *D*	Observer *B*	Observer *C*	Observer *D*
I.	4.180±.075	4.139±.075	5.211±.094	8.331	8.128	9.989
II.	3.552±.069	3.774±.074	4.407±.086	7.032	7.397	8.711
III.	2.899±.056	3.243±.063	4.137±.081	5.739	6.414	8.335
II.+III.	3.242±.045	3.527±.049	4.301±.059	6.418	6.943	8.583
IV.	6.839±.133	7.160±.139	7.635±.149	6.785	7.086	7.651
V.	5.498±.107	5.682±.111	6.248±.122	5.483	5.632	6.193
IV.+V.	6.210±.086	6.464±.089	6.998±.096	6.177	6.402	6.974
VI.	11.928±.040	12.089±.041	16.172±.055	5.940	5.986	7.964
VII.	....	1.937±.044	3.072±.069	.....	7.663	11.929
VIII.	....	....	2.314±.045	.....	.....	9.179
IX.	2.333±.048	2.013±.041	....	9.042	8.072	.....
X.	2.010±.045	1.714±.039	....	7.904	6.813	.....
IX.+X.	2.202±.033	1.887±.028	....	8.587	7.535	.....
XI.	3.255±.063	3.147±.061	4.399±.086	6.486	6.266	8.738

For steadiness of judgment, we have no absolute standard comparable to a mean deviation of 0 in the personal equation test. The accuracy of an observer must be estimated by comparison with others. A relative measure of steadiness of judgment permitting comparison between different kinds of experiments is desirable. Since it is reasonable to suppose *a priori* that errors of estimation will be larger when the observer is attempting to lay out samples of a large number of seeds than when she is dealing with a small number,[1] this relative measure is best furnished by the biometrican's coefficient of variation, which is obtained in this case by dividing the standard deviation multiplied by 100 by the ideal number plus or minus the observed personal equation, as the sign of the latter may indicate.

These relative measures of steadiness of judgment are given in the last three columns of Table V.

The differences in standard deviation measuring steadiness

[1] Should the number be made very small indeed there would be practically no error of estimation after a little experience, since the observer could all but invariably lay out the correct number.

of judgment between the three observers are set forth with their probable errors and their ratios to their probable errors in Table VI.

Table VI

Difference in Standard Deviation for Individual Observers, that is, Difference in Steadiness of Judgment

Experiment	$D-C$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	$D-B$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	$C-B$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$
I.	$+1.072\pm.120$	$+8.93$	$+1.031\pm.120$	$+8.59$	$-.041\pm.106$	-0.39
II.	$+.633\pm.113$	$+5.60$	$+.855\pm.110$	$+7.77$	$+.222\pm.101$	$+2.20$
III.	$+.894\pm.102$	$+8.76$	$+1.238\pm.098$	$+12.63$	$+.344\pm.084$	$+4.10$
II.+III.	$+.774\pm.076$	$+10.18$	$+1.059\pm.074$	$+14.31$	$+.285\pm.066$	$+4.32$
IV.	$+.475\pm.204$	$+2.33$	$+.796\pm.200$	$+3.98$	$+.321\pm.192$	$+1.67$
V.	$+.566\pm.165$	$+3.43$	$+.750\pm.162$	$+4.63$	$+.184\pm.154$	$+1.19$
IV.+V.	$+.534\pm.131$	$+4.08$	$+.788\pm.129$	$+6.11$	$+.254\pm.124$	$+2.05$
VI.	$+4.083\pm.069$	$+59.17$	$+4.244\pm.068$	$+62.41$	$+.161\pm.057$	$+2.82$
VII.	$+1.135\pm.082$	$+13.84$	....	.....	....	.....
VIII.	....	.....	....	.....	....	.....
IX.	....	.....	....	.....	$-.320\pm.063$	-5.08
X.	....	.....	....	.....	$-.296\pm.059$	-5.02
IX.+X.	....	.....	....	.....	$-.315\pm.043$	-7.33
XI.	$+1.252\pm.105$	$+11.92$	$+1.144\pm.107$	$+10.69$	$-.108\pm.087$	-1.24

The differences for the standard deviations of the three observers are more consistent than those for personal equation. Observer *B* has more erratic judgment than *C* in 4 cases, less erratic in 5 cases. In 5 out of the 9 cases the difference may be considered to be significant with regard to its probable errors. In 2 of the experiments it is Observer *B* who is significantly more variable in estimation, while in 3 cases it is Observer *C* who has the most irregular estimates. These significant differences which differ in sign from experiment to experiment are probably to be explained in the same way as those in personal equation discussed above. The relative steadiness of judgment as measured by the coefficient of variation shows a mean of 6.971 in the case of Observer *B* as compared with 6.946 in the case of Observer *C*, a difference of only 0.025! Thus in the long run there is no discernible difference in the steadiness of judgment of *B* and *C*, although in the case of individual experiments now one, now the other, may be higher.

For the comparison between both *B* and *D* and *C* and *D* the case is quite different. *In every individual experiment Observer D has* a higher standard deviation, or in other words *less steady judgment*, than either *B* or *C*. In practically every instance the differences may be considered significant in comparison with their probable errors. The average relative scatter of estimates of Observer *D* as measured by the coefficient of variation is 8.743, a value about 1.80 higher than that of either of the other observers.

III. Recapitulation

The purpose of this paper, and of another on the influence of previous experience upon errors of judgment which is to follow, is the presentation in terms as succinct as possible of the results of a series of experiments on errors of judgment in the estimation of moderately large numbers of objects.

Data Table *A*

Amount of Error	I. *A*		I. *B*		I. *C*		I. *D*		II. *B*		II. *C*		II. *D*		III. *B*		III. *C*		III. *D*	
0	4		61		79		41		62		64		61		76		80		61	
	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−
1	7	—	60	73	64	58	50	42	77	62	73	63	60	58	76	69	77	64	55	48
2	8	9	61	53	63	65	63	43	56	57	65	34	51	58	75	54	53	56	44	61
3	8	9	65	55	58	46	56	42	38	44	49	44	53	43	70	38	56	43	43	47
4	6	3	52	32	44	36	49	22	38	41	51	15	27	34	50	21	38	28	30	51
5	7	9	33	36	42	22	46	18	35	20	39	16	27	22	18	22	35	16	21	32
6	8	1	19	18	32	14	48	8	18	9	22	12	20	14	8	6	17	11	13	18
7	7	6	14	20	17	7	35	11	14	6	14	4	19	9	8	4	10	4	19	13
8	4	3	6	9	15	6	33	10	9	4	12	3	10	6	3	1	4	2	6	16
9	5	1	9	7	7	2	19	6	6	—	7	2	5	5	1	—	1	—	5	5
10	5	3	4	3	7	2	12	3	2	—	2	2	5	1	—	—	3	—	3	3
11	7	—	2	1	3	1	13	2	—	—	1	2	3	1	—	—	2	—	1	—
12	1	—	1	2	2	1	7	3	2	—	1	1	—	—	—	—	—	—	2	2
13	3	1	3	—	4	—	5	—	—	—	1	—	1	—	—	—	—	—	—	—
14	2	—	—	—	1	—	4	—	—	—	—	—	2	—	—	—	—	—	1	—
15	3	—	—	—	1	—	1	1	—	—	—	—	2	—	—	—	—	—	—	—
16	3	—	1	—	—	—	1	—	—	—	—	—	1	—	—	—	—	—	—	—
17	4	1	—	—	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—
18	—	—	—	—	—	1	3	—	—	—	—	—	—	—	—	—	—	—	—	—
19	1	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—
20	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
21	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—
22	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—
23	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
32	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	91	46	330	309	361	260	447	212	295	243	338	198	288	251	309	215	296	224	243	296

The experiments consisted in attempts to lay out samples of a definite number of small objects. The number which the observer was attempting to obtain in each sample (25, 50, 100, or 200) was constant for considerable periods. The error of each estimate was at once determined and recorded by the experimenter, who on the basis of these known errors made a continuous effort to improve in accuracy of estimating.

Two characteristics of the series of errors of estimation made by the three observers are here considered—personal equation and steadiness of judgment. By personal equation

DATA TABLE *B*

Amount of Error	IV. *B*		IV. *C*		IV. *D*		V. *B*		V. *C*		V. *D*		VII. *C*		VII. *D*	
0	31		37		27		47		30		47		95		66	
	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−
1	37	34	32	28	31	35	49	42	47	43	35	32	86	69	63	57
2	34	34	32	31	27	30	44	42	46	40	47	36	63	52	54	52
3	34	31	44	27	18	42	36	29	34	37	38	29	28	20	36	25
4	33	31	34	25	23	35	36	28	29	24	33	29	21	9	28	22
5	25	25	20	22	22	31	27	28	33	30	34	27	4	1	16	4
6	20	29	30	20	31	35	23	30	31	19	29	16	1	1	8	1
7	20	17	14	24	24	17	16	22	23	21	20	16	—	—	6	—
8	12	22	19	10	13	19	10	12	25	16	20	15	—	—	4	—
9	29	10	17	14	9	18	11	18	14	10	12	12	—	—	3	—
10	10	7	21	8	10	16	11	2	7	5	9	6	—	—	1	—
11	9	8	14	12	8	9	10	6	4	2	5	4	—	—	1	—
12	6	5	11	6	9	6	3	4	7	3	13	5	—	—	1	—
13	11	5	8	4	7	5	3	1	10	1	3	2	—	—	—	—
14	3	3	6	1	9	2	2	1	2	1	6	2	—	—	—	—
15	6	3	7	6	4	4	3	—	—	1	1	3	—	—	1	—
16	4	1	3	1	—	2	3	—	1	—	1	2	—	—	—	—
17	4	1	3	—	4	—	1	—	1	—	6	1	—	—	—	—
18	1	—	2	1	1	2	—	—	1	—	1	—	—	—	—	—
19	4	—	1	1	2	1	—	—	—	—	—	1	—	—	—	—
20	—	—	—	1	2	—	—	—	—	—	1	—	—	—	1	—
21	—	—	—	—	3	1	—	—	1	—	1	—	—	—	—	—
22	—	—	1	—	—	—	—	—	1	—	—	—	—	—	—	—
23	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—
24	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—
25	—	—	1	—	—	3	—	—	—	—	—	—	—	—	—	—
27	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—
32	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—
	303	266	320	243	259	314	288	265	317	253	315	238	203	152	223	161

we understand a bias in a given direction—a tendency to estimate too high or too low. By steadiness of judgment we mean consistency in estimation as measured by the closeness with which the errors of estimation clusters around their mean value.

Personal equation is measured by the mean (regarding signs) of the deviations of the samples from their ideal value.

Steadiness of judgment is expressed in the absolute terms of the standard deviation of the errors of estimation about their mean, or in the relative terms of the coefficient of variation.

In the case of all three observers there is a slight but significant personal equation, which, notwithstanding the

DATA TABLE *C*

Amount of Error	VI. *B*		VI. *C*		VI. *D*	
0	6		8		1	
	+	−	+	−	+	−
1	7	5	3	7	8	3
2	8	5	4	8	6	5
3	3	7	1	4	6	4
4	—	4	10	5	7	2
5	5	8	5	3	6	4
6	8	2	10	3	7	4
7	8	1	5	8	5	4
8	3	3	5	—	5	2
9	8	4	10	4	4	2
10	9	4	9	2	7	5
11	6	5	8	6	5	—
12	5	3	2	2	5	5
13	1	5	2	2	9	6
14	4	2	6	4	4	3
15	3	2	4	3	1	—
16	6	4	3	1	2	3
17	1	1	5	2	2	2
18	2	—	2	4	4	1
19	3	1	3	2	4	3
20	2	2	—	—	—	2
21	2	1	—	1	3	3
22	2	2	2	—	3	2
23	2	—	1	1	2	1
24	2	5	2	1	—	2
25	—	1	1	1	3	2
26	3	—	2	—	—	1
27	1	2	2	2	2	1
28	—	3	1	—	2	—
29	—	—	—	2	2	1
30	—	2	1	—	3	—
31	—	1	—	—	—	—
32	—	1	—	—	—	—
33	1	—	—	1	1	—
34	—	1	—	—	—	—
35	—	—	1	1	1	1
36	—	—	—	—	—	—
37	1	—	—	1	1	—
38	—	—	1	—	—	1
39	—	—	—	—	—	—
40	—	—	—	—	1	—
42	—	—	—	—	1	1
43	1	—	—	—	—	—
71	—	—	—	—	1	—
	107	87	111	81	123	76

constant effort to improve, persisted throughout the two years during which the experiments were intermittently made. In only three out of the twenty-eight experiments did the observer lay out samples of too small average size. In a large number of the individual experiments the personal equation is certainly statistically significant (trustworthy) in comparison with its probable error.

It is impossible on the basis of the present series of experiments, extensive though it is, to assert that the personal

DATA TABLE *D*

Amount of Error	VIII. *D*		IX. *B*		IX. *C*		X. *B*		X. *C*	
0	103		83		116		82		109	
	+	−	+	−	+	−	+	−	+	−
1	97	97	91	70	91	104	74	72	99	82
2	79	71	94	52	50	68	79	45	48	48
3	41	35	56	24	34	38	34	23	28	19
4	28	20	35	13	22	16	23	9	9	4
5	8	7	13	2	5	3	7	1	2	—
6	8	1	11	1	1	2	1	—	—	1
7	1	2	3	—	—	—	—	—	1	—
8	1	—	1	—	—	—	—	—	—	—
9	1	—	—	—	—	—	—	—	—	—
10	—	—	1	—	—	—	—	—	—	—
	264	233	305	162	203	231	218	150	187	154

equation of any one of the three observers is on the whole higher than that of the others, although the figures do suggest that the bias of observer *D* may be slightly greater than that of either of the others. In the case of individual experiments there may be significant differences between two observers. In one experiment x may have a decidedly lower personal equation than y, while in another period of observation exactly the reverse condition may be found. This is taken to indicate a variation in the magnitude of the personal equation of an observer from experiment to experiment.

For steadiness of judgment there is no absolute standard comparable with the zero mean deviation of the personal equation. The data show a coefficient of variation about 6.9 per cent. in the case of Observers *B* and *C*, and of 8.7 per cent. the case of Observer *D*, who has a decidedly greater scatter in her estimates—that is a far less steady judgment—than either of the other observers. Indeed, in every individual experiment her standard deviation is higher than that of either of the two other experimenters.

Thus while there is no certain differentiation among the experimenters in personal equation, they differ distinctly in steadiness of judgment.

For a more detailed consideration of these two characteristics the reader must see the subsequent paper.

Finally, I must emphasize again the fact that these data

DATA TABLE *E*

Amount of Error	XI. *B*		XI. *C*		XI. *D*	
0	71		78		52	
	+	−	+	−	+	−
1	68	72	66	86	55	50
2	57	57	64	59	50	55
3	58	49	48	50	51	40
4	40	28	33	23	40	36
5	23	26	25	23	24	27
6	12	12	13	7	20	22
7	11	6	10	6	17	11
8	4	1	2	2	11	10
9	1	2	2	1	7	4
10	—	1	—	—	5	1
11	—	—	1	—	3	1
12	1	—	—	—	3	—
13	—	—	—	—	1	1
14	—	—	—	—	—	—
15	—	—	1	—	—	—
16	—	—	—	—	2	—
19	—	—	—	—	—	1
	275	254	265	257	289	259

are presented purely for their intrinsic value. They were secured quite incidentally in the carrying out of large plant breeding experiments. The chief value of the observations perhaps lies in the fact that they represent far larger experiments than the average professional psychologist is able to make. Comprising as they do 28 experiments due to three observers all of whom carried on the work at considerably separated intervals over a period of two years, during which they made over 15,000 estimates, the constants have a reliability which cannot possibly be attributed to short series. Purely psychological discussions, even the review of literature with some of which the writer is quite familiar, is left to specialists.

ORIGIN OF HIGHER ORDERS OF COMBINATION TONES[1]

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It is well-known that Helmholtz gave three or more different explanations of the origin of combination tones. According to his own statements combination tones may be generated (1) from the clicking action between the hammer and the anvil of the ear, when the primaries are powerful;[2] (2) from the asymmetry in vibration of the tympanum; and (3) from disturbed superposition of vibrations due to some objective connection between the primary periodicities, such as a common windchest found in the polyphonic siren or the harmonium, making the air puffs for each tone weaken periodically the puffs for the other tone. The tones generated by conditions (1) and (2) were called subjective; those by condition (3), objective. Helmholtz did not seem to regard case (3) as a condition of disturbed superposition as demanded by his mathematical explanation based upon vibrations '*so large that the square of the displacements has a sensible influence on the motions*';[3] for he states explicitly of this case that he will 'draw attention to a third case, where combinational tones may also arise from *infinitely small vibrations*.'[4] This is of course an error. Helmholtz admitted that in conditions favoring objective combination tones conditions (1) and (2) were also operative, thus making all audible objective combination tones also largely 'subjective.'[5] I have suggested[6] that conditions (2) and (3) are practically identical physically

[1] Read before the Utah Academy of Sciences, April 3, 1915.

[2] 'Sensations of Tone,' p. 158.

[3] *Ibid.*, Appendix XII., 412.

[4] *Ibid.*, 419. Italics mine.

[5] *Ibid.*, 157.

[6] 'Combination Tones,' etc., Psychological Review Monograph, No. 39, 1908, 17 ff., 103 ff.

both being dependent upon the principle of disturbed superposition of vibrations; that such superposition is in all probability in case (2) most pronounced in the liquids of the inner ear. This view has subsequently been supported by Clemens Schaefer.[1]

The view of Helmholtz, expressed under case (1) above, has not been substantiated by recent research. My own experiments lend support to the conclusion of Helmholtz respecting objective combination tones, that they are in large part 'subjective,' *i. e.*, that they are in a considerable measure due to disturbed superposition of vibrations of the primaries within the ear itself.

As to the nature of higher orders of combination tones two contradictory views were expressed by Helmholtz. One was that they are higher order difference tones in the sense suggested by Hällstrom, that they originate from a first difference tone and a primary tone or from two difference tones.[2] His other view was that all higher order combination tones take origin *directly* from the primaries. This is in agreement with a theory developed mathematically in 1881 by R. H. M. Bosanquet,[3] and more recently by Clemens Schaefer in the article referred to in a preceding paragraph. This view that so-called higher order combination tones take their origin directly from the primaries is also supported by the experimental results of a number of recent investigators. From the method of his statement of the laws of the occurrence of difference tones, Krueger has been interpreted by some writers to favor the Hällstrom view of 'higher order' combination tones. This interpretation, which Krueger assured me personally in a conversation[4] is wrong, is used by R. M. Ogden as evidence against Krueger's theory of consonance. Ogden writes that "Stumpf's investigations indicate that combination tones are always directly derived from the objective tones, and not from beats, nor, except in the highest ranges of the scale, from one another as Krueger

[1] *Annalen der Physik*, Bd. 33, 1910, 1216–1226.

[2] 'Sensations of Tone,' p. 154.

[3] *Phil. Mag.*, 5th Series, XI., 1881.

[4] At Cleveland, Ohio, December, 1912.

maintains."[1] This is an unfortunate mistake as to Krueger's view, and, as I hope to show in another place, the actual facts really support rather than refute his theory of consonance. As to the facts, I am willing to go farther than either Stumpf or Ogden and assert that there is no satisfactory evidence in existence to support the view quoted above, that 'in the highest ranges of the scale' combination tones may originate from other combination tones. This is a matter of importance in relation to theories of consonance.

Though Stumpf quotes approvingly my own view as to the so-called higher order combination tones, and seems to accept and to use my experimental evidence, he registers his unwillingness to dispute so generally as I have done the possibility of difference tones being derived from overtones of the primaries.[2] No evidence is offered for such difference tones except that Stumpf has perceived, as other investigators have perceived, difference tones lying in pitch between the primaries. Such tones are, of course, easily accounted for on the theory of direct origin from the primary tones on the basis of disturbed superposition; but the fact of their existence is contradictory to theories like that of Max Meyer. Rücker and Edser[3] established the existence of objective intermediate difference tones, and similar 'subjective' tones are to be expected on the view that disturbed superposition is the cause of all combination tones.

While Helmholtz held to the position maintained by Stumpf as to the derivation of certain difference tones from upper partials, experimental evidence indicates that few difference tones, if any at all, have such origin. Though the possibility of such tones is not disputed by theory—for these tones would follow the same laws as combination tones from the primaries—there is, so far as I know, no evidence at all to indicate that audible difference tones derived from upper partials exist. This is not incomprehensible when one takes into consideration the fact that the overtones of the primaries

[1] In a review of Stumpf in *Psychol. Bul.*, 9, 1912, 117.

[2] Stumpf, C., 'Beobachtungen über Kombinationstöne,' *Zeit. f. Psych.*, 55, 1910, 1 ff.

[3] 'Objective Reality of Combination Tones,' *Phil. Mag.*, 5th Series, 39, 1895.

are themselves to a considerable extent 'subjective,' or dependent upon periodicities arising in the liquids of the inner ear.

In the case of the second difference tone, D_2, which is easily perceptible with a number of intervals, is there any evidence that it can be generated by the higher primary tone and the second partial of the lower primary, as suggested by the formula $D_2 = 2l - h$? In the psychological laboratory of the University of Chicago the resonated forks $Ut_4 : Mi_4$ (4 : 5) give an exceptionally prominent second difference tone, 3, on very gentle sounding, *i. e.*, at an intensity of the primaries which leaves the first difference tone, D_1, entirely inaudible.[1] D_2 can evidently not arise from the first difference tone and the lower primary. On substituting Ut_5 for Ut_4—sounding gently Ut_5 with Mi_4—one should make the tone D_2 even more prominent, if it depends upon the tone corresponding to Ut_5, by the formula $4 \cdot 2 - 5 = 3$. But with such substitution a much greater intensity of the primaries is required to make D_2 (3), now D_1, audible at all. The first upper partial of piano tones in the middle of the register is easily perceptible even to the unaided ear—as are several others of the higher partials. In the case of the major third (4 : 5) with the following tones $c^2 : e^2$, $b^2 : e\flat^2$, and $d^2 : g\flat^2$ the respective second difference tones at approximately the pitch of g^1, $g\flat^1$ and a^1 were easily audible, though the several first difference tones were inaudible. But when the octave of the lower tone in each pair was substituted for this tone (*i. e.*, c^3 for c^2, b^3 for b^2, d^3 for d^2) the difference tone in question in each case became inaudible even though the intensity of the primaries remained the same as before the substitution, or was slightly increased. The tone in question was therefore evidently not dependent upon the upper partials.

In the case of forks $Ut_4 : Mi_4$ (4 : 5) the first difference

[1] It is well known to those who have experimented with tones that such things as tuning forks have individualities almost as marked in certain respects as those of persons. Certain pairs of forks of the few sets with which I have become well acquainted give clear difference tones while others of precisely the same pitch, and resonated similarly, give very weak ones or none at all. The absolute pitch of the primaries is also an important factor in determining the presence or absence of combination tones.

tone, 1, is easily audible when the forks are loudly sounded. Is the second difference tone dependent upon this one? The fork Ut_2 gives a tone coincident in pitch with this first difference tone and, on gentle sounding, almost indistinguishable from it in timbre. Even though this is the case, when Ut_2 is substituted for the primary Mi_4, that is when the interval is represented by the forks $Ut_2 : Ut_4$, the second difference tone, 3, now made the first, disappears altogether. This is true whether Ut_2 is sounded gently—to represent the difference tone for which it is substituted—or loudly. It is evident, therefore, that the second difference tone, 3, of the major third in question does not arise from the first with the lower primary, by the formula $l - (h - l) = l - D_1 = D_2$ or $4 - (5 - 4) = 4 - 1 = 3$. If this is true of the second difference tone it is also true of summation tones if they are really difference tones of higher orders.

But there is yet a better test available. In the case of summation tones theoretically explained according to the formulæ

$$h + l = n(h - l) \quad \text{and} \quad h + l = nh - ml$$

a conclusive quantitative test is applicable, one, however, that was first worked out by the writer only a few years ago. In the case of the fifth (2 : 3) the difference tone of the fifth partials (10, 15) coincides with the summation tone (5). The summation tone is not easy to hear unless its origin is objective to the ear. However with the use of resonated tuning forks I found that it was perceptible to the practiced ear when the forks $Ut_3 : Sol_3$, or $Re\flat_3 : La\flat_3$ were sounded a little above medium intensity. As an objective check a number of students trained in experimental psychology were asked to select from among a number of high pitched forks the tones that were audible in addition to the primaries. With the intervals given were also $Fa_3 : La_3$ (4 : 5) and $Ut_3 : La_3$ (3 : 5). The fork representing the summation tone was in each case selected, as well as forks representing certain first upper partials that were easily perceptible to a trained ear. Occasionally I suggested to the subject a wrong fork, but it was in every case rejected finally. One of the subjects readily sang the note representing the summation tone. There could be no doubt as to the existence of the tone in question.

By tuning an auxiliary tone to very nearly the pitch of these summation tones well-marked beats were obtained. The auxiliary tone is generated by an unresonated fork sounded gently and held close to the ear. After the auxiliary tone is exactly tuned to the pitch of the summation tone, any change in pitch of the primaries is at once noted by a beating between the auxiliary and the summation tone. On this principle it was found possible to determine the true nature of the latter. Take, for instance, the fifth. The primary tones and their respective upper partials are represented by the following numbers:

2, 4, 6, 8, 10, etc.
3, 6, 9, 12, 15, "

Now it is evident that if the tone in question is a true summation tone, lowering one of the primaries one beat per second will also lower it one beat. But if it is really a difference tone it will in this case take origin from the fifth partials of the primaries, *i. e.*, from 10 and 15. In such a case lowering one of the primaries one beat will lower or raise the tone in question five beats per second. But in the practical working out of this test a difficulty was encountered. Lowering the pitch of a primary tone makes the interval imperfect, and the beats of interference prevent a careful study of the summation tone with the auxiliary. To get rid of these irrelevant beats the following procedure was adopted: After depressing one of the primaries one vibration per second, the auxiliary fork was attuned to the summation tone, a change that could be accomplished with certainty. The primary tone was then brought back to its true pitch. The only beats then remaining were those due to the interference of the auxiliary with the summation tone. The demonstration was convincing; there was absolutely no trace of rapid beats, but slow beats with a rate of one per second were heard. If these beats were due to interference between a difference tone of the auxiliary and a primary with the other primary, as might be argued, one should hear also rapid beats of the auxiliary with this high tone, if the latter originated from the upper partials. It will be remembered that only such summation tones as were audible were used in the test. Besides, rapid beats of five per second are more easily perceptible than slow ones of one per second. No such beats, however, were audible, and the auxiliary was so weak that no difference tones were probably generated; certainly none was perceptible. The summation tones tested were consequently not explicable as difference tones of upper partials, but were real summation tones in the Helmholtzian sense. These experiments, then, all contradict the view that combination tones may originate from upper partials. While they do not show the absolute impossibility of such origin, we must remember that no case to prove the derivation or combination tones from upper partials exists, and that all the audible overtones of musical intervals are likely to a considerable extent intra-aural in origin.

XXIII. PRACTICE IN GRADING AND IDENTIFYING SHADES OF GRAY

BY WARNER BROWN

The following report consists of two parts. The first part deals with the effects of practice in grading during ten sittings in which an attempt was made to arrange fifty gray cards in the order of their apparent brightness. No corrections were made. The result shows that there was no improvement in the accuracy of the arrangement. The second part, on the other hand, shows that in the course of the ten sittings there was a substantial improvement in the ability to remember and recognize four of the fifty cards.

1. Practice in Grading

The experiment was performed twice a week for five weeks. Each worker was provided with two sets of fifty cards 2.5 cm. wide and 6.5 cm. high. The cards were covered with gray paper ranging from nearly white to nearly black. After being shuffled the cards of one set were laid out by the learner in a row according to the following directions:

> The fifty gray cards are numbered on the backs according to an arbitrary system. You are to arrange these cards according to their apparent brightness, with the lightest on the left, the darkest on the right, and the others graded in between. Do not take more than 15 minutes for this part of the experiment. After you have arranged the cards write down, in the table below, the numbers on the backs of the cards in order.

These directions were printed on a record-blank which also contained the table with fifty spaces for entering the numbers found on the cards. A fresh copy of the blank was supplied to the worker at each practice sitting. Emphasis was laid on the fact that the numbers on the backs of the cards were unreliable and that there was no such thing as a standard arrangement of the cards; but at the same time it was confidently asserted that a good record in the later part of the experiment (to be described shortly), depended upon a scrupulously careful arrangement of the cards in order of brightness, for the amount of error in the memory and recognition test was to be measured in terms of the worker's own scale of brightness, and any irregularities in the scale would tend to increase the score of his errors.

In order to determine whether any improvement in the accuracy of the arrangement resulted from practice it was necessary to discover the correct arrangement of the cards. As the cards differ considerably in color and in character of surface it was thought that an empirical determination of the correct order would be preferable to a physical measurement of brightness in which these disturbing factors would escape consideration.[1]

The empirical determination of the order of brightness of the cards was made from the records of 37 persons. As each worker had made 10 arrangements there were 370 judgments upon which to base the average position of each card. The data from this computation are presented in Table I. This table shows not only the average position assigned to each card (with the mean variation) but also, under the heading "most probable position," the position which each card would occupy in a series of fifty if they were arranged as nearly as possible in order in a single series according to the average judgment. This last arrangement takes account of the fact that while some of the cards have almost the same average position they can not occupy the same position in an actual arrangement.

When the correct position for any particular shade has been determined it is possible to measure the accuracy of the arrangement for any of the ten practice sittings by the amount of the average deviation of the different persons from that

[1] The cards used in this experiment were prepared from a set of 'Hering' gray papers. The numbers given in the first column of Table I. are those found stamped on the original rolls, but it is evident from the data of Table I. that many of these numbers are without meaning, either because they fail to designate the approximate position in the series (compare No. 20, No. 25, or No. 31), or because different numbers are assigned to papers of almost the same brightness (compare Nos. 20 and 28, or Nos. 25 and 32). Enquiry elicited the information from the manufacturers that they made up different shades at different times, according to the demand, and that the different batches were not alike, so that when they sold an entire set the purchaser received portions of different and dissimilar sets.

TABLE I

Arbitrary Number to Designate the Shade[1]	Average Position	Mean Variation	Most Probable Position
1	1.00	0.00	1
2	2.00	0.00	2
3	3.00	0.00	3
4	4.06	0.12	4
5	5.21	0.37	5
6	5.76	0.39	6
7	6.97	0.08	7
8	8.00	0.02	8
9	9.39	0.48	9
11	10.27	0.78	10
10	11.45	1.04	11
12	11.71	0.95	12
13	13.10	0.85	13
14	13.16	0.83	14
15	15.56	0.87	15
17	17.49	1.45	16
16	17.64	1.53	17
18	17.84	1.42	18
19	18.39	1.56	19
23	20.82	1.54	20
21	21.35	1.66	21
24	21.47	1.73	22
22	21.86	1.53	23
20	25.24	1.86	24
28	25.36	1.63	25
29	26.26	1.77	26
27	26.67	1.51	27
30	27.29	1.70	28
33	29.46	1.52	29
26	30.04	1.72	30
32	30.42	1.37	31
25	30.60	1.52	32
35	34.35	1.43	33
37	34.59	1.53	34
34	35.64	1.60	35
39	35.96	1.52	36
40	36.40	1.75	37
36	37.66	1.41	38
31	38.23	1.74	39
38	38.70	1.21	40
45	42.09	1.21	41
43	42.86	0.96	42
44	42.90	1.12	43
41	43.04	1.18	44
42	44.75	1.04	45
48	46.02	0.80	46
47	46.56	0.55	47
46	47.79	0.39	48
49	49.00	0.10	49
50	49.94	0.11	50

position. A larger deviation means a larger number of cards wrongly placed. Table II. shows the average deviation for each of the ten days of practice. Fewer cards were wrongly

[1] See foot-note on preceding page.

placed the first day than any other day. There is not the slightest tendency to reduce the number of misplaced cards as practice advances. These conclusions are the same whether the deviations are calculated from the average positions or from the 'most probable positions' as defined above. Practice does not increase the accuracy of the work of grading the shades of gray.

Table II

The Amount of Inaccuracy in Grading the 50 Gray Cards on Each of 10 Trials

The average amount of displacement from the true position has been calculated for 37 persons for each of the 50 cards. The 50 figures so obtained have been averaged to give the figures in the table.

Trial	Av. Deviation from the Av. Position of the Card; All Cards Combined	Av. Deviation from the Most Probable Position All Cards Combined
1	0.95	0.93
2	1.04	1.03
3	1.08	1.10
4	1.13	1.13
5	1.11	1.11
6	1.04	1.04
7	1.11	1.12
8	1.06	1.07
9	1.08	1.08
10	1.06	1.07

2. Memory Training with Shades of Gray

Most experiments intended to demonstrate an improvement of memory with practice require so much time and such an unusual amount of patience that they can not be made use of in connection with an ordinary course of laboratory experiments. The present experiment shows a very substantial improvement of memory, or at least a form of memory, in the course of no more than five hours of actual work and with very little of the effort usually involved in learning. The portion of the printed directions dealing with the test of memory is given herewith; the conditions of work will be made more clear in what follows.

According to the program for each day's work, given below, pick out four cards from the series as you have arranged it, and observe them carefully, without looking at the numbers on the backs of the cards. Put the first set of cards away. Spread out the other set, face up, on the table. Pick out the same four shades, as nearly as you can, from this second set. Mark in the table the numbers found on the backs of these four cards.

PROGRAM

First day.	Pick out the	10th,	20th,	30th,	and 40th.
Second day.	" " "	11th,	21st,	31st,	and 41st.
Third day.	" " "	9th,	19th,	29th,	and 39th.
Fourth day.	" " "	12th,	22d,	32d,	and 42d.
Fifth day.	" " "	8th,	18th,	28th,	and 38th.
Sixth day.	" " "	13th,	23d,	33d,	and 43d.
Seventh day.	" " "	7th,	17th,	27th,	and 37th.
Eighth day.	" " "	14th,	24th,	34th,	and 44th.
Ninth day.	" " "	6th,	16th,	26th,	and 36th.
Tenth day.	" " "	10th,	20th,	30th,	and 40th.

The record of the day's work, when filled out, shows: (1) the actual series of grays as arranged by the student; (2) the four shades selected by him in the memory test; (3) the amount of his error for each shade. The amount of the error depends upon the worker's own arrangement of the cards. Suppose he has sought for the 10th, 20th, 30th, and 40th cards, and that the shades which he has picked out in the memory test, as they lay scattered in irregular order on the table, are the 11th, 20th, 33d and 39th, according to his own arrangement as shown in his record and regardless of the true brightness or arbitrary designations of the cards; the errors in this case would be *plus one*, *zero*, *plus three* and *minus one*, or disregarding the direction of error, a total of *five*. The worker was informed at once of the actual numerical amount of his errors, but he was not allowed to look again at the cards themselves. It will have been observed that different shades were used for the tests on different days so that the practice did not consist of learning to recognize certain shades but was confined to the more general process of learning to perform the mental operation of recognizing this kind of thing.

The actual amount of improvement in the memory test was large. Table III. shows the work of three seasons. In 1911 the recognition test was made with the same set of cards that was used in the memorizing by simply shuffling them up. This involved the difficulty that if there were any spots or other marks on the cards studied they might help in the work of picking out. As the cards suffer considerably from wear

Table III

The Daily Amount of Error in Recognizing 4 Shades of Gray Out of a Set of 50

The figure is the average per person of the difference between the position-numbers of the 4 shades which were selected and the position-numbers of the 4 shades which were sought and should have been selected. The amount of error decreases with practice.

Trial	1911, 33 Persons	1913, 37 Persons	1913, Errors on Probable' Scale	1914, 22 Persons
1	15.5	14.8	14.9	14.2
2	13.4	16.4	16.3	11.3
3	12.7	11.8	12.8	9.9
4	8.3	12.3	12.2	9.6
5	9.1	9.6	10.0	7.5
6	8.2	11.6	11.1	9.9
7	8.2	10.5	11.4	6.4
8	9.5	8.6	9.0	9.8
9	5.9	8.2	8.2	8.3
10	5.8	9.0	9.9	10.0

during the practice such spots multiply, and better records can be made with their help at the end of the period of practice than at the beginning. None of the students of the 1911 group realized that he was making much use of these marks but the result shows that this group made a larger apparent improvement than the latter group who were deprived of this help by being required to select the cards from another set. In 1913 this source of error was eliminated. The subject scattered the cards of a second set and selected from among them according to the directions which have been reproduced above.

The experiment of 1914 was designed to eliminate anothe possible source of error. It was felt by many of the worker that the familiarity with the cards which was acquired in the course of arranging them in order of brightness might be responsible for a large amount of the apparent improvement of memory. Accordingly the experiment was so arranged as to reduce the amount of this factor to a minimum. On the first day the procedure was the same as in the preceding season in order that the first test of memory might be made under exactly the same conditions, but after the first day there was no arranging of the cards in order by degrees of brightness.

Instead, the cards were arranged by the subject according to number with their faces down (they were numbered according to the 'most probable order' described above), then the whole set was turned over and the four cards for the memory test were selected. The worker then proceeded to scatter another set and pick out the cards which seemed to be the same, just as in the experiment of the year before. The experiment was identical with the one of the year before in every respect except that the workers were not familiarized with the material through practice in arranging it. Precisely the same opportunities were afforded in 1914 as in 1913 for the employment of special devices, illegitimate as well as legitimate, as helps in choosing the shades.[1]

The scoring in 1914 was on the basis of the average of previous determinations as shown in Table I. under the heading 'most probable position.' In order to make a comparison possible between these results and those of 1913 the latter were rescored on the same basis; that is to say, the error was stated in both cases as the difference in position between the card sought and the card found if they were all arranged in the 'most probable' order. The results obtained for the 1913 experiment according to this method of scoring are not essentially different from those obtained when the scoring was done on the basis of the worker's own arrangement for the day, and are only a little more regular.

The improvement of recognition through practice is no less rapid in 1914, without the additional familiarity which comes from arranging the cards in order, than in 1913 when this factor was fully operative. With this assurance that the factor of familiarity is of no great importance, and with the evidence that the results can be scored satisfactorily on the basis of the learner's own daily arrangements of the cards without undertaking the laborious process of measuring the actual brightness of the different ones, this experiment gives a simple and easily workable demonstration of the improvement of a form of memory through practice.

[1] The most important of these devices is the estimation of the distance from the end of the scale to the lightest or darkest of the four cards to be memorized. It is

comparatively easy to find these cards by simply counting up mentally, as the cards lie scattered at random on the table, from the white or black extreme to the eighth or whatever one is desired. The other two cards are frequently found by grading in between the two extremes. Only half as many errors were made on the lightest shades as on the middle ones and fewer errors occurred on the darkest shades than on the middle ones. There was no general tendency to under- or over-estimate the shades of gray in the memory test, but too light a shade was selected for the lightest and too dark a shade for the darkest, and the same tendency appeared with the other two shades, as may be seen from the following tabulation of the average number of errors per card.

	Too Light	Too Dark
Lightest card	1.08	0.77
Second card	1.90	1.57
Third card	1.72	1.80
Darkest card	1.17	1.52
Average all four cards	1.47	1.42