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CONTENTS OF VOLUME III.

ALPHABETICAL INDICES OF NAMES AND SUBJECTS WILL BE FOUND AT THE END OF THE VOLUME.

ARTICLES.

DISCUSSION AND REPORTS.

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THE PSYCHOLOGICAL REVIEW.

PSYCHOLOGY AND PHYSIOLOGY.[1]

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In a paper which I read two years ago before this Association, I endeavored to make clear the nature of the work done by the psychologist, and to set forth the assumptions upon which he must proceed and the method he must employ. I maintained that he must assume the existence of an external physical world, and the existence of certain copies or representatives of it intimately related to particular bodily organisms. These transcripts of the external world, supplemented by certain elements not supposed to have their prototypes without (feelings of pleasure and pain, etc.) are called minds. I stated that it was the task of the psychologist, with the aid of introspection, observation and experiment, to obtain a knowledge of such minds, and to reduce their phenomena to laws. I held further that, whether we regard mental phenomena as parallel with nervous processes, or as belonging to the same series with them and forming a part of the one chain, that does not affect the fundamental assumption of the psychologist, the assumption of an external world and of minds which mirror it, nor does it affect his general method of procedure, the employment of introspection, observation and experiment.

These positions seem to me to be commonplaces of psychology, and so generally accepted, explicitly or implicitly, that they may be taken without question. They appear also to de-

[1] Read before the Philadelphia meeting of the *American Psychological Association*, *1895*.

fine with some exactness the field which belongs to the psychologist, and to make possible a line of demarkation between psychology and other scientific disciplines. As, however, the sciences differentiate themselves clearly from one another, and acquire definiteness, only as they approach a high state of development, and as psychology and the sciences which lie nearest to it must be admitted to be still in their infancy, it is not to be wondered at that the question of boundaries should often be mooted, and charges and countercharges of trespass made with some warrant. It was but lately that psychology was scarcely recognized as a separate science at all, being treated as a branch of philosophy, and psychological facts being served in a sauce of epistemological speculations. From this condition of affairs the science is gradually emerging. The separation is by no means complete in fact, as a glance at many of our psychologies will show, but we may console ourselves with the thought that the state of affairs is better than it was, and that human knowledge is gaining through the change.

In our own day the living question is that of the relation of psychology to physiology, and of the line of demarkation between them. We hear charges that the psychologists sometimes occupy themselves in doing work which is purely physiological, and one who reads the text-books of physiology cannot but see that the writer is frequently on ground not properly his own. Where are we to draw the line between the two fields? And if a clear line can be drawn, how far is it desirable that a division of labor should take place? It is to a brief discussion of these questions that this paper is devoted.

I have said above that whether we regard mental phenomena as parallel with nervous processes, or as belonging to the same series with them and in causal relation with the world of things, it need not affect our view of the fundamental assumptions of psychology or of psychological method. But in discussing the line of demarkation between psychology and physiology, this question of the nature of the relation between mind and body may become an important one, and it will be convenient to treat my subject under two heads; that is, to inquire into the relations of these sciences on the assumption that mental states and bodily do not belong to the same series, but are merely parallel (the so-called automaton theory); and then to consider the effect of regarding the two sets of phenomena as forming one causally related whole.

I.

Assuming, then, that mental states have no influence upon bodily, and that the so-called sensory-motor arc consists of an unbroken chain of physical processes, what are the limits of the science of physiology? The task of the physiologist lies in the study of the functioning of living bodies. These bodies form a part of the physical world, a world complete in itself, and which demands for none of its phenomena an explanation drawn from any other sphere. To explain physical actions, however complicated, the physiologist should have recourse to bodily processes, which in turn find their explanation in other physical processes, and these in still others, and so on without end. The rhythmic contraction of a heart, the fall of an eyelid stimulated by an irritation of the conjunctiva, the unconscious gnawing at a fingernail, and the intricate chain of actions which result in the production of a work of art or a scientific treatise, all must be explained in the same way, as one explains the unfolding of a leaf or the reddening of an apple. In each case we have the functioning of a living body, a physical thing, and our causes and effects must all be physical.

This complete physical explanation of the functioning of organisms is, of course, only an ideal, and an ideal which, in the present condition of the science of physiology, smiles at us from a hopeless distance. Whether it be the contraction of a heart, or the fall of an eye-lid, or the biting of a finger-nail, or the penning of a sentence, the chain of physical causes which bring about these results lies hidden in that darkness which encloses the glimmering taper of our science. Exact knowledge of the antecedents of any bodily movement does not exist, and in its absence the physiologist is forced to give such fragmentary explanations as he can, often even overstepping the limits of his own science and using conceptions which are really out of place in it, but which he seems to be compelled to use *faute* *de mieux*. He has no right to speak of sensations, of feelings, of ideas; they are not in his world. The functioning of a brain, as he is concerned with it, results in motions immediate or remote, not in feelings and thoughts; and to make use of such in his reasonings amounts to confessing, either that he chooses to be a psychologist as well as a physiologist, or that, having found his own road impassable, he has been forced to continue his journey upon that of his neighbor.

How little the physiologist is in a position to furnish such an explanation of the functioning of organisms as I have outlined above is impressed upon one who reads critically our standard text-books upon physiology. One sees that, if we eliminate from the chapters which treat of the nervous system the anatomical portions and the psychological portions, the residue is surprisingly small. Certainly nowhere do we find such a description of the antecedents of a bodily movement as I have held up as our ideal. Let me take for illustration the well known work by Professor Foster, which is so widely used as a text-book. The learning and candor of the author, as well as his caution in the expression of opinions, make him, I think, a desirable representative of his class. I shall quote a few passages from various parts of his book.[1]

The necessary limits of such a paper as this force me to omit much that directly bears upon the subject under discussion. The question is as to the exact chain of physiological events between a sensory stimulus and the resultant muscular movements. We have to consider the occurrences in the nerves and in the nervous centres, both spinal and cerebral. As Dr. Foster begins with the motor processes, I shall consider these first.

As to the changes in a nerve during the passage of the nervous impulse our author is frank in his admission of ignorance. He regards it as clear that the impulse is something quite dif-

[1] I shall quote from the sixth London edition. That the book may stand as a representative of its class becomes clear when one examines almost any of the more recent works on the subject; *e. g.*, Waller's 'Introduction to Human Physiology, (London, 1893); Bernstein's 'Lehrbuch der Physiologie' (Stuttgart, 1894); Munk's 'Physiologie des Menschen und der Saügethiere' (Berlin, 1892); or the 'Vergleichende Physiologie der Haussaügethiere', edited by Ellenberger (Berlin, 1892).

ferent from the ordinary electric current, but what it is he does not venture to say (Part I, pp. 127, 156).

The mechanisms with which the spinal cord is provided appear also to be mere matter of conjecture. Concerning these mechanisms Dr. Foster speaks as follows: "If we regard the spinal cord, and apparently we have a right to do so, as resulting from the fusion of a series of segments or metameres, each segment, represented by a pair of spinal nerves, being a ganglionic mass, that is to say, a mass containing nerve cells with which nerve fibres are connected, we should expect to find that the fibres of a spinal nerve soon after entering in, or before issuing from the spinal cord, are connected with nerve cells lying in the neighborhood of the attachment of the nerve to the cord. We should, we say, expect to find this; but owing to the difficulty of tracing individual nerve fibres through the tangled mass of the substance of the cord, our actual knowledge of the termination of the fibres of the posterior root, and origin of the fibres of the anterior root, is at present far from complete" (III, 876). In a later section we come upon this passage: "From these and similar phenomena we may infer that the nervous network spoken of above[1] is, so to speak, mapped out into nervous mechanisms by the establishment of lines of greater or less resistance, so that the disturbances in it generated by certain afferent impulses are directed into certain efferent channels. It may be added that though conspicuously purposeful movements seem to need the concurrent action of several segments of the cord, and as a rule the greater the length of the cord involved, the more complex and the more distinctly purposeful the movement, still the movements evoked by even a segment of the cord may be purposeful in character; hence we must conclude that every segment of the nervous network is mapped out into mechanisms" (III., 909). A little further we find: "But if the spinal cord possesses mechanisms for carrying out coördinated movements, which in the case of voluntary movements are discharged by nervous impulses descending from the brain, we may infer that in reflex actions the same me-

[1] *i. e.*, The grey matter of the cord.

chanisms are brought into action, though they are discharged by afferent impulses coming along afferent nerves instead of by impulses descending from the brain. The movements of reflex origin, in all their features, except their exciting cause, appear identical with voluntary movements; the two can only be distinguished from each other by a knowledge of the exciting cause. And it seems unreasonable to suppose that the spinal cord should possess two sets of mechanisms in all respects identical, save that the one is discharged by volitional impulses from the brain and the other by afferent impulses from afferent nerves" (III., 910).

We are then, it seems, forced to assume the existence in the cord of various mechanisms for carrying out movements. What these mechanisms are and how they act we do not know. We know only that *something* happens in the cord, not *what* happens.

Our ignorance regarding the structure and functions of the bulb appears to be also great. I shall cite but two extracts: "Thus of the various tracts or strands of the spinal cord two only are known definitely and certainly to pass as conspicuous unbroken strands through the bulb to or from higher parts; namely, the pyramidal tract to the cerebrum and the cerebellar tract to the cerebellum; all or nearly all the rest of the longitudinal fibres of the cord reaching the bulb end, as far as we know at present, in some part or other of the bulb; and we may infer that some or other nerve cells of the bulb serve as relays to connect these fibres of the cord with other parts of the brain" (III., 949). "Meanwhile enough has been said to show that the bulb differs very materially in structure from the spinal cord. The grey matter of the bulb is far more complex in its nature than is that of any part of the cord; and the arrangement of the several strands and tracts of fibres is far more intricate. The structural features on the whole, perhaps, suggest that the main functions of the bulb are two-fold; on the one hand, it seems fitted to serve as a head centre governing the spinal cord, the various reins of which, with the exceptions noted, it holds, as it were, in its hands; on the other hand, it appears no less adapted to act as a middleman between parts of the spinal cord below and various regions of the brain above. As we shall see, experiment and observation give support to these suggestions" (951). It is scarcely necessary for me to add that the experiment and observation referred to do not remove the questions as to the functions of the bulb and the mechanisms it contains from the field of conjecture.

The section entitled "The Disposition and Connections of the Grey and White Matter of the Brain" opens with the following passage: "As we pass up from the bulb to the higher parts of the brain, the differentiation of the grey matter into more or less separate masses, which we have seen begin in the bulb, becomes still more striking. We have to distinguish a large number of areas or collections of grey matter more or less regular in form and more or less sharply defined from the surrounding white matter; to such collections the several terms corpus, locus, nucleus and the like have from time to time been given. These areas or collections vary greatly in size, in form and in histological characters; they differ from each other in the form, size, features and arrangement of the nerve cells, in the characters of the nervous network of which the nerve cells form a part, and especially perhaps in the extent to which the more distinctly grey matter is traversed and broken up by bundles of white fibres. Guided by the analogy of the spinal cord, as well as by the results of experiments and observations directed to the brain itself, we are led to believe that the complex functions of the brain are intimately associated with this grey matter; and a full knowledge of the working of the brain will carry with it a knowledge of the nature and meaning of the intricate arrangement of the cerebral grey matter. At present, however, our ignorance as to these things is great; and, though various theoretical classifications of the several collections of grey matter have been proposed, it will perhaps be wisest to content ourselves here with a very broad and simple arrangement" (III., 952).

This modest exordium is followed by a number of frank confessions of ignorance which appear fully to justify it. We find such statements as: "Our knowledge of the finer histological details of the various masses of grey matter is at present too imperfect to afford any basis whatever for physiological deductions, and it will be hardly profitable to dwell upon them" (1022). "In the present state of knowledge it is impossible to come to any satisfactory conclusion concerning the meaning of the variety and arrangement of the cells and other constituents of the cortex" (1032). These two citations are of a sufficiently sweeping character to cover the whole ground; I shall, however, allow myself the space necessary to present two somewhat more lengthy extracts. They are as follows: "In the spinal cord we were able to divide all the fibres into afferent and efferent respectively, though even here we meet with some difficulty. Dealing with the cerebral cortex, which, as we have already seen, is certainly especially concerned in voluntary movements and in the development of full sensations, we may be tempted to consider the fibres connected with the grey matter similarly divisible into motor and sensory; and we may go on to suppose that the fibres joining the cortex as axis cylinder processes of recognizable cells are motor fibres, and that all the other fibres joining the grey matter in some way are sensory fibres. But in doing so we are going beyond our tether; in all probability the nervous processes going on in the cortex are far too complex to permit such a simple classification of the functions of fibres as that into motor and sensory; and any attempt to arrange either fibres or regions of the cortex as simply motor or sensory is probably misleading" (1033). "The exact nature of the part played by the cortex and the pyramidal tract in voluntary movements our present knowledge is inadequate to define. When we pass in review a series of brains from the lower to the higher and see how the pyramidal system is, so to speak, grafted on to the rest of the brain, when we observe how the increasing differentiation of the motor cortex runs parallel to the increasing possession of skilled educated movements, we may perhaps suppose that 'a short cut' from the cortex to the origins of the several motor nerves, such as is afforded by the pyramidal fibres, from the advantages it offers to the more primitive path from segment to segment along the cerebro-spinal axis, has by natural selection been developed into being in man the chief and most important instrument for carrying out voluntary movements; but, we repeat, it remains even in its highest development a link in a chain, and a knowledge of how the whole chain works is at present hidden from us" (1063).

So much for the nervous antecedents of movements. The few extracts I have given justify, I think, my statement that the physiologist is not in a position to give any accurate account of the chain of causes which led to the fall of an eyelid or the penning of a sentence. What happens in the brain is unknown; what happens in the lower centres is also unknown; the nature of the nervous impulse is still problematic. It is not for the psychologist to throw stones, and I lay emphasis upon this ignorance on the part of our fellow-workers in science only because it seems to me an important source of confusion as to the limits of the science of physiology. Sciences grow in definiteness as they develop, and the lines which mark them out from one another become more distinct. Here we are dealing with something very vague and very dim, and one may expect a body of knowledge so dim and vague to have a misty and uncertain boundary.

Our author expresses in various places a desire to remain on purely physiological ground and to avoid a mixture of psychology in his discussions. He puts forward a few cautious statements which would rather incline one to believe that he sympathizes with the view of the relation of nervous processes to mental phenomena assumed to be true in this part of my paper. "Looking at the matter," he says, "from a purely physiological point of view (the only one which has a right to be employed in these pages), the real difference between an automatic act and a voluntary act is that the chain of physiological events between the act and its physiological cause is in the one case short and simple, in the other long and complex" (III., 1004). A little further we find the same thought: "In short, the more we study the phenomena exhibited by animals possessing a part only of their brain, the closer we are pushed to the conclusion that no sharp line can be drawn between volition and the lack of volition, or between the possession and absence of intelligence. Between the muscle-nerve preparation at the one limit, and our conscious willing selves at the other, there is a continuous gradation without a break; we cannot fix on any linear barrier in the brain or in the general nervous system, and say, 'beyond this there is volition and intelligence, but up to this there is none'" (III., 1007). And at the close of the discussion of voluntary movements we come upon still another striking passage: "Lastly, without attempting to enter into psychological questions, we may at least say that the birthplace of what we call the 'will' is not conterminous with the motor area; the will arises from a complex series of events, some of which take place in other regions of the cortex, and probably in other parts of the brain as well. With these parts the motor area has ties concerned not in the carrying out of volition, but in the generation of the will. So that, looking round on all sides, it is obvious, as we have said, that the motor area is a mere link in a complex chain" (III., 1069).

These passages are, to be sure, capable of more than one interpretation, and I shall again refer to them later; but it is at least clear from them and from others that the author has desired to avoid unnecessary trespass on psychological ground. That he constantly make use, however, of psychological conceptions the most cursory examination of his book makes evident. We read that a common effect of the arrival at the central nervous system of impulses passing along afferent nerves is a change in consciousness, or a sensation (III., 850, 851); that the effects of 'shock' may be a temporary diminution or loss of consciousness, of volition, of reflex movements and other nervous actions (903); that a muscle may be thrown into contraction by the will (906); that choice may be determined in some cases by an intelligence (909); that mechanisms in the lumbar cord may be brought into play by the will (914); that, in the case of a frog deprived of its whole brain, the signs of the working of an intelligent volition are either wholly absent or extremely rare (999); that the operations of the will are limited by the machinery at its command (1002); that we may, perhaps, speak of a mutilated animal as the subject of sensations, but that there is no satisfactory evidence that it possesses either visual or other perceptions, or that the sensations which it experiences give rise to ideas (1006); that in an ordinary voluntary movement an intelligent consciousness is an essential element (1068), and that the will, blundering at first in the maze of the nervous network, gradually establishes easy paths (1069).

These statements from one who declares that in his pages things must be looked at from a purely physiological point of view, and who realizes that the science of psychology occupies a distinct field upon which it is not desirable that he should encroach, are very suggestive. May we not assume that they find their explanation in the fact that poverty of physiological data forces the physiologist off his own ground? A physiologist, like everyone else, is conscious that he experiences sensations, has perceptions, reflects and wills. What actions of the brain correspond to these physiological facts? Dr. Foster has frankly admitted that he does not know. Yet we must assume that there are nervous occurrences which thus correspond, and it is desirable to mark distinctions between these hypothetical occurrences. How mark these distinctions? There appears to be no other way to do it than to abandon physiology and turn to psychology. It ought, however, in the interests of clear thinking, to be distinctly recognized that this is a makeshift, and argues that the science which must thus be pieced out by scraps taken from another one is in a very imperfect state of development. Such a makeshift ought not to be allowed to obliterate the line dividing the two sciences thus forcibly brought together.

If the parts of Dr. Foster's treatise concerned with the motor aspects of the nervous system have seemed to wander from the field of pure physiology, the parts concerned with its sensory aspects must be regarded as sinning in a still higher degree. The discussion opens with a section entitled "On the Development within the Central Nervous System of Visual and of some other Sensations," and this is followed by one entitled "On the Development of Cutaneous and some other Sensations."

To the thoughtful reader of his pages the author's reasons for selecting these psychological titles seems clear. We are informed that "in dealing with sensory effects we must expect and be content for the present with conclusions less definite and more uncertain even than those gained by the study of motor effects" (III., 1077). We find that, speaking of the functioning of the cortex in vision, "the only clear and consistent statement which can be made with any confidence is the broad and simple one that the hind region of the cortex is in some way intimately concerned in vision" (III., 1083); and that "although the matter is thus in many of its details at present outside our exact knowledge, we may probably conclude that in the complex act of complete vision, while part, especially the more psychical part, is carried out in the cortex, more particularly of the occipital region, part is accomplished in the lower centres, the tegmental masses. As to the several functions of the three masses,[1] we know almost absolutely nothing" (1084). We learn that the olfactory nerve "is undoubtedly the nerve of smell" (1085), and that, "though the evidence on the whole goes to show that the cortex at the front end of the hyppocampal gyrus is especially connected with smell * * * * yet the whole matter stands on a somewhat different footing from the sense of sight" (1087). We learn further that "though sensations of taste enter largely into the life of animals, and indeed of man himself, we have no satisfactory indications which will enable us to connect this special sense with any part of the cortex" (1088). We are told that "the connections of the auditory nerve with the cerebral hemisphere belong to the same category as those of other afferent cranial, and, we may add, spinal, nerves; we have no very clear anatomical guide toward any particular part of the cortex" (1088); and that though the method of degeneration suggests a connection with the cortex of the temporal lobe, "the matter needs further investigation" (1089). As to cutaneous and other sensations arising through impulses along the nerves of the body generally, our author speaks as follows: "The fairly convincing evidence that the occipital cortex has special relations with vision, and the less clear evidence that other regions have special relations with smell and hearing, suggest that special parts of the cortex have special relations with the sensations now under consideration. But in the cases of the senses of sight and smell we had a distinct anatomical

[1] *i. e.*, The lateral corpus geniculatum, the pulvinar, and the anterior corpus quadrigeminum.

leading; and we have seen how uncertain is the evidence where such an anatomical leading fails, as in hearing and taste. In the case of sensations of the body at large, the anatomical leading similarly fails" (1091).

In view of the above statements we cannot regard it as surprising that the author comes to the conclusion that "it is difficult to say anything definite concerning the transmission of sensory impulses and the development of sensations" (III., 1109). Neither is it surprising that he has chosen psychological titles for his discussions. The only thing which appears to be known with sufficient definiteness to be named appears to be the sensation. The corresponding nervous process is covered with thick darkness—darkness which may be felt. And it is not surprising that in a section of his work with the unexceptionable physiological title, 'On the Time Taken up by Cerebral Operations,' we should find the following odd mixture of physiology and psychology: "The events taking place in the central stage are of course complex, and this stage may be subdivided into several stages. Without attempting to enter into psychological questions, we may at least recognize certain elementary distinctions. The afferent impulses started by the stimulus, whatever be their nature, when they reach the central nervous system undergo changes, and, as we have seen, probably complex changes, before they become sensations; and further changes, now of a more distinctly psychical character, are necessary before the mind can duly appreciate the characters of these sensations and act accordingly. Then come the psychical processes through which these appreciated sensations, or perceptions, or apperceptions, as they are sometimes called, determine an act of volition. Lastly, there are the executive processes of volition, the processes which, physical to begin with, end in the issue of coördinate motor impulses, or, in other words, start the distinctly physiological processes of the efferent stage. We may thus speak of the time required for the perception of the stimulation, of the time required for the action of the will, and of the time required for the complex psychical processes which link these two together" (III., 1122). We may admit that the author has not attempted to enter into psychological questions; we may even admit that he has attempted to keep out of them; but surely he has wandered on to ground which the most liberal use of language would not permit us to call physiological; and we cannot help raising the question whether what is not psychological is to be distinguished from what is simply by the fact that it is briefly and superficially treated. Sensations, perceptions, apperceptions, volition—are not these the things with which psychology deals?

In his chapters on the senses (IV., pp. 1–305) Dr. Foster appears to have forgotten that he has resolved to avoid psychological questions. These chapters cover some three hundred pages, and may, I think, be fairly described as a treatise on the peripheral sense organs, with rather full psychological appendices. The eye is discussed at length, and from that one passes to visual sensations, visual perceptions and visual judgments. What happens between the retina and the 'hind part of the cortex,' and what happens in that region of the cortex, are passed over in silence. The reason for these omissions the previous section on the development of sensations within the central nervous system makes clear. What happens between the retina and the cortex is not known. The chapter on sight is accordingly necessarily restricted to a discussion of the eye and of the psychology of vision. In the next chapter we similarly pass from a study of the ear to auditory sensations, perceptions and judgments. In the chapter following that, we find a section on the olfactory mucous membrane followed by one on olfactory sensations; and one on the peripheral organs of taste followed by one on gustatory sensations. The chapter on 'Cutaneous and Some Other Sensations' resembles those which precede it. There is some discussion of peripheral organs and much psychological material. It seems evident to the thoughtful reader of Dr. Foster's pages that he is everywhere forced out of his field by poverty of established physiological data. He travels on a parallel road because he finds his own impassable.

In the preceding I have confined myself to the examination of a single work on physiology. I have done so for convenience. The work is fairly representative of its class, and

I might have chosen in its place any one of a considerable number. The results of the examination appear to me to make it evident that we are as yet very far indeed from having realized the ideal set for the physiologist in the explanation of bodily movements. They appear also to make it evident that the physiologist is much given to trespassing on psychological ground. Far be it from me to imply that physiologists have no right to do psychological work, or that some of them may not do certain kinds of such work better than many psychologists. I do not even mean to maintain that, in the existing state of the science of physiology, it may not be wise for the physiologist to occasionally trespass in the interests of his own proper work. On this point I shall speak further in a few moments. What I wish to emphasize now is this: a completed science of physiology would, on the hypothesis which serves as a basis to this part of my paper, be wholly independent of psychology, and a book on physiology would have no excuse for containing psychology. As it is, such books contain a great deal of psychological material; and it should not be overlooked that this is psychological, and that, in dealing with it, the usual psychological method must be followed. It should never be assumed that, because it is found in works professedly concerned with another science, it is anything more than a 'quatorzième,' invited, nay, compelled to come in, to fill an unwelcome gap. Its presence in physiological discussions should not be allowed to obscure the line dividing two sciences, each of which has its appropriate method of investigation.

II.

In what precedes I have rested upon the assumption that bodily states and mental are not causally related in the strict sense of the words—that the two series are, so to speak, parallel. In other words I have assumed the truth of the so-called 'automaton' theory. It is to be noted, however, that, whatever may be the opinion of the physiologist on this point, his language does not favor such a view of the relation of mind and body. One who repudiates the theory—and I think it is a bold man who will dare to maintain that the present state of our knowledge justifies us in holding that the theory is proved to be true and must necessarily be accepted—one who repudiates the theory may view the relation between mind and body in either of two other ways. He may regard mental states as belonging to the physical series in the sense that they are effects of physical causes, and in their turn causes of physical effects; or he may regard the mind as a something at least partially independent of the physical series, and, as it were, breaking in upon it.

In either case the chain of purely physical events between the peripheral stimulus and the resultant movement is broken by the interpolation of something of a different kind. The sensory-motor arc is partly physical and partly psychical. How does this effect our views as to the relations of the two sciences, physiology and psychology?

The former of these two ways of viewing the relation beween mind and body is, I think, most in harmony with the language used by physiologists generally. Certainly, it is most in harmony with that used by Dr. Foster, as the extracts already given sufficiently indicate. Even the passages which, as I said above, might be taken as indicating that Dr. Foster favored the 'automaton' theory, may perhaps be understood as supporting this doctrine. The afferent impulses started by a physical stimulus are supposed, when they reach the central nervous system, to become sensations; the will is said to arise from a complex series of events which take place in various regions of the cortex and probably in other parts of the brain as well; and we are told that mechanisms in the lumbar cord may be brought into play by the will. Here we have, if we take the author's words as they stand, a composite arc—physical, psychical, and physical.

I am not inclined, however, to take such statements too seriously. Physiologists do not appear to pick their words very carefully, nor do they appear to have given much serious thought to this question of the relation of mind and body. It would be obviously unfair to read into their statements more than they have themselves seen in them. Nevertheless, it is worthy of mention that, even if their language is chosen only for convenience and is meant to be interpreted loosely, it is clearly misleading in case they do not hold to the view I am discussing; and it would be much better did they exercise a little care in expression. If, on the other hand, looseness of expression is an indication of looseness and vagueness of thought, it is highly desirable that it be vigorously attacked and speedily brought to an end.

But whatever be the real opinion of the physiologist regarding the matter, it remains to discuss this view of the relation of mind and body. If we accept it we have, it is true, from initial stimulus to resulting movement, but the one causal series. It is, however, a series made up of two quite different kinds of elements. We have, on the one hand, physical changes which may be studied, as are all physical changes, by directly objective methods. We now know very little about the changes in a nerve during the passage of the nervous impulse, but there is no reason to think that we may not justly expect to investigate these changes by the same methods as those employed in the investigation of physical and chemical problems. On the other hand, we have also to reckon with psychical facts, sensations, perceptions, volitions; and in whatever series one may be inclined to place these, it seems incredible that one should expect to study them just as one would study the changes in a muscle during contraction. It is not inconceivable that with improved apparatus we may some day arrive at an ocular demonstration of the translocation of molecules there supposed to take place. But would the most ardent physiologist expect an exhaustive study of the brain to reveal directly sensations of color or sound, or feelings of pleasure or pain? I am not now speaking of molecular changes corresponding to such psychical facts, but of the facts themselves. Surely there is but one way of reaching such facts, and that is by the use of introspection; and there is but one method by which they may be studied—the psychological method of introspection, observation and interpretation. Hence, even if we have to do with the one causal series, we have two kinds of facts and two distinct methods, and it seems, on the whole, convenient that the work should be divided between two men. We have abundant evidence that a given man may employ the one method very well and the other very badly.

On this view of the relation of mind and body the physiologist would not, it is true, be wholly independent of psychology; he would be occupied with series of occurrences which end in or are initiated by psychical facts, and he would be interested in these facts as he is interested in the physical stimuli which give rise to nervous impulses. They would, however, constitute no part of his own proper field of labor; and to give the total antecedents of a given bodily movement, the combined work of the physiologist and psychologist would be needed. It is hardly worthy of remark that, in the present state of our knowledge, the question as to the exact spot in the sensory-motor arc at which the psychical patch is to be inserted, is one which no sensible person will give himself the trouble of asking. On the wisdom of making mental states effects of bodily causes and setting them in the one series with these, it is not necessary for me here to comment.

If the mind be regarded as a something independent of the physical series of causes and effects, and, so to speak, breaking in upon it, the case is much the same as in the above. We have a physical series interrupted at a given point by something of a different nature, and which must be investigated by a different method. The physiologist appears to have a definite task—the study of the physical series; he may leave the examination of the gap between its two parts to the psychologist, whose work is sufficiently marked out from his own by the method employed, the method of introspection, observation and experiment, and interpretation.

So much for the theoretical boundary line between physiology and psychology. It is, I think, a sufficiently definite one. It is, however, a line, and not a fence; one may easily step over it, as, indeed, many do step over it. The question naturally arises, is it wise to step over it, and if so, when? I think this question may be answered in a general way by saying that, when, for any reason, an excursion into other territory will further one's progress in one's own, such an excursion is justifiable. If the physiologist can, through a study of psychical phenomena, arrive at some hint of their physical concomitants, or, if you will, causes, it seems quite right that he should make use of such a means to his end. The two theories of color vision commonly discussed in books on physiology very well illustrate this point. I have said somewhere above that practically nothing is known about the occurrences between the retina and the 'hind part of the cortex' in the act of vision. I might have added that comparatively little is known about what goes on in the eye itself. What has taken place in the retina when one has become conscious of seeing the color red or the color blue, physiology has never succeeded in directly demonstrating. Both the Young-Helmholtz and the Hering theories are attempts to guess at the nature of such physical occurrences by the aid of knowledge gained in another field, the psychological. Such a mode of procedure seems proper enough, but it is well to remember that were the science of physiology more completely developed, this excursus into psychology would be unnecessary. Where such an excursus has not a physiological end in view, but is merely psychological throughout, it does not appear to me justifiable. There are a number of chapters in Dr. Foster's fourth volume which seem to be of this nature. Their place is in a text-book on psychology and not one on physiology. In the place which they actually occupy they serve, I think, only to conceal poverty of physiological material and to confuse the reader's mind as to the limits of the two sciences.

The above sentences and, indeed, the whole argument of this paper support the conclusion that, with increase of knowledge, the amount of psychology to be found in text-books on physiology will be a diminishing quantity. This does not, however, imply that psychology will grow independent of physiology, as the latter will grow independent of the former. Physics and chemistry are independent of physiology, but it is not independent of them. The psychological method includes introspection, observation and experiment, and interpretation of what is thus brought to light. And the difference between guessing roughly at what is passing in a man's mind by watching the movements of his face, and studying systematically and minutely the human body with the same end in view, is not a difference in kind. The objective method in psychology implies the employment of physiology in the search for psychological truth; and, as I pointed out two years since in the paper already mentioned, the every-day psychology of the practical man who needs to know something about what is passing in his neighbor's mind is, after all, psychology, and only differs from that of the scholar in being less systematic, exact and reflective. If a study of the cerebral cortex will better reveal what we are seeking to discover than a study of the face, then by all means let us transfer our attention to that. Let us not, however, grow so interested in the study of the body as to forget that we are psychologists. Let us not take up physiological work which has no psychological aim. Now and then, I think, psychologists do this. When they do it I believe they are guilty of unjustifiable trespass, and would probably better serve the world by remaining on their own ground.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY. (III.)

COMMUNICATED BY PROFESSOR HUGO MÜNSTERBERG.

A. The Place of Repetition in Memory.

By W. G. Smith.

Smith College.

The investigation of which I wish to give a short account was undertaken with the view of affording material for a further step in the experimental analysis of the processes involved in learning and recollection.[1] Every one knows that repetition plays an important part in the process of acquiring knowledge, but hitherto there has been no attempt experimentally to study this factor beyond the experiments of Ebbinghaus relating to the effect of repetition on the duration of memory. The aim of the following experiments has been to determine the extent and character of memory at different stages of repetition. Series of nonsense syllables formed the subject-matter which had to be learned; the reagent made no attempt to learn a series by heart, but simply reproduced as much as he could recollect after he had repeated it a certain number of times.

The experiments were carried on in the Harvard Psychological Laboratory with the kind assistance of Prof. Münsterberg in the spring and summer of 1895. I am able to present the results gained from eight subjects. In some cases the experiments are not so numerous as might be desired; on the other hand, owing to the large number of subjects, any conclusion which may be drawn can hardly be vitiated by merely individual peculiarities. Only the initial stage of the research can be presented here, but as I have no immediate prospect of making any substantial advance in the investigation, it seems best to bring forward now the results so far as they have been gained.

[1]This research may be regarded as a continuation of the work on memory which formed the subject of an article in *Mind*, N. S., IV., p. 47.

The following method was adopted in the experiments. Series of syllables were printed on slips of paper by means of the typewriter in such a form that the subject could easily read what was printed. In each series there were ten syllables forming one line; in each syllable there were three letters, the vowel being in the middle. Syllables which were too harsh in sound, or which might suggest too easily an intelligible word or phrase were rejected. No two successive syllables were allowed to have the same vowel, and the same consonant could recur only after several others had intervened. Modified vowels were not used, and consonants whose pronunciation was ambiguous, *e. g.*, h, c, were either not used at all, or were used only under certain conditions. The syllables were formed and arranged after a method similar in certain respects to that followed by Müller and Schumann; the object was to let chance rule as far as possible in the formation of the series. When the supply of new and unobjectionable syllables was exhausted the syllables which had been already used were rearranged to form fresh series.

In the actual experiments the slip of paper bearing the syllables was inserted in a frame which was fastened behind an oblong horizontal opening in a screen made of black cardboard. Behind this opening and before the slip of paper was a shutter which could be raised or lowered at any moment. The subject, who sat at his ease before the screen, was required to read the series aloud, one syllable after another, at a rate determined by a metronome standing near him. The rate of the metronome varied with the different individuals, that rate being chosen for each reagent which seemed to be most convenient for him. As a matter of fact the two rates chosen were 80 and 100 per minute. Only in one case was the rate changed in the course of the experiments; this was done because the subject complained that the old rate had become too slow for him. The object of introducing the metronome was to secure that the subject should, as far as possible, give the same time and attention to each syllable. Where a series had to be repeated several times the subject made a pause of two beats each time he came to the end, and then began the repetition again. The shutter covering the series was raised only after the subject had given a signal that he was ready and had accommodated himself to the rhythm; before the experiment began he was told whether one,[1] few, or many repetitions were required. The signal for closing was given, except when there was only one repetition, by a tap on the table which came in the pause preceding the last repetition. The subjects were asked to repeat the series with regular attention and without any special effort or strain at any point; the purpose of the closing signal was to secure that the value of the experiment should not be lowered by any accidental fluctuation of attention just before the close of the experiment. By these precautions the disturbing effects of fatigue, of variations of attention and of emotional changes were to a large extent avoided. Where, notwithstanding the precautions, there occurred a disturbance of any kind which seemed to endanger the value of the result a new experiment was made. Irregularities in the formation of the series, or in the conduct of the experiments, were made a ground of rejection of the result when the subject had been disturbed thereby, or when the character of the irregularity seemed to render the value of the experiment doubtful.

The experiments were arranged with a view to ascertaining the value of the memory at five stages in the process of learning, the series being repeated, according to the directions of the experimenter, once, thrice, six times, nine times or twelve times. As far as possible an equal number of experiments was made each day for the various stages of repetition. Owing, however, to various distractions and also to the loss of time involved in cross-questioning the subjects in regard to their state of mind during the course of the experiments, this rule could not always be carried out. In no case have the experiments of any day been accepted on which there was not at least one experiment with each stage. Preliminary experiments for practice were made with each subject both at the beginning of the investigation and at the beginning of each day's work.

[1] The phrase 'one repetition' is so convenient that the inaccuracy involved in its use may be pardoned.

The results of these experiments are presented in the tables given below. In the first Table are given the numbers which represent the values of the memory at each stage of repetition, these numbers being the final averages gained by taking together the averages of the eight reagents. The object of the second Table is to show the relative frequency with which syllables in the various parts of the series are recollected. In Table III., which is printed at the end of this article, are given the individual averages which form the basis of the values given in Table I. The description of the divisions and details of Table I. applies without alteration to Table III.

The written records handed in by the subjects have been analyzed in Tables I. and III. from two points of view, and the resulting values have been arranged in two divisions. In the first division, on the left hand of the page, the records are analyzed from the point of view of the syllable. The first column gives the average number of syllables in each experiment, which are correct both in their component letters and in the position assigned to them by the subject, while in the second column are collected the syllables whose only fault is that they have been put in the wrong place. In the third and fourth columns are given the incomplete syllables, *i. e.*, those which have dropped a letter or exchanged one of their letters for a false one; in the third column appear the incomplete syllables whose place is correct, while those whose position is wrongly given are in the fourth. In the second division, where the different classes of error are marked by *Arabic* letters, the syllables are regarded as made up of separate letters; in this way several points which could not well be brought out in the first division receive recognition. In column *a* is given the average number of letters which are omitted. In the next two columns are recorded the letters which are rightly recollected, but have been put in a wrong position; those under *b* have retained their position in complete or incomplete syllables, while the syllables themselves have been wrongly placed; those under *c* have lost all trace of their original arrangement. The next column, *d*, contains the letters which have been reproduced oftener than they appeared in the original series. Column *e* is intended to give material for a further analysis of the errors recorded under *c*, and contains the average number of *vowels* in each experiment whose original arrangement has been entirely lost. The average of all errors taken together is given under *m*; the figures in this column have been gained, not by adding the averages in the other columns, but by a separate summation of the errors in each experiment. Cases of inversion where the original order of the letters is simply reversed occurred so rarely that the column which had been set apart for their reception was left unused; errors of this kind found a place in column 3 or 4 in the first division, and in column *b* in the second division, the mode of estimation being slightly modified for them. Errors due to insertion of a wrong letter were likewise rare, and appear only in the total averages under *m*. The Roman numerals in the first vertical column represent the different stages in repetition.

In Table II. the numbers in the horizontal columns opposite the Roman numerals give the percentage of times that the syllables in the ten places in the series, whether in complete or incomplete state, are reproduced by the subject; the analysis takes into account only the original position of the syllable and neglects entirely the place assigned to it by the subject. Owing probably to the fact that the experiments were not sufficiently numerous to eliminate accidental variations, the results of the analysis regarding the original position of the recollected syllables are somewhat irregular if we look only at the individual results. The general tendency, however, is plain and since that tendency is expressed with sufficient clearness in the figures gained by taking together the averages of all the subjects I have decided to present only the final averages.

TABLE I.

	1	2	3	4	a	b	c	d	e	m
I. . .	2.2	0.35	1.1	0.6	15.5	2.5	3.0	1.0	0.7	22.2
III. . .	2.5	0.9	1.1	0.9	13.0	4.3	2.5	1.35	0.6	21.4
VI. . .	2.8	0.9	1.1	0.9	11.9	4.5	2.6	1.5	0.5	20.5
IX. . .	3.4	0.9	1.1	0.6	10.9	3.95	2.2	1.5	0.6	18.9
XII. . .	3.9	0.8	1.0	0.7	10.0	3.75	2.1	1.3	0.6	17.3

TABLE II.

	1	2	3	4	5	6	7	8	9	10
I. . .	81	52	24	16	16	24	26	26	62	84
III. . .	84	67.5	39	38	34	33	29	44	69	92
VI. . .	81	61	42	42	34	32	46	54	74	85
IX. . .	89	67	49	41	32	33	48	64	77	93
XII. . .	92	58	46	41	56	57	57	61	84	91

It will be noted on comparing the values given in Table I. with those in Table III. that, while the general features of the results are reproduced in Table I. with great distinctness, there is yet among the different individuals a considerable amount of variation. The values given by the subject St. in col. *m* are opposed to those of all the other subjects, though in the case of two others, H. and Sn., the numbers do not conform very closely to the typical curve. It is unfortunate that another subject, whose memory proved itself better than that of any other, was unable to continue his attendance long enough to give a satisfactory number of experiments. The three subjects, H., Cu. and P., who have carried out the largest number of experiments, present fairly typical examples of the different kinds of memory; in order to give some proof of the trustworthiness of the average values assigned, the probable error of the averages in col. *m* has been calculated[1] and the figures inserted to one place of decimals under *r*.

Before going on to draw any conclusion from the results we may note shortly the limitations of the research. The results obviously can only be taken as representative of the process of learning series of syllables of a certain length, repeated aloud in a more or less artificial manner. The only test of the value of the memory at the different stages lay in the accuracy with which the subject recollected the syllables immediately after the learning was finished. Without doubt the results would be different if we allowed some time to elapse between learning and recol-

[1] In experiments such as those of Ebbinghaus, as has been remarked, the probable error is an unsatisfactory test, because while the number of repetitions may become indefinitely large it can never fall below 1. Here, on the other hand, the total number of errors may be zero, but it can never rise above a certain point.

lection. Probably in this case the errors in the first stages would increase much faster than those in the later stages of repetition. Finally, in these as in other memory experiments, we have a very mixed result in which factors, such as the memory images of sight and hearing, are inextricably mingled together.

The results given in the tables confirm in general the accepted fact of the efficacy of continued repetition in impressing any kind of subject-matter on the memory. That even with the reagents who remember best its effect is so small is somewhat surprising. Probably the explanation of this feature is to be found partly in the artificiality of the experimental conditions; partly, also, in the fact that the subjects were directed not to try and learn as much as possible, but simply to repeat, with all possible regularity, what was presented to them. The advantage of this rule was that there was very seldom any complaint made of fatigue due to the experiments. A comparison of the average values in the earlier and later halves of the series of experiments carried out by the subjects who have furnished the largest number of experiments shows that in the majority of cases there is a slight increase in the value of the memory in the second half, a result probably due to practice.

It is interesting to observe a confirmation here of another fact which meets us in common life. In any pursuit or competition the candidates start fairly equal; it is towards the end that they begin to separate from each other. Here we are met by the fact that on the whole the different individuals do not differ very greatly in the number of errors which they commit after one repetition, while as we go on to twelve repetitions the difference increases markedly. The difference between the best and the worst memory after twelve repetitions is very much greater than after one repetition. A better way of proving the same fact consists in giving the mean variation of the final averages (Table I. *m*) at each stage:—

	I.	III.	IV.	IX.	XII.
mv.	1.8	3.0	3.8	3.7	5.1

The first repetition is undoubtedly the best; *i. e.*, more is learned by it than by any other repetition, or, in fact, by all the other repetitions put together. There seems to be a slight increase in the value of a repetition as we pass from the third to the twelfth; this result shows itself in cols. 1 and *m*, but not in col. *a*, where errors of omission alone appear; in fact the change in col. *a* is in the opposite direction, the increase in the number of letters recollected, caused by the successive repetitions, appearing to grow smaller as the number of repetitions increases.

If we look more closely into the figures for each stage we find certain regularities which hold for almost every subject. The number of syllables which are correctly remembered (col. 1) increases regularly with the increase in number of repetitions, while the total of errors (col. *m*) and also the errors of omission (col. *a*) decrease as regularly. The other classes in both divisions comprising the errors of disorder show values which remain pretty constant throughout; *i. e.*, the number of errors, while remaining absolutely constant, decreases relatively to the total number of syllables and letters remembered. It is one of the limitations of this investigation that it does not enable us to analyze exactly the errors due to the various kinds of confusion and disorder and separate them from errors of omission. To do this it would be necessary to employ a method which was followed by Bigham in his research on memory.[1] According to this method the subject would be supplied with a list of the syllables, arranged in chance order, which were being used in an experiment and would be required to rearrange them after the repetitions were finished. What we seem to have in the present experiments is a continual process of promotion during the learning; a syllable or letter, at first forgotten, appears bye and bye in one of the classes which represent failure to remember the right order and then passes into the classes of syllables or letters correctly remembered; in this way the figures representing errors of disorder might be expected to remain fairly steady.

Cases of inversion of syllables practically did not occur at all; inversions of letters and insertion of false letters occurred rarely, as before remarked. What the precise explanation of these facts may be I have no means of saying. With Ca. and R. the figures in col. 3 are much larger than in cols. 2 or 4 at each stage, while with H. and St. the figures in col. 4 are regularly the largest. Sn., on the other hand, shows the

[1] PSYCHOLOGICAL REVIEW, I. pp. 34, 453.

largest numbers in cols. 2, *b* and *c*. Such results point to the need for purposes of explanation of a more exact knowledge of the psychical processes of each individual. Observations were made in the course of the experiments on the nature of the memory and its variations at the different stages, but I have not been able to any great extent to correlate these observations with the numerical values given in the tables. The memory in every case seemed to be of a mixed character, now visual, now auditory and now motor images being more prominent. A comparison of the figures in cols. *c* and *e* seems both interesting and significant. If consonants retained their hold on memory to the same extent as vowels the figures in the last column ought to range about a third of those in col. *c*; as a matter of fact they range somewhere about a fourth, the figures tending to approach nearer to a third in the later stages. The conclusion seems justified that vowels impress themselves better on the memory than consonants. There was a tendency in most subjects to associate foreign ideas with the syllables or make the syllables into intelligible phrases, though towards the end this tendency was lessened. With one individual, Ca., this was a very troublesome feature from beginning to end, and there was hardly an experiment where I had not one or more instances of this associating tendency. I have summed up the associations made at each stage by this subject, and without any great stress being laid on the figures they may be presented as an illustration of the fact, which was otherwise confirmed, that this associative tendency grows with the number of repetitions.

	I.	III.	VI.	IX.	XII.
No. of Assns.	6	9	23	34	33

It was decided that an experiment should be rejected only where connecting associations were formed, *i. e.*, associations which connected two syllables in the series into a single intelligible phrase.[1] This rule proved in the end too severe, as the associations very often occurred in the more laborious experiments of the later stages, and in the end it was decided to ac-

[1] Examples: div nur—divine nurture; mon sud—Monday Sunday. The range of these associations will be understood when it is mentioned that they included English, Scotch, German, French, Russian, Latin, Greek and Hebrew words.

cept the experiment when an association was formed between the first two or last two syllables at any stage, or between syllables in any part of the series when the number of repetitions was twelve; in all these cases there was a considerable probability that the syllables would have been remembered in the absence of the association.

There does not seem to be any definite connection traceable here between excellence of memory and the mode of reproduction. The subject with the best memory and the subject with the worst both wrote from the beginning straight on, the syllables at the end of the series being thus written last. In the great majority of cases the first two syllables are reproduced first; often the last two come next; this is specially marked in the case of the reagent P. However, although the last syllable does not come first in the reproduction, it is in most cases best remembered.

The subjects were left free throughout the experiments to introduce a rhythm into the repetition if they pleased. In most cases there was a slight rhythm present. In a few instances its effect is visible in the detailed results which form the basis of the second table; in these cases there is a greater difference between the figures in the second and third and also the eighth and ninth places than between those in the first two and last two places. On the whole, however, its effect is less than might have been expected. It appears from Table II. that a syllable in the second half of a series has a somewhat greater chance of being remembered than one in the first half; the best places are at the beginning and the end, the chance of being recollected lessening at first rapidly, then more slowly as the middle of the series is approached. During the pause of two beats between the repetitions the subjects waited without trying to memorize; in most cases their eyes were fixed inattentively on the beginning or end of the series which was being presented. Two of them complained that in this way an undue advantage seemed to be given to the first and last syllables. One of the two adopted the device of shutting the eyes during the pause; in spite of this, the first and last pairs of syllables are in this case specially well remembered. There does not seem any reason to suppose that looking at the syllables in this inattentive way has any very marked effect upon the memory.

TABLE III.[1]

		1	2	3	4	a	b	c	d	e	m	r
H. 21	I. . .	0.7	0.1	1.1	1.0	16.9	2.6	5.1	2.0	1.4	27.1	0.5
	III. . .	0.7	0.5	1.3	1.5	15.3	5.0	4.6	1.6	1.4	27.2	0.4
	VI. . .	1.0	0.5	1.0	1.7	14.2	4.9	4.6	1.4	1.0	25.7	0.4
	IX. . .	1.4	0.4	1.3	0.9	14.5	3.2	4.8	1.8	1.3	24.5	0.5
	XII. . .	1.0	0.4	1.7	1.3	13.7	4.0	5.3	1.1	1.2	24.2	0.5
Cu. 21	I. . .	2.0	0.3	1.3	0.4	16.7	1.8	2.7	0.6	0.5	21.8	0.3
	III. . .	2.6	0.6	1.7	0.6	13.2	2.7	2.2	1.0	0.4	19.2	0.5
	VI. . .	3.2	0.5	1.7	0.8	11.3	3.0	2.0	1.3	0.3	17.7	0.6
	IX. . .	4.6	0.3	1.4	0.5	9.2	2.0	1.7	2.8	0.4	16.0	0.9
	XII. . .	4.9	0.6	1.3	0.4	8.1	2.7	1.4	2.0	0.4	14.3	0.7
P. 20	I. . .	2.6	0.2	1.25	0.5	15.3	2.15	2.1	0.35	0.5	20.05	0.5
	III. . .	3.0	0.9	1.1	0.6	13.6	3.5	1.1	0.9	0.3	19.2	0.6
	VI. . .	4.0	0.35	0.95	0.65	11.3	2.5	1.85	0.7	0.5	16.4	0.6
	IX. . .	4.25	0.6	1.0	0.5	10.45	2.95	1.35	0.8	0.35	15.6	0.7
	XII. . .	4.75	0.65	1.0	0.25	9.1	2.5	1.55	0.55	0.4	13.7	0.7
Ca. 18	I. . .	3.3	0.5	1.0	0.6	13.8	2.6	2.1	0.7	0.7	19.3	
	III. . .	3.7	0.7	0.9	0.8	11.4	3.4	1.6	1.1	0.4	17.7	
	VI. . .	3.9	1.3	1.25	0.6	8.0	5.3	2.6	1.4	0.9	17.0	
	IX. . .	4.4	0.8	1.0	0.7	7.2	3.7	1.8	0.8	0.6	13.7	
	XII. . .	6.2	0.7	0.8	0.3	6.0	2.8	0.7	1.2	0.3	11.0	
L. 16	I. . .	2.3	0.6	0.6	0.4	17.5	2.6	1.8	0.25	0.4	22.1	
	III. . .	2.4	1.1	0.75	0.5	14.5	4.4	2.0	0.6	0.4	21.6	
	VI. . .	2.3	0.6	0.9	0.8	15.6	3.5	1.7	0.6	0.1	21.4	
	IX. . .	3.5	0.9	0.9	0.2	13.4	3.25	1.4	0.3	0.2	17.9	
	XII. . .	3.1	0.7	0.5	0.5	15.1	2.9	1.5	0.25	0.4	19.9	
St. 12	I. . .	2.75	0.2	1.6	0.8	10.6	2.3	4.9	3.6	1.2	21.9	
	III. . .	2.25	0.4	1.7	1.7	9.5	5.0	4.5	4.7	1.2	23.8	
	VI. . .	2.1	1.1	1.1	1.1	10.5	5.4	4.5	4.1	1.5	24.9	
	IX. . .	2.1	1.4	1.4	1.3	9.0	7.1	4.1	3.9	1.5	24.75	
	XII. . .	2.8	0.7	1.3	1.7	9.2	5.5	3.9	3.6	1.2	22.5	
Sn. 12	I. . .	1.4	0.9	0.5	0.7	18.5	4.25	1.6	0.25	0.3	24.6	
	III. . .	1.5	2.25	0.25	0.6	15.1	7.9	1.75	0.2	0.5	25.1	
	VI. . .	1.6	2.3	0.4	0.9	13.2	9.0	2.1	0.8	0.4	25.2	
	IX. . .	2.6	2.1	0.5	0.4	13.0	7.8	1.2	0.25	0.2	22.25	
	XII. . .	2.25	2.2	0.4	0.9	12.7	8.3	1.3	0.4	0.3	22.75	
R. 12	I. . .	2.2	0.0	1.4	0.75	14.7	1.5	3.75	0.9	0.7	20.8	
	III. . .	3.75	0.4	1.25	0.6	11.7	2.7	1.9	0.7	0.4	17.2	
	VI. . .	4.25	0.3	1.3	0.6	10.75	2.2	1.3	1.5	0.25	15.9	
	IX. . .	4.3	0.4	1.25	0.2	11.0	1.6	1.8	1.4	0.25	16.2	
	XII. . .	6.1	0.2	1.3	0.4	6.1	1.3	1.2	1.3	0.3	9.9	

[1] The letters in the first vertical column represent the names of the reagents, while the figures give the total number of experiments made at each stage. It may be mentioned that Ca. made 16 instead of 18 experiments with stage VI.

B. Association. (II.)

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Experimental investigation may best supplement the purely introspective study of the nature of association by describing in relatively concrete terms the probable direction of trains of associated images. To this end there is necessary such a consideration of the so-called suggestibility of objects of consciousness as shall answer the question: what one of the numberless images which might conceivably follow upon the present percept or image will actually be associated with it?

Ordinary self-observation has long recognized that the readily associated objects are the 'interesting' ones, and has further enumerated frequency, recency, vividness or impressiveness, and primacy (the earliest position in a definite series of events) as the factors of interest, and therefore the conditions of association. A given object, then, is likely to be suggested by one with which it was frequently, recently or vividly connected, and by one with which it stood at the beginning of a series.

Logically prior to the discussion of suggestibility is the study of the suggestiveness of objects of consciousness, that is, the consideration of the question: what part of the present total content of consciousness will be associated with a following image? The suggesting object may, of course, be of varied extent. In the rare cases of 'total redintegration,' practically the entire present content is connected, as a whole, with what follows. Far more often, some one accentuated part of the total object of consciousness is the starting point of the association; and this emphasis of attention is once more upon the 'interesting' part of the entire content, that is upon some vivid, recent or repeated object, or upon one which has had the early place in a series. Finally, neither the total content of consciousness, nor a single accentuated portion of that total, but a group of these single factors or objects of consciousness may form the starting point of the association.

These distinctions may be summarized, somewhat as follows:

I. Contents of consciousness are 'suggestive.'

a. As totals (Total Redintegration.)

b. As complex.

1. *Groups* of objects are suggestive (through 'constellation.')

2. *Single* portions are suggestive, through their interest, due to

(a) Repetition (Frequency.)

(b) Vividness.

(c) Recency.

(d) Primacy.

II. Objects of consciousness are 'suggestible,' through their interest, due to

a. Frequency of connection.

b. Vividness " "

c. Recency " "

d. Primacy " "

The experimental investigation whose results are here reported concerned itself with the conditions of suggestibility. The massed records of the first part of the study were published in this REVIEW, volume I, pages 476 to 483. The figures of this earlier summary are incorporated with those of the later experiments in this paper, and the account of the methods used and of certain of the conclusions reached is here in part repeated to secure completeness. All the results were twice set down, once in the books kept for the individual subjects, and again in the books which contained the grouped records of the different sorts of experiment. These experimental ledger pages have been balanced, and all the figures given in the tables represent the concurring results of both forms of record. Constant notes were kept of subjective experiences, but have not been reported, for none of them tended to modify the conclusions drawn from the experiments themselves except where the occurrence of natural associations made it necessary to reject entirely the results of particular experiments.

Experimental Investigation of the Conditions of Suggestibility.

The relative significance of frequency, recency, primacy and vividness, was studied in about 2,200 experiments. This number does not include the introductory experiments undertaken in order to select satisfactory methods nor the practice experiments of each subject. There were 17 subjects, no one of whom assisted in more than 275 nor in less than 40 experiments; and the average number was 130 for each subject. Most of the visual experiments were repeated with 40 members of the writer's Wellesley College class, with an average of 12 experiments each. The results coincide very closely with those of the more extended study in the Harvard laboratory; they are not included except in one or two instances which will be noticed. All the subjects were entirely or comparatively ignorant of the aims and the problems of the investigation, which was not discussed until the conclusion of the work.

The experiments were of two main types, visual and auditory; the visual experiments are divided again into the successive and the simultaneous; finally, all the experiments may be classed, with reference to their purpose, as simple or comparative.

I. Simple Series.

a. 1. Successive Arrangement. Visual Series.

The method already described[1] was retained throughout, except that the time was kept, in the second half of the experiments, by listening to the beats of a metronome, which rung a bell every four seconds; the metronome was enclosed in a sound-proof box, so that the subjects were not disturbed by the beats, which reached the experimenter through a rubber tube. A color was shown during four seconds, against a white background, followed by a numeral, also exposed four seconds. Each series consisted of 7, 10 or 12 such pairs of quickly succeeding color and numeral, each presentation lasting only four

[1] Psychological Review, I., p. 477.

seconds and each pair of stimuli separated from the next by an interval of eight seconds; at the close a test series was shown, made up of the colors only, in changed order, and the subjects wrote down whatever numeral, if any, was suggested by each color. The experimenter was hidden from view throughout.

In the first group of experiments, some one color appeared several times in each series, once in an unimportant position with any chance numeral, but also once or more in some emphasized connection—either repeatedly with the same numeral (a 'frequent' combination), or at the very beginning or very end of a series (cases of 'primacy' and of 'recency'), or with a numeral of unusual size or color (an instance of 'vividness').

The following are representative series:

Visual Series, 213. Vividness.

First Series: Vivid, 4. Second Series, 5.

I. Brown, 34; peacock, 65; orange, 51; *green, 792 (v);* blue, 19; violet, 48; *green, 27 (n);* grey, 36; strawberry, 87; dark red, 54.

II. Blue, grey, dark red, brown, *green* (*v*), orange, strawberry, grey, peacock.

Visual Series, 127. Recency.

I. Peacock, 46; *blue, 38 (n);* brown, 51; grey, 74; yellow, 29; *blue, 52 (r).*

II. *Grey, blue* (*r*), peacock, yellow, strawberry, brown.

The problem of the experiment is the discovery of the proportion of cases in which the accentuated color, *e. g.*, green (as in series 213, above), suggests the numeral—here 792—with which it was emphatically combined, instead of suggesting the other numeral with which also it was shown.

The later experiments, in the first place, fully corroborated the results already published. Thus the general likelihood of the recall of numerals in series of this character, leaving out of consideration all the emphasized numerals, was 26.1% in the long, 35.1% in the short series.[1] No new series were intro-

[1] Cf. for per cents. of earlier results (26.4% and 35.3%) PSYCHOLOGICAL REVIEW I., p. 479.

duced with only two occurences of the repeated numeral, since the per cent. of recall in these cases[1] had been so little above the normal; but the likelihood of associating the numeral three times repeated with a color was 63.7%, while the normal or unaccentuated numeral appeared in only 24.9% of the cases.[2] In 19.2% of all the test series, 'both' the frequent and the normal were remembered. This is easily explained when the normal comes late in the series, for the recurrence of the color, already repeated, draws attention to the following numeral, even when that is not accentuated. To eliminate this influence of position, the place of the normal in the series was constantly changed from beginning to middle and end. The table of individual records is given only for the one-fourth (or 3 of 12) frequency series; it shows that the results are not due to any misleading massing of the figures, for the preponderance of frequency associations appears for each subject. As before, the column headed 'Half' includes cases in which one digit only was recalled, and these are estimated in calculating the per cents., as half correct.

TABLE I. FREQUENCY (3:12), VISUAL.

Names.	Number of Series.	Both. Full.	Both. Half.	%	Normal only. Full.	Normal only. Half.	%	Frequent only. Full.	Frequent only. Half.	%
B.	20	4	1					5	2	
C.	24	3	1		2	3		9	1	
Ha.	13	2				2		6		
Ns.	5							3		
Pt.	22	3			1			14	1	
Shp.	6	2						2	1	
St.	17	2			1	1		12		
Lg.	11	3						5	1	
Mc.	6							3		
N.	11	3				1		2		
E.P.	6				1	2		2		
J.P.	12	1						2	2	
R.	12	3			1			5	1	
Sh.	12	8						3		
Si.	11		1		1			3	1	
So.	12	3						8		
Total,	200	37	3	(19.2%)	7	9	(5.7%)	84	10	(44.5%)

[1] cf. PSYCHOLOGICAL REVIEW, IV., p. 475

[2] Cf. for per cents. of earlier results (63.4% and 23.3%), PSYCHOLOGICAL REVIEW, I., p. 149.

The greatest difficulty of these experiments was unquestionably in the study of vividness as a condition of suggestibility. The category is a vague and elusive one, seeming to include all those forms of the interesting which cannot be referred to the repetition, the recency or the primacy of the experience. In the main, therefore, the 'vivid' is either the 'unusual,' or it is the object of instinctive, and therefore of psychologically inexplicable, interest.

TABLE II. VIVIDNESS, VISUAL.

Names.	Number of Series.	Both. Full.	Both. Half.	%	Normal Only. Full.	Normal Only. Half.	%	Vivid Only. Full.	Vivid Only. Half.	%
B.	33				1			13	6	
C.	8				2	1		4		
Ha.	39	4			2			16	3	
Mi.	42	3			9	1		12	7	
Ns.	47	6	1		1			17	2	
Lg.	10	2			1			6	1	
Lh.	35	5	1					7	8	
Mc.	43	11			5	2		10	6	
N.	9					1		1	1	
E.P.	29	4						12	7	
J.P.	10				2			1	2	
R.	11	3						4	1	
Sh.	9	3						1	2	
Si.	10	1			1	1		3	1	
So.	11	1	1		1			6		
Total,	346	43	3	(12.8%)	25	6	(8%)	113	47	(39.4%)

Thus, in close likeness to the results of the former experiments,[1] the vividly-associated numerals are remembered in about one-half (52.2%)[1] of the series, while the normal associations with the same colors are only one-fifth (20.8%)[1] of the entire number. The lessened strength of these sorts of vividness, as compared with that of the three repetitions, is shown by the greater number of cases in which neither numeral is remembered. J. P., however, is the only one of the subjects whose records, only 10 in number, show no influence at all of vividness.

The individual records in the experiments on recency[2] offer only one variation from the type, again in the case of J. P.

[1] Cf. for earlier results (48% and 20.8%) *op. cit.*, page 481.

[2] Cf. for earlier results (53.7% and 22.2%) *op. cit.*, page 480.

The last numeral is recalled in 53.7%[1] of the possible cases; the other numeral associated with the same color, only in 25.7%.[1]

TABLE III. RECENCY, VISUAL.

Names.	Number of Series.	Both. Full.	Half.	%	Normal Only. Full.	Half.	%	Recent Only. Full.	Half.	%
Hy.	4	1						1	1	
Lg.	9	2						3		
Lh.	19	3						11	1	
Mc.	27	6			1	1		8	3	
Nr.	9				1			3	2	
E.P.	18	2			3			9	1	
J.P.	18	2	1		4	1		3	1	
R.	17	2			2			8	1	
Sh.	12	4			1			4	3	
Si.	15				3	1		3		
So.	17	2			1	3		8	2	
Mi.	6				1			1	1	
B.	10	1			1	1		2	1	
Ha.	9				2			3	1	
Ns.	10	2				1		4		
Total,	200	27	1	(13.7%)	20	8	(12%)	71	18	(40%)

The influence of recency has been studied also in the series which were arranged without this particular purpose, by recording all cases in which the last numeral was correctly associated with the color on which it had followed. In these cases the likelihood of recall does not surpass that of the average numeral, though the 'recent' color was shown third in the second half-series: the recall of the recent numeral occurred only in 26.4% of 276 series. The swiftly decreasing influence of recency, well-known from such experiments as those of Ebbinghaus on memory, is thus clearly indicated: even the intervention of only two colors between the last combination of color and numeral and the reappearance of the color was sufficient to annihilate the effect of the recency.

Finally, the suggestibility of a numeral which had already appeared at the very beginning of a series was compared with that of another numeral combined with the same color midway in the series.

[1] Cf. for earlier results (53.7% and 22.2%) *op. cit.*, page 480.

TABLE IV. PRIMACY, VISUAL.

Names.	Number of Series.	Both. Full.	Both. Half.	%	Normal Only. Full.	Normal Only. Half.	%	Primacy Only. Full.	Primacy Only. Half.	%
Hy.	8				1			2		
Lg.	14				1					
Lh.	20	2			8	3		2	1	
Mc.	19	2			2	3		6	2	
Mi.	2				1	1				
N.	18	1			2			6	2	
E.P.	20	4			3	3		2	1	
J.P.	21				3	1		5		
R.	22	1			3	1		12	1	
Sh.	17	3			2	3		6	2	
Si.	17	2			1	2		4	1	
So.	22	3	2		4	1		3	2	
Total,	200	18	2	(9.5%)	31	18	(20%)	48	12	(27%)

The table shows very clearly that with long series, primacy is a significant factor only in individual cases. Thus, its influence is very marked on R.'s associations, and may be observed in the records Mc. and Sh. Lh. on the other hand associates the later numeral, that is the 'normal,' much more often, and with four of the other subjects the normal has a slight advantage. A record of cases was also kept in which the first of the series was remembered, without special competition with any other numeral, but the proportion was barely the average one in the long series; in the short series on the other hand the first numeral was associated in more than two-fifths of the cases—in 43%, that is 8% more often than the average numeral and only 8% less often than the recent.

The ineffectiveness of primacy in the long series seems at first sight to contradict the testimony of common experience and of experiment,[1] for, in committing long series to memory, the learner is certainly very apt to remember the first presentation. This difference, however, is easily explained: in memorizing the subject sets himself to learn the series as a whole, and he may not only accentuate the first presentation, but recur to it while learning the rest of the series; moreover, when he repeats the series, or records it in writing, he almost invariably gives first the earliest presentation. In the associa-

[1] Cf. the work of Dr. W. G. Smith on memory.

tion experiment on the contrary, the first presentation was always repeated toward the middle of the test-series, thus multiplying the chances that the combination would be crowded out of the memory.

2. Simultaneous Arrangement.

These general results have been amplified, and at the same time verified, by introducing series in which the connected color and numeral were simultaneously shown. This method might have been used more often, since the simultaneous combination of Stimuli is perhaps more common in ordinary experience than the successive; but the experiments of the successive type, in which the combination of color and numeral is emphasized by the long pause between each pair, were employed as affording a close comparison between the visual and the auditory series. So far, however, as these subjects are concerned, the results of the simultaneous series are so closely parallel with those of the successive ones, that no characteristic differences appear. Color and numeral were shown side by side in an opening 10x4 cm., by slipping them into double passe-partout frames, made for the purpose. Each frame held a color and a numeral separated by a narrow band of white. The intervals of exposure were six seconds, and in a few series four seconds; the pauses were usually six seconds, occasionally four seconds. In each of the three most important simple forms of the experiment, 50 tests were made. The average of recall, leaving out of account the emphasized numerals was 25.4% for the 100 long series and 30% for the 50 short series, thus falling, as has been said, slightly below the average of recall in the successive series. Moreover the percentage of emphasized numerals which were associated was slightly greater than in the successive series, because of the larger number of cases in which *both* numerals were recalled. This result, however, may be due to the greater degree of practice when these simultaneous tests were made.

The number of experiments is so small that the individual records are not given, but they are closely parallel to those of the successive series. In the table which follows, the figures for the 'half' correct, which are small, are combined with those of the fully correct, and the corresponding per cents., with those of the successive series, are added in parenthesis.

TABLE V. SIMULTANEOUS COMBINATION.

Nature of Series.	Number of Series.	Both. Sim. No.	%	Suc. %	Normal only. Sim. No.	%	Suc. %	Emphasized. Sim. No.	%	Suc. %
Freq.	50	11	22%	(19%)	1½	3%	(5.7%)	24	48%	(44.5%)
Viv.	50	15	30"	(12.8")	4½	9"	(8 ")	19	38"	(39.4 ")
Rec.	50	10	21"	(13.7")	5½	11"	(12 ")	19	38"	(40.7 ")

b. Auditory Series.

All the varieties of experiment which have so far been described, except those in primacy, were repeated with nonsense syllables and numerals, as the association-elements, both pronounced to the subjects. These series were arranged in pairs of a nonsense syllable and a numeral each, with four seconds allowed to the pronunciation of each pair, and four seconds interval both between the pairs and between the two parts of the series. One series will serve as illustration of all.

Series 335b. Vivid, Auditory.

I. Zet, 24; Kip, 62; Tox, 96; *Wez, 319 (v); Vit, 38;* Lup, 45; Nuk, 29; *Wez, 73 (n); Vab, 57; Muv, 41.*

II. Vit, Kip, Muv, Zet, *Wez*, Nuk, Lup, Vab, Tox.

The results of the experiments are generally parallel to those of the visual tests, with certain suggestive variations which will be noticed later. The general average of recall, disregarding the accentuated pairs is shown in

TABLE VI. CORRECT ASSOCIATIONS, AUDITORY.

Series.	Number of Series.	Possible Correct Associations.	Actual Correct Associations. Full.	Half.	%
Long.	254	2405	498	22	(25.3%)
Short.	100	581	118	39	(23.6%)

TABLE VII. FREQUENCY (3:12) AUDITORY.

Names.	Number of Series.	Both. Full.	Half,	%	Normal only. Full.	Half.	%	Frequent only. Full.	Half.	%
Hy.	5	1						1		
Lg.	14	8			1			3	1	
Lh.	12	3				1		5	2	
Mc.	15	9						4	2	
Nr.	14		1			1		7	2	
E.P.	14	5	1					5	2	
J.P.	14	9						2	3	
R.	15	4						7	3	
Sh.	17	8				1		6	2	
Si.	14	8						3	3	
So.	16	2				1		9	3	
Total,	150	57	2	(38%)	1	4	(2%)	52	23	(42%)

The position of the normal in the series was carefully varied, as in the visual experiments. The following table shows, however, that whatever the position of the normal, associations with the repeated numeral are much in excess, though they decrease where the normal is midway in the series so that the repetition affects it also.

TABLE VIII. FREQUENCY, AUDITORY.

Position of Normal.	Number of Series.	Both. Full.	Half.	%	Normal Only. Full.	Half.	%	Frequent Only. Full.	Half.	%
Early.	42	11		(26 %)				25	3	(63 %)
Middle.	57	26		(45.6")		3	(2.6%)	10	13	(28.9")
Late.	51	20	2	(41 ")	1	1	(3 ")	17	7	(40 ")
	150	57	2	(38 ")	1	4	(2 ")	52	23	(42 ")

Two methods of making a numeral impressive were employed. Sometimes, as in the example given, a numeral of three digits was used. At other times the emphasized numeral was read in a very loud tone. The next summary shows that both methods were effective, but that the voice-stress was a little more impressive.

TABLE IX. VIVID, AUDITORY.

Nature of Vivid.	Number of Series.	Both. Full.	Half.	%	Normal Only. Full.	Half.	%	Frequent Only. Full.	Half.	%
Digits.	97	14	3	(15.9%)	4	9	(9.7%)	26	29	(41.7%)
Loud.	103	22	1	(21.8 ")	6	6	(8.7 ")	31	7	(33.4 ")
Total,	200	36	4	(19 %)	10	15	(8.7%)	57	36	(37.5%)

The individual records show greater variation from the type than the reports of frequency-association.

TABLE X. VIVID, AUDITORY.

Names.	Number of Series.	Both. Full.	Half.	%	Normal only. Full.	Half.	%	Vivid only. Full.	Half.	%
Hy.	4							1		
Lg.	19	2				2		6	4	
Lh.	14	2	2			1		6	2	
Mc.	22	5	1		1	2		10	2	
Nr.	10					1			3	
E.P.	12	2			1			7	2	
J.P.	23	8	1		1			4	5	
R.	26	3			4	5		7	4	
Sh.	23	7				1		6	8	
Si.	20	3			3	1		2	3	
So.	27	4				2		8	3	
Total,	200	36	4	(19%)	10	15	(8.7%)	57	36	(37.5%)

The influence of the position of the normal shows itself, as in the other series, in the larger number of cases in which 'both' are remembered, when the normal comes after the vivid combination.

TABLE XI. VIVID, AUDITORY.

Position of Normal.	Number of Series.	Both. Full.	Half.	%	Normal only Full.	Half.	%	Vivid only. Full.	Half.	%
Early.	108	13		(12%)	7	4	(8%)	40	22	(46%)
Late.	92	23	4	(27 ")	3	11	(9 ")	17	14	(26 ")
Total,	200	36	4	(19%)	10	15	(8.7%)	57	36	(37.5%)

The records of the recency experiments show the very striking effect of auditory recency. There are no individual variations from the general type, and the number of cases in which the normal is remembered does not rise above one-eighth. In about half the records the 'recent' is wholly or partially remembered in every case.

TABLE XII. RECENCY, AUDITORY.

Names.	Number of Series.	Both. Full.	Both. Half.	%	Normal Only. Full.	Normal Only. Half.	%	Recent Only. Full.	Recent Only. Half.	%
Hy.	5							5		
Lg.	9	1						8		
Lh.	6							5	1	
Mc.	9	3						4	1	
N.	8							4	3	
E. P.	10							9	1	
J. P.	10	2				1		4	2	
R.	11	1			1			7	1	
Sh.	10	1						5	1	
Si.	11	2			1	1		5	2	
So.	11							10	1	
Total,	100	10		(10%)	2	2	(3%)	66	13	(72.5%)

Auditory experiments to determine the effectiveness of primacy were undertaken, but were soon discontinued because they showed from the beginning the insignificance of this factor in long series. In the short auditory series, however, as in the visual, the first position proved very important: the first numeral was associated in 38.4% of the possible cases, that is, in 14% more than the average number.

The general relations of the auditory to the visual series appear in the next table in which only per cents. are given:

XIII. COMPARISON OF VISUAL AND AUDITORY ASSOCIATIONS.

Type of Series.	Correct Ass.	Both.	Normal.	F, V or R.	Total F, V or R.	Total Normal.
F. Vis.	26%	19%	6 %	44.5%	63.5%	25 %
F. Aud.	25 "	38 "	2 "	42 "	80 "	40 "
Viv. Vis.	26 "	13 "	8 "	39.4 "	52 "	21 "
Viv. Aud.	25 "	19 "	8.7 "	37.5 "	56.5"	27.7 "
Rec. Vis.	33 "	14 "	12 "	40. "	54 "	26 "
Rec. Aud.	23 "	10 "	3 "	72.5 "	82.5"	13 "

II. COMPARATIVE SERIES.

In showing that frequency, vividness, primacy and recentness are conditions of association these experiments have so far, of course, merely substantiated ordinary observation. The real purpose of the investigation is attained only by a comparison of these factors. Already it has appeared that the per cent. of correct 'frequency' associations is slightly the largest, and that recency is the principle of the combination in the next greatest number of cases. In order, however, to carry out the comparison under like conditions, these principles of combination were compared within the same series. To this end, long 'successive' series were arranged in which the significance of frequency was contrasted with that of vividness by showing a color three times with the same two-digit numeral (f) and once with a three-digit numeral (v); others, in which the color three times shown with a numeral (f) appeared also at the first of the series with another numeral (p). Short 'successive' series were formed in which the last color (r) had appeared once before with a three-digit numeral (v), or at the very beginning of the series (p), or twice before with a repeated numeral (f).

In the following summary of results of the comparison of frequency and primacy, half the records are those of Wellesley subjects. The individual records are not given because they are few in number and show no variation. The experiments were not continued further because their result was so unmistakable verifying the conclusion already reached by the study of primacy alone, that this is evidently an unimportant feature of long series.

TABLE XIV. FREQUENCY AND PRIMACY.

Number of Series.	Both. Full.	Half.	%	Prim. Only Full.	Half.	%	Freq. Only Full.	Half.	%
80	15	2	20%	3	2	5%	44	3	56.8%

The comparison of frequency with vividness shows far less inequality, and yet there is a definite excess of correct associations with frequency. In half the cases where there was any association at all, both the frequent and the vivid numeral were recalled. The records are these:

TABLE XV. FREQUENCY AND VIVIDNESS.

Names.	Number of Series.	Both. Full.	Half.	%	Vivid Full.	Half.	%	Frequent. Full.	Half.	%
Hy.	7							2	2	
Lg.	13	8							2	
Lh.	23	15			1			3	4	
Mc.	26	12			3			6	4	
Na.	17	2			1	1		8	2	
E. P.	20	16				1		2	1	
J. P.	18	4			7			3	1	
R.	23	13			1			2	6	
Sh.	16	11				1		3		
Si.	14	4			3			6		
So.	23	6				1		9	5	
Total,	200	91		(45.5%)	16	4	(9%)	44	27	(28.7%)

This shows a total of 74.2% (28.7+45.5) of associations with the numeral frequently combined with the color presented, and 54.9% (9+45.5) of associations with the numeral vividly combined. Frequency, however, is not invariably the more determining factor: the records of E. P., Lh., and Sh. show only a small difference between 'frequent' and 'vivid' associations, while J. P. has more with the vividly combined numeral.

The greater significance of frequency of combination was brought out more strongly by lengthening and filling the interval between the half-series. After the pairs of colors and numerals had been shown to the subjects, short anecdotes or news-items, of about one hundred and fifty words were rapidly read aloud. The test series, of colors only, was then shown and the subjects tried as usual to associate the numerals. The table shows that the per cent. of association was a little lowered, but that the per cent. of frequency associations is greater than after the unfilled interlude. The frequently combined numerals seem to be more tenaciously associated. This method might with advantage have been extended to the other experiments.

TABLE XVI. FREQUENCY AND VIVIDNESS.

INFLUENCE OF FILLED INTERLUDE.

Interlude.	No. of Series.	Both. Full.	Half.	%	Viv. Only. Full.	Half.	%	Freq. Only. Full.	Half.	%
Unfilled.	89	49		(55 %)	7	1	(8.4%)	16	10	(23.6%)
Filled.	111	42		(37.8 ")	9	3	(9.4 ")	28	17	(32.8 ")
Total,	200	91		(45.5 ")	16	4	(9 ")	44	27	(28.7 ")

The influence of position in the series does not alter the general relation of frequent and vivid associations, though the greatest number of 'frequent associations only' does occur where the vivid numeral is nearest the beginning of the first half-series and so at a relative disadvantage. The greatest likelihood of remembering 'both' occurs when the vivid is near the middle of the series so that it is influenced by the repetition and itself influences the remaining repetitions. All this appears in the following table:

TABLE XVII. FREQUENCY AND VIVIDNESS.

INFLUENCE OF POSITION IN SERIES.

Position of Vivid.	Number of Series.	Both Full.	Both Half.	%	Vivid. Full.	Vivid. Half.	%	Freq. Sec. Full.	Freq. Sec. Half,	%
Early.	68	25		(36.7%)	7	1	(11 %)	20	9	(36%)
Midway.	72	42		(58.3 ")	5	1	(7.6 ")	12	5	(20 ")
Late.	60	24		(40 ")	4	2	(8.3 ")	12	13	(30.8")
Total,	200	91		(45.5 ")	16	4	(9 ")	44	27	(28.7 ")

The results of the comparison of recency with the other conditions of suggestibility is made in the three following tables:

TABLE XVIII. RECENCY AND VIVIDNESS.

Name.	Number of Series.	Both. Full.	Both. Half.	%	Vivid Only. Full.	Vivid Only. Half.	%	Rec. Only. Full.	Rec. Only. Half.	%
Hy.	5	1								
Lg.	9	6			1					
Lh.	26	6			5	7		2	3	
Mc.	22	4			7	4		2		
Mi.	10	2			2			2	1	
Nh.	10	3			4			1		
E.P.	24	13			4	1		3	1	
J.P.	17	3	1		2	1		1		
R.	17	8	1		2	3			1	
Sh.	11	6			3	2				
Si.	9							2		
So.	17	2			1	1		6	3	
B.	6				2	1				
Ha.	8	3			1	2				
Ns.	9	2			2	1		3		
Total,	200	59	2	(30%)	36	23	(23.7%)	22	9	(13.2%)

TABLE XIX. RECENCY AND FREQUENCY.

Name.	Number of Series.	Both. Full.	Both. Half,	%	Frequent Only. Full.	Frequent Only. Half.	%	Recent Only. Full.	Recent Only. Half.	%
B.	6					2				
Ha.	8	2			3	2		1		
Lg.	9	6						1		
Lh.	11	2			1	2		2	1	
Mc.	17	7			5	2			1	
Mi.	10	6			3			1		
Nr.	3	1	1					1		
Ns.	9	3			2	2		1		
E.P.	8	3			1	2		2		
J.P.	7	3			1			1	1	
R.	10	7	2					1		
Sh.	10	6			2			1		
Si.	7				2			2	1	
So.	10	4			2	1		3		
Total,	125	50	3	(41.2%)	22	13	(22.8%)	17	4	(15.2%)

TABLE XX. RECENCY AND PRIMACY.

Name.	Number of Series.	Both. Full.	Half.	%	Primacy Only. Full.	Half.	%	Recent Only. Full.	Half.	%
Ha.	4	1						1	1	
Lg.	13	6			1	1		3		
Lh.	4	2			1					
Mc.	8	2			1	1		1	2	
Mi.	4	1						3		
Na.	8	1			2	1		1		
Ns.	3	1			1					
E.P	3	2								
J.P.	4							2	1	
R.	13	4				4		2	1	
Sh.	12	2	1		1	3		3	1	
Si.	10							3	1	
So.	14	3			3	1		6		
Total,	100	25	1	(25.5%)	10	11	(15.5%)	25	7	(28.5%)

The discussion of these results will be facilitated by comparing the. per cents. of the total number of the recent and of the contrasted associations in the different cases:

	RECENT ASSOC. %	CONTRASTED ASSOC. %
Rec. and Viv.	43.2%	(v) 53.7%
Rec. and Freq.	56.2 "	(F) 64 "
Rec. and Prim.	54 "	(P) 41 "

It appears that in this direct competition recency yields both to frequency and to vividness as a condition of suggestibility. The vivid numeral seems even to suppress the recent, for in the recent-vivid series the recent is recalled 10% less often than in the series where the recent is compared with an ordinary numeral (See Table VI.). On the other hand, the effect of recency is as usual, to raise the likelihood of the recall of the contrasted numeral, but not to the level of the frequent associations.

The associations with the first numeral of the series are decidedly less than those with the recent, though far more numerous than in the longer series. Individual differences, however, are to be noticed here, and would doubtless appear more strongly in a larger number of experiments; they may also be observed in a few records of the other short series, as in that of So., who has few vivid, and many recent, associations.

From this mass of figures a few conclusions emerge into prominence. Some of these have been already formulated, but the more important ones may be briefly stated again:

In these experiments frequency has been the most constant condition of suggestibility. The proportion of the frequent as compared with the normal associations is one-tenth greater than that of the vivid or of the recent. When directly compared with the vivid and the recent the proportion is still greater, though the number of associations of the contrasted numeral is larger than that of the associations with an ordinary one, because of the tendency of the repetition to accentuate the compared factor.

This significance of frequency is rather surprising. For though everybody recognizes the importance of repetition in forming associations, we are yet more accustomed to 'account for' these by referring to recent or to impressive combinations. The possibility that the prominence of frequency in our results is not fairly representative of ordinary trains of association is strengthened by the fact that it is contrasted with forms of vividness which are only two or three of many, and which do not approach the impressiveness, for instance, of richly emotional experiences. But this does not affect the importance of frequency as a corrective influence. Granted a sufficient number of repetitions, it seems possible to supplement, if not actually to supplant, associations which have been formed through impressive or through recent experiences. Moreover, the trustworthiness of the ordinary observation, which relegates frequency to a comparatively unimportant place among the factors of suggestibility, may be seriously questioned: I have found many cases, during experiments in free association in which the subject, asked to explain the association, does not always mention repetition, even when it has obviously occurred, but seems, as it were, to take it for granted. The prominence of frequency is of course of grave importance, for it means the possibility of exercising some control over the life of the imagination and of definitely combating harmful or troublesome associations.

None of our generalized totals, it must be added, are proof against the caprice of the individual, who may have his own favorite type of association which resists opposition. So the preference of one of our subjects—So.—for the recent may be traced through almost all the series, often in contradiction of the general result.

C. The Saturation of Colors.

By L. M. Solomons.

The experiments of which a provisional account is given here were the outgrowth of an effort to determine whether least perceptible differences of color saturation obeyed Weber's law, and though they have branched out into the wider field of the general relation of white and black to the colors they are still best presented from this point of view.

In any color mixture we may distinguish two kinds of intensity: the intensity of coloring, and general light intensity. For example, if we take a red disk and compare it with a color wheel containing a large amount of white and a little red, the merest novice at color judgments will, say that the red disk is the more intense red, while the wheel possesses greater general light intensity. To the former element, the intensity of coloration, we give the name *saturation*, reserving intensity for general light intensity.

Now, if in a color wheel we increase the amount of color, we change in general both the saturation and the intensity. Therefore in determining least perceptible differences (which will hereafter be denoted by the abbreviation L. P. D.) it is necessary to make sure that we are not judging by intensity. Our first plan was to mix the color—red—with a gray of the same intensity, so that increasing the red decreased the white, thus keeping the intensity constant. These experiments gave no very satisfactory results, though the failure to obey Weber's law was manifest. The reason soon became clear. When we increase the red we decrease the white. Now to assume that the saturation increment is measured by the increase of red is to assume that the saturation of a mixture depends only on the amount of color, and not at all upon the amount of white. This is not true.

If we take two color wheels putting in one, say 180° red and 180 black, and in the other 180° red and 180 white, the former appears very much more saturated than the latter, though the actual amount of red is the same. With such large differences as in the above example the difference of saturations is obvious to anyone. But to compare two mixtures of very different intensity with regard to their saturation, with any degree of accuracy, seems at first almost a hopeless task. But with a little practice, beginning with large differences and working down, the judgment becomes quite possible, and eventually exceedingly accurate. Owing to the training required the experiments were made only by Miss Stein and the writer.

The result of a long series of observations showed that the saturation of a mixture of color and white is entirely independent of the intensity, and of the actual quantity of color, and depends only on the ratio of the color to the white. The law is perfectly obeyed within the limits of experimental error (a few degrees). The equality point was always determined by the method of least observable difference, though it was not long before the judgment of the equality point became more accurate than in most judgments, being nearly always placed in the same position, for movements in both directions. The colors used were red and blue. The teleological significance of the law is obvious. It enables us to identify objects in varying light intensity. The characteristic of a colored object is the proportion of the colored light to the white light that it reflects. The actual quantity of colored light depends upon the intensity of the incident light. It is therefore of the greatest importance for the recognition of objects that the intensity of coloration should depend upon the ratio of colored light to white, and not upon the actual quantity of colored light.

Meantime a series of measurements of L. P. D. made out the following facts: For a constant saturation the L. P. D. is constant measured in terms of actual amount of color added, that is, if in a mixture of 50° white and 50° red the red must be increased by 4° to give a L. P. D; in a mixture of 100° red and 100° white, the red must also be increased by 4°; secondly, the L. P. D. increases with the saturation. To find out the exact law of increase it is necessary to have a measure of saturation.

By direct observation we only determine when two saturations are equal. Now the law that they are equal when the ratios of the color to the white are equal admits of more than one interpretation. For when the ratios of color to white are equal the ratios of color to white + color or any proportion thereof, as white + ½ color, are also equal. Calling S the saturation, we have the general formula $S = \frac{c}{w+ac}$ satisfying the law of equality of saturation for all values of a. We have seen that for constant saturation the saturation increment for a L. P. D. varied inversely as the intensity—for the actual color increment being constant, the saturation increment corresponding to it will vary inversely as the total quantity of light. Assuming it to vary directly as the saturation, we should have the formula $\Delta S \left[= \frac{\Delta c}{W+ac} \right] \propto \frac{c}{W+ac} \frac{1}{I}$, I being the intensity, that is, the actual increment of color, Δc, varies directly as the ratio of color to intensity. Since the result is independent of the quantity W+ac it might seem preferable to give the law the simple, verifiable formulation $\Delta c \propto \frac{c}{I}$, and from a physical standpoint this would of course be preferable. But psychologically it is bad because the quantity c has no psychological equivalent. The psychical fact, intensity of coloration, depends upon a physical ratio $\frac{c}{W+ac}$. If we are to keep to psychical facts we must use the quantities saturation and intensity. Remembering therefore that $\Delta c \propto \frac{c}{I}$ is the best expression of the observed physical fact it is yet well, I think, to retain the somewhat hypothetical formula $\Delta S \propto \frac{S}{I}$, as more suggestive from the psychological point of view.

As to the accuracy with which the law $\Delta c \propto \frac{c}{I}$ is obeyed, many difficulties have arisen in the effort to fully verify it. Several very short series of observations have obeyed it within the limits of experimental error. In attempting to get long series of observations it was found that owing to the constant increase of skill in the subject, as well as other causes of variation, the different parts of the series are not strictly comparable. By planning the series with these facts in view, however, accurate results may I think be obtained.

The above L. P. D. law contains two anomalies which require investigation. The first is that though the saturation increment varies inversely as the intensity when the change is produced by increasing the proportion of color and white in the wheel, it is not affected, at least not to any easily observable extent, by variations in the intensity of the incident light. The other is that if we adopt the usual conception of a L. P. D., viz., that it represents a simple increment of sensation, the L. P. D. law contradicts the saturation law. For if we call the sensation of saturation S and the physical quantity corresponding to it $\left(\frac{c}{W+ae}\right)$ s, we have the law $dS = \frac{ds}{s} I$

By integration this formula gives $S=I \log s$, which contradicts the saturation law that S depends on s only and is independent of I. A similar contradiction exists in the other formulation of the law.

The explanation of the above brings up two questions. What is the general relation between intensity and color quality, and what is the real significance of a L. P. D.?

A number of experiments were carried out in connection with the former problem, most of which have no immediate bearing on the subject in hand. I wish to describe only one series, the results of which are important. An apparatus was arranged, whereby two color wheels were placed in lights of different intensities. The arrangement was a very simple one, the wheels being placed opposite a window divided into two portions by a vertical board. By placing a screen between the two wheels perpendicular to their plane and that of the window, each wheel received light only from its own side of the window. The subject sat in front of the board dividing the window and had both wheels well in view. By varying the size of the openings the light could be varied at pleasure.

Place a white disk in a weak light, and a black and white in a strong light. It is not possible, by varying the proportion of black and white in the well-lit disk to get the two to look alike. It is possible to get them of the same general light intensity, or of the same shade of gray, but not both together. When the light intensity is the same the well-lit disk is a very dark gray and the other a white, dimly seen. When of the same shade, the well-lit disk is very much more intense. It is the same with colors. A blue disk is seen distinctly as a pure blue, even when the light is so feeble as to make it scarcely visible, while a blue and black disk appears a dark navy blue, no matter how strong the light. There is much individual difference here. A white disk in weak light appeared much more like a gray to Miss Stein than to me, but in no way could either of us get equality between the strong and weak light wheels. It should perhaps be stated that these experiments were first carried out with the object of really securing such an equality, and our inability to do so was a serious inconvenience; so that the result was anything but desired by us. We made every effort to see the disks alike.

If, however, we look at the disks through black tubes, held to the eye so as to shut out everything else from the field of view, there is no trouble about perfect equality. The white disk in dim light looks gray, the blue, navy blue, etc.

The conclusions are obvious. Intensity as such does not affect color quality *at all.* It remains a separate and distinct element in every color presentation. Blackness cannot be regarded as the inverse of intensity, nor as a sensational element at all. For it depends not upon the character of the light coming from the given body, but upon its relation to the immediate field of view. It must be regarded as an element added to every presentation by some reflex process, and giving the relation of the object to its immediate field of view—or to the incident light. It is not a mere question of comparison with other objects, for in all the above experiments there were two objects seen, yet the most intense disk was also the blackest. Nor was it simply a question of seeing objects 'as we know them to be,' instead of as they appear. For in our efforts to obtain equality all sorts of variations were made in the proportion of black and white and color in the two disks, of which the subject was unaware; yet it was not possible to get equality as long as the two disks were seen in different backgrounds. The teleological significance of the law is obvious. It makes blackness a 'body property,' independent of the intensity of the illumination.

This compels us to adopt a four-fold, instead of the usual three-fold, representations of colored objects. They can vary in four independent ways: 1. color quality, or tone; 2. saturation;

3. intensity; 4. blackness. Any one of these may be made to vary while the others remain constant. This is a purely psychological classification of course, giving the different subjective effects which a colored object produces. That color quality may vary, the other elements remaining constant, is clear theoretically, though to actually compare the saturation of different colors is difficult. Saturation may be made to vary independently by simply changing the proportion of color to white, while keeping the sum of their intensities constant. Intensity by simply increasing the incident light, and blackness by increasing the incident light and at the same time decreasing the amount of color and white in the disk so as to keep the intensity constant, while its relation to the intensity of the field changes. When the saturation of any color becomes zero we call it a gray, and grays may vary in intensity and blackness. The above four elements, and no fewer, completely describe any color combination. White is not given explicitly, but saturation and intensity together determine the amount of white, if whiteness is different from intensity, so that the above formulation is entirely independent of all special color theories. The general result of all this, it will be noticed, is to accentuate the subjective aspect of color theory.

We can now understand the law of L. P. D. of saturation. Consider for a moment the process of making a judgment of saturation. Suppose we have one disk of 40° red and 20° white, and another 120° red and 60° white. The only difference between them is in blackness and intensity. The intensity, however, is easily abstracted from. It does not 'fuse' into the general presentation but remains as a fairly distinct element. The black, however, is an organic part of the percept bound up with the rest. The process of perceiving the two disks to be equal is abstracting from the black element. Once able to separate that, and they are seen equal. The training required for judging saturation is simply the training in isolating the black element in a color presentation. Our experience amply confirms this theoretical deduction. This has actually been the difficulty encountered in making judgments, and our records are full of such notes as 'judgment uncertain on account of inability to separate black'—notes taken, it should be stated long before their theoretical significance was suspected.

It is clear now why the L. P. D. varies inversely as the quantity of color and white in the disk, but not as the intensity of the incident light. Changing the intensity in the first way changes the blackness, while changing it in the second way does not. The law should really be stated $\Delta S \propto SB$ where B is blackness. If we regard the L. P. D. as measuring primarily the ease or difficulty of a judgment, then we can understand why it varies directly as the amount of black. The process of isolating the black becomes the more difficult as the amount of black becomes greater—as the black becomes a more prominent feature of the presentation. The ordinary conception of a L. P. D. leads to a contradiction in the case of saturation because we have not here the simple case of comparing two quantities; but there is another process to be gone through in addition to the primary judgment—the isolation of the black. It is necessary therefore to go back to the primary significance of a L. P. D. in order to properly understand the law.

We have begun a series of experiments on the effect of tiring on saturation. The results are very encouraging, but as yet too few to permit of much theorizing. On tiring with white, the saturation of a color is increased by a constant proportion of its value for the same time of tiring. The increase seems to be proportional to the time of tiring for the times tried—5 to 15 seconds—but the experiments have not gone far enough yet to give more than provisional results.

D. Fluctuations of the Attention (I.).

By J. B. Hylan.

The facts of the oscillation of feeble impressions are still under discussion. The alternate increase and decrease of weak sensations may be of peripheral or of central origin; the peripheral sources may be nervous or muscular, the central process may go on in the cortical end apparatus of the sensory nerves or in that psycho-physical system which we call attention. It is clear that only in the last of these four cases is the fluctuation really fluctuation of the attention, the name usually given to the phenomenon. There is no doubt but that the discussion has so far been based on a rather small number of facts; it was the purpose of my investigation to secure more experimental results which might throw light on the question. I used optical, tactual and thermal stimuli. The subjects were Messrs. Singer, Hooper, Gehring, Logan, Rice, Hart, J. Pierce, Miss Stein, Miss Shipman, Miss Miles and the writer. The fluctuations were registered by small finger movements of the subjects and measured in fifths of a second.

We began for practice with the grey circle of Masson's disc. The results were as usual; the fluctuations are irregular, but the average of the periods during which the circle is seen and the average of the time during which it is not seen balance each other for most of the subjects. There was no conscious strain upon the eyes; to the one observer the disc seemed to move to and from the eyes and the eyes seemed to fail rather than the circle to disappear. Our new experiments were made with dark grey spots as a background instead of the circle and the first question was as to how the oscillations vary if several spots are in the field of vision.

A square of black cardboard had in the center a dark gray spot 2 mm. across, and both 10 cm. above and 10 cm. below this were other similar spots. The subject was placed at a distance of about 120 cm. from the square, at which distance the two outer spots were just visible. The line of vision was directed to the middle spot, which never disappeared, and the fluctuations of the two others were registered independently of each other. Each subject received at first some training in the attentive observation of the indirect field of vision. In the first group of experiments the fluctuations of the upper or of the lower spot alone were examined and no attention was given to the other. The result was that the oscillations of the spot below the center were slower than those of the spot above, and the periods of appearance show a clear preponderance over the periods of disappearance for the lower spot, while they balance each other for the upper spot. With three subjects the lower spot would disappear and then come back immediately. In the second group the attention was divided between the upper and lower spots without any intentional fluctuation between them. While in the first group the fluctuation of the lower spot was slower, here the periods of fluctuation for both spots coincide; for instance, with one observer, Hr., the periods were: "Both seen 7 sec., both unseen 3, both seen 7, both unseen 1, both seen 2, both unseen 5, both seen 3, both unseen 3, both seen 4, both unseen 7." But there is also here a difference between the upper and the lower spot; the lower tends to remain longer in view. The entire oscillation is equally long for both, but the proportion between the seen and the unseen part is often different; the lower often disappears later and appears earlier; the time difference is mostly too short to be registered. No difference as to the duration of the disappearance of the spots was discovered whether one or two eyes were used, but some subjects noticed a tendency to see the fluctuations of the two spots somewhat independently of each other when one eye only was used. In the next group one gray spot 10 cm. to the right and one 10 cm. to the left of the center were added to those above and below. The constant result was that the fluctuations become more independent; the four points might disappear together, but often some are visible and others invisible. The upper spot always disappeared first, then the lower and then the horizontal ones; the time of disappearance is in the same order, being longest for the upper. If one eye only was used, the time of disappearance in general increases, the upper spot often dropping out altogether; the right and left spots fluctuate more readily with one eye than with two.

Going back to the two spots, only one above and one below the center, we studied the influence of an intentional variation of attention. The object was to fix the eyes continually on the center, but to direct the attention alternately to the one or the other spot seen in indirect vision. With some practice all learned to alter the attention without moving the eyes. As the attention changed, some had a feeling of muscular movement in the head. When the attention changed, some (Sr., J. P., H., M.) usually noticed that the spot grew gradually brighter for some seconds, but with others it grew dimmer or remained unchanged. With all the observers the spot to which the attention was changed disappeared from vision after a short time, while the object from which the attention shifted often remained bright and with some (R., G., P.) it grew even brighter when the change took place. If the one spot disappears from view in spite of the subject's special effort to see it, and the other grows brighter when the attention is consciously diminished, we have probably no right to call the oscillations of intensity fluctuations of attention.

It is a question whether, perhaps, the muscles directing the movement of the eye might become fatigued, so that a drooping of the eye would bring a fresh part of the retina in range after the spot had disappeared. This suggested the following experiment. A heavy black cross was placed upon a white background, with a small gray spot beneath it. After the cross had been looked at a moment, an after-image was seen on any part of the background to which the eyes were turned. The subject fixed his eyes upon the center of the black cross, marked by a small gray spot. If the drooping of the eye causes the disappearance or reappearance, it is evident that an after-image of the cross will extend over the outlines of the figure in the direction of the movement. The results were uniform for all subjects. It was observed that while the eyes remained fixed, and the attention wandered to different parts of the cross, the luminous after-image would shift a little towards the part to which the attention was directed, though no conscious change of the eye's position took place. After looking at the cross for a little time, the after-image seemed to be placed behind the cross and to be seen all around it. The gray spot fluctuated, though the after-image did not shift with the fluctuations. Sometimes the fluctuations took place when the after-image was shifting or had slightly shifted, but oftener when the after-image was concentric with the cross. This seems to show that there is no relation between the fluctuations of the gray spot and unconscious movements of the eye. The results of experiments with regard to the inner muscles of the eye were not quite so uniform. One spot was fixed on a vertical glass plate, the other spot on a card 50 cm. behind it; if the eyes accommodate for one, it is distinctly seen, while the other is blurred. The results show that with all subjects there is a tendency to alternate between the accommodation for the nearer and the farther spot, but with most subjects this fluctuation is much slower than the oscillation of one spot alone, and especially the oscillation of the spot on the cardboard was often observed without corresponding variation in the distinctness of the spot on the glass; the one might be visible while the other was distinct, and might become invisible while the other was blurred. The oscillation seems consequently to be independent of the ciliary muscles, a conclusion which earlier experiments had already suggested, and which results also from our experiments with four spots which were in the same plane, and did not necessarily disappear together.

The second research had to do with touch and temperature. As here the same object gives at the same place both a touch sensation and a cold sensation, their fluctuation and their relations must be suggestive for the understanding of the process. To the beam of a balance weighing two-tenths of a gram was attached a metal tube for studying cold and hot spots. The tube allowed a current of water supplied by rubber tubing to pass continually through it, thus keeping the temperature constant. The point of the tube applied to the skin was about 1 mm. in diameter. Light flexible tubing was used which allowed the balance to move freely. The temperature of the water both before and after passing through the tubing was taken and the average used for the temperature of the point. The water was carried through the tube on the principle of a siphon. The removal of weights from the opposite pan of the balance gives a known pressure of the point upon the subject's hand. As the hands could not be moved, the signals were given by spoken words which the experimenter registered, the time lost by the reaction being of no importance compared with the long periods in question.

In passing the cold point over the hand, all the subjects found three distinct effects according to the location of the point. Some spots were entirely insensible to cold. Passing from that a moderately cold spot would often appear from which the sensation would be dull and not definitely located beneath the point.

Next would come a cold spot from which the sensation would be intense and definitely located beneath the point. After a long application of the cold point for several minutes the cold sensation often spreads around the point of application and sometimes streams up and down the hand and arm. Sometimes the cold sensation is located after a few minutes at a distance of 2 to 3 cm. from the point, usually coming back after a time. To the measurements of fluctuations that follow, acute cold spots only were taken, which were marked and tested as cold spots on several days. The back of the hand or wrist was always used.

It was first found that a less pressure of a cold object was necessary to give a cold sensation than a touch sensation. With ice water of 2–3°C. the cold sensation came out strongly under a pressure of 0.2 gr., while a touch sensation was not perceived unless the pressure was 0.5 gr., for several subjects 0.8–1.0 gr. In the first group we studied cold sensation only, using water of 2°C. with a pressure of 0.2 gr.; none of the subjects had any touch sensations. With two subjects no fluctuations were observed, as the cold sensation after its final disappearance did not return for five minutes or longer. The others felt the oscillations distinctly, for instance: Nr. cold sensation unfelt 34 sec., felt 20 sec., unfelt 23, felt 18.4, unfelt 15, felt 7, unfelt 8.6, felt 17, unfelt 29.2, felt 22.4, unfelt 6, felt 11; on another day with Hn. unfelt 46 sec., felt 30, unfelt 9, felt 14, unfelt 30, felt 24, unfelt 6.5, felt 22.4, unfelt 7.5, felt 13.5. With some subjects the fluctuations were quicker, for instance: Ht. unfelt 9 sec., felt 3, unfelt 2, felt 4.5, unfelt 4, felt 25, unfelt 50, felt 9, unfelt 2, felt 6, etc. The sensations of cold are much stronger when the point is first applied than afterwards. The feeling grows gradually less, often changing into a dull ache before disappearing. When it appears again it is mostly less intense than at first, though it sometimes gradually increases again to a high degree of intensity. As a rule there is a gradual decrease of intensity as the sensations successively return. The times during which the cold is not felt are not the only fluctuations; very often while the cold is being felt, it fluctuates in its intensity.

When the pressure was as much as 1 gr. all the subjects felt a tactual sensation in addition to the temperature sensation, and with all subjects the two fluctuated independently. While with the higher pressure some of the subjects got no continuous fluctuation, as the sensation did not return after the first or second disappearance; here there was practically no limit to each series. A typical series would be: 2°C. 1 gr. pressure. At first cold and touch then after 45 sec. cold disappears while touch remains, 23 sec. later cold appears again, 42 sec. later cold disappears, 20 later cold appears, 21 later cold disappears, 54 later touch also disappears, 81 later cold appears again, 59 later touch appears again, 106 later cold disappears, 15 later touch disappears, etc. A prolonged series may be characterized by the following case (a=appears, d=disappears, t=touch, c=cold): Hn. 2°C. 1 gr. pressure, c. t.—7 sec. d: c. —5 sec. a: c.—6 sec. d: c.—42 sec. a: c.—13 sec. d: c.—64 sec. d: t.—17 sec. a: c.—11 sec. a: t.—7.5 sec. d: c.— 9 sec. d: t.—8.5 sec. a: t.—21.5 sec. a. c.—2.5 sec. d: t.—6 sec. d: c.—2 sec. a: t.—14 sec. d: t.—2.5 sec. a: c.—22.5 sec. d: c. 1 sec. a: t.—7 sec. a: c.—11 sec. d: c.—2.5 sec. d: t.—6 sec. a: c.—15 sec. d: c.—61.5 sec. a: t.—8 sec. d: t.—31.5 sec. a: c.—2 sec. a: t.—6.5 sec. d: c.—27.5 sec. a: c.—12 sec. d: c.—19 sec. a: c.—5 sec. d: t.—1 sec. d: c.—15.5 sec. a: t.

The same experiments were made with a pressure of 3 gr. and of 5 gr. The general type of the results was the same, but a curve representing the average of all subjects, and separating the touch fluctuations from the cold fluctuations, shows distinctly that the periods of disappearance for the touch sensation increase with increasing pressure. The stronger the pressure, the greater the tendency of the pauses to exceed the periods of sensation. This is contrary to the results with light and, as we shall see, also contrary to the results with temperature. It may be explained, perhaps, by the fact that an unusual pressure keeps the blood away from the place of contact and brings on numbness, but it is difficult to see why that numbness disappears again. The two kinds of fluctuations tend unmistakably to be independent of each other, but a constant law of the disappearance and reappearance cannot be formulated, as the fluctuation of the temperature sensation especially seems dependent upon a great variety of conditions. The central psychophysical control of the peripheral blood supply seems an important factor among others. The position of the hand and the temperature of the room was of course kept as constant as possible.

In the next group of experiments the pressure remained unvaried, always 2 gr., but the temperature changed between 2° C. and 18° C., producing, therefore, cold sensation of different intensity, but the sensations, at least at first, were always of cold. The individual results show great differences and irregularities, but the average of all experiments with all subjects shows a distinct tendency for the intervals without sensation to become longer as the temperature rises. In general with 2° C. the duration of the sensations and of the pauses are nearly equal. With 10–12° C. the pauses are more than twice as long, and with 18° C. three to four times as long as the period of sensation. In some experiments, however, the temperature seemed to have no influence at all on the length of the fluctuations. It might be asked whether the cold sensations did not interfere with the touch sensation, so as to produce an abnormal result. Experiments were made, therefore, with the temperature of the water too high to give a cold sensation or in spots which are not sensitive to cold, but nothing in the results indicated that the cold tended to interfere with the fluctuations of the touch sensation.

In experiments to be described later two tubes were used, suspended from the beam of the balance, giving two cold sensations and two touch sensations at the same time, or cold and hot sensations together.

DISCUSSION AND REPORTS.

PHYSICAL PAIN AND PAIN NERVES.

My article in the July number of this Review on 'The Psychology of Pain' has had the good fortune to elicit discussion from Dr. Nichols in the September number and from Dr. Marshall in the November number, and I trust I may now be permitted to say a few words in reply to their criticisms.

My thanks are due to Dr. Nichols for making it plain that the conditions known as analgesia and hyperthermalgesia may be explained on a different hypothesis from that advocated in my article; on the hypothesis, namely, that the skin possesses, in addition to the ordinary nerves of touch and temperature, three distinct sets of pain nerves, one for tactile pains, another for heat pains, and a third for cold pains. Dr. Nichols is better acquainted than I with the literature of the subject, and will know whether the hypothesis of pain nerves commonly takes this form. The hypothesis I had in mind was that of a single set of pain nerves, excitable indifferently by all kinds of painful stimuli; and I think it will be admitted that the fact that tactile pains and temperature pains may be exaggerated or lost independently has the appearance of disposing of this hypothesis. At least I find a careful physiologist like Foster arguing without hesitation that, where sensations are lost independently, the impulses must proceed by separate paths. On this principle, the alternate loss of tactile pains and temperature pains would oblige us to choose between three sets of pain nerves and no pain nerves at all. I am gratified to gather from Dr. Nichols' remarks that he agrees with me on this point, and frankly assumes not one but three sets of cutaneous pain fibres.

As between this modified view and the 'shunt theory' of Wundt, it seems to me that the arguments are not so clearly in favor of the former as Dr. Nichols would have us suppose. In the first place, the occurrence of pains unconnected with tactile and temperature sensations is just as explicable on the Wundtian theory as on that of Dr.

Nichols. In pathological cases the path of the moderate impulses is presumably blocked by lesion; in normal cases these impulses may reach the brain, but be lost in the tumult of the excessive impulses: in neither case could they produce their usual effect in consciousness.

But Dr. Nichols objects to Wundt's theory on the ground of "its demand for a much more complicated and duplex arrangement of our sensory nervous systems—cranial as well as cord—than present anatomy gives any suggestion of." This is a most unfortunate objection in the mouth of Dr. Nichols, since it applies with even greater force to his own theory. The two theories agree in assuming six distinct or partially distinct paths in the cord—for touch, heat, cold, tactile pains, heat pains, and cold pains—the partial anæsthesias not being explicable as due to blocking on the assumption of any smaller number. But on the Wundtian theory the three pain paths may be partially distinct without being distinct throughout; the grey matter may form a common path for pain impulses, and the different kinds be distinct only at their entrance points; while on Dr. Nichols' theory they must be distinct throughout. So that in the most unfavorable case the Wundtian theory assumes no greater complexity in the cord; whereas the theory of Dr. Nichols assumes double the complexity in the peripheral nerves.

When therefore Dr. Nichols speaks of his own as 'a very simple theory,' it is evident that the word 'simple' is to be taken in the sense of 'comprehensible,' not in that of 'economical.' That Nature should have provided for our protection against injury by equipping us with a special set of pain nerves seemed plausible enough; but if it should turn out that she has supplied us with three such sets (not to mention special nerves for muscular pains, colics, toothaches, etc.), the discovery would be calculated to enlarge somewhat our notions of her beneficence. Meanwhile I see nothing in the facts to compel our assent to so prodigal an hypothesis.

Mr. Marshall, not alive to the advantages of the Wundtian theory, had explained analgesia and the 'lateness of pain' as due to the retarding or blocking of the impulses of a fourth cutaneous sense, additional to those of touch, heat and cold. I pointed out that on this theory the affective coloring of touch, heat and cold can never amount to positive pain, and that in a painful burn the pain and the heat must be called forth by different nerve fibres. To this deduction Mr. Marshall demurs, suggesting that the anæsthesia and analgesia of his 'cutting-pricking sense' may be accompanied by analgesia without anæsthesia of the other three senses. I reply that this rather arbitrary suggestion deprives the fourth sense of the theoretical value which was Mr. Marshall's original ground for assuming it. If the three other senses have, to use his own phrase, any 'pain-giving capacities' at all, then abolition of the fourth sense will not explain analgesia, and the introduction of such a sense is an unnecessary complication of the problem. But, as I have already said, the fact that tactile and temperature pains may be separately lost seems to disprove the view that pain is the exclusive function of a fourth sense.

Turning now to the introspective question, Mr. Marshall thinks that many of my objections to the 'aspect theory' do not touch the 'quale theory' which he advocates. The 'quale theory,' as I understand him, recognizes no such duality within the mental state as would justify our speaking of two aspects. The hedonic coloring is a mere attribute, or *dimension*, of the tactile or temperature sensation (I hope I give his idea correctly), not a new content additional thereto. And yet, in discussing the 'lateness of pain,' Mr. Marshall does not hesitate to speak of 'a certain sensation other than the pain to which this pain belongs' (*Pain, Pleasure and Æsthetics*, p. 18). So that even on Mr. Marshall's view the relation is not such as to forbid our inquiring: (1) whether the conjunction of the pleasure or pain with the sensation or other cognitive element is a necessary one, in such wise that we can never have sensations uncolored by pleasure or pain, and never have pure pains, that is, pains unattached to sensations or other cognitive elements; (2) whether the pleasure or pain is rightly conceived as an attribute of the sensation, analogous to intensity. The affirmative answer to the first question is what I have called the 'aspect theory;' the affirmative answer to the second question is the theory of feeling-tone. Now Mr. Marshall holds that pleasure or pain is an attribute like intensity. He also holds that we never have pure pains. And he repeatedly asserts that either pleasure or pain 'must . . . belong to every element of consciousness' (Pain, Pleasure and Æsthetics, pp. 3, 45, 47); though this does not prevent his admitting that 'there are cases where it must be supposed that neither pleasure nor pain exists' (PSYCH. REV., Nov. 1895, p. 597). Mr. Marshall thus subscribes to the theory of feeling-tone, and if he escapes being a thorough-going subscriber to the aspect theory, it is only by inconsistency and self-contradiction. I therefore cannot help thinking that, so far as my arguments against these theories have cogency at all, they are equally destructive of his 'quale theory.'

Whether we ever have indifferent sensations and pure pains is, of course, a question of fact. But, granting that we do, the gist of my argument was, that an attribute which may modify its subject but need not, and which may exist by itself and in that case has an intensity of its own, is not an attribute, but a separate sensation. Now the existence of indifferent sensations Mr. Marshall inconsistently admits. And his explanation of pure pains—of the fact that "in cases of extreme pain we usually fail to distinguish the forms of sensibility to which the pain is attached"—is in my opinion so artificial as practically to surrender the case. This fact he explains as a phenomenon of attention. He says that, just as an experimental psychologist may become so absorbed in attending to the intensity of a sensation as to lose appreciation of its quality, so in this case we fail to distinguish the sensations because our attention is wrapped up in the pain. I should reply by denying that the experimental psychologist can perform any such feat. If it is a real intensity to which he is attending, and not a mere thought about intensity—if, for instance, he is trying to decide which is the louder of two tones—surely he must keep the qualities sensationally present in order to do so, as much as if he were deciding as to the comparative length of two lines. Whereas no amount of introspective search on the part of the sufferer from toothache suffices to discover a sensational quality connected with the pain. If the predominance of the pain were a phenomenon of attention, we ought to be able to turn our attention from the pain to the accompanying sensational quality, which we are not. This quality simply is not there—'we only feel the pain,' says Professor James. But, if so, the pain is not an attribute, but a substantive content, a sensation.

But, even admitting all this, Mr. Marshall would still object to calling pain a sensation, on the ground that it answers to no special form of stimulus in the environment. I reply that neither do hunger, thirst, nausea, and fatigue, yet we classify them as sensations. The attempt to analyze these states into 'cognitive elements' on the one side and pain on the other seems to me most futile and absurd. But, though themselves simple, they usually call forth an emotional reaction in the shape of a feeling of displeasure, in virtue of which we say, for example, that hunger is unpleasant. And I hold that what is true of these organic sensations is also true of pain. The proposition that pain is unpleasant is no more a tautology than the proposition that hunger is unpleasant. These are not, in other words, analytic judgments, but synthetic ones. I am gratified to find my own introspection on this point confirmed by so high an authority as Professor James, who says in a recent article, speaking of localized bodily pain: "I think that even here a distinction needs to be made between the primary consciousness of the pain's ***intrinsic quality***, and the consciousness of its degree of ***intolerability***, which is a secondary affair, seemingly connected with reflex organic irradiations" (PSYCH. REV., Sept. 1894, p. 523, note). This puts the whole matter in a nutshell. The total experience of extreme pain, which on the traditional theory could only be classed as a 'feeling,' something neither a cognition nor a volition, now falls apart into a sensation on the one hand and an emotional reaction on the other. And this explains how slight pains may sometimes be interesting and almost pleasant, and how bad tastes and odors may be excessively unpleasant without being in the proper sense painful. The separation of physical pain from displeasure, in short, though it may seem at first sight 'a bold assumption,' will, I think, be found both a necessary and a fruitful one.

C. A. STRONG.

COMMUNITY OF IDEAS OF MEN AND WOMEN.

I was pleased to learn by the July number of the PSYCHOLOGICAL REVIEW that my experiments upon mental community had been repeated at Wellesley College; but before reading far in the report of the experiments, my pleasure was changed to regret by finding that the method of experimentation and of computation had been diverged from in essential points. It did not surprise me, therefore, that the results reached were in part different from those published by me. I think it can be readily shown, however, that the Wellesley results do in no serious way tend to invalidate those reached upon Wisconsin students; and that on the one hand in the Wellesley report the contradiction between the two is exaggerated, and on the other the reflections made upon results reached by such statistical methods at Wisconsin or elsewhere are unwarranted.

The first of the two points at issue relates to the ratio of different words found amongst lists of natural associations prepared by groups of men and women students. The lists each contain one hundred words. I had found in 50 such lists prepared by students at the University of Wisconsin, only 2024 different words; among 25 men's lists, 1375 different words; among 25 women's lists, 1123 different words; or in percentages, 40.5 %, 55.0 %, 44.9 %. At Wellesley, although 25 lists prepared by women students were available, only 15 (why this was done is not told) were used in the computation; and because in these 15 lists as many as 1103 different words are found, the results are supposed to antagonize those published by me. But the most essential requisite for the fairness of such a comparison has been neglected, namely, that the number of lists in the two cases shall be the same. I had taken special pains to call attention to this point in a footnote in my first article (New Review, Dec., 1891, p. 562), where it is distinctly stated that the ratio of repetition depends upon ***the number of persons writing the lists*** as well as upon other factors; and again, in my second article in an experiment involving a different kind of word association (Educational Review, December 1891, footnote to p. 448), I had shown the general course of the law connecting frequency of repetition with the number of contributors to the word associations. Indeed, the mere fact that as given above the percentage of different words for 100 students is 40.5; while that for the groups of 50 students composing the same 100 it is 55.0 % and 44.9 % respectively, is a sufficiently obvious indication of the phenomenon in question. It is therefore entirely to be expected that the number of different words in the 15 Wellesley lists will be relatively larger than in either of the 25 Wisconsin lists. The law above referred to demands this. A fair comparison must be between two sets of 15 lists each from Wellesley and Wisconsin, or sets of 25 each from the two colleges. But, further, I do not hesitate to predict that even on the basis of such a comparison the Wellesley words will be found to show a smaller degree of community than the Wisconsin lists, and that because, as I shall attempt to show presently, the words written at Wellesley seem to be less natural and unreflective than those written at Wisconsin; and, as indicated in the note to my first article, the ratio of repetition depends, too, upon the character of the task. I had shown, for instance, that the repetition of words is greatest amongst the first words of each list, where the associations are most spontaneous and natural.

The second point at issue relates to the manner of distribution of the words written by the students, into twenty-five different classes as indicative of the relative prominence of these categories in the masculine and in the feminine mind. The strong preference of the feminine mind for certain concrete and familiar classes of words, in particular for articles of dress, interior furnishings, foods, etc., and the absence of abstract words, which appeared in the Wisconsin lists, entirely fail to appear in the Wellesley lists. The clue to this difference is to be found in the manner in which the lists were prepared. The lists which I used were written as rapidly as possible, and by each student at his or her own home, under as natural surroundings as possible. The Wellesley process is thus described: "That the thought process might be as free as possible, no restriction was made. The students were not even asked, as in the case of Dr. Jastrow's class, to write as rapidly as possible, but this difference in the method cannot possibly be supposed to account for the wide difference in results." Here I must beg to differ; I am of the opinion that it does very largely account for the difference in the results and I am glad to be able to strengthen my opinion by that of Mr. Havelock Ellis, who in his work on "Man and Woman" (pp. 166–170), extensively cites my results. In a card to the Editor of this REVIEW he wrote as follows: "In the July Psychological Review I noticed a record of experiments supposed to invalidate Jastrow's on community of ideas. I am sorry it has not been pointed out that they do nothing of the kind. It is essential that the words should be written *as rapidly as possible* (the italics are Mr. Ellis's). In this case ample time was given for conscious or unconscious selection. The results showed a difference which might largely have been foretold." The large number of abstract words is one of many indications of the unconscious selection going on in the Wellesley lists, and one list alone contained fifty abstract terms. I lay especial stress in the comparison of masculine and feminine mental traits upon securing as natural a material as possible, and the writing as rapidly as possible is a help toward this result. I remember that in writing my first paper I hesitated between using only the first fifty or the entire one hundred words of each list, feeling that the first half, when the words were natural and spontaneous, was in many respects the more typical. In brief, then, I regard the Wellesley lists as more reflective, less spontaneous than my own and the differences between us as in large measure due to this difference in method.

It remains to add (1) that as above indicated the proportion of different words will be larger when the words are unduly of the remote and abstract kind, so that the difference in method in the two results also goes to account for the higher percentage of different words in the Wellesley lists, and (2) that as I have indicated elsewhere (Educational Review, December 1891), it is only in the unrestricted spontaneous kinds of association that I found community of ideas greater in women than in men, and further that in dealing with such small groups as fifteen or twenty-five persons large room must be allowed for accidental variation. (See PSYCHOLOGICAL REVIEW, Vol. I., No. 2, pp. 152–158).

I, therefore, see in the Wellesley attempt to corroborate my results nothing that markedly conflicts with the conclusions I drew from my own experiments, and furthermore I find in them a positive contribution in that they show that a difference in methods of experimentation and in the treatment of material will bring about definite and predictable differences in the results reached; and that they thus emphasize the value and reliability of the statistical method, when efficiently applied, in the study of mental phenomena.

JOSEPH JASTROW.

MADISON, WIS., October 14, 1895.[1]

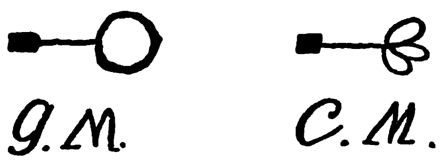
THE FUNCTIONS OF THE RODS OF THE RETINA.

v. Kries has written a long article (Zeitschr. f. Psych., IX., 81–123) in which he sets forth the reasons for considering that the rods are the seat of the faint-light sensation (which is the name by which I have designated the sensation of gray which remains after colors are no longer distinguishable), of the peripheral sensation, and of the sensation of the totally color-blind. His argument is extremely effective, and ought to carry conviction to every one who studies it thoroughly. I confess that I am somewhat surprised at his constantly referring to this idea as *his* hypothesis, and as the 'just developed' hypothesis. I had supposed that it was a fundamental part of my theory of light-sensation; and I am the more surprised at this because v. Kries expressly says in one place: "It may here be mentioned that the assumption according to which the rods are capable only of the production of the colorless sensation is found in the theory which has been developed by Chr. Ladd-Franklin." Apparently it is because he is unable to adopt my theory (nor even to understand it, he says) that he considers it proper to ignore the fact that the hypothesis in regard to the function of the rods is not now put forward, with any strong evidence in its favor, for the first time. (Max Schultze already in 1866 suggested this as the function of the rods, on the ground that many night-seeing animals have rods only, or chiefly, in the retina.)

As regards v. Kries' criticism of my theory, I have two remarks to make. In the first place, the assumption which he considers so objectionable a feature, and which he finds it impossible to form any conception of—the assumption, namely, "that the atoms of the outer layer have become separated into three groups at right angles to each other," is not an essential part of the theory—is, in fact, merely a mode of expression adopted for the purpose of giving the molecules

[1] I must explain that the delay in the appearance of the above rejoinder is due to a long illness and resulting accumulations of duties.

conceived of a certain degree of symmetry. All that is essential in the idea is that a photochemical substance which in the rods goes to pieces all at once under the influence of light of any kind has been so modified in the cones that it can go to pieces in three different stages, under the influence respectively of three different groups of wave-length. Merely to give a resting-place to the imagination, I make a diagrammatic representation of two molecules, of a just sufficient degree of complexity to answer this purpose, in this way, for instance:



The real molecules (if such exist) are, of course, of very different appearance from this, and of immensely greater complexity. My hypothesis that the vibrations which are going on in the outer portions of the molecule are so timed as to cause the molecule to be disintegrated by ether vibrations of the velocity of the visible portion of the spectrum, but not by those which are either more rapid or less rapid, is at the same time an hypothesis to account for selective chemical dissociation in organic substances in general. It is far from being remote from current physiological or chemical speculation. Jensen, in a late number of *Pflüger's Archiv* (LXII., 172–201) makes use of it to account for the extraordinary fact that, in animals so low down as the foraminifera, a state of contractory excitation is caused by the cut off pseudopodia of a different individual, while the pseudopodia of the same individual, though cut off in exactly the same way, produce no effect whatever; he makes the suggestion, since no morphological ground can be assigned for this difference, that an explanation must be sought in the idea, first made use of by Pflüger in his memorable paper of 1875, that every portion of living matter is a system of countless little differently tuned harps, and that non-synchronously vibrating portions of protoplasm act destructively upon one another when brought into contiguity. The origin of the idea in my own mind dates from the reading of a paper by Ebbinghaus.

My second remark is this: The very difficulty which my theory was gotten up to meet (given a separate grey process and complementary, not antagonistic, colors) has not apparently occurred to v. Kries as being a difficulty at all, and hence it is not surprising that he does not feel the necessity for my assumption. He says that in laying down a definite relation between the monochromatic and the trichromatic elements, I give up the advantage which should be gained by separating them. But is there not a tremendously definite relation between the *sensations* in question? The grey sensation due to the decomposition in the rods is absolutely indistinguishable in quality from the grey sensation due to the decomposition in the cones. What could be more natural then—more indispensable in fact—than to give this remarkable resemblance a physical basis in the theory? Nor can I see that anything whatever is lost by so doing. Far from my not having 'remarked' the connection between my assumptions and the Purkinje phenomenon, I had already suggested an explanation of that phenomenon in my paper in *Mind*, Vol. III., N. S., p. 103 (which v. Kries seems to have overlooked) and have since pointed out the inevitableness of this explanation in the light of the more recently added facts.

Prof. v. Kries attributes importance to the observation of Ebbinghaus and myself that a grey made of red and green is a very different thing from a grey made of blue and yellow, and considers that Hering himself must admit that it is thoroughly destructive of his theory, so soon as Hering shall have convinced himself of the correctness of the observation. v. Kries himself finds it extremely easy of confirmation.

C. LADD FRANKLIN.

SOMETHING MORE ABOUT THE 'PROSPECTIVE REFERENCE' OF MIND.

In the last number of this REVIEW (November, 1895), Prof. Baldwin handled the problem of the completeness and satisfactoriness of the purely scientific answer as to the nature of the functions of knowledge. After showing the impossibility inherent in the very nature of the scientific historical categories of their saying the last word about any organized developing real, he applies the argument, *a fortiori*, to those developing reals which we call the functions of consciousness. Any thing of organization is only known by its activities, and my present conception of it is of the sum of its known activities up to the present moment. This is the scientific or historical view of a thing, or to use Prof. Baldwin's term, the 'retrospective reference' of mind. Under this view we can determine the 'how,' the manner of the development of a given thing; but does this give us the right to consider its past history the whole reality, the 'what' of the object of our study? Assuredly not, for we are immediately confronted with a new series of activities, which could not be predicted and which may change our entire conception of the thing. Thus was reached, by an elaboration of this idea, a theory which makes an element of teleology necessary to the worth of the historical fragments themselves. It is seen that the mind works equally under the category of description or retrospective reference, and teleology or prospective reference, if it wishes to conceive the 'what,' the reality, of a thing. One must remain a positivist, concern himself alone with the 'how' and give up the problem of the 'what,' if he denies the validity of the prospective way of looking at things; at least so should he do, if he would be consistent. And this is especially true of the functions of mind, which to know aright implies not only an understanding of their historical evolution, or of their present epistemological meaning, but likewise of the ideal end toward which they point.

But says the Naturalist: All this is true enough psychologically; yet this very prospective way of looking at things, on account of the possession of which you are dissatisfied with the historical categories, can be shown to have been naturally evolved, and, proud as it is, must owe its existence to the very past which it claims to transcend.

"If the mind has developed under constant stimulus from the external world, and if its progress consists essentially in a more and more adequate representation in consciousness of relations already existing in the external world, then it follows that these internal representations can never do more than reflect the historical events of experience." How then can there be *any* phase of reality not subject to plain statement in terms of natural law? This is, however, but a new attempt to state the whole nature of a still active developing real in terms of its past, in this case the category of teleology itself. But the error rises likewise from a second and more subtle cause, namely, the failure to recognize the real relation between the historical categories and teleology, as it is deeply rooted in the psychology of knowledge.

This relation we may state, at least tentatively, in the following way: What we call the category of teleology is simply an induction from, or a statement in historical terms of, just those elements in each of the historical categories that escape our description. Or, better, it is an *attempt* so to describe these prospective indescribable elements. This may seem to be so many words, or, if to be understood, to be a direct violation of our principle which says that the prospective reference must not be put into historical terms. But let us explain. In our study of the 'what' of mind, its 'behavior generalized,' we find one peculiarity about its activity, that is not open to observation as in the case of other organized developing things. It is true that every growing, moving organism will have much more to tell us of its nature years hence; but while, as we have seen, this may then throw our past reckoning out of count, at present it tells us nothing of the future. It is nothing more than a 'vague pressure toward the infinite.' But in the activities of mind we think there is something more. They have, as it were, taken us into confidence and revealed to us their hopes for the perfect, the highest, the absolute. Each historical category, as expressed in the judgments of time, space, causality, etc., contains, we shall attempt to show, a 'strain of prospective reference,' which is the very life-blood of its function. Spencer recognizes this infinite reference of the categories, but fails to make use of its implications for his theory of knowledge, seeing in it only an argument for his metaphysical assumption of an unknowable but absolute ground. We may very properly ask why do these categories look toward an absolute, of which we know nothing; why, if they have nothing but the phenomenal in themselves, do they look for that with which they have no kinship? Extend these modes of thought to infinity, and unless there be something of the absolute in their constitution, the journey will have failed to bring them there. As a matter of fact this infinite prospective reference has not only a meaning for metaphysics, but for the very psychology of knowledge itself; it is the moving principle of the categories, the constitutive element in their activity.

To discover this we must analyze a little more minutely the psychological character of the 'infinite prospective reference.' The Old Psychology[1] placed among the fundamental intuitions of mind, as fulfilling in inductive search the criteria of universality and necessity, the two categories of teleology and the infinite. In a general way, this seems to be true to the facts of psychology; but their close relation to each other and to the other categories of mind is not indicated. From a psychological point of view the intuitions of the infinite and of teleology are really one and the same, or rather have their roots in the same psychological principle. The intuition of the infinite possible future is simply the prospective reference in its 'first intention' devoid of reflection or application to the explanation of particular phenomena. The idea of telos or end is understood, however, when the vague, infinite reference of mind is reflected upon in connection with the application of the retrospective categories to the explanation of the particular phenomena of the world series.

[1] James McCosh, for instance.

This makes clearer the conception already brought forward that the teleological principle in mind is simply the prospective reference of all the historical categories, brought under one descriptive term. For when we apply any one of the descriptive categories like time or causation to particular phenomena, this vague infinite reference compels us to look forward as well as backward, and as we are then dealing with particular phenomena or representations, the end or telos of this infinite reference must likewise be of the nature of a representation, if it is to explain the representations, and thus is the element of ideality or teleology introduced. Against the objection, already suggested, to thus characterizing the general prospective reference, or teleology, as the prospective reference of all the historical categories, put under one general term, the answer can be made that such a description is only symbolic; for we are simply describing it negatively, as that part of the retrospective categories that forever escapes description in their own terms, in terms of natural law.

It now remains for us to make good, by psychological analysis of the retrospective categories, the claim that each contains this strain of 'prospective reference.' For then we shall have shown that teleology is a constitutive element in each, and, in the second place, secured a new point of view from which to consider the problem of knowledge.

That we may not take our categories at random—and also for another reason which will appear later—in prosecuting this research, let us make use of the schematism of Schopenhauer's 'Vierfache Wurzel.' Following the static analysis of Kant, he proceeds to analyze the laws of Vorstellen—that narrow knife-edge of representations that lies between the two halves of the universe, subject and object—into four distinct classes, each of which has its own category and is ruled by a particular application of the 'Law of Ground.' Beginning, then, with the most mechanical of the categories, those of the second class, Space and Time (and for the reason that they are so mechanical, we shall find them the least propitious for our search), let us see if they do not contain also a strain of prospective reference.

The space of our study, it must be remembered, is not the space of geometry, of the so-called pure intuition, from whatever source that may come, but of the empirical intuition involved in our intuition of the external world as it may be shown to be historically evolved—in short, psychological space. For it is only this space which is a category of description, of history. Here, it is true, as well as in the sphere of geometry, the law of ground is simply the law of place, which says that any point determines as ground the position of every other point. But when this law of ground is applied geometrically it is essentially a retrospective, reflective, point of view, and is discernible only by reflectively impressing upon the empirical vision the laws of an abstract geometrical space. It is a necessity, which, just like logical necessity, is of the second intention; the law of the simple space intuition, as of all intuition of reality, is simply, as Paulsen has shown, an aesthetic 'Zusammenhang' or harmony.[1]

Now what is the nature of this primary empirical space intuition, which we hold in common with lower forms of the animal world? Its chief characteristic is that it is subjective and psychological. It is an intuition, an outreaching from a particular 'here.' It becomes such by the very fact that it has the 'here.' Space without the 'here' is objective and geometrical. As empirical intuition it may be studied from two points of view, that of natural history, and, secondly, that of its meaning for the intuiting consciousness. As historically evolved, as seen under the aspect of natural causation, there is no reason for doubting that the empirical space consciousness is, as Spencer claims, but a more complex expression of the primitive adjustments of rudimentary organisms to environment. The only thing to be avoided is the tendency to become metaphysical, to leave the outer world of adjustments and find a metaphysical explanation for space in time or still lower in sensation. But the historical side is not the whole of the category. This gives its past. It has also, as we have seen, its epistemological present with its sharply defined 'law of ground' for dealing reflectively with the details of the intuition. It has also, finally, a future reference, a teleological meaning for the 'here,' from which the spatialization goes out. If genetically, we must construe the space intuition as a growing complex of adjustments to environment, we surely cannot say, *a priori*, that its development is complete. As a matter of fact, the synthesis is constantly growing and including new elements in its grasp. To be sure, the geometrical law of ground always does remain ruling, as a matter of history. But the reason we can say that things are necessarily in certain relations of place is simply because our historical experience of space has been such as to make this law of ground always applicable. But space as a function is nothing more than a growing grasp of the manifold of experience, and its only principle from the point of view of its prospective reference is a certain æsthetic harmony of place.

[1] 'Einleitung in die Philosophie,' p. 229. Man kann es nicht stark genug betonen: Notwendigkeit ist im logiken Denken, aber nicht in der Natur; alle Naturgemässigkeit ist spontane Zusammenhang aller Teile.

This teleological harmony—the ruling motive of the activity of space intuition—has come about on the following wise. Or, rather, one should not say come about, but made its appearance to consciousness. In the primitive animal the motive to the rudimentary adjustment to environment was an external one, the pressing of sensational environment upon him and thus the necessity of getting into harmony with it. In the spiritual human consciousness, however, the motive to spatialization with the extension of the category is the harmonization of all representations of a spatial nature, no matter by what means they have entered consciousness, in one all-inclusive ken. To this end it works not alone through sight and touch, which are the historical media of the intuition, but by the imaginative use of the mathematical symbols. Can it be said that the planets are not in my space because I have not measured their distances with the naked eye, and can only express their relations in the borrowed symbols of numbers? If so, then the house across the river, which I see from my window, is not in my space. For it is quite sure that geometrically its relation to the river is quite different from that which it holds in my perspective.

It becomes, then, mere foolishness to attempt to take all of the teleology out of the dynamic space intuition, to separate it from its empirical content, and subject it as a dead, statical *res completa*, to analysis; for contradictions immediately develop themselves, such as all keen critical thinkers from Zeno to Bradley have had no difficulty in bringing against its reality. The reality of space exists, however, for the *intuiting subject*, before whom lies the spacial ideal, unconscious, perhaps, of finding in the *composition* of all the representations that have entered his spacial consciousness, a *place* for each in harmony with the great whole. Its ideal, its striving, is ever to overcome the limitations of the individual 'here,' and bring all reality that is external, the limitless world of a limitless space, into the ken of the knowing subject.

In the category of time the prospective reference is still more clearly shown. Here again we must distinguish between the time of mathematics and that of the empirical intuition with its 'now;' for only as it is related to this empirical 'now' is time the form of inner experience. This gives it the psychological character of an intuition, just as did the 'here' in the case of space. Succession is its law, to be sure, but as pure succession, independent of the 'now' of the intuition, it offers to reflection the same sort of difficulties as did space. Its nature refuses to be completely stated in retrospective terms.

Bradley's criticism shows here likewise that, taken as a *res completa*, abstracted from its content and from the dynamic synthesis which is its nature, succession immediately develops intellectual contradictions. If the 'now' is but a point in the succession, and through its mediation one attempts to understand the connection of the past with the future, the 'now' will itself break up into atomistic and mutually repelling moments, so that the series will fall into contradiction. The reality of time consists, however, in the fact that it goes out as a dynamic synthesis from a 'now;' and the latter, instead of being a point of connection between the past and future of a series, is in reality the measure of our grasp upon the changing content of consciousness. The reason that the present can be a bridge between the past and the future is simply that in a vague indefinite sense it already feels the future. Historically, time, like space, was evolved through the reaction of primitive sensibility upon a manifold of sensations and was simply the successful attempt to hold them in its grasp. But the motive of time is now no longer one of sensation; it has to do with the harmonious grouping of *all* the contents of consciousness, no matter by what means they have entered. Not all are equally definitely placed, for the law here is not one of simple succession, but rather an æsthetic principle of temporal subordination according as they have meaning for the 'now.' This now is continually prospective and is ever looking forward to the wider complex which it will grasp in the hand of the future 'now.' And as the category of time develops genetically, the 'specious present,' by its growing richness of meaning, marks what of the flowing stream the individual has been able to synthesize, and again points to an intuition of things which shall grasp all in a timeless 'now.'[1]

We now seek to discover the prospective reference in the two important retrospective categories of science; namely, Causation and Identity, or (lest Identity have a too metaphysical sound) the 'Same and the Different,' according to Mr. Spencer's terminology. Causation, the typical category of the Understanding, is, as an intuition, dependent upon time and space relations; but, when considered intellectually, it is an attempt to account rationally for change in space and time. But if we take the temporal relations existing between A and B, and try analytically to discover a real bond between them, we find, as Bradley points out, the same difficulties that appeared in the case of space and time, in fact, in relations of any kind. The A and B will persist in falling apart, for

[1] Prof. A. T. Ormond 'Basal Concepts in Philosophy.' Chapter on Time.

every attempt to introduce a mediating term ends in further disremption. But that this is so, follows from a static and analytical view of what is a dynamic intuition of the subject—from a strange oblivion to the prospective element in this, as in all intuitions. Lotze, equally well, saw the difficulties that gather around the causal relation when it is analyzed statically into its merely spatial and temporal conditions. For once analyzed, the space and time as well as the causal idea itself are then seen only under the retrospective point of view. For consider, that the judgment of causation is primarily not due to a definite knowledge of the space and time relations. These are only analyzed after the intuition has taken place, in order to give analytical *grounds* for the intuitive judgment. In order to explain the causal judgment itself and, indeed, in order to make it consistent and rational when analyzed into its grounds, the element of teleology or organization must be recognized in it. Says Lotze in his Metaphysic: "The natures of things that act on each other, the inner states in which, for the moment, they happen to be, and the exact relations which exist between them, all constitute the complete ground or reason from which the resulting effect issues. Thus the consequence is contained in the reason." This is, of course, only discoverable, however, analytically in retrospective thinking. But, he continues, there is resident in the notion of causation, "the idea of some one plan, which is the complex of reality, which only once completes itself and nowhere hovers as a universal law over an indefinite number of instances, and which assigns to each state of facts that consequence which belongs to it as a further step in the realization of the one history." [1] This is the essential prospective reference of the category. It is this persuasion that in the harmony of the whole there is a necessary place for every experience of nature in relation to the others, that compels us to order the particulars under this rubric of causal relations. As an intuition this category presents to us a union of the prospective elements of both time and space, so that it seeks a harmony which includes in its plan both relations of place and of succession. As a matter of fact we do tacitly assume such a state of affairs, for every time we make an hypothesis, under the guidance of which we seek to discover causal relations, we rest upon the teleological element in our causal notion, which says to us that the particular facts *must mean* something like this hypothesis.

In regard to the typical category of the Reason, Identity, or, in its empirical expression, the 'same and the different,' only a few words

[1] Metaphysic, p. 107.

are necessary. Natural science has very properly followed Hume in saying that in the sphere of perception, 'first intention,' there is no such thing as identity, but only close resemblance; and he is likewise perfectly justified in saying that these empirical judgments, historically considered, may be all reduced to habit and custom. But that ideal identity, which lies at the root of our judgments, the ideal which is so strong that we are always compelled to say that particulars are the same, although our experience afterwards (when we historically and analytically investigate the grounds for the judgment) invariably shows us that we were mistaken and had to do only with close resemblances—this side of the judgment requires other explanation than that of history, of custom or habit. It is really none other than the prospective reference to be found in this category; absolute identity is the distant ideal to which in its empirical expression the judgment never attains. Like a will-o'-the-wisp, it always escapes us, and, when we come up to our actual judgments and historically examine them they are seen to be concerned alone with close resemblances. But the genetic development of this category in an individual consciousness shows a closer and closer approximation to the 'norm' or ideal, showing that it does function as a regulation element in experience.

To attempt to show this prospective element in the sphere of ethics or in the will would be gratuitous, for motive, end, is the peculiar law of activity in this sphere. All empirical expressions of the will can be understood only under the law of motivation. Whatever be its historical origin, the *existence* of a prospective 'must' in this sphere is never denied; it is in the historical categories that the problem of the prospective reference lies, for here, so it is thought, 'is,' actuality, expresses all.

So much for the psychological analysis of the categories themselves, by means of which we were to discover in their very constitution a strain of prospective reference—not only an infinite reference which points vaguely to an absolute ground, but their very lifeblood, the withdrawal of which causes them to fall into pieces, giving us only appearance and illusion.

This is not so very different from the Platonic doctrine that all knowledge is only a remembrance, long since held for philosophical poesy. That doctrine is, however, but a symbolic way of expressing a fact that cannot fail to impress the mind that ponders the problem of knowledge. Is the present, individual knowing consciousness simply a spider at the end of a thread of its own spinning; or is there an instinct which determines the point to which that thread shall reach, a vital living connection with the consciousness that lies in the future time as well as with that of the historic past? How else shall I express those prospective judgments that do not seem to implicate will but only memory? The past alone does not explain them, 'nervous habit' and 'social custom' express only one side of the truth. Paradoxical and vague as the terms may seem, the prospective element in our knowledge functions can best be described as a *future forward memory* which, equally with the past, governs the activity of the present.

This becomes still more clear if these categories be united under some more ultimate one. It is in the basal category of sufficient reason, which has its peculiar law in each of these retrospective categories, that the prospective reference is most clearly marked. On its historical side, as an evolved psychological principle, it is explainable in terms of 'nervous habit' and 'accommodation;' it is the simple psychological principle of *interest*, with reactions made definite by habit. As an epistemological principle it is also seen under historic categories, for the law of ground in these different spheres of space and time, causality or the understanding, identity as typical of the reason, and motivation in the case of the will, is only discoverable when these judgments have taken their place as states in the historical, psychological series. For the descriptive terms of universality and necessity by which we test them are only discoverable in an inductive study of the static consciousness as instanced in the case of both Kant and the Natural Realists. In the case both of history and analysis we look upon them as definite formulas or laws and by that very fact are compelled to put them under retrospective categories. But the principle of sufficient reason, as well as the particular categories in which it finds application, has a third and more ultimate side. As such it is simply the dynamic impulse to knowledge which presses on to further and more complete synthesis of mental content, using the categories as its instruments; it is prospective always; its grounds only coming into conscious recognition when the judgments are viewed historically. But now arises a most important question. If historically Sufficient Reason is nothing more than nervous habit, if its epistemological grounds are likewise purely retrospective, what can be said of its prospective reference, except that it is a blind forward impulse? Of what value is it to have shown the individual categories to be prospective in their nature, if the active principle which gets them in motion cannot be defined more definitely than that it is an impulse to know? Have we not gotten back again to the 'vague infinite' reference, into which we attempted to infuse an element of teleology? The strength of this criticism cannot be well overrated, and at first it may seem that, in having escaped the relativity that arises out of the natural history view of Spencer, we have fallen into the pessimistic fatalism of Schopenhauer. For this is none other than the position of this famous Kantian. Epistemologically the categories are absolutely valid in their own spheres, for phenomena, but they are simply necessary unchangeable mirrors through which the otherwise blind Will looks upon itself. But all movement is in Will; therefore no teleology to knowledge, for Will is blind. What difference for knowledge whether the principle that has brought its categories into being is one of blind force, operating under the law of natural selection, or a blind irrational evil, with no meaning in its movements?

Now, it cannot be denied that from one point of view there is an element of blind fatalism in the psychological principle of Sufficient Reason. The *act* of judgment itself, which is the expression of the subjective impulse called Sufficient Reason, is really a leap into the dark, in its first movement.[1] Its synthesis of elements is always prospective, and it is only in the light of this synthesis, largely æsthetic, that the grounds arise upon which we develop our reasons for the same. But by this time the judgment has already become an event of history. So that the synthetic act itself is always without conscious grounds, always remains mysterious and illusive, making its necessity something almost fatalistic.

This is, undoubtedly, a true picture of the simple psychological impulse to knowledge, objectively considered. There is, however, a subjective concomitant, a reflex, so to speak, in the case of every judgment, which is so uniform in its meaning that it cannot fail to suggest a teleology to the forward movement of the psychological impulse itself. I refer to the element of necessity or *belief* with which we are compelled to pronounce a positive or negative judgment on any complex of form and content. In the sphere of 'first intention,' of sensation and perception, this is pure psychological necessity, or, in Prof. Baldwin's terms, 'reality feeling.' In the sphere of reflective judgment it becomes grounded or logical necessity, and its corresponding descriptive expression is belief. Now, it is important for our purpose that we see that there really exists no essential distinction between the absoluteness of these two necessities. Whatever may be

[1] Kant has the same idea of the Judgment (Kritik der reinen Vernunft, Ed. 1781, p. 78):" Die Synthesis überhaupt ist die blosse Wirkung der Einbildungskraft, einer blinden, obgleich unentbehrlichen Funcktion der Seele, ohne die wir überall gar keine Erkentniss haben würden, der wir uns aber selten nur einmal bewusst sind."

the difference in their knowledge-content, as functions they are one and the same. In one the grounds are in the elements of the percept; in the other they lie in the conceptual relations of the elements in the judgment; but in each case it is a necessary response to a complex of form and content, and the response itself, as long as it remains undisturbed by any new elements of content, is absolute. Sigwart recognizes this in his doctrine of the necessity of all judgments; although the grounds in one may be psychological, while in another logical. Likewise Newman, in his 'Grammar of Assent,' argues keenly for the essential likeness of both kinds of assent, although 'inferences' may afterward enhance the value of the belief for the logical understanding. This is belief in all its aspects, when viewed as a psychological function. But is not this also as fatal and irrational as Sufficient Reason as a psychological impulse? Yes, viewed alone as a function it is.

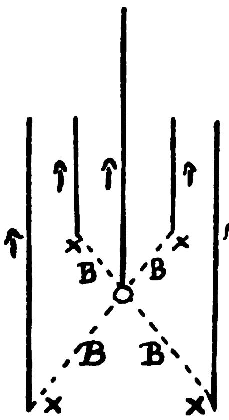
Yet *forces* in the psychological sphere are as dark and inexplicable as in the physical. It is only as a *bond* connecting the concept of the movements of the earth and its surrounding planets that Gravitation has any meaning. As a pure force it is absolutely without any content for thought—must be relegated to the limbo of fantastic powers of enchantment and wilful activity. In the same way the pure reflex function of belief has no meaning in our study of consciousness, unless it be a *bond* between two elements of content that are ideal. Thus to say that belief is the reflex movement of consciousness upon any complex of form and content describes it psychologically; but it is only when we conceive it as a *bond between the knowing self and its complexes of content* that it has any but a descriptive meaning for us.

As a matter of fact, belief is essentially an act of appropriation to the *subject*, of that which Sufficient Reason, as an impulse to knowledge, has brought before the bar of consciousness. Belief is, above all, self-reference. This self-reference of belief is always manifest to one who is not prejudiced in favor of a sensational philosophy, and it is not without meaning that both Hume and Spencer find difficulty in giving even a satisfactory psychological explanation of belief. Now my final aim is simply this: to show that *the continual self-reference of belief is the bond which unites the movement of Sufficient Reason, otherwise irrational, to a developing self, whose ideal is the end toward which the impulse to knowledge, in Sufficient Reason, is blindly moving, and that this teleology is what gives meaning to the prospective reference of the categories, which* *this teleology has generated.* The psychological forces of Sufcient Reason and Belief are blind only as forces abstracted from the ideal self-content to which they relate.

But to make good this claim, this self-reference must be analytically shown to be psychologically true—from the lowest form of judgment to the highest. First in the reality-feeling that accompanies sensation and perception; here we must not fall into the error of the intellectual Idealist, who commits the 'psychologist's fallacy' of making every feeling *explicitly for* a knowing self. Yet we must believe that the self-reference is at least implicit, else how (on the side of knowledge) could sensations ever be held together long enough for comparison and for the emergence of relations. The sensation is not first of all for a self, consciously, but it points vaguely to a self for whom it will become explicit later on in perception. As a matter of fact, recent studies in genetic psychology[1] point out that, in the development of the category of personality in the child, there are at first certain personality suggestions, very vague, to be sure, but nevertheless present, in the touch sensations that the infant receives in its earliest days. Already, in mere feeling, he learns to distinguish the personal in his external surroundings, and this reacts upon this budding notion of the self. As we pass from one higher synthesis to another, the self-reference becomes more marked. Time connects in a series the vanishing experiences, and by the mechanism of memory affords the possibility of an empirical self, which in turn by that constant increase of its grasp, points to a self which shall see all things *sub specie æternitatis.* Space brings with it the external world, both personal and impersonal; and by setting this over against the subject he further intensifies the self notion. With the advent of the category of causation comes a fuller notion of the self, for here energy is interpreted in terms of the activity of the self as revealed in the acts of will. The growth from the perception of close resemblances to the judgment of identity again brings the identical self into view as the norm and source of the judgment. The self in these last categories always reacts in the form of belief, and all these relations thus believed in are taken up and unified by the knowledge of a self as the source from which they depend and the end for which they have meaning. So much Kant saw, from a purely statical and analytical point of view. Whatever may be the metaphysical worth of the category of the self, it is at least the conceptual source of unity for all the other categories of consciousness.

[1] Professor Baldwin's 'Mental Development in the Child and the Race.'

There are thus discoverable two important lines of 'prospective reference:' a main line in the development of the self-notion, and a number of independent forward references in the particular categories themselves—a constitutive element in their growth. Each of these lower categories is in turn connected by the self-reference of belief to the category of the self as the developing *motif* of the whole movement of Sufficient Reason. The following diagram will show this more clearly:



The x's are the four principal retrospective categories, each with its prospective reference. The o is the category of the self, with its forward reference always in advance of the others. And by the bonds of Belief, the B's, each category is involved, in each of its activities or judgments, in the movement of the self, which is the richest category of consciousness and can then be used to interpret the others.

But suppose we seek for this unifying and explaining self as something among the complexes of content of which it is the ground and end. We shall then be looking for that which is the prospective reference of all the categories—all the syntheses of consciousness—among states of mind that have already taken their place in the historical empirical series. Our self, which seemed so much *en evidence* as it functioned ideally, has now withdrawn its support from the mechanical or retrospective categories, or, to employ a better figure, has fallen into lifeless dust among their atomic and disintegrated materials. But the self is just that point which can never be past, and for that reason can never be treated historically or found phenomenally. Just because it is the prospective reference of the emperical self, by which the latter is to be explained, does it refuse to be contained within those empirical limits. As Bradley argues, endeavoring to reduce the self like the other categories to illusion, psychologically (or under historic categories) the identity of the self or *ego* cannot be determined; for it must then be put somewhere within the temporal series and suffer the fate of time in his critical hands. But for that reason shall we call it illusion! If so we are reducing to illusion that which a study of the development of consciousness has shown to be the one element which has saved the whole movement from irrationality and confusion.

What then is the self? How is it to be construed in this connection? It is the empirical states, but more. As far as the retrospective categories can take hold of it is this the self. Nor is alone the unmediated intuition of the will the essential self, as Schopenhauer claims,[1] for this cannot explain the teleological function of the self in consciousness. Such an intuition has no ideal element whatever. It is pure present or past and can only be stated in terms of experienced acts of that will. This intuition, however, is a part of my self-consciousness, in that it gives me my notion of self as active and forceful. There is yet a third element of prospective or ideal significance which, just because it is prospective, will almost escape all statement in descriptive terms. It is that æsthetic harmony of our conscious states which we project as an ideal, that confidence (which is such a *ground-motif* of self-conscious life) that every element of consciousness has a meaning for the general harmony. This may, perhaps, be better suggested by a figure. The detail of the landscape before me is made up of rocks, trees, etc. As such, when I come up to them and subject them to study under the categories of description, they lose all the meaning that lay in the grouping of the perspective. The very value of the perspective is the æsthetic unity in which it reduces mere detail to its place in the whole. The same way of looking at things may be applied to consciousness. There is an element in the self whose very value lies in the æsthetic reduction of the indefiniteness which pervades the detail. This real psychological characteristic of consciousness can never be stated in descriptive terms, for as soon as we approach the scene with the instruments of science we have nothing but gross, crass details bound by nothing but mechanical laws. The prospective side of the self always escapes description, although for that reason it is no less an important psychological characteristic. Though rejoicing in the freedom from the thraldom of a metaphysical conception of the self as substance, he who fails to see

[1] Vierfache Wurzel, end of paragraph 42.

nothing more in self-consciousness than an aggregate of empirical states, or an unmediated intuition of a feeling called will, is not yet entirely free from the chains of the 'stuff' idea.

With the preceding discussion before our eyes, there seems now some hope, if not of correcting the faults, at least of understanding the weakness of some current epistemological ideas. At the present day the static analysis of Kant has been extended in three important directions, each of these movements being animated by the important modern conceptions of the flux of things. To view knowledge as an activity, and, above all, as a development, to infuse into the rigidity of the intellectual categories the life and movement which appears in the volitional sphere—this has been the *motif* since the Kantian disremption of the Pure from the Practical Reason. Historically the idealistic movement came first with a thought-evolution, in which the categories are the steps of a development, with the 'Idea' as its goal. The Evolution Theory in the hands of science, which knows no permanent or static forms in any of its spheres, makes of the absolute functions of Kant evolved products of the interaction of a primary sensibility with its environment. Like the *fauna* and *flora* of the biological world, they have taken their place historically according to natural law, and therefore get their whole meaning from the nature out of which they spring. Any forward movement is vague, and infinite in its possibilities. The Schopenhauerian conception which puts the whole movement of knowledge in the hands of a blind Will, and conceives the categories as complete existences, is but a purely metaphysical restoration of the breach between the Reason and the Will.

This is, however, the fault with all these theories: the movement is externally and metaphysically explained. It is not grounded in a psychological analysis of the knowledge factors themselves and of the self in its relation to knowledge. In the case of the Idealists the actual empirical development of knowledge is reduced to a mere *conceptual* relation of ideas, and, to the extent that the psychological roots of the concept are not known, is unpsychological. The Natural Science view, in so far as it finds the *origin* of the knowledge processes in the interaction of subject and object, of sensibility and environment, and by this seeks to explain them, is also metaphysical, either naïvely dualistic or somewhat materialistic in its monism. The third union of Will and Knowledge by Schopenhauer, is, of course, purely metaphysical.

If then the ontological teleology of Hegel is untenable, there is left either the pure relativity of Spencer, or the absolutely blind unteleological movement of the Schopenhauerian Will.

There is every motive then to look for a teleological, prospective reference of mind, as a constitutive element in the retrospective categories themselves which the Kantian critique had looked upon as static and unchangeable: above all to give it a psychological basis, for the metaphysical application will not be far in the rear.

Though the preceding study may not have been in any way of the nature of a supply to this demand, it yet affords grounds, we are convinced, for a somewhat more emphatic repetition of the poetical but keenly intuitive protest of Emerson against the Kantian description of Intellect and Will: "Our intellections are mainly *prospective*. The immortality of man is as legitimately preached from the intellect as from the moral volitions. Every intellection is mainly prospective; its present value is its least."[1]

JENA. WILBUR M. URBAN.

OUR LOCALIZATION IN SPACE.

The title to these notes may appear misleading, but I know of no other to describe the phenomena which I wish to illustrate by an interesting experience that occured to me about a year ago and that I have narrated to my classes for suitable purposes. Perhaps it will be interesting to others.

Only twice in my life have I awakened in dream and at the same time had the dream images continue for a short period so as to watch them as apparently real objects. The first one was a dream of a mountain scene in a valley with a lake and summer hotels on its shore. I watched the view for perhaps a full minute with my eyes still closed, but conscious of being awake and lying in bed. The disappearance of the scene was marked by the visible occurrence of small clefts or openings in the rocks nearest where I appeared to be standing. The scene was perfectly vivid and real, an exact representation of what such a scene would be, if I were actually looking at a landscape, projected outside of me. The *eject* of reality and actual space relations, perspective, color and all were as distinct as when walking in the fields or the streets. But this is not the characteristic which I wish to describe or illustrate. I have probably only narrated what is a common experience with others who have awakened in a dream and watched it, though it may, nevertheless, be interesting to note the fact

[1] Essay on 'Intellect.'

that sensory action without its appropriate stimulus is as definite and complete as either in reality or in halucinations. The fact, however, to which I call special attention in it is that I cannot recall or did not have the peculiar feature of the second dream to be narrated, which resembled the first in its main characteristics; that is, the visual reality and projection of the apparent object.

I dreamed that I was in my old bed-room where I slept when I was a child. It was oblong in shape and I recognized it, my view of it appearing as it would if I were lying on the bed. I awakened in the midst of the dream and keeping my eyes shut (there being no reason to open them as no darkness appeared, though where I was actually sleeping it was quite dark), I noticed paper on the walls, a kind I had never seen in my recollection. Now there never had been any paper on the room represented in the bed-room of my childhood, and observing it in the dream image I felt some surprise, because I knew that my bed-room had never had paper of any kind. This discrepancy at once convinced me that I must be wrong about the room, the moment I compared what I saw with what I remembered, a comparison which did not suggest itself during sleep. The discrepancy had no effect. But, strangest of all, the moment that I saw the discrepancy and saw that I was not in the room as I had known it, I became confused as to where I was. I noted the resemblance in shape to my old bed-room, and tried to recognize where I was and though wide awake I could not think of myself as in my apartment in New York. I had not the slightest conception where I was. I could only see the walls and wall paper of my old bed-room. After the lapse of about a minute the paper and walls vanished quite suddenly, though a general mass of Eigenlicht remained, and I at once recognized that I was in bed in my apartment. I then opened my eyes. It is remarkable that the tactual sensations did not avail to localize me, but they did not. I felt myself lying down, but I could not obtain the least conception of where I was until the vision of the wall paper and walls disappeared, when I could *recall to the visual imagination and memory the shape of the room and position in it* in which I was actually sleeping. Had it not been for the discrepancy between what I saw and my memory of my old room at home, I might have still imagined that I was there. But I knew from the wall paper that this could not be, and I was puzzled to know where I was until the visual image began to break up and vanish, when I at once pictured to my mind where I was in reality.

Now, the question is, was my localization conditioned upon a memory image in the visual center which could not be found until the real image vanished? Of course the identification, when it did come, represented my past experience with my bed-room in the apartment, and the assumption that I was where I had gone to sleep the evening before, but this had no effect until the visual image of my old room at home vanished. Nor did the tactual sensations to which I consciously deferred help me in the slightest degree to determine where I was. It all seemed to hinge on the representation in the visual memory of the room in the apartment after the real image of the old room at home had disappeared. Unfortunately I am not able to corroborate the supposition involved in the above question by any recollection of actually localizing myself in bed in the first dream which I have narrated. I only recall the fact that I was awake looking apparently at a beautiful landscape of mountain scenery, and that I was much interested in the nature of the phenomenon. But I am not certain that I knew I was lying in bed. This may have been after the image began to disappear. I do remember that I was lying on my stomach, but I do not recall that I was conscious of this fact before the picture vanished. Hence I can get in it no confirmation of the possibility that the localization depended upon a visual representation of my room as it was in the memory continuum of experience. Moreover, objection might be made to such a supposition from the fact that the possibility of memory representation conditioned the consciousness of the discrepancy between what I saw and what I recalled of my old room at home. Hence it seems all the more puzzling to note the fact that tactual sensations did not tell me where I was and that the localization did not occur until the visual memory became active.

But I had a waking experience which at least seems to confirm the supposition, though it may not be conclusive. I was riding in the cars of the New York Elevated Railway and had reached the Thirty-third street station. Just as the train left it I noticed across on the south side of Broadway the sign of a store for the *Microbe Killer*. I said to myself, "Well, this store has moved; it used to be around the corner of the next street north" (Thirty-fourth street). I fully expected to see it where it had been as the train moved. I looked up and saw a church (Dr. Taylor's) on the north corner of Thirty-fourth and Broadway, and I said to myself, "No, this cannot be; there was no church near where I had seen the *Microbe Killer* store." But I was not positively convinced of the error until I could see up the street as we crossed it. I felt puzzled for a few moments to know where I had seen the store. All at once there emerged in my memory a visual representation of Broad and Arch streets, in Philadelphia, where I had seen a store at which the *Microbe Killer* was sold, the store being on that side of the street where it would have been in New York on Thirty-fourth street, if I had been correct in my first impression. Now the interest of the case lies, not merely in its being an ordinary case of redintegration (was there any association between the words Broad street and Broadway?), but in the fact that the space relations in the false and the true recollections were the same and that my illusion about the store was not discoverable until I formed a visual representation in memory of what I had seen in Philadelphia and could compare it with the knowledge or consciousness of any actual place in New York.

But I will not urge the case as proving anything. I narrate it here with the dreams only to encourage observations of others in the same direction. I do not know that such a phenomenon as is narrated in my second dream and the waking state following it is at all common. I should like to know whether others have had a like experience. It is of special interest as suggesting how little tactual sensations have to do with space perception and localization in it except as tactual experience is conceived in terms of visual space. Not that I mean to imply that we cannot obtain any notion of space whatever by tactual and muscular sensations, but that in this case at least they seemed to have no power whatever to determine it. I certainly find in my own case no reason to accept the Berkeleian doctrine of space and our localization in it, and this wholly apart from the dream experience just narrated. In this case, however, the localization was definitely related to the visual representation of my place of living. The only question that remains is to know whether such a phenomenon occurs often enough in the experience of others to give it anything more than individual significance and interest.

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THREE CASES OF SYNÆSTHESIA.

The subjects of this report are three sisters, D, C and K, aged respectively 9, 10 and 12. Their father and mother are good visualizers, the father having definite number forms. There are also two younger brothers one of whom, aged about 5, visualises his alphabet so vividly as to be able to read it off backwards with unexpected rapidity. His alphabet form is traced to the perpendicular series from which he learned his letters. No such early association can be discovered in the case of the three sisters, though they too have elaborate forms for numbers, months, days of the week and the alphabet. They are not musical.

D sees the letters black on a background of indefinite color, but as if they were *behind* the patches of the color to which the letters correspond. The color is seen only when she thinks the words separately, not when she reads them or hears them spoken connectedly in a sentence. The position of the word and color is close to the eyes or in the head.

C sees the words from a foot to a yard away. Sounds and smells are yellow to her except thunder, which is black; but the color is very dim and she herself is somewhat uncertain about it.

To K the colors are 'far away,' but seem to come nearer when closely attended to. Her brightest words are the yellow ones.

All three have had these pseudo-sensations as long as they can remember, but their peculiarity was not noticed until about a year ago. They have not influenced one another in the coloring of letters or words, as they have been observed always to disagree about the same letters in the same way.

Subjoined is a table giving in the children's own language the colors, if any, of all the letters of the alphabet, days, months, certain proper names, certain common nouns selected for their phonetic or orthographical peculiarities and certain numbers. Roman numerals are colored after the letters (I, V, L, C, etc.) composing them.

	D	C	K
A	white	reddish brown	white
B	blackish blue	white	bluish white
C	white	white	blue and white
D	white	green	white
E	blue	greenish yellow	brown
F	brownish	black	reddish
G	green	brown	grey
H	brown or green	brown or black	red
I	black	black	yellow
J	red	brown	dull red
K	crimson	black	white
L	yellow	white	yellow
M	blackish red	red	bluish black
N	red	brown	light brown
O*	white	white	white
P	black	white	black
Q	yellow	white	yellowish
R	pink	blue or as initial red	red
S	white	yellow	very light yellow
T*	black	black	black
U	yellow	greenish white	yellow

V	blue	white	grey
W	brownish	green	blue black
X	no color	yellow	brown
Y	yellowish black	black	yellow
Z	black	yellow or white	brown
&	yellow	black	no color
1	black	black	white
2	white	brown	blue
3	red	white	brown
4	blackish or no color	black	red
5	yellow	green	bluish white
6	black	red and white	red
7	black	black	light yellow
8	brown	green and white	bright yellow
9	black	brown	crimson
10	white	1 black, 0 white	black
11	yellow	black	dark
12	white	black and brown	darker than 11
13	red	black and white	brown
14	no color	and so on to 20	red
15	yellow		white
16	white		red, duller than 14
17	black		yellow
18	yellow		yellow
19	black		crimson
20	white	brown and white	"dull white, like steel"
30	red	white	brown
40	no color	black and white	red
50	yellow	and so on to 90	like 20
60	black		duller red than 40
70	red		yellow
80	white		yellow
90	no color		dark red 99 red
100*	white	white	white
200	white	brown and white	white like 20
300	red	white	3 brown 00 no color
400	no color	black and white	red + no color
500	yellow	green and white	white
1000	blackish white	greenish or white	no color
2000	white	brown and white	no color
347	red	3 white 47 black	3 brown 4 red 7 yellow
896	red	8 green 9 brown 6 white	8 yellow 9 crimson 6 red
Dorothy*	white	white	white
Quincy	yellow	white	yellow
Grinnell	green + red	brownish green	greenish brown
Charlotte	white + bluish	red	bluish black
Katharine	red	black	white
Laurence	yellow	white	reddish brown
Robert*	red	red	red
Morgan	blackish white	red	blue and black
Maria	yellow	red	M light I red rest indistinct
Isabel	*Is* brown; *a* white; *bel* yellow	yellowish	I yellow, rest yellowish brown
John	reddish	brown	black
Sally*	white	yellowish white	white
Stephen	brownish	yellow	brown
Spencer	no color	Spen yellow; cer white	brown

Hilda	yellowish	brown	red
Madeleine	whitish yellow	dark red	not distinct
Louise	yellow	black and white	red like Hilda
Mary	white	red, sometimes white	like Maria
Edith	yellow	white	white
hurt	brown	brownish red	dull brown
pert	black	green [red]	purplish black R red
smell	always yellow	yellow	brown
spell	all colors	yellow	brown
stop	*st* black *op* white	yellow	lighter brown
break	brown	black and white	no color
try	black	black	blue
house *	brown	brown	dull red
Cæsar	white	white	C and A white
fairy	white	white with black spots	yellow, R red
how	white	brown	red
few	black	red	yellow brownish red
straight	black sometimes white	yellow	doesn't know; yellow
trait	black	black	no color
rate	red	RA red TE black	R red
ate	yellowish black	doesn't know	no color
at	written, A white, T black	black or no color	no color
hat	black and white	brown	H red, rest no color
that	black and white	black	T's have black back ground
handy	black and green and white	brown	red (dull)
hand	black and white	brown	red (dull)
and	white	green	no color
an	white and green	brown	no color
a	white	red sometimes brown	no color
eight	blackish yellow	greenish white	yellow
ate	blue and white	doesn't know	no color
bow (=bough)	white	white	no color
bow (=bō)	white	white	no color
Monday	red	pink	blackish blue
Tuesday	black	red with yellow stripes	yellowish black
Wednesday	dark orange	green	blackish blue
Thursday	black and white	black and brown	dark brownish black
Friday	light red	black	greenish
Saturday	white	green and white	brown
Sunday	light yellow	color of the sun	very light yellow
Jan.	red	green and black	reddish brown
Feb.	black	darker	brown
Mar.	green	red and white	red
April	yellowish white	red and white	red
May	white	pink	white
June	dark red	yellow	red
July	darker red	yellow	red
Aug.	hay color	yellow and black	yellow
Sept.	black and white	yellow and black	brownish yellow
Oct.	black and white	white and black	grey
Nov.	red	red and white	no color
Dec.	white	white and red	white

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PSYCHOLOGICAL LITERATURE.

GENERAL.

Elements de psychologie humaine, cours professé à l'université de Gand. T. T. VAN BIERVLIET Gand, Lepper. 1895. 8°. Pp. 317 and 34 fig.

This is an elementary treatise on psychology intended especially for students studying for the B. A. degree, who as a rule lack physiological and anatomical knowledge. A large part of the book, almost half of it, is taken up with descriptions of the nervous system and of the organs of sense and movement, descriptions which are to be found in every physiology. The entire work is imbued with the physiological spirit, as may be gathered by observing the clear and precise language of the author, whose metaphors and similies are almost always borrowed from the natural sciences. It is evident that the author is not in the unhappy position of some of his contemporaries who having received a special literary education forget this when they begin to write. It is worthy of note that the physiological tendencies of the author do not lead him to materialism. He urges, on the contrary, that mental processes are entirely distinct from cerebral and do not correspond to anything material, that judgment and reason are not functions of the brain but faculties of an immaterial soul, and that the immateriality of the soul does not require proof, as it is practically doubted by no one.

A third characteristic of this book is the complete absence of experimental psychology. Researches on reaction-times are only noted in the appendix. This omission, which is apparently intentional, is surprising, as the author is the director of a psychological laboratory; and for this reason M. Biervliet's book cannot be considered as representative of the actual state of psychology.

The general plan of the work may now be indicated. After an introduction on the human body in which the author studies cells, tissues, and more especially the circulatory, respiratory, muscular and nervous systems, including the latest views of Cajal and of Golgi, we have the first part covering the *physiology of conscious phenomena*. It includes sensations and movements, but scarcely anything else, being merely a repetition of what may be found in general treatises on physiology. The second part on the *psychology of conscious phenomena* contains definitions of ideas, judgment, reason and will, including a defense of the doctrine of the immateriality of the mind and of free will. The third part on the *psychophysiology of conscious phenomena* includes imagination, memory, motor expression, character, personality and measurement of reaction-time. In speaking of memory the author develops interesting though somewhat theoretical ideas on the mechanism of recognition and on localization.

In spite of some drawbacks the book is certainly the best elementary treatise on psychology in the French language.

Psychologie des Foules. G. Le. BON. Paris, Alcan. 1895. Pp. 200.

We have here a book that treats a subject with which the psychological laboratories scarcely concern themselves. The reading of such a work cannot but be salutary for the professional psychologist, if only to teach him that there is more in mental life than reaction-times. The author studies the 'crowd,' understanding by this word, which he uses in a wide sense, a number of individuals who think and feel in the same way, but who are not necessarily collected together in one place. Thus he introduces into his book a study of the curious popular movement produced in France by General Boulanger a few years ago. Two principal conclusions are drawn: 1st. That the importance of 'crowds' is growing daily and will continue to be a factor of increasing importance in the future. 2d. That the 'crowd' is of low intelligence, without reflection, reasoning or moderation, a prey to all extreme emotions, good or bad, incapable of self-guidance and without the power to construct or to originate. How in the face of these results an optimistic conclusion and a faith auguring well for the political future can be drawn we do not understand.

A. B.

TELEPATHY, ETC.

Ueber unwillkürliches Flüstern, eine kritische und experimentelle Untersuchung der sogenannten Gedankenübertragung. F. C. C. HANSEN UND ALFRED LEHMAN. Philosophische Studien, XI. 4. pp. 471–530,

In the S. P. R. Proceedings, VI., 128, is a series of experiments by Prof. and Mrs. Sidgwick on the transference of numbers from the mind of Mr. Smith to two young men hypnotized by him. The numbers were bi-digital, running from 10 to 90, drawn from a bag and silently looked at by Mr. S. The subjects named whatever numbers they saw appear in their mental field of vision. There were 1,356 trials, with the result that any digit 'seen' or 'named' by the subject invariably corresponded much more often to the digit 'drawn' than to any other digit. In table I., for example, in a series of 354 trials, both digits were named rightly 79 times instead of the 'probable' number of four or five times. Some cause was evidently at work inclining the subjects to guess right. The Sidgwicks think that this cause cannot have been vocal indications given by Smith and hyperæsthetically heard by the subjects, because if the latter had been guided by sound their mistakes would have shown the effect of sound as well as their successes; that is, the numbers named wrongly by them would have also tended to resemble in sound the numbers actually drawn from the bag, which the Sidgwicks try to show by a comparative table was not the case.

The Danish writers subject this opinion to a careful criticism. Repeating the experiment with two hemispherical mirrors, 90 cm. wide, opposite each other, the head of the agent being in the focus of one, and that of the percipient in the focus of the other, they found that the numbers could be *heard* by the percipient, and consequently named rightly; when the agent inwardly articulated them, even the bystanders could hear nothing and the agent's lips were tightly closed. They also found certain parts of the room within which the sound of a grain of shot dropping on a plate could be heard, whereas it could not be heard from other places. The percipient, if in such a favored place, might of course catch a vocal indication to which bystanders would be deaf. Subjecting the whole number of 'guesses,' right and wrong, to a laborious phonic analysis, they prove moreover that the mistakes made by the English subjects, mistakes whose nature, according to the Sidgwicks, was such as to exclude their being due to imperfect hearing, showed a striking analogy to those made by themselves, which positively *were* due to imperfect hearing. In the English observations, namely, the numbers oftenest substituted for each other were those whose common phonetic elements were the same that caused the most frequent confusions of hearing in Messrs. H. and L. The Sidgwicks' opinion is, therefore, Messrs. H. and L. conclude, superficial and hasty, and hyperæsthesia of hearing remains 4,000 times more probable than any other assignable cause, of the amount of 'thought-transference' recorded in their experiments. The authors point also to the facility with which, in diagram-guessing, figures may be considered 'right' which really represent quite different objects from those meant by the agent, if only the two objects have analogous elements. The paper is a genuinely scientific contribution to the elucidation of so-called thought-transference phenomena, and contrasts most agreeably with the random abuse to which their recorders are accustomed.

Telepathic Dreams Experimentally Induced. G. B. ERMACORA. Proceedings of the Society for Psychical Research. Vol. XI. Pp. 235–308.

This is a startling experimental record of a new genus of thought transference. The personages are: Dr. Ermacora; the Signora Maria, a young woman with trances and automatic writing in which she manifests a secondary personality alleging itself to be a spirit named Elvira; Angelina, Maria's cousin, a child in her fifth year; and, finally, the Signora Annetta, Maria's mother. The two ladies and the child live together at Padua, and Dr. Ermacora is a familiar visitor at the house. A certain spontaneous dream of Angelina's, in which she seemed to see the so-called Elvira, led Dr. E. to try systematically whether he could determine Angelina's dreams by ordering 'Elvira' to appear to her in sleep and make her dream according to his prescription. The experiments made were seventy in number and almost every one succeeded. Dr. Ermacora, for reasons that he does not give, was unable to isolate Angelina from the two ladies, so the physical possibility was not precluded of Siga. Maria telling the child every night, after the details of the dream had been dictated in the evening, what she must report next morning. He considers it morally impossible, however, that the ladies should wilfully play a trick on him; and believing that Signora Maria, if she coached Angelina at all, could only do so whilst herself asleep, he habitually locked and sealed Angelina into a separate room, and got Signora Annetta to sleep with Signora Maria, so as to detect any possible somnambulism. This nevertheless was not reported. He moreover prescribed dreams, the nature of whose details was incommunicable verbally, such as dreams of persons shown in photograph to Maria-Elvira, and afterwards identified in photograph by the child as having been seen in dreams; or dreams of instruments pictured in manufacturers' catalogues, and similarly discriminated in Maria's absence by the child from amongst other figures of instruments that contained the same mechanical elements and would have had to be described in the same words. The child's accounts also made it clear that the suggestion, whatever it was, must have been in optical, and not in verbal terms; for she often gave circumstances of the dream in words of her own limited experience that differed from the names used in prescribing the dream—'dog' for lamb, *e. g.* (she had never seen a lamb); 'hail' for snow; 'dark place down stairs' for cellar (she had never been in a cellar); 'tramway' for ship (the steamboats at Venice which was the child's home are known as tramways) etc.

Dr. E's conclusion is that there was communication between the subliminal selves of Angelina and Maria. It is clear, in spite of the precautions taken, that much of the evidence hinges on the honesty of Siga. Maria and her mother, which Dr. Ermacora says it is impossible for him to doubt. I, knowing Dr. E. personally, and having been present at one of his experiments, do not doubt *his* honesty. He is a trained physicist, author of a thick book on electricity, and possesses an unusual experience of 'psychic' phenomena, and a shrewd mind in comparing hypotheses. The editors do not doubt *my* honesty, or they will not print this report. But the facts are so unprecedented that the whole chain of honesties will seem a weak one, and the 'rigorously scientific' mind will exercise its natural privilege, and doubtless promptly and authoritatively dismiss the narrative as 'rot.'

W. J.

The Present Condition of Experimental Psychology, its Methods and its Problems. VICTOR HENRI. Woprosi Philosophie.

The author reviews the rapid development of the science, closing the paragraph with the assertion that psychology is passing through a transition stage at present, in which the school of Fechner and Wundt is disappearing and new school takes its place. The first experimental psychologists arose in opposition to the old metaphysical psychology, sought to place the science on a similar basis with the natural sciences, and therefore abandoned the use of self-observation, applied physical and mathematical laws to psychic facts, and emphasized the external conditions of psychic life. The result was automatic methods and results which include much hypothesis, illustrated in Weber's experiments in skin-sensations where 'one point' or 'two points' were the only answers requested or allowed to the experimenter's subject. The Psycho-Physic of Fechner made the matter worse, and experiments similar to Weber's in automatic character were carried out in the laboratories of Wundt and others. In 1868 Donder's reaction-time experiments opened up a new field for the application of these methods destitute of self-observation—a field which was rapidly investigated in a manner prolific of results. The difference between sensory and motor reactions, discovered by Ludwig Lange, can never be explained, says the author, until the method of self-observation is again resorted to. Following these experiments came others concerning the time-sense, the general sense, contrast, after-images, abnormalities, etc., all carried out without self-observation. These experiments were chiefly conducted in Germany and America, where the first laboratory was founded in 1883, by pupils of Fechner and Wundt. In France and England the writings of Comte, Hume, Mill, Spencer, Bain and Darwin laid the foundation for psychology which in these lands did not come to such sharp opposition to the old metaphysical conceptions and never neglected self-observation nor ceased to employ it. The aim in these countries was, through experiments, to establish constancy in outer conditions and so to secure a control for self-observation. Attention was chiefly directed, not to theories of sensation and psychometry as in Germany, but to the higher psychic functions; for example, the works of Spencer, Bain, Galton, Sully, Charcot, Ribot, Binet and James. In France attention was mostly given to pathological states, and in America to the practical application of psychological results in the field of pedagogics. Thus we see, writes the author, how a German school whose chief representatives are Fechner and Wundt has developed side by side with a French-English school whose chief representatives are Galton, James, Binet and Ribot. The former seeks exactness in the measurement of the simplest processes, investigates small details and neglects self-observation; the latter investigates the complex processes, gives attention to self-observation, and studies psychic phenomena as they appear in reality.

The author seems to have confused experimental psychology with psychology as a general discipline, forgetting that the writings of Prof. Wundt, and his pupils are by no means confined to the experimental branch of the subject. The fact that he is the representative of a conception of the will which some have designated metaphysical is enough to establish for him a relation to general psychology. It is not true that his work has been confined to the 'measurement of the simplest processes to little things and details.' On the other hand, how can the author classify such writers as Spencer, Comte and Mill as experimental psychologists, or even founders of experimental psychology? The two volumes of Spencer's Principles of Psychology do not contain a single psychological experiment, properly so called. The same is, in general, true of Comte, Bain and Sully. The author finds that in this French-English school alone, self-observance has been used and given its due importance. But looking at the works of the authors cited, what is the fact? The relative importance of self-observance and experiment in psychology is seldom, if ever, discussed. The System of Logic of J. S. Mill emphasizes the importance of experiment in all empirical sciences, but aside from this, what is there? Experimental psychology has been comparatively little pursued in England. Comte denied the possibility of direct self-observation and with it the possibility of such a science as psychology. Locke and Hume can be classed as experimental psychologists as well as Mill, Comte, Bain, Sully or Spencer.

Furthermore, the author somewhat misrepresents the psychological work done in America, in speaking of it as a practical application of psychological results to pedagogics. He seems to forget that Prof. James, whom he classes with the French-English school is an American, that the latest and most adequate discussion of the philogenesis and ontogenesis of mind is from the pen of Prof. Baldwin, and that Ladd has probably done more work in physiological and experimental psychology than any other English writer.

The author's representation of the German 'school of Fechner and Wundt,' as omitting all self-observation in their methods, is surely inaccurate, to say the least. Space cannot be taken to quote from the many utterances of Wundt in the Theorie der Sinneswahrnehmung, in the Grundzüge, in the Menschen und Thierseele, and in the Philosophische Studien, all mentioning experiment as a help, a means of regulating and controling self-observation. Looking alone at the experiments which are conducted in Prof. Wundt's Institute, as well as at those proposed but rejected, it is clear that the primal requisite to successful experimentation is, to his mind, that all the conditions of the state or process to be investigated be subject to the control of the observer, so that the experimenter is not left to choose any one of a number of unknown processes in forming his judgment, thus introducing an equal number of unknown variable factors into the results. Just in this requisite, that the method of self observation be known, lies the limitation of experimental psychology. In Prof. Wundt's words, 'only those psychic phenomena can be influenced through experiment which are open to direct physical influence.' * * * * Every psychological is therefore at the same time a physiological experiment, just as there are physical processes corresponding to the psychic processes of sensation, representation and will."[1] The object of Prof. Wundt has never been, as is here represented, merely to minimize the importance and the actual use of self observation; but rather to control and systematize it. A psychology totally devoid of self-observation is impossible. The author has exaggerated some features of experiments in Wundt's Institute and made it appear that the value of self-observation is here unrecognized and its necessity denied. Probably no method other than Wundt's, or one in all essentials similar to his, could have been adopted in investigating the phenomena which he has investigated.

At best the author's division of psychologists on the basis of the principle of self-observation is not a happy one. Would not some such division as the following be better: I. Psychology of the *nature* and relations of the functions of adult consciousness, including (1) general psychology of individuals, psychometry, psycho-physic, physiological psychology, etc., and (2) psychology of races and crowds; and II. Psychology of the *development* of consciousness, (1) in the race and (2) in the child?

The author divides methods on the basis of the steps involved, and not on the basis of the nature of the objects investigated, into eight classes. (1) Experiments where one stimulus is given and the experimenter simply reports what he experiences. Such are all threshold determinations, experiments concerning the clearness of perception, the analysis of musical cords into single tones, elementary experiments in æsthetic pleasures, the localization of tones and localization in general, etc. (2) Two stimuli are given, either simultaneously or successively, for comparison, as in experiments concerning the sensibility to difference (Unterschiedsemfindlichkeit), etc. (3) Several stimuli are given and the experimenter is asked to choose one possessing a certain characteristic. (4) The experimenter has a certain movement to make, either as he chooses or as directed, to a given stimulus—psychometry and muscular-contraction experiments. (5) A copy is given and the experimenter repeats or imitates it, or seeks another which

[1] Menschen und Thierseele (1892), pp. 11, 12.

stands in a given relation to it, as in memory drawings, and the localization of a stimulated spot of skin. (6) A series of objects given to be arranged in a certain order.

These six classes embrace all the psychological experiments which are possible. Two others which are properly physiological are conducted by means of psychological observations. (7) Reflex and voluntary movements which follow certain stimuli. (8) Pure self-observation, where, *e. g.*, one requests an author to describe his methods of work

The author next discusses the method of gathering together answers and working out the results. The experiment is always influenced by factors dependent upon the observer, upon the experimenter, or upon accidents. Each such factor is to be investigated together with their relations of mutual interdependence, and they thus furnish principles for the gathering together of results. In skin experiment, *e. g.*, the strength of the stimulus, the strength of the sensation, the character of the sensation, the concentration of attention, the knowledge and previous experience of the observer, etc., are all to be considered. Each factor in turn is to be altered while the others remain constant. As a matter of fact, the others do not remain unchanged from one experiment to another. Habit, adaptation, weariness, variations of attention, etc., make it impossible to retain them all unchanged; and this makes it necessary that the observer alter his plan and method somewhat with each new experiment, and exercise the utmost possible care and foresight. After each experiment any unusual experiences or side-factors should be described; but questions must not be asked in a fixed order or number, as this leads the experimenter to devote his attention to these side phenomena and thus prevent the normal progress of the experiments.

In the choice of experimenters it should be remembered that some have prejudices either as to the experiments or the method; some soon form a theory as to the problem investigated and answer according to their theory; and very many are curious to know results and accordingly ask questions in regard to them. Among the general conditions are to be mentioned variations of the attention, adaptation through exercise and practice, knowledge of the object and of the method, the mood of the experimenter, sleep, hypnotism and many others.

The problems of experimental psychology are represented as (1) to describe psychic phenomena as accurately and completely as possible under different conditions, (2) their relations of interdependence, (3) their influence on each other, and (4) their relation to outer processes. One may investigate these problems (1) in respect to those processes which are common to all men; (2) among processes which are shared only by particular classes of human beings, children, the abnormal, the extremely aged, etc.; (3) with regard to the individual differences of men in their psychic processes. The first group of the last classification embraces the whole of general psychology; the last two, individual psychology. G. TAWNEY.

LEIPZIG.

Der Hypnotismus. Seine psycho-physiologische, medicinische, strafrechtliche Bedeutung und seine Behandlung. AUGUST FOREL. 3. verbesserte Auflage mit Annotationen von Dr. O. Vogt. Stuttgart, Verl. v. Ferdinand Enke. 1895.

In the new and enlarged edition of the work of this well-known author special reference must be made to the annotations written by Dr. Oscar Vogt, of Leipzig, a pupil of Forel's, in which he—an adherent like Forel of the so-called association psychology—endeavors to explain the effects of suggestion as arising from the brain mechanism. It is, doubtless, well known that to Prof. Forel are due in a great measure the scientific diffusion and increased recognition of the suggestion theory in Germany, as well as the destruction of the Charcot theory. Since he first became interested in the subject through Bernheim, he has fought with indefatigable zeal on the side of the school of Nancy. This work, written in the same spirit, forces the conviction upon the reader that the author is sure of himself and that entire recognition of his point of view is no longer far distant. The great value of the book lies in its practical usefulness. The author understands how to initiate the beginner in the clearest and most intelligible way in the practical management of all branches of the theory of suggestion with reference at the same time to all related literature. There can be no doubt but that this new edition of his work will win new friends for both author and subject far beyond the bounds of his native country. Besides the practical introduction to the subject itself, the author presents a theory of consciousness on which he lays great stress, rightly holding a psychological comprehension of hypnotism indispensable to the successful execution of hypnotic experiments. Forel holds monistic views. Conciousness is to him only 'the subjective form of appearance of the activity of the nerves,' or 'the inner reflection of a part of the activity of our cerebrum.' "Living nerve-substance, nerve-activity and consciousness are only three forms of appearance of the same thing in relation to us, analytically abstracted by us and in no way differing from each other. Subjectivism, power and matter are in essence the same and appear on earth in their most perfect and complicated form as cerebrum and the soul of man." Without entering fully into these questions, I may remark respecting the theory of consciousness and the psychological deductions of the work that, holding other fundamental views, I cannot agree in all particulars with the explanations and consequences either of Forel or of Vogt, notwithstanding that to the latter I owe personal thanks for some enlightenment as to the nature of hypnotism. In conclusion I refer the reader to Wundt's 'Hypnotismus and Suggestion,' a work, I may here add, described by Dr. O. Vogt also in his latest publication (Zeitschrift für Hypnotismus, etc., III, Juli–Sept.-Heft.) as of the highest importance.

LEIPZIG. FRIEDR. KIESOW.

VISION.

Die Arten des Sehpurpurs in der Wirbelthierreihe. ELSE KÖTTGEN und DR. GEORG ABELSDORFF. Sitzungsber. der Akad. d. Wissensch. zu Berlin, 25. Juli, 1895.

It is known that there is more than one form of the visual purple, but Kühne was not able to determine whether the different forms consist of two definite types or whether there are intermediate stages. Miss Köttgen and Dr. Abelsdorff now show that the former is the case. They examined specimens of all the classes of vertebrates—sixteen species in all—and they find very close coincidence in the absorption curve of the fishes, on the one hand (of which eight different kinds were examined), and of all the other vertebrates, including man, on the other hand; for the other vertebrates the maximum absorption is at 500 $\mu\mu$, and for the fishes at 540 $\mu\mu$, more in the yellow green, corresponding to the fact that is more bluish in appearance. The fact that there is no visual purple in the rodless retinas of most reptiles they confirmed in the case of the turtle—even a concentrated solution of sixteen retinas, extracted with the greatest care in red light, gave no trace of it. The reptiles which have rods, the chameleon, the crocodile and the boa, they did not examine on account of the costliness of the material. [The reviewer does not find that the absorption spectrum of sea water has been determined. It would be interesting if it should turn out that the agent for absorption in the eye of fishes is adapted to the light to be absorbed in deep water, which is the fish's darkness.]

Sur la théorie de la vision des couleurs. DUFOUR. Congrès intern. de médecine. Rome. 1894.

This paper deserves mention as one more instance of the apparent impossibility of bringing about a widespread knowledge of facts of color-vision which ought by this time to be the property of everyone, at least, who writes upon the subject. Dufour had several cases of total color-blindness, and by experiments in the sorting of colored wools in accordance with their brightness, and by unquantitative estimations of the brightness of the different parts of the spectrum, he comes to the conclusion that the brightness' maximum for the totally color-blind lies in the green. But it has already been shown by Hering and Hillebrand, and by König and Dieterici, by means of the most accurate measurements, not only that the maximum is in the green, but that it is at a definite wave-length in the green. The author then maintains that upon the theory of Hering, according to which only the sensations of black and white and their mixtures remain in cases of total color-blindness, it is impossible to explain why the maximum should fall in the green; he does not say, however, why we should find it any more easy to explain its falling in any other part of the spectrum. The fact in question is, according to Dufour, readily explained upon the theory of Helmholtz, with the aid of the assumption that what the individuals in question see is really green and not grey. In saying this the writer merely shows that he is unaware that we all have this same colorless scale of sensation in a faint illumination, and in the periphery of the eye at all illuminations, and that its curve of distribution through the spectrum is coincident with that of the color-blind. It would, therefore, be impossible to suppose that the sensation of the totally color-blind is green, even if it were not for the fact that we have cases of monocular total color-blindness, in which it is known to be grey; and Helmholtz himself had, in fact, long ago virtually given up this position. It is far more important that whoever argues the intricate question of color-vision should argue within the bounds of easily accessible facts, and also of elementary principles of logic, than that the theories of Hering or of Helmholtz, inadequate as they are, should be disproved in the briefest possible time.

As this was the only contribution to color theory made by the Congress at Rome, and as it was received without discussion, apparently, it would not seem to indicate a very great interest on the part of physicians in color sensations or in their theoretical handling.

Étude sur les Cones et les bâtonnets dans la région de la fovea centralis de la rétine chez l'homme W. KOSTER (Utrecht). Arch. d'Ophtalm. V. 428–437. July, 1895.

Koster has considered it to be desirable, before finishing his study of the Purkinje phenomenon, to re-examine the retina carefully with the purpose of determining the exact extent of the coneless region about the fovea; this has been done hitherto only incidentally, as it is only since controversy has arisen as to whether the rods alone are the seat of the Purkinje phenomenon or not, that the subject has been of so much importance. Koster had only a small amount of very good material, but the material is so difficult to get (it is useless to examine an eye so late as two hours after death) that he publishes his method at once in order that others may be spared the loss of time involved in tentative experimenting. His conclusions, based upon four cases, are as follows:

Region in which the cones dominate (diam.).........8 mm.
Region in which there are no cones at all...............5 mm.
Bed of the fovea.. .2 mm.

Dimmer gives 1.4 to 2 mm as the diameter of the fovea, but he counts from the beginning of the declivity. Koster reserves discussion of this result until a later occasion.

Die Cardinalpunkte des Auges für Verschiedenfarbiges Licht. W. EINTHOVEN. Pflüger's Archiv., LXI. 1895.

The effect of dispersion upon the cardinal points of the eye has not been calculated except in the case of the focal points, and in that case only for Listing's reduced eye with one refracting surface. In view of recent discussion by Schapringer, König and others, Einthoven has found it desirable to carry out the entire calculation for the actual eye. Of chief importance for the phenomena of color diffusion in the eye is the position of the second nodal point and of the second focal point. He finds that the former is for blue rays 3μ in front of its position for yellow rays, a difference so small that it can be neglected in cases where a relative change of position of differently colored retinal images is to be investigated. The distance between the focal points for blue light and for red light is .248 mm., as against .193 for the reduced eye.

C. LADD FRANKLIN.

BALTIMORE, MD.

MEMORY.

On Memory and the Specific Energies of the Nervous System. PROFESSOR EWALD HERING. Eng. trans. * * * Chicago Open Court Publishing Company. 1895. Pp. 50.

This is a good translation of two brief essays, the first being a popular address delivered in 1870 before the Imperial Academy at Vienna. Like all of Hering's work, it is vigorous and suggestive, and will prove especially so to lay readers. To those at all closely in touch with contemporary psychology it will possess little interest beyond that which always attaches to a clear statement of any doctrine, for most of its contents concern matters which are to-day psychological commonplace. The general thesis is the dependence of reproductive mental processes, both sensory and motor, upon the retention in protoplasmic structures, such as the nervous system, of modifications occasioned by previous experiences. The important distinction between the mere reproduction, or representation, of mental states and the reproductions of true memory—in Prof. James' sense, for instance, involving the conscious recognition that the reproduced fact has been a part of one's own past experience at a definite time—is never alluded to. The point, so often misty in other writers, is clearly made, that unconscious memory (?) and unconscious mental (?) processes are simply tantamount to neural activities of such character and intensity as do not awaken their counterparts in consciousness. In the broad sense all organic structures manifest a kind of memory, in so far as they retain the modifications of past experience. The more permanent among these modifications occurring in the nervous system are transmitted from generation to generation, emerging in the new-born individual as instinctive acts—a statement which may require to be edited anew in the light of Weismann's work.

In the second essay, which is much less clearly written, the problem of the specific energies of the nerves is discussed more or less in the light of the foregoing doctrine. The author apparently posits ultimate and specific differences of function as properties of protoplasm, which differences are called out, developed, and at length firmly embedded in the growing nervous system through the agency of repeated stimulations of similar character. The manifold views of other writers upon this topic gain no notice, and some of the statements made are flatly contradictory of the widely-credited work of other scientists—for instance, Goldscheider's work on temperature sensations. Still, it is all very entertaining, and we venture to hope the translator will see fit to render accessible to English readers Hering's much more important work upon the color sense.

UNIVERSITY OF CHICAGO. JAMES R. ANGELL.

EXPERIMENTAL.

Observations comparatives sur la reconnaissance, la discrimination et l'association. B. BOURDON. *Revue philosophique,* XX. 154–185. August, 1895.

As the title indicates M. Bourdon proposed to investigate the interrelations of recognition, discrimination and association, but the result is rather three minor studies.

(1) *Recognition.* Series of words, or letters, were read aloud in which one of the words occurring near the beginning of the series was repeated later on, and the number of times its occurrence was recognized was determined. Thus, for example, in a series in which the word *restaurant* was the fifth of the series and again the twenty-second it was recognized 60 times in 65 trials. The word was of course more likely to be recognized if first in the series or if the intervening words were few. Words were more likely to be recognized than letters, and dissyllables than monosyllables. A word is more likely to be recognized if interesting, and thus the method may be used to determine what ideas are of most interest. It would seem that those concerned with eating and drinking attract the attention most forcibly.

(2) *Discrimination.* Three series of printed letters were used—one a passage from a book, one of letters 1.75 mm. high not making words and one of letters 1.25 mm. high not making words, and the observer was required to mark as many letters of a given sort as he could in four minutes. Thus for example in four minutes 1,693 letters were read, and 216 of the 223 a's were marked. When it was necessary to mark six different letters 503 letters were read and 255 of those 273 occurring were marked. The size of the letters did not make any evident difference. M. Bourdon concludes that the letters not marked take up about one-tenth as much time as those marked. He notes the interesting fact that most observers can mark all the a's in a list more quickly than they can discriminate all the letters, and attributes this to the circumstance that in discriminating the letters there is with most observers a tendency to articulate them. This, however, is probably not the correct explanation. The present writer has found that observers can discriminate and articulate about six letters per sec. when the letters make words, and about three letters per sec. when they do not make words. The rate is limited by the time of discrimination, not by the time of articulation, which is reflex and overlaps the discrimination of the following letters. Observers can mark 100 A's

on a list of 500 letters at the rate of about one per sec., in which case they must cursorily discriminate about four letters per sec. in addition to the A's. This cursory discrimination consists in seeing that a letter is not A, which is easier than seeing what letter it is. In making the experiment the A's seem to stand out from an undiscriminated complex. In reading proof one can often see an inaccurately printed word by glancing at a page, and one can see that the word is incorrect before one recognizes the nature of the error.

(3) *Association.* M. Bourdon collected verbal associations on the familiar lines of exhibiting words and letting the observer write down the suggested words. He classifies the results according to the percentages of nouns, verbs and adjectives suggested, which would scarcely seem to be as satisfactory as the classifications used in similar experiments by others. M. Bourdon concludes that students of letters show greater versatility, and students of science greater stability, in their associations.

It will be seen that M. Bourdon's experiments are of interest, but they were not conducted nor are they described in accordance with what the present writer regards as the best scientific method. Basing new work on work already accomplished, and giving such statement of results as may be the basis of further work, is the method that advances science. J. McK. C.

De l'influence de la perception visuelle des corps sur leur poids apparent. TH. FLOURNOY. L'Année psychologique, 1894. I., 198–208.

Although psychologists generally agree in discarding innervation sensations, says M. Flournoy, yet the immediate knowledge of the outgo of energy in voluntary effort seems so directly evident to consciousness, that there is a call for some thorough and crucial demonstration of its fallacy, apart from pathological cases. For this purpose the writer selected ten objects of different sizes, but exactly the same weight (112 grams); the largest was a wooden box of 2,100 cu. cm. content, the smallest a metal case of 10 cu. cm. filled with lead. The subjects (50 in number, in the first set of experiments) were asked to arrange the objects in order of weight. The wooden box was judged lightest by 84%; the next largest article was given second place by 50%; and throughout the series the average judgment made the object heavier as it decreased in size, the metal case being placed last by 90%. The individual variations show, however, that habitual associations also affect the judgment of certain objects.

To eliminate any possible effect from the area of skin touched, M. Flournoy devised a second set of tests, in which the weights were lifted only by means of a string and ring. Out of 31 persons, 29 placed the box first (*i. e.*, as lightest), and 30 placed the metal case last. These results directly contradict those reported by Charpentier, in the Arch. de Physiologie (1891, p. 127). On closing the eyes, the difference disappeared.

Of 44 persons asked to state the weight of the objects, the average made the (supposed) heaviest 253 g. and the lightest 52 g. The divergence was greater in women than in men, and in literary men than in scientists. Of 30 asked to assign the relative weight, the average made the (supposed) heaviest 5.2 times the weight of the lightest; but when asked to add weights to the lightest till it equalled the heaviest, the average result was a mere doubling of its weight.

The illusion persists even after its illusory character is known; it is present in persons of every age. It is shown to be due to the volume of the object, rather than the area of contact. Finally, it is a direct argument against the innervation feelings, which ought to correct or even over-balance such an error of judgment. As an important datum bearing on the subject, I should suggest to M. Flournoy that he examine the case of postal clerks, who are commonly supposed to be able to detect slight differences of weight by mere lifting.

PRINCETON. H. C. WARREN.

Recherches graphiques sur la musique. A. BINET ET J. COURTIER. Revue Scientifique, 6 Juillet, 1895.

MM. Binet and Courtier have devised an apparatus for recording the normal movements of piano players which promises results valuable to both music and psychology. It consists of a rubber tube six millimeters in diameter running along directly under the keys and connected at both ends with an elastic drum which carries the recording style. The tracing is taken, as usual, on smoked paper. Errors that might arise from inertia of the apparatus are avoided by inserting in the tube a diaphragm with capillary opening. When a key is struck the style is deflected in such a way that the height of the deflection is proportional to the force of the pressure; the length of the deflection records the time; and finally the form of the curve gives a detailed account of the manner in which the movement was carried out. The whole apparatus is out of the player's sight and the tube is so adjusted that it does not increase appreciably the resistance of the keys, thus insuring entirely normal movements.

A number of interesting results are described, showing that irregularities which escape even trained ears are plainly seen in the curves and that the differences which distinguish good execution from poor are easily studied.

A series of experiments made with this apparatus will be reported in the next number of l'Année psychologique. C. H. JUDD.

LEIPZIG.

THE FEELINGS.

Review of A. Lehmann's Work, '*Die Hauptgesetze des Menschlichen Gefühlslebens.*' TH. LIPPS. Göttingsehen gelehrten Anzeigen, 1894, Nr. 2, pp. 85–117.

A strongly written attack on the 'Lange-James' theory of emotion, Lipps maintaining that the primary psychological phenomenon in feeling is always the disturbance produced by the stimulus upon the Subject's system of *ideas*. The consequent bodily alterations hardly contribute at all to the emotion properly so-called, since the sensations which they yield are easily distinguished therefrom, as when Lipps himself is so 'touched' with sympathetic happiness in reading of romantic situations that he gets a distinctly painful constriction of the throat, which not only does not constitute, but positively conflicts with his emotional happiness. Moreover, if feelings were made of sensations, how could a 'self' arise? A certain group of sensations makes *my* body, because it is tied to feelings and strivings. These latter are the immediate *I*, and render *mine* whatever sensational content they go with, rendering alien whatever sensational content they separate from. They cannot themselves be sensational content. W. J.

Les emotions: Étude psycho-physiologique. Lange. Trad. Francaise de G. DUMAS. Paris, Alcan. 1895. Pp. 198.

A few words only are needed to anounce the French translation of this well-known book for the theory of emotions, which it elaborates, is at present causing much discussion among psychologists. It is, however, interesting to note that the principal argument by which Lange seeks to prove that the emotions are the result of vasomotor changes is that it is incontestable that many emotions have a purely physical cause; for example, the exhilaration due to wine, the excitement and anger produced by certain drugs and the various emotions which in many diseases accompany abnormal conditions of the body. It would seem that the critics of the new theory of Lange and of James have not taken sufficient account of this argument.

A. B.

The Conscience, Its Nature and Origin. WILLIAM W. CARLILE. International Journal of Ethics, October, 1895. Pp. 63–77.

This article is in the main a protest against the view of Herbert Spencer that it is the experience of rewards and punishments accorded in this world during many generations to right and wrong action respectively that has evolved the conception of duty. Such a view postulates a psychological hypothesis of transformations that follow the analogy of chemistry, *i. e.*, a metamorphosis from pleasure and pain elements into ethical ideas of justice, the right, the true, etc., which we cannot expect to understand. Such hypotheses, transcending the understanding, lead to paradoxes and absurdities, and rule out the *reductio ad impossibile* in mental science, where a refutation of a false induction may be attained by a simple appeal to fact. In psychology, it is impossible to appeal to a fact, to an ethical sentiment, for instance, in the same way as appeal may be made to the physical phenomena evidenced by the senses; nevertheless, appeal may be made to the circumstances which occasion the sentiment in question. Thus an objective reference may be made indirectly. And this is analogous to the operation of mechanical forces, that is, where the cause is seen in the effect, and not an obscure metamorphosis akin to chemical changes. Causation in the sphere of ethical phenomena must be intelligible. It will not do, therefore, to derive a sense of justice, either directly or indirectly, from fear of punishment.

How, then, can a law enforce itself without hint of penalty? The answer may be suggested by two acknowledged psychological phenomena. One is that a representation in the mind of the act contemplated always precedes the actual volition and consequent realization of the volition in conduct. And the second is that the conception of ourselves is moulded on the conception of others. A judgment of self is the reflected judgment of others. Therefore when we are tempted to a mean or wrong act it must first appear before our minds as a presentation that may be rendered actual in conduct, and, moreover, it is represented *sub specie alius*. The contemplated act arouses resentment against any who would commit, or whom we think of at the moment as committing it, but wholly in an impersonal way; then we transfer the resentment to ourselves considered as committing it. This transfer makes the act personal. The consequent feeling of disapprobation has arisen through a direct line of causation, after a mechanical and not a chemical analogy. This transfer of resentment accounts for the prohibitive aspect of conscience; its positive aspect as an incentive to all virtue is accounted for by a like transfer of gratitude. This view is, therefore, superior to the Kantian view which accounts only for a negative virtue. This view is illustrated in the ethical grandeur of Hellenism, which the author thinks has been unfairly disparaged by Matthew Arnold.

I am, on the whole, in sympathy with the writer's main contention that fear of punishment, however developed, and through increasingly complex associations, be they ever so many, can never be transformed through a psychological metamorphosis into a sense of honor, or of justice, or a regard of duty for duty's sake alone. Yet I feel that he can not summarily rule out all explanations of psychological phenomena, which are of the chemical rather than the mechanical type; for instance, the relation of the purely physiological to the psychological phenomena can not be explained by transformations of the mechanical kind, where the cause is seen in the effect. Moreover, the comparison between self and others and the consequent transfer of the feeling of resentment or of approbation does not wholly account for the rise of the moral sentiments. We might ask, whence the original feeling of resentment in an impersonal way concerning the acts of others? And in the transfer to self there seems to me to be a supplementary comparison overlooked by Mr. Carlile, namely, the comparison between the possible self, conceived as agent of the contemplated act, and the ideal self, of which the act in question would be unworthy. There is, moreover, a tendency on the part of the writer to identify the true and the right, where, for instance, he says that 'Ought' is the formula of deduction in speculative truth as well as in ethics. The ethical 'ought,' however, has a deeper significance and produces peculiar psychological effects, as the consequent emotions attendant upon its presence in the mind, and depending upon the will's response to its behests. The true may cause but the minimum of emotional functioning, and may deliver no command to the will; the right, however, speaks always with authority, stirring emotional depths, and resulting in conduct accordingly.

JOHN GRIER HIBBEN.

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The Philosophy of Lotze — Doctrine of Thought. H. JONES. New York. Macmillan & Co. 1895. Pp. VIII.+375.

This volume, the first of two on the philosophy of Lotze—the second will deal with his metaphysics—is a criticism from the Neo-Hegelian standpoint of the Lotzean Epistemology contained in the

Logic. In brief, it attempts to show that the Lotzean view, by its inherent contradictions, refutes itself, and thereby demonstrates more clearly the validity of a Hegelian position. The author's English is fresh and vigorous, and usually clear; but at times his desire to be lucid, especially in expounding Lotze, leads him into needless and oftentimes wearisome repetitions.

The first chapter outlines Lotze's general position in philosophy. He is critical rather than constructive; in fact, says Jones, he has no real system. He attempts a *via media*, avoiding on one side the limitations of the merely scientific view, and on the other the extreme panlogismus of Hegel. 'The main endeavor of his life was to refute' the Hegelian identification of knowledge and reality (p. 34). He also undertook to vindicate the unrecognized claims of feeling. The resemblance between Kant and Lotze that each bases his metaphysics upon ethics is emphasized and made the premise for the very doubtful conclusion that 'those who really know Kant have little need of Lotze' (p. 17). Now since, reasons Jones, Lotze's extreme divergence in his Metaphysics from Hegel is based logically upon his divergence from the latter in his view of thought, it follows that the refutation of the Lotzean Epistemology will involve the complete destruction of his Metaphysics. The remainder of the book is the attempted refutation.

The essence of Ch. II. is an elaboration of the three following 'limitations' of thought made by Lotze: 1. Thought is not reality, nor is reality thought; knowledge by its very nature is subjective. This is not scepticism, says Lotze, for thought should not claim *to be* things, but only *to be valid* of them. 2. Not only is thought simply an activity of the soul, it further is only a part of the soul's activity. Feeling and conation are equally coördinate, incommensurable ultimates. Lotze argues (Jones disagreeing) that since to the feelings, we owe our impulses towards, and our ideals of, the True and the Good, and hence the data for our judgments of worth, a philosophy of the feelings is more important than a philosophy of mere thought. 3. A third 'limitation' is the assignment of thought to only the higher (formal) intellectual functions.

Ch. III., IV., V., and VI., deal with the third 'limitation,' aiming to show that 'thought shorn of its pretensions' is nugatory. Ch. III. treats of Perception and Conception. Lotze defines the function of thought as making coherent the ideas given by associative consciousness as merely coincident. Jones points out that Lotze leaves it ambiguous whether thought discovers the coherence or produces it.

He evidently misinterprets Lotze's position, representing him (p. 105) as holding that we first *know* a subjective state, which thought then objectifies. On the contrary, one of Lotze's firmest convictions is that the first mental act involves no knowledge whatever.

Viewing thought as formal, Lotze soon recognizes that conception cannot furnish necessary coherence among coincident phenomena, and so passes to Judgment (Ch. IV.). But beginning with the categorical, and passing successively through the Conditional and Disjunctive, he fails to find the necessary coherence. Nor is he more successful in Inference (Ch. V.). Subsumptive Inference in its three forms, Syllogism, Induction and Analogy, cannot produce coherence. "These processes, if synthetic, appear invalid, and if valid, they seem tautological." Inference by Substitution can give us coherence, but only of abstract quantity and not of real phenomena. Inference by Proportion possesses a similar weakness. Classification by type fails because it is not concerned with the real world of change. Finally, Systematic Explanation can give coherence, only provided the whole system is known, but since this is impossible, this method also is inadequate.

In Ch. VI., Jones argues that thought as formal cannot perform even the function assigned it by Lotze. The argument does not seem conclusive. He says: "We have to condemn either Lotze's view of thought as formal, or all knowledge as uncertain, except mathematics." (p. 257.) A few pages further he admits the uncertainty of all knowledge. "Scientific systems, including mathematics itself, will remain hypothetical, and the truth they contain will rest upon unverified assumptions." (p. 266.)

Ch. VII. and VIII. deal with the first and second limitations of thought advanced by Lotze in Book III. Part of Ch. VII. is devoted to an exposition of the first limitation, criticism being reserved for Ch. VIII. The rest of the chapter treats of the second limitation, being a criticism of Lotze's view of feeling. Lotze regards feeling as the test of the ultimate principles of knowledge. Jones demurs. The very fact that Lotze regards these principles as needing scrutiny proves that the test is really logical, viz., the coherence of elements in a system. Again, he attacks Lotze's view that our belief in a real world is based on feeling, not thought, because feeling is the source of our judgments of value. But, says Jones, it is absurd to speak of judgments of any kind without thought. Lotze confuses feeling with the knowledge of objects as worthful that accompanies the feeling (p. 298). This criticism seems irrelevant. Lotze did not mean that a judgment of worth could be made without thought, but that feeling supplies the content or data (moral emotions, etc.), which thought can never furnish.

In Ch. VIII. on The Principle of Reality in Thought and its Processes, one is surprised to find the easy manner in which Jones disposes of Lotze's basal position that 'Thought is *valid* of reality.' It is not 'worthy of serious discussion' (p. 333, note). The ignoring of this point materially strengthens the criticisms upon Lotze.

The value of the work, as a whole, lies chiefly in the many admirably clear expositions of Lotzean theory, its criticisms and the ambiguity of several of the terms used constituting its chief weakness.

W. J. SHAW.

WESLEYAN UNIVERSITY.

Time and the Succession of Events. J. L. McINTYRE. Mind, July, 1895. Pp. 334.

Mr. McIntyre in general criticizes the Kantian doctrine and expounds Lotze.

The modern problem is—'Does time belong to the ultimatey real'? If it is only an appearance for us, ultimate reality becomes incomprehensible, whether it be called the 'unknowable' of Spencer or the 'harmonious experience' of Bradley. If it belongs to the Real, we must show in what sense it can be predicated.

Our criterion must come from experience. Merely logical criteria, as Bradley's 'self contradiction' too plainly shows, are purely formal and tell us nothing. For him there can be no positive statement. Any such would involve unity and diversity. We are reduced to a barren identity.

Leibnitz denied the absolute reality of time, but admitted a real succession of events. Kant held that time was an *a priori* form. Lotze took up the argument where Kant left off and showed, that while empty time is a mental abstraction, the appearance of change involves change in the Real. A concrete succession of events must be admitted in the real. Every event is an act, every act is the act of a subject, not it, but expressed in it. The succession of events implies the immanent action of an absolute subject. As unity it is above time and incomprehensible. But the subject can only be a unity in expressing itself in the diverse succession of events. These are inseparable aspects of the real. Past and future are alike constructions: The permanent changing present alone is the real. An adequate conception of this 'present' is difficult to form.

S. F. McLENNAN.

NEW BOOKS.

Der Kampf um einen Geistigen Lebensinhalt. R. EUCKEN. Leipzig, Veit. 1895. Pp. viii+400. 7.50 M.

Die Moderne Physiologische Psychologie in Deutschland, mit Besonderer Berüchsichtigung des Problems der Aufmerksamkeit. W. HEINRICH. Zurich, Speidel. 1895. Pp. iv+205. 40 M.

Psicologia per le Scuole. G. SERGI. 2d ed., revised. Milano, Dumolard. 1895. Pp. vii+227.

Mental Development in the Child and the Race. Methods and Processes. J. MARK BALDWN. 2d ed., corrected. New York and London, Macmillan & Co. 1895. Pp. xvi+496. $2.60.

Johnson's Universal Cyclopedia. New edition, Vols. V–VIII. New York, Johnson Co. 1895.

The Conception of God. J. ROYCE. Address before the Philosophical Union of the University of California. With comments by S. E. MEZES, J. LE CONTE and G. H. HOWISON. Berkeley, California, Executive Council of the Union. 1895. Pp. 84. 50 cts.

Mental Physiology. T. B. HYSLOP. London, J. & A. Churchill. 1895. Pp. viii+539.

Sur le mécanisme du sommeil. L. ERRERA. Bruxelles, Hayez. 1895.

Histoire de la philosophie atomistique. LÉOPOLD MABILLEAU. Paris, Alcan. 1895. Pp. vii+560.

Etude sur le temps et l'espace. LECHALAS. Paris, Alcan. 1895.

Le psychisme expérimental. A. ERNY. Paris. Flammariom. 1895.

Théorie de l'âme. ALAUX. Paris, Alcan. 1895.

The Psychic Development of Young Animals. WESLEY MILLS. Montreal. 1895.

Geschichte der neueren Philosophie, 1ter Bd. Aus dänischen übersetzt. REISSLAND, Leipzig. 1895.

NOTES.

THE *Archiv für system. Philosophie* (Heft 4, Bd. I., Oct., 1895) publishes a 'Bibliographie der philosophischen Litteratur des Jahres 1894' comprising 1298 titles, together with a 'Namenregister' to the same. It gives no arrangement of titles, either alphabetical or other, under the different sections, but, on the other hand, the headings are frequent and the classification detailed. There is no section devoted to 'Neurology.' 'Biology' has 11 titles, and 'Physiologisches Grundlagen' (under 'Psychologie'), 35.

THE first number of a new journal called *Zeitschrift für immanente Philosophie* has appeared. It is edited by M. R. Kauffmann, with the coöperation of W. Schappe and R. v. Schubert-Soldern, all of whom have articles in the first number. (Berlin, Salinger, quarterly, 9 M. per volume.) The editor states the object of the journal as follows: "Noch Möglichkeit alle Anhänger der Grundprincipien des idealestischen Monismus und diesem verwandten Auffassungen in gemeinsamer Thätigkeit zu vereinigen." It has no literature or other auxilliary sections.

IN May of the present year the Universities of St. Petersburg, Moscow and Kieff replied to an inquiry from the Minister of Education unanimously favoring the establishment of laboratories of psychology in all of these universities. A committee of eight professors from the University of Kieff has petitioned for about $3,000 for the establishment of a laboratory of psychology, and a yearly appropriation of $300.

A LABORATORY of experimental psychology has been fitted up at the University of Kansas under the charge of Olin Templin, professor of philosophy.

KANT's manuscripts, belonging to the University of Dorpat, have been placed by the Russian Government at the disposal of the Berlin Academy of Sciences, which is preparing to issue a complete edition of the philosopher's works.

PROFESSOR E. HERING, who succeeds Ludwig at Leipzig, offers lectures on the 'Physiology of Sensations and Movements.'

DR. HERBERT NICHOLS, formerly instructor in psychology in Harvard University, has been appointed lecturer in psychology in Johns Hopkins University.

DR. EDGAR PIERCE has been appointed instructor in psychology in the University of Michigan.

DR. C. VON TWARDOWSKY, *privatdocent* in the University of Vienna, has been elected assistant professor in philosophy in the University of Lemburg.

DR. JOHANNES GAD, of Berlin, has accepted a call to the chair of physiology in the University of Prague, vacated by Professor Hering.

MSS. intended for publication in THE PSYCHOLOGICAL REVIEW or in the MONOGRAPH SUPPLEMENTS, and books, etc., intended for review during the year 1896 should be sent to Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

VOL. III. No. 2. MARCH, 1896.

THE PSYCHOLOGICAL REVIEW.

PROCEEDINGS OF THE FOURTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, 1895.

REPORT OF THE SECRETARY AND TREASURER FOR 1895.

The fourth annual meeting of the American Psychological Association was held at the University of Pennsylvania, December 27 and 28, 1895, that place having been selected with reference to the simultaneous meetings of the American Society of Naturalists and other affiliated societies. There were present the following members: Baldwin, Cattell, Chrysostom, Farrand, Fullerton, Gardiner, Griffin, Hyslop, James, Ladd, MacDonald, Marshall, Miller, Mills, Newbold, Nichols, Patrick, Sanford, Seth, Shaw, Strong, Warren and Witmer—twenty-three in all. Morning and afternoon sessions were held on both days, President J. McKeen Cattell presiding. Abstracts of the papers presented so far as they have been received by the Secretary are appended.

At the regular business meeting and in the intervals of the regular program, the following business was transacted. Election of officers for the ensuing year: *President*, Prof. George S. Fullerton; *Secretary and Treasurer*, Dr. Livingston Farrand; *Members of the Council*, Profs. E. H. Griffin and E. C. Sanford. Elected to membership on nomination of the Council: Dr. H. Austin Aikins, Western Reserve University; Dr. C. H. Bliss, University of the City of New York; Dr. Franz Boas, Museum of Natural History, New York; Prof. E. D. Cope, University of Pennsylvania; Prof. J. E. Creighton, Cornell University; Prof. Warner Fite, Williams College; Mr. J. E.

Lough, Harvard University; Prof. C. S. Minot, Harvard Medical School; Dr. E. A. Singer, Harvard University; Dr. W. G. Smith, Smith College; Dr. Norman Wilde, Columbia University.

On motion of Professor Baldwin, the Association voted that a committee of five, including the President of the Association, be appointed to consider the feasibility of coöperation among the various psychological laboratories in the collection of mental and physical statistics, the committee to report at the next regular meeting of the Association. The following members were later appointed on this committee: Baldwin, Jastrow, Sanford, Witmer and Cattell (chairman). The question of the formation of a philosophical society or a philosophical section within the present Association was, after a brief discussion, referred to the Council with full power to act. It was voted that members attending the International Psychological Congress in Munich, in 1896, be empowered to act as delegates of the Association, when qualified by notice given by them to the Secretary of the Association. A vote of thanks for the hospitality shown by the University of Pennsylvania and the local committee of arrangements was unanimously passed. The time and place of meeting were, by vote, referred to the incoming President to be fixed by him in conference with the presidents of the Society of Naturalists and other societies meeting simultaneously.

REPORT OF THE TREASURER:

Receipts:

From retiring Treasurer (Prof. J. McKeen Cattell)	$127 17	
Dues	180 00	
Sale of Proceedings	50	
		$307 67

Expenditures:

Printing	$13 60	
Postage	7 15	
Stationery	1 73	
Expressage	90	
		$23 38
Balance on hand		$284 29

Of this amount $100.00 is on deposit in the Mechanic's Savings Bank, Worcester, Mass., and $158.00 in the Worcester Institution of Savings. These deposits have drawn interest to an estimated amount of $7.00.

Audited by the Council and found correct.

E. C. SANFORD.
Secretary and Treasurer, 1894–95.

ABSTRACTS OF PAPERS.

(1) *Address of the President.* By PROF. J. MCKEEN CATTELL, Columbia University.

The address reviewed the history and recent progress of psychology and the part played in its development by experiment and measurement. Psychology is by no means a new science, but its growth during the last few years has been rapid, and it now rivals the other leading sciences in productiveness of research and publication and in academic position. Science is either genetic or quantitative, and psychology is advancing in both directions. The problems that can be treated in the laboratory were reviewed, and it was claimed that these have added directly and indirectly new subject-matter and methods, have set a higher standard of accuracy and objectivity, have made some part of the subject an applied science with useful applications, and have enlarged the field and improved the methods of teaching psychology. In conclusion, the relations of psychology to the other sciences and to philosophy were reviewed, and their interdependence was emphasized.

[This address is printed in the present number of THE PSYCHOLOGICAL REVIEW.]

(2) *Psychology and Physiology.* By PROF. GEORGE S. FULLERTON, University of Pennsylvania.

This paper was in a sense a sequel to the paper read two years before, entitled 'The Psychological Standpoint.' It attempted to draw the line between two sciences which touch each other closely, and was divided into two parts. The first part assumed the 'automaton' theory of mind to be the true one, *i. e.*, the theory that mental states form no part of the sensory-motor arc, but are, so to speak, parallel with neural changes. The second part assumed ideas to be either a part of the chain of causes resulting in bodily motions or a something standing outside of that chain and, as it were, breaking in upon it. The author expressed no opinion as to the truth of any one of these theories, but merely inquired where the line dividing psychology from physiology should be drawn in any case. He examined at length a standard work on physiology, that of Professor Foster, and showed that dearth of established physiological data forced the author constantly to abandon his own field and take to psychology. In general, he maintained the thesis that psychology is sufficiently marked out from other sciences by the method it must employ, the method of introspection, observation and experiment, and interpretation; and he deprecated the introduction into text-books on physiology of psychological material as tending to conceal lack of physiological knowledge and to lead to confusion as to the boundaries of the two sciences. [The paper appeared in the January number of THE PSYCHOLOGICAL REVIEW, 1896.]

(3) *Series of Physical and Mental Tests on the Students of Columbia College.* By DR. LIVINGSTON FARRAND, Columbia University.

The tests described are made on the undergraduates of the College at entrance and repeated on the same students at the end of their Sophomore and Senior years.

The object of the tests is to obtain a record for comparative purposes of certain mental and physical characteristics of the students at different times during a period of rather active intellectual growth and at the same time to furnish material for a statistical study of the particular points examined. Stress is laid to a certain extent upon the more purely mental inquiries, such as memory, rate of perception and motor response, accuracy of perception, color vision, etc., but enough physical tests are included to afford a comparison between bodily and mental development, if any relation between the two exists.

(4) *Neuro-Social Data.* By DR. ARTHUR MACDONALD, Bureau of Education, Washington.

TABULAR STATEMENT GIVING QUANTITATIVE MEASUREMENTS OF SENSIBILITY IN PERSONS OF DIFFERENT AGES AND DIFFERENT CLASSES OF SOCIETY.

No.	Classification of Individuals.	Number of persons.	Ages and average ages.	Average least sensibility to distance (locality) bet. two points on volar surface of wrists.		Average least sensibility to heat on volar surface of wrists.		Average least sensibility to pain (by pressure) on temporal muscles and on palm of hand.	
				r.wr.	l.wr.	r.wr.	l.wr.	right.	left.
		1	2	3	4	5	6	7	8
				mm.	mm.	cent.	cent.	kilos.	kilos.
I	Women (highly educated)	23	av. 38	17.3	16.2	2°.1	1°.7	1.253	1.224 (T. mus.)
II	Young Women (wealthy classes)..	11	un. 30	13.6	12.4	4.6	4.4	2.9	2.4 (hand.)
III	Young Men (wealthy classes)...............	10	" 30	12.4	12.7	4.4	3.7	4.7	4.2 "
IV	Boston, Army of ?...	35	av. 28	16.1	15.6			9.5	9.5 "
V	Washington School Children (boys)...	526	6–18	16.3	15.5	3.9	3.8		
VI	Washington School Children (girls)...	551	6–18	14.8	13.8	4.5	3.9		
VII	Boys (parents well-to-do)	205	6–18	16.2	15.2	4.0	3.9		
VIII	Boys (parents poor)..	119	6–18	16.6	15.9	4.0	3.7		
IX	Girls (parents well-to-do)	183	6–18	14.3	13.5	3.9	3.5		
X	Girls (parents poor)..	133	6–18	14.9	13.8	3.9	3.6		
XI	Boys, bef. pub.........	318	6–14	15.7	14.9	3.9	3.6		
XII	Boys, aft. "	208	15–18	17.2	16.3	4.5	4.2		
XIII	Girls, bef. "	186	6–12	14.5	13.8	4.8	3.8		
XIV	Girls, aft. "	362	13–18	15.1	14.0	4.3	4.0		
XV	Col. Chil., boys......	33	6–19	13.9	13.5	2.0	1.7		
XVI	" " girls......	58	6–16	15.2	14.1	2.5	2.4		

The tests for temperature discrimination were made with Eulenberg's thermæsthesiometer; those for pain with the author's own algometer applied to the temporal muscle. All the psychical conditions were made as uniform as possible, especially with the children. Should these results be confirmed by experiments on larger numbers of individuals, the following statements would be probable:

Middle-aged women of the educated classes are much less acute in the sense of locality on the wrist, but much more acute to heat than young women of the wealthy classes (Nos. I. and II., columns 2, 3, 4, 5, 6).

Young men of the wealthy classes are much more sensitive to locality and pain than the men in the Boston Army of the Unemployed (Nos. III. and IV., columns 3, 4, 7, 8).

Young women of the wealthy classes are much less sensitive to locality and heat, but much more sensitive to pain than young men of the wealthy classes (Nos. II., III., columns 3, 4, 5, 6, 7, 8). As to pain, it is true in general that women are more sensitive than men, as shown in a former investigation. But as remarked then, it does not necessarily follow that women cannot endure more pain than men.

Boys are more sensitive to locality and heat before puberty than after. Girls are more sensitive to locality before puberty, but their sensibility to heat is about the same before and after puberty (Nos. XI.—XIV., columns 3, 4, 5, 6).

Colored boys are more sensitive to locality and heat than white boys. Colored girls are less sensitive to locality, but more sensitive to heat than white girls (Nos. VI., and XVI., columns 3, 4, 5, 6). Colored boys are more sensitive to locality and heat than colored girls (Nos. XV. and XVI., columns 3, 4, 5, 6).

The left wrist is more sensitive to locality, heat and pain than the right wrist; only one exception. (No. III., columns 3, 4).

(5) *An Experimental Investigation of the Processes of Ideation.* By MR. OLIVER CORNMAN. Introduced by PROF. LIGHTNER WITMER.

(6) *On Direct Control of the Retinal Light.* An informal Communication by PROF. GEORGE T. LADD, Yale University.

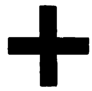
After a few explanatory remarks Prof. Ladd presented the following syllabus of experiments on the phenomena in question which he illustrated by reading extracts from one of the detailed reports secured by one of his experimenters.

VOLUNTARY CONTROL OF THE EIGENLICHT.

Be seated in a quiet room, with the face turned away from the direct light, and shut the eyes long enough to let the after-images fade away before beginning the experiment.

In a preliminary way, note the presence of the 'Eigenlicht,' and record your opinion as to its (1) color, (2) persistence and (3) shape or distribution.

If successful in this, try the following:

EXPERIMENT I.—By persistent and attentive willing, arrange the color-mass of the 'Eigenlicht' into the shape of a cross composed of two equal bars at right angles, thus: 

Record your experience in regard to (1) the time it took to produce the cross, (2) how long it could be retained, (3) modifications of shape, if any, (4) color, and (5) effect of fatigue.

If successful in the above, try the following:

EXPERIMENT II.—Produce, as in Exp. I. (1) a circle, (2) two concentric circles, and (3) a triangle.

Record results in full.

EXPERIMENT III.—Try to produce the circle successively in the colors red, green and violet. Note your power to control the color by will.

EXPERIMENT IV.—Try to project the image of the circle in each of the above three colors separately on a blank sheet of white paper, and note what you see.

Try Exp. I. twice a day for ten successive days (morning and evening), and record date, time of the day, and duration of the experiment.

(7) *Consciousness and Time.* BY PROF. C. A. STRONG, University of Chicago.

The paper presented objections to the account of the relations between consciousness and present time given by Professor James in his Presidential Address at the Princeton meeting (December, 1894), and suggested an alternative account.

Professor James held that the present instant is a mere boundary line between the past and the future; that both the time consciousness is *in* and the time consciousness is *of* must be durations or intervals, and that consciousness must therefore include within its span those portions of the past and of the future which lie nearest to the present instant. Hence the view that "there is literally no such datum" as that of present time, and that "past and future are already parts of the least experience that can really be." Hence, also, the need of assuming, in addition to the simultaneous unity of consciousness implied in the perception of likeness or difference, a successive unity, as implied in the perception of passing time.

In opposition to this account it was urged that, since the present instant is a mere boundary line, an interval or duration may be considered *abstractly*, from the point of view of that instant, and it will then appear to consist of a portion of the past plus a portion of the future; but that, if we wish to consider it *concretely*, and in the way it actually happened, we must conceive it as a line of successive presents, as an onward progress from an earlier present to a later present. Though present time is a point, it is not a resting, but a moving point, and it is this character of motion which affords the room consciousness needs in order to exist. Though change is thus a character of present time and of consciousness which is in it, consciousness may be in present time without being aware of change; for change, when real, is always infinitesimal, an invisible feature of the histology of consciousness.

The consciousness of the succession of two feelings cannot arise simultaneously with the first, nor midway between the two, but only when their succession is an accomplished fact. This consciousness is thus in its nature retrospective, and the relation is perceived between images existing in consciousness simultaneously. The consciousness of succession therefore implies only a simultaneous unity of consciousness, the same unity implied in the consciousness of likeness or difference; not a successive unity, which is a monstrosity, a contradiction in terms. Knowledge even of the nearest past is representative, not intuitive, and involves no self-transcendence except that involved in ordinary memory.

[This paper appears in THE PSYCHOLOGICAL REVIEW for March, 1896.]

(8) *Some Conditions of Will Development.* By BROTHER CHRYSOSTOM, Manhattan College.

The conditions of Will Development naturally fall under two heads: *intrinsic*, or such as are part and parcel of the voluntary agent; and *extrinsic*, or such as act upon him from without. The first of the intrinsic conditions is the nature of the will itself, which, according to the testimony of consciousness, is indetermined at least in the means which it employs to attain a given end. The objection against such testimony raised in the name of Double Consciousness does not hold, for it is based upon a confusion of the *idea of the ego* with the *ego*, or self, the former, however, varying with the normal or pathological state of the subject. A *succeeding* state of consciousness cannot *result* from an *antecedent* state, since the latter has already passed away. Hence a *unity* of subject must be granted; but, since it is variously affected, its phenomena are *many*. The subject is therefore, really distinct from its phenomena, the *ego* from its idea.

The will is, however, determined to a certain degree by acquired habit or disposition and by intellect. Indirectly, heredity exercises a marked influence over it. The law of heredity applies with special force to the exercise of external sense and of imagination. Its influence is modified by environment, and this, in turn, is partly subject to will. Herein lies the great opportunity of teacher and pupil, the former aiding the latter to build up a manly and well-balanced character, and both utilizing for this end the occasions presented by the events of daily life.

(9) *A Psychological Interpretation of the Rules of Definition in Logic.* By PROF. ALFRED H. LLOYD, University of Michigan.

This paper, though in the hands of the Secretary, was omitted for lack of time.

(10) *Discussion on Consciousness and Evolution.* By PROFS. WILLIAM JAMES, E. D. COPE, J. MARK BALDWIN, CHARLES S. MINOT AND GEORGE T. LADD.

[It is hoped that this discussion may be printed in full at some later time.]

(11) *An Experiment on the Effects of the Loss of Sleep.* By PROF. G. T. W. PATRICK, University of Iowa.

The patient, a healthy young man of 28, accustomed to regular sound sleep of eight hours, abstained wholly from sleep for ninety hours, having watchers all the time. At intervals of six hours tests were made upon him in respect to pulse, temperature, weight, steadiness, discrimination-time, simple reaction-time, discrimination of taste and smell sensations, sharpness of vision, lower and upper threshold of pain, strength of grip and pull with dynamometer, memory and attention, time of adding columns of figures, discriminative sensibility of the skin, muscle sense, motor ability, fatigue, pulse after fatigue, measurement and analysis of urine. At the end of ninety hours the subject was allowed to sleep. He slept soundly ten and a half hours and awoke wholly refreshed. He made up altogether but 25% of the sleep lost.

The result of the various tests and their comparison with normal condition will be published in full later. Persistent hallucinations of sight were one marked result of the sleep fast. Pulse, reaction-time and muscular strength decreased. The weight of the subject, sharpness of vision, and discriminative sensibility for taste and for sound increased.

During the ten and-a-half hours' heavy sleep which followed the experiment, the subject was awakened every hour for the purpose for ascertaining the depth of sleep and constructing an 'absolute' sleep curve to compare with the normal sleep curve. The subject was awakened by an electric current passing through the leg, the strength of current necessary being taken as a measure of the depth of sleep. The deepest sleep was found at the end of the second hour, next the first hour, then the third hour, decreasing then rapidly till waking.

(12) *Further Researches on the Psychic Development of Young Animals and its Physical Correlation.* By PROF. WESLEY MILLS, McGill University.

The author announced that he had made investigations on several other animals, viz: The Mongrel Dog, the Cat, the Rabbit, the Guinea Pig, and on Birds. The subjects had been treated as in his first contribution, on The Pure-Bred Dogs, and the papers embodying the facts, etc., were now in the press. As the handling of the subject in its present form involved an enormous mass of details he would not attempt more than the mere announcement of his work now, but hoped to read a generalized account of the whole at the next meeting.

(13) *Variations in the Patellar Reflex as an Aid to the Mental Analysis.* By PROF. LIGHTNER WITMER, University of Pennsylvania.

(14) *Experiments on Induced Hallucinations.* By PROF. JAMES H. HYSLOP, Columbia University.

The experiments reported in this paper were undertaken at my suggestion by a lady acquaintance whose experiences in early life suggested the possibility that looking into a crystal would induce hallucinations. She herself was doubtful about the undertaking and disbelieved in its possibility. The trial, however, showed that they would occur very easily, and twenty-three of them were recorded and described as having some interest. Some of them appeared to be located on the surface and some to originate from the center of the crystal. Some were clearly reproductions of past experiences or scenes modified by association or the addition of materials not in the original experience. One especially seemed to show association between two possible experiences of a different type, but without any element of recognition. All of them exhibited imagery which illustrated the constructive action of dreams, and emerged as capriciously and as independent of the present mental state as dreams.

The two cases in which coincidental features were afterward discovered represented nothing of an objective value in this respect and deserved mention only as subjective facts which occultists might confuse with objective evidence. In other respects their value could only be to show the existence of an influence by the crystal to produce, under proper conditions, genuine hallucinations and to show the capricious character of the effects. (This paper will appear in full in the *Proceedings of the Society for Psychical Research.*)

(15) *Cases of Dream Reasoning*. By PROF. W. ROMAINE NEWBOLD, University of Pennsylvania.

Prof. Newbold reported three cases of what is loosely called dream reasoning. The first occurred in the experience of Dr. W. A. Lamberton, Professor of Greek in the University of Pennsylvania. In the spring of 1870 he worked for several weeks upon the problem: Given an ellipse, to find the locus of the foot of the perpendicular let fall from either focus upon a tangent to this ellipse at any point. Finding all attempts at an analytical solution fail, he gave it over, intending to return to it when his thoughts had got out of the rut in which they had been running. About a week later upon awaking one morning he saw projected upon a blackboard in his bedroom a complete figure containing not only the lines given by the problems, but also a number of auxiliary lines, thus giving at a glance a geometrical solution of it. The case presents two special points of interest.

In the first place, it proves the existence of complex processes corresponding to those of ordinary reasoning, but existing apart from the personal consciousness. It is impossible to determine whether these processes took place during sleep or subconsciously in waking life. Second, the sensory externalization of the solution is a most curious feature. Professor Lamberton is a very poor visualizer and has never in his life had any other hallucination.

The other two cases reported were experiences of Dr. H. V. Hilprecht, Professor of Assyriology in the University of Pennsylvania. When a student under Professor Delitsch he dreamed that the real meaning of the name Nebuchadnezzar was 'Nebu protect my boundary,' deriving the element 'kudurru' from the, at that time, little known verb 'kadâru,' to enclose. This explanation has been universallv accepted. This easily explicable case is of interest chiefly in view of a later and more remarkable dream.

In March of 1893 Dr. Hilprecht dreamed that an Assyrian priest appeared to him and informed him that two fragments which he had been in vain endeavoring to decipher belonged together and were portions of a votive tablet erected by King

Kurigalzu. This was found to be true. Other details in the dream could be neither verified or disproved. No information was given in this dream which could not have been reached by normal processes of reasoning, but its dramatic form makes it most interesting. We are compelled to suppose that the two items of information—namely, that the fragments were parts of one original and that that original was a votive tablet—were reached by normal associative processes and that the old priest and other dramatic details were afterwards thrown about the conceptual elements as one drapes a gown on a lay-figure. This would involve the assumption of a translocation of the time series; the conclusion must have been first given and the dream must have been constructed, as it were, backwards. For such time hallucinations in dreams there is considerable independent evidence.

ADDRESS OF THE PRESIDENT BEFORE THE AMERICAN PSYCHOLOGICAL ASSOCIATION, 1895.

PROFESSOR J. McKEEN CATTELL,

Columbia University.

In the struggle for existence that obtains among the sciences psychology is continually gaining ground. We bear witness to the fact meeting here on terms of equality with the other natural sciences. This Association demonstrates the organic unity of psychology, while the wide range of our individual interests proves our adjustment to a complex environment.

While our confidence in the future of psychology rests on a knowledge of its intrinsic vitality, we are able for the convincing of others to offer the brute argument of material success. The academic growth of psychology in America during the past few years is almost without precedent. The work begun by James at Harvard, Ladd at Yale, and Hall at Johns Hopkins not more than about fifteen years ago has become an important factor in our universities. Psychology is a required subject in the undergraduate curriculum wherever studies are required, and among university courses psychology now rivals the other leading sciences in the number of students attracted and in the amount of original work accomplished.

In addition to the objective test of university recognition we may regard productiveness in publication. There are in America three journals of general science, in all of which psychology is treated as are the other sciences, and there are special journals as follows: mathematics, 3; astronomy, 3; physics, 1; chemistry, 2; geology, 2; botany, 2; zoölogy, 1; physiology, 0; psychology, 2. A comparison of these journals will not discredit those devoted to psychology; and it should be noted that we have in addition to these at least two journals of philosophy and two journals of education in which psychology occupies a prominent place. It would be difficult to select by an objective criterion the most important books published in America during the past ten years, but if we may regard the judgment of foreign nations as the most probable verdict of posterity, the books written by members of this Association will stand well to the front among American contributions to science.

We must admit that the rapid growth of psychology in America has been due to conditions of the soil as well as to vitality of the germ. The more complete absorption of the college president by executive work has made necessary the transferring of his former prerogative of teacher of philosophy to the special student, while the development of the university with elective courses has permitted the easy introduction of a new study.

It follows that a comparison of the progress of psychology in America with its progress in other countries is less flattering to our *amour propre* as psychologists than to our patriotism. Still Germany maintains its prestige in psychology, and psychology maintains its prestige in Germany. Psychological courses are an increasingly large part of the philosophical courses, and the laboratories of psychology are being acknowledged as equal in rank to those in other sciences. There are two excellent journals of psychology, one of which is attracting part of the best work formerly published in physiological and physical journals. In France the *Année psychologique* bears witness to much recent work in experimental psychology, while interest in social, individual and pathological psychology is unabated. In England the traditional psychology is being enriched by absorption of the most important foreign work, while new contributions are offered on the side of philosophy and on the side of the biological and the medical sciences. In Russia, in Scandinavia and in Italy professorships and laboratories are being established.

While the recent progress of our science has been great, we do not admit that psychology is a new science. It is not a 'sport,' not even a fortuitous variation. If science is to date from the year of 'the master of those who know,' then we may take pride in the beginnings of psychology whose foundations were more securely laid by Aristotle than those of any other science. Like the little boy answering the first question of the Catechism we may say "God made one foot big and I growed the rest." But with our superior knowledge of embryology we may further believe that we did not start even as an infant of the size of the famous one in 'Midshipman Easy,' but began our inarticulate growth long before 'mewing and puking' we came hither.

Even the 'new psychology' began at the beginning, and developed *pari passu* with the other sciences. Take, for example, a subject, not of leading importance, but typical of the problems studied in our laboratories—after-images. We have in after-images a case where we investigate the relations of the change in consciousness to the physical stimulus on the one hand and to the bodily organism on the other, where we make experiments and measurements on phenomena known to us only on the side of the individual consciousness, a case where we may hope for useful applications in education, in medicine, etc. We have in after-images phenomena related to and throwing light on a large range of mental activity—imagery, memory, hallucinations, space-perception, etc., and even of interest (see, for example, what Royce has to say in Vol. III. of the *Philosophical Review*) in their bearing on epistemology and metaphysics. Now after-images, phenomena thus typical of modern experimental psychology, were described by Aristotle with such exactness that we may feel sure that he himself made experiments upon them, whereas he refers to them as though they were familiar to his readers. Experiments upon after-images have been made by men eminent in widely separated fields of mental activity—by Augustine, Newton, Buffon, the elder Darwin, Goethe and many more—long before the date usually assigned to the development of psychology as an experimental science. I have perhaps selected a favorable example, but I think there are but few subjects now in course of investigation in our laboratories whose origin and gradual development could not be traced a long way back.

I may mention parenthetically that the earliest explicit formulation of the problems of experimental psychology, as I understand them, is to be found not in Lotze, nor in Fechner, nor in Wundt, but in the most visionary and poetic of poets, himself a problem in heredity, character and intellect far beyond the reach of our psychology. Shelley wrote[1] about eighty years ago:

> "A scale might be formed, graduated according to the degrees of a combined scale of intensity, duration, connexion, periods of recurrence, and utility, which would be the standard, according to which all ideas might be measured, and an uninterrupted chain of nicely shadowed distinctions would be observed, from the faintest impression on the senses, to the most distinct combination of those impressions; from the simplest of those combinations, to that mass of knowledge which, including our own nature, constitutes what we call the universe."

While psychology traces its descent through a long and noble line, we need not hesitate to mark a natural, but at the same time a notable, development under our own eyes and hands. A little while ago the psychologist might still say with Browning's *Cleon:*

> "And I have written three books on the soul,
> Proving absurd all written hitherto,
> And putting us to ignorance again."

But we are past the time for systems of psychology; now handbooks of psychology are prepared. We have, like the other sciences, a small area lighted by ascertained fact and accepted theory, outside this is the penumbra, and beyond is darkness through which none of us pretends to see. We, indeed, estimate differently the importance of different departments and the hopefulness of different lines of research, but in this respect we only exhibit the human nature in whose study we are engaged. The student of mechanics proposes to account for all physical phenomena by Newton's laws, the student of electricity by electric vibrations, etc. An eminent chemist recently remarked that chemistry is evidently the basis of psychology.

It is, however, possible that we over-emphasize the differences that do exist. Certainly there is no member of this Association who believes that science should be a *tohu-wabohu* of facts, nor any who believes that reasonable theories can be de-

[1] Shelley's Works, Forman's Edition, VI., 285. *Speculations on Metaphysics.*

vised without regard to facts. Probably none of us would claim that he could draw a straight line and say: "on this side is science, on that side is philosophy." Possibly none would say: "these observations have no scientific validity, because they rest on introspection," or "these determinations have no psychological interest, because they are mere measurements." Rather we all join in the admirable words of our President at the New York meeting:

"Let us all always be just; nay, let us be something more than merely just; let us be generous. And let our generosity include all workmen of all times, with their works, from Aristotle's *De Anima* to the latest thesis by the youngest aspirant for the degree of Doctor of Philosophy."

While science advances along many paths, there are certain highways most traveled and most direct. What these are in psychology at the present time the analogy of the other sciences may perhaps indicate. We cannot, I think, doubt, but that modern science is either quantitative or genetic. I say 'either' because there is at present a partial divorce between the physical sciences in which the relative permanence of the phenomena makes the quantitative point of view easy and the genetic difficult, and the biological sciences in which the converse conditions obtain. This divorce is, however, due rather to our ignorance than to our knowledge. In the progress of science the physical sciences will become increasingly genetic and the biological sciences increasingly quantitative. Astronomy learned the laws of Kepler before it learned the nebular hypothesis. The physicist could not find in the star-dust the problems of modern physics and chemistry. There is variation and survival in the inorganic as well as in the organic world. The biologist in turn should no longer rest content with describing the genesis of species and of individuals, but should measure variations and changes, and determine causal relations by the methods of exact science.

It would seem likely that methods prevailing in the other sciences should also hold in psychology, more especially as we must admit that most of these sciences have passed through the stage in which psychology now is—or until recently was—and have reached a clearer self-consciousness. But we do not need to depend exclusively on the often delusive argument from analogy. Recent work in psychology speaks with sufficient emphasis in favor of tracing the genesis and the degeneration of mental states on the one hand, and their quantitative definition on the other.

I think we may claim without undue self-assertion that the most important contributions to genetic psychology made during the past year have come from members of this Association. Baldwin has treated with an elaboration hitherto unequalled, the mental development of the child and of the race; Stanley has studied the evolutionary psychology of feeling; and Royce has analysed the genesis of the contents of the individual consciousness in its dependence on social environment and evolution with great acuteness. While much of the definite outcome is still *sub judice*, there is none to question the validity of the genetic method.

When we turn to the quantitative method in psychology we find, I fear, more difference of opinion. We have, indeed, our many laboratories, all of which are at least silent witnesses in its favor. But several of our leading members have expressed, at the meetings of this Association and in their published writings, doubts as to the validity, or at all events as to the value, of mental measurements.

Now it is easy to adopt a skeptical point of view in such a matter. By the nature of things men of science and students of philosophy are quit of the enthusiasm of the proselytizer and reformer. The every-day up-hill work of the laboratory is scarcely more stimulating than the routine of the factory or of the farm. Each, as Clough's *Dipsychus* tells us,

> "Must slave, a meager coral-worm
> To build beneath the tide with excrement
> What one day will be island, or be reef,
> And will feed men, or wreck them."

But this skeptical point of view can be applied with equal success and equal futility to any science, or to the conduct of daily life. We may, if we see fit, wonder why anybody does anything. By common consent the discovery of argon in the atmosphere was the most important scientific advance of the past year, but it has not as yet been found that argon is of any practical use, and, so far from helping us to understand the universe, this substance but adds to its apparent complexity. Why not let the last decimal be, and enjoy the air in the springtime—or devise means to keep out of it if one happen to live near Boston?

Münsterberg has written: "Die Messung ist niemals Selbstzweck in der Psychologie, eben dadurch unterscheidet sie sich von der Physik." Now it seems to me, on the contrary, that measurements have just the same place in psychology as in the material sciences, except in so far as they have not been as yet so successfully prosecuted. The immediate end of science, whether physical or mental, is to describe the world—it may be added, and to explain it, though if we had a complete and unified description, it is not clear what would be left to explain. We wish to describe the world, partly because our knowledge can be applied in useful ways, and partly because the effort satisfies mental needs, as do art and religion. Measurements in the physical sciences are in a way means to the ends mentioned, but in so far as a description of the world is an end in itself, measurements are a part of this description, and by far the most exact, general and economical method of description hitherto devised.

It may be that in psychology the field for quantitative definition is more limited than in the case of the physical sciences. The lack of many or wide-reaching numerical formulas expressing mental relations may be due not so much to the recentness of our attempts to discover these as to the nature of the subject-matter with which we are dealing. Indeed it is evident that as a mere matter of definition we have to a large extent limited the physical sciences to a quantitative treatment of time, space and energy, relegating qualitative differences to consciousness. But it is also true that the quantitative point of view in physical science has only gradually and but recently emerged from a chaos of animistic and teleological conceptions. The unitary point of view developed by physical science is truly a matter for marvel. The awe inspired in the great mystic of rationalism by the starry heavens and the moral law may well pervade the student of physics in the presence of the unforeseen grandeur and simplicity of his own handiwork.

It is, indeed, true that measurements give as yet only a partial description even of the physical world, and the progress of science may make this less rather than more adequate. The ether, elastic and solid as it is supposed to be, seems on the point of breaking under the load laid upon it. Atoms are stumbling blocks, however small we may assume them. Ludwig, chiefly instrumental in establishing for a while a mechanical theory of living tissues, lived long enough to witness the emergence of a neo-vitalism. Indeed Ludwig himself, on being asked why he did not prepare a new edition of his *Physiology* said, "Such a work must be written by a young man; an old man is too well aware of his ignorance." We are as the earth in the Hebrew cosmogony, when the light had been separated from the darkness, but the sun had not yet been set in the firmament.

Both the success and the failure of material science may encourage the experimental psychologist. He may hope to accomplish much by measurements, even though he may foresee that he will not accomplish everything. Whether he be entrusted with one talent or with many, he can serve in better ways than in standing and waiting.

This commingling of hopeful endeavor and hopeless limitation drawn from the analogy of physical science may be more directly deduced from the work hitherto accomplished in the psychological laboratory. This has indeed been called trivial and pedantic, and has been wounded even in the house of its friends. Statistics, averages and probable errors seem remote from the complex fulness of human nature. There seems imminent danger from a neo-scholasticism with measurements in the place of definitions.

Now when it is said that nothing has been done beyond measuring time, intensity, and complexity of sensations, movements and mental processes, the reply may readily be made that nothing further can be desired. Physical science measures only time, space and energy. If psychology can do as much it has the same abundance of individual problems; if it need do no more it has the same great simplicity as its goal.

The use of averages and probable errors in psychology is not pedantic, except when attempted by those not acquainted with their meaning. The probable error tells us just how many experiments we ought to make and just what reliance may be placed upon them. The theory of probabilities, enabling us to measure both our knowledge and our ignorance, is one of the great achievements of the human intellect, and is equally applicable in sciences attaining varying degrees of exactness. It was, indeed, pedantic for Helmholtz to give the velocity of the nervous impulse as 37.4927 m. per second, when the average velocity and the individual variations are not known within tens of meters. But it is exactly an application of probable errors that would prevent such pedantry, and if the greatest of physicists has on occasion indulged in it, we need not too severely blame the student preparing his doctor's thesis for carrying his average a decimal beyond what is warranted by the theory of probabilities.

In some cases a very considerable degree of accuracy is attainable and necessary in psycho-physical measurements. Thus doubling the intensity of the stimulus shortens the reaction-time about 0.001 sec., and if we wish to determine the relation between intensity of stimulus and duration of reaction we must measure to the ten-thousandth of a second, and the averages and probable errors show that such a degree of accuracy is attainable. The apparatus used for the purpose in the psychological laboratory of Columbia College has this year been borrowed by professors in the physical department to measure the rate of fall of bodies in liquids, and it has resulted that the constancy of the physical motions is less than that of the psychophysical processes.

Such problems—the correlations of quantities—are those ultimate in exact science, and in so far as they can be undertaken in psychology it becomes an exact science. It may be said that in the example given we are concerned with the nervous system rather than with consciousness. But even if this be exclusively the case it may be urged that a dynamics of the nervous system is essential to a final psychology. Further, the correlations of quantities may be investigated in cases in which we know the changes in consciousness, but are completely ignorant of the neural processes which may accompany them. Thus we all are familiar with the many investigations on the discrimination of differences in intensity. There is room for various interpretations of the meaning of a just noticeable difference, but by definition differences in sensation are equal if they be equally perceptible. We can find when differences are equally perceptible by determining the percentage of times in which they are in fact correctly perceived. But we can approach the problem from a new point of view, first defined, I believe, at the New York meeting of this Association, involving the correlation of mental magnitudes.

If differences be equally perceptible it takes equal times to discriminate them, and the less the difference in sensation the greater the time required. By measuring the time of discrimination it is possible to determine differences in the intensity of sensation. We can thus investigate the relation of the intensity of stimulus to the accuracy of discrimination (Weber's law), and can even use the method for the comparison of disparate sensations. We may find that the difference between red and blu is equal, say, to the difference between sensations due to lights from 10 and 1,000 candles, or even that the difference between the tones C and c is equal to the difference between certain visual sensations.

The appeal made to different minds by such problems as part of a worthy description of the world must of necessity vary with the individual mind. Our goals in religion, art, philosophy and science are not only wide apart, but they also shift even as we run. Our science and our philosophy are but as a doll in the arms of a little girl, who does not know what it means nor what the years will bring.

I think, however, that conclusive testimony may be advanced to prove that psychological experiment has had and will have both practical applications and an important share in psychology as a whole, whether regard be had to its individual development or to its relations with the other sciences.

Professor Burdon Sanderson, in his presidential address before the Ipswich meeting of the British Association, said that he was not aware of any useful application of experimental psychology, and Professor Morley, president of the American Association, said, at the recent Springfield meeting, "science cannot change human natures or the social order." These selected representatives of science in England and America, however, both hold that science has an adequate end in the satisfaction of intellectual curiosity. On the other hand Franklin, the father of American science, speaks of new discoveries as important only because they tend "to extend the power of man over matter, avert or diminish the evils he is subject to, or augment the number of his enjoyments." Franklin's point of view may be regarded as materialistic, but science for the sake of science is in turn in danger of dillitanteism.

We may be glad that experimental psychology has practical applications in spite of quasi-official dicta to the contrary. In the United States more than one hundred and fifty million dollars, collected by enforced taxation, is spent annually on public schools in the attempt to 'change human natures.' President Eliot says that nothing is accomplished in these schools except the training of the memory, and his colleague, our retiring president, tells us that the memory cannot be trained.[1] Surely in education, which extends from birth to death, we can learn by experiments on the senses and the mind what may be done to fit the individual to his environment. It should not be forgotten that we not only hold the clay in our hands to mold for honor or dishonor, but we also have the ultimate decision as to what material we shall use. The physicist can turn his pig iron into steel, and so can we ours; but he cannot alter the quantities of gold and iron in his world, whereas we can in ours. Our responsibility is, indeed, very great. By one psychological experiment we injure the eysight of our children in the schools, and by another psychological experiment we discover the defect and fit glasses to correct it. It seems to me certain that experimental psychology has wide-reaching practical applications, not only in education, but also in medicine, in the fine arts, in political economy and, indeed, in the whole conduct of life.

[1] As Professor James said at the meeting, he only holds that native retentiveness is unchangeable. It was not, of course, intended in this paradox to adequately represent the views of President Eliot and Professor James.

It also seems evident that experimental psychology has recently become an important factor in the development of psychology as a whole, both by its actual contributions and by the changes in method to which it has given rise. The psycho-physical camel will never be able to exclude the psychological Arab from his tent, but it must be welcomed, or at least tolerated. A comparison of modern text-books of psychology, such as those by James, Ladd, Baldwin, and Dewey, with older works bears irrefutable witness to the introduction of the results of physiological and psychological experiment. I shall undertake the *argumentum ad hominem* in the case of James, who said at our last meeting that "curious phenomena of the dissociation of consciousness * * * throw more new light on human nature than the work of all psycho-physical laboratories put together." On taking down James' *Psychology*—which has breathed the breath of life into the dust of psychology—and turning at random to the even hundred pages, I find that the first is entirely taken up with the measurement of the temperature of the brain in relation to thought; the second is on continuity of consciousness with time measurements; the third a description of the bodily movements giving the consciousness of self; the fourth on the relation of the two hemispheres of the brain and of bodily movements to self-consciousness; the fifth on the relative intensity of sensations and images; the sixth on the association of ideas; the seventh on observations and experiments on the mistaken interpretation of sense-stimuli; the eighth on the relation of movements of the eyes to the perception of space; the ninth on the factors distinguishing the perception of reality; the tenth on instinctive actions; the eleventh on muscular sensations; the twelfth on hypnotic suggestion. These topics illustrate very fairly the field covered by modern psychology. They nearly all rest upon psycho-physical observations and experiments, and in cases where observations predominate it is evident that they will soon be superseded by actual experiments from our laboratories yielding quantitative results.

Even in directions where experiment has not yet offered considerable contributions, it has performed an important service in setting a standard of carefulness and objectivity. It may also be urged that experiment serves as a stimulus and starting point for thought. Thus Wundt states that his theory of the development of the will and of its relation to 'apperception' had its origin in observations made during the course of experiments on the reaction-time. It requires peculiar genius to sit down at a desk and write out observations and theories that are new and true; they are more likely to occur during actual work of some sort. Further, there are many who can carry out experiments in the laboratory who are incapable of constructive work. The data obtained by them may be seen by others in their larger relations. The generalizations of a Newton must be based upon the observations of a Flamsteed.

The introduction of experiment has also made the teaching of psychology easier and more useful. Laboratory work by students is by common consent an important part of their training. Whether the experiments be in chemistry or in psychology may not greatly matter. In the chemical laboratory, when the course is intended for liberal training, the experiments are meant to educate the senses and the mind, rather than to give information concerning metals and acids. When the object of an experiment is not to learn what happens when two solutions are mixed, but to teach the student to observe what happens, we may perhaps claim that a psychological experiment has been undertaken. Whether experiments directed to the senses and the mind would serve better or worse than others for the purpose in view, or whether it is practicable to introduce a new study into the preparatory school or the early college years, are matters that can themselves only be settled by experiment. It is, however, certain that such preliminary work, or, lacking it, some experiments introduced into the course in psychology commonly offered and even required in the junior year, would enable the average student to follow this course with greater interest and intelligence, so that he would be less likely to regard it as:

> "A tale
> Told by an idiot, full of sound and fury,
> Signifying nothing."

I venture to maintain that the introduction of experiment and measurement into psychology has added directly and indirectly new subject-matter and methods, has set a higher standard of accuracy and objectivity, has made some part of the subject an applied science with useful applications, and has enlarged the field and improved the methods of teaching psychology. In conclusion I wish to urge that experiment in psychology has made its relations with the other sciences more intimate and productive of common good.

In courses in physics, for example, certain psychological subjects, vision, hearing, etc., have always been included. The treatment has of necessity been inadequate, and the student, if not the teacher, may have left the subject with confused notions, *e. g.*, as to the distinction between light as a mode of motion and color as sensation. Or it may be incidentally stated that color, pitch and warmth are 'subjective,' while matter in motion is alone 'real.' Now the treatment of certain subjects in common with physics has set for the psychologist a higher scientific standard, whereas it may be hoped that the physicist has learned that processes of perception and thought are part of the real world which science as a whole must take into account.

In physiology the treatment of certain subjects in common must ultimately result in mutual benefit. Our host Fullerton showed this morning how largely the physiology of the nervous system leaves its proper field for that of psychology. The physiologist must face the problem as to whether consciousness shall be assumed in causal interaction with the nervous system. It may not matter greatly to us whether cerebral functions are located here or there, but it would be a survival or an atavism to hold that we can fully treat processes of perception, ideation, feeling or will without regard to sense organs, movements, paths of conduction and nervous centers, or even apart from metabolism and circulation of the blood.

In general biology, whose great problem is the development of life, zoology-botany and psychology cannot advance excepting hand in hand. Darwin did not hesitate to use consciousness as a *vera causa* in the preservation of species, and Cope, now presiding over our sister society, urges that it is a preëminent cause in their origin. Sensations, movements, instincts and habits are prominent in any theory of the evolution of species, and they must be treated in common by physical zoology and psychology. The importance of these problems is borne witness to by the fact that we have selected them for special discussion to-morrow morning. For many the leading interest in organic evolution is in its application to social evolution, and it is scarcely necessary to mention the relation of psychology to sociology, which science is indeed simply collective psychology.

Psychology has long been and properly remains the gateway to architectonic philosophy. It may be that experiment cannot answer the final questions of philosophy, but the world-view of each of us depends increasingly on what the natural and exact sciences contribute to it. The white light of philosophy can only result from the proper commingling of the colors of the sciences. Systems of philosophy, elaborated prior to the development of modern science or without regard to this, may receive our admiration as poetry, but they cannot claim our adherence as truth. To allot to science those subjects concerning which we have knowledge, and to reserve for philosophy those questions concerning which we know nothing, is evidently subversive of philosophy. Epistemology, ethics, logic and æsthetics are regarded as philosophic disciplines, but they rest increasingly on psychology. Epistemology depends on the psychology of perception and may be nothing else. Works on ethics, logic and æsthetics take increasing account of psychological facts; indeed, as our knowledge increases, the distinction between a normative and a descriptive science becomes somewhat tenuous. The twilight of philosophy can be changed to its dawn only by the light of science, and psychology can contribute more light than any other science.

CONSCIOUSNESS AND TIME.[1]

BY C. A. STRONG,

Columbia University.

The question to which I invite your attention is this: What is present time, and what is the relation of consciousness to present time? The answer that naturally occurs to one is, that consciousness is *in* present time, that present time is *present* time, not past or future, and that there is an end of the matter. The answer given by Professor James in his Presidential Address a year ago, which is the answer of Shadworth Hodgson, is a very different one; and as this answer seems to me unsatisfactory, I should like, with your permission, to present my objections to it and my reasons for preferring the natural answer.[2]

According to Professor James, there is "literally no such datum" as that of present time. The present is merely a boundary-line between the past and the future, and is not itself extended or capable of containing anything real. The only real datum, the only thing that is ever really present to us, is a short bit of duration, an *interval* of time, extending a little way back and a little way forward from the present instant. Or, rather, the datum is the feeling that occupies this interval and has this duration. And there is a minimum duration which the feeling must have in order to exist at all. Such a minimal feeling, when reflectively examined, is found to consist of two sub-feelings, one earlier and the other later, and the feeling of their succession. Thus the simplest feeling we can have already includes the consciousness of change, of passing time. The boundary-line which separates the two sub-feelings is the pres-

[1] Read before the American Psychological Association at Philadelphia, December, 1895.

[2] Prof. James's address appeared in the PSYCH. REV. for March, 1895, pp. 105 ff; see esp. pp. 111–113. Mr. Hodgson's statement is found in his *Philosophy of Reflection* I., pp. 248 ff. The view criticised is also held by Fouillée, *Psychologie des Idées-Forces*, II., pp. 81 ff.

ent instant, and with reference to it the one appears as past and the other as future. Thus past and future are "already parts of the least experience that can really be."

But the consciousness of change, of passing time, is a relational state, and therefore necessarily unitary. Just as the consciousness of the difference of two things requires that they should both be present to a unitary state which knows them and their difference, so the consciousness of succession requires that the earlier and the later state should both be present to a single thought. And as the consciousness of likeness or difference implies a simultaneous unity of consciousness, a unity running, so to speak, across the stream, so the consciousness of change, of passing time, implies a successive unity of consciousness, a unity running a short distance up and down the stream. Such a successive unity is, to be sure, a mysterious thing; for the knowing state has to reach out beyond itself, and intuitively know together things that do not exist together. Yet such self-transcendence, such "presence in absence" we must nevertheless assume, if the relation of consciousness to time is to be made intelligible at all.

In presenting my objections to this account of the matter, I must point out, to begin with, that there are two distinct relations here involved: the relation of consciousness to the time in which it exists, and its relation to the time of which it is aware. I shall take for granted that consciousness does as a matter of fact exist in time, and shall inquire first as to the characters of the time in which it exists.

The present instant, as a mere boundary-line, is too small for consciousness to exist in: this, I think, is Professor James's doctrine. It must be freely admitted that the present instant is a mere boundary-line, and that consciousness, in order to exist at all, must have duration. But the consequence which is supposed to follow from this—namely, that consciousness, driven out of the present, can find room for itself only in a little bit of the past plus a little bit of the future—does not really follow from it at all.

This might appear, for one thing, from the consideration that the past means that which once was present, and the future that which will be present; that the reality of the past and the future is thus derivative from the reality of the present; and that to deny the reality of the present is therefore to knock the bottom out of reality altogether. But it might be contended that this applies only to the 'specious present.'

That the consequence does not really follow, appears when we consider that the conception of an extended present involves the same false abstraction as the conception it is intended to replace. The two sub-feelings AB are supposed to be given at once, A being known as past and B as future. But this state of things exists, not for any finite length of time, but only for an indivisible instant, only at the instant x. Only at that instant is it true that A is entirely past and B entirely future. But it is in just such an instant that nothing can be real. When we look at the feelings AB from the point of view of this instant, we are conceiving the flight of time as arrested, and therefore considering the feelings in abstraction from real time. If we are to find room for consciousness, we must abandon the point of view of any single instant, and take up the innumerable points of view of the duration itself. We must remind ourselves that the feeling A has been present and the feeling B will be present; in other words, we must consider A and B as feelings that become successively present; or, rather, we must consider these feelings as parts of a stream of consciousness every instant of which becomes successively present. Only by substituting the point of view of the interval wy for that of the instant x, and only by considering this interval, not as a bit of the past plus a bit of the future, but as *a line of successive presents*, shall we succeed in considering the feelings concretely, and in the way they actually happened.

By adopting a sort of algebraic statement the case may be presented in its essence. Such a stream of consciousness, such a line of successive presents, may be conceived as composed of n parts, and these parts will always be successive, thus: $1, 2, 3 \ldots\ldots n-2, n-1, n$. However great the value assigned to n, the parts will always be successive. It is evident that from these essential relations there is in the nature of the case no escape. No fraction of a second can possibly be taken so small that all of its parts will be given at once. Succession, in other words, is spun out infinitely fine, and the only thing that can be given at once is an indivisible instant.

Here doubtless some hearer says to himself that I have given away my case. I have sharpened the succession down to a point, and in so doing have left no room for consciousness. I reply that this objection rests on the very same false abstraction against which I have been protesting. For what sort of a point is it which we now have left? It is no longer a resting point, but a moving point; it is the onward motion of a point from an earlier instant to a later one; and what is such onward motion but actual duration? The resting point was a mere abstraction; it left out the motion, the onward passage, the fugitive quality, which is the essence of time, without which it would not be time at all; and since time itself was left out, there was naturally no room for consciousness. But the moving point is only another name for actual duration, and in present time, when so conceived, consciousness finds all the room it needs. In order adequately to conceive present time, we must combine the two conceptions of movement and a point. The conception of movement is necessary in order to render time's flight, its fugitive quality. The conception of a point is necessary in order to render its infinite successiveness.

But here, again, some hearer doubtless says to himself that I have given away my case in another direction. In modifying the conception of present time from that of a resting to that of a moving point, I have virtually admitted that change is directly given. And, if change, then the *terminus a quo* and the *terminus ad quem*, the two sub-feelings and their succession, the immediate past and the immediate future. I may reply by asking the objector to consider what sort of change it is which he here assumes, and what sort of change it is which is actually given. The change which he assumes is a finite change, the transition from feeling *A* to feeling *B*, these two feelings not being real at the same time. The change actually given is an infinitesimal change, the onward passage in the act of occurring—that onward passage in virtue of which we say that time is infinitely successive, that the succession is spun out infinitely fine. The fact is, that the succession is spun out very much too fine to be visible to the naked eye of consciousness. The succession, in other words, enters into the tissue of consciousness, histologically speaking, but is not an obvious feature of its gross anatomy. Consciousness is in its nature a sort of change, conscious states are nothing if not events, but this is an account of what consciousness *is*, not an account of what we are conscious *of*. A sudden, brief pain is in its nature an event, a change, but what we are conscious of is not the change, but the pain. A changing consciousness, in short, is not the same thing as a consciousness of change.

I hope I have now met the nearest-lying objections, and made it clear that present time, when rightly conceived, furnishes all the room that consciousness needs. Reality, as we know it, may in fact be said to be ***nothing but one ever-changing present.*** So far is it from being true that "past and future are already parts of the least experience that can really be," and that there is "literally no such datum" as that of the present, that I think we may rather say that there is literally no other conceivable datum, and that the present is the only time that ever was, or is, or shall be.

I pass now to the second of the two relations, that between consciousness and the time of which it is aware. This relation may be disposed of briefly. Though consciousness exists only in the present, and the present is a moving boundary-line, yet it must be admitted that the time of which we seem to be directly aware is not a mere boundary-line, but an actual interval, an interval the parts of which are given not successively, but all at once. And this is true, in spite of the fact that the consciousness of this interval exists only in the moving boundary-line. Just as, in external perception, what is really given is only a subjective state, yet what we are for practical purposes aware of is a real world of space; so, in our consciousness of time, what is really given is only the moving boundary-line, but what we are for practical purposes aware of is, say, the last half-second, the 'specious present.'

The first point that requires to be settled is the temporal relation between the knowing state and the 'specious present which it knows. *When*, with reference to the 'specious present,' does this knowing state exist? We have all along assumed that it, as well as other states of consciousness, truly exists in time. The view that the consciousness of time is itself timeless is a Hegelism which I should be loth to attribute to Professor James. When, then, does it exist?

Professor James's view seems to be that it exists midway between the two sub-feelings, in some sort after the first of them and before the second. At least it is only from this position that the knowing state would have the right to regard the first sub-feeling as past and the second as future. But has this state such clairvoyant power as to be able to foresee what the second sub-feeling will be, before this sub-feeling has actually appeared on the scene, or even to foresee that there will be a second sub-feeling at all? A direct intuition of the past seems marvelous enough, without assuming a direct intuition of the future. I know Professor James will reply that I am reading a separate individuality into the sub-feelings which does not exist in his account. My answer is, that such a separate individuality is inevitable, if the sub-feelings are truly to exist in time. If one sub-feeling follows the other in time, a knowing state that coexists with the first will have no inkling of the second, and no inkling of their succession. If the knowing state exists midway between the two, it will still have no inkling of the second sub-feeling or of their succession. Only when the second sub-feeling has appeared can there be a knowledge of the two sub-feelings and their succession. Only when the succession is an accomplished fact can a consciousness of that fact arise. In other words, the actual succession must precede the knowledge of the succession. When the knowledge arises, the feelings that succeeded each other are past and gone. But, if so, this knowledge is in its nature *retrospective*. The succession is not perceived between the feelings A and B as they occur, but between the images a and b which they leave behind. And psychologists have long been discussing the 'temporal marks,' discernible in these images, which prompt the mind to perceive between them the relation of succession.

This view of the case is so generally accepted, and Professor James himself has given so lucid an account of it in his chapter on Time, that I am at a loss to understand his now falling back into a radically inconsistent view. I can only attribute his lapse to the confusion, already referred to, between the finite succession of the feelings A and B, and the infinitesimal succession which enters into the tissue of each.

Dr. Ward, in his Britannica article, aptly expresses the view here defended, the view that a succession of ideas is not an idea of succession, by comparing the actual stream of consciousness to a horizontal line, and the consciousness of time to a vertical line erected upon it. We are aware of time, he says, not through the feelings A and B in the horizontal line, but through the images a and b in the vertical. Our knowledge of time thus involves a perspective effect, similar to that by which we perceive the third dimension of space. "We are aware of time," he insists, "only through the time-perspective."[1]

The lapse of time is, therefore, not directly experienced, but constructed after the event. The time we are directly conscious of is not the real time that elapsed. The succession of our feelings is a fact external to the feelings themselves. If it were not for memory, we should never have any consciousness of succession at all, any more than of past time. And this is true, because succession is essentially a relation between past and present, or between an earlier and a later past. We never lift ourselves up out of the stream of time and view it as a stream except representatively, except through memory. To wish to apprehend succession, or change, or the lapse of time directly, and not through memory, is as foolish as to wish to apprehend the past directly, and not through memory.

But now, if this account of the case is correct, it seems to me that the fiction of an intuitive knowledge of the immediate past and future falls to the ground of itself. Later states can have no direct and intuitive dealings with earlier states, for the simple reason that the two do not exist at once. When the earlier state is present the later state is non-existent, and when the later state appears the earlier one is non-existent. Our apparently direct consciousness of the immediate past is an illusion,

[1] Encycl. Brit., 9th ed., art. 'Psychology,' pp. 64-5.

of the same character as that which leads us to attribute extramental reality to material objects. To take this illusion seriously is to be guilty of a sort of naive realism in the field of time.

The impossibility of such a direct consciousness of past time appears, further, from the consequences to which it should lead if true. If we can be directly conscious of a feeling that occurred half a second ago, in spite of the fact that the feeling is now past and gone, why not also of a feeling that occurred a whole second ago, or a minute ago, or an hour, or a day, or a week? The consciousness would be in no wise more miraculous. Why cannot we be directly conscious of any past experience, no matter how remote? But, if such consciousness is not to be thought of, then for the same reason the direct consciousness of half a second ago is not to be thought of. Our consciousness of even the nearest past must be ideal, not actual; representative, not intuitive.

But again, if this is true, the successive unity of consciousness falls to the ground likewise. "In reality," says Dr. Ward, "past, present and future are differences of time, but in presentation all that corresponds to these differences is in consciousness simultaneously." But, if so, there is no need of a successive unity of consciousness to account for the consciousness of succession. Earlier and later states cannot be bound up into a successive unity, because they do not exist together, and because they are past and gone when the perception of succession arises. When this perception arises, the relation is perceived between images existing in consciousness simultaneously. It therefore implies only a simultaneous unity of consciousness, the same unity that is implied in the perception of likeness or difference; not a successive unity, which, to tell the truth, is a monstrosity, a contradiction in terms. Successive states of consciousness are not one with each other, but continuous with each other; we may speak of a continuity, but not of a unity. The only unity is the unity of that which is in consciousness at once.

A word, in closing, in regard to the broader issues involved in this discussion. The legitimate and necessary reaction from the psychological atomism of Hume and his school has led to the recognition of relational states, of the unity of consciousness, of 'mental synthesis.' This reaction was a necessary and legitimate one; these were features of consciousness which the English school overlooked: but it would ill become us, in interpreting them, to fail to imitate that sobriety and economy of thought which has always been characteristic of the English school. No psychologist has done more to illustrate and commend these qualities than Professor James. There is no one to whom we are so accustomed to look for plain, honest, intelligible psychology, and no one to whom we so seldom look for it in vain. We have not looked for it altogether in vain in this very matter. There is one mystification, the most insidious and fatal in this subject, from which Professor James's account is signally free. I mean the conception of 'mental synthesis' as involving an agent, who finds feelings apart and puts them together into a unity. When we turn from this mystification to the facts of consciousness, all that we find really given is the essential unity of the relational state itself. We enjoy these states when they arise; they constitute an increment to our intellectual being; we may even put ourselves in the way of getting them. But may Heaven preserve us from the arrogance of supposing that it is we who create them. We no more create them than we create the sensations which form their terms. We do not create them, but *are* them. And may Heaven preserve us, not less, from supposing that, being them, we may nevertheless transcend them and hold direct converse with feelings that are past and gone. Our knowledge of the past involves self-transcendence, but the self-transcendence is representative, it is ideal. A self-transcendence that is other than ideal is neither plain, honest, intelligible psychology, nor plain, honest, intelligible metaphysics.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY. (IV.)

COMMUNICATED BY HUGO MÜNSTERBERG.

THE PHYSICAL CHARACTERISTICS OF ATTENTION.

BY R. MACDOUGALL.

The researches here to be reported upon were concerned with the phenomena of functional disturbance which accompany various forms and degrees of perceptual and reflective attention.

The subject was seated in a comfortable position with body relaxed and eyes closed, beside a table upon which the instruments were placed. The conductor of the experiments, who gave the signals and applied the stimuli, stood at his back, by which arrangement undesired knowledge of the nature of the stimulus or other matter of technique was avoided.

Records were taken of the character of the breathing, of the changes in pulse form and blood supply in the left forearm and of the alterations in muscle tension in the fingers of the right hand.

Upon the subject's breast was fastened a Marey tambour pneumograph, held in position by tapes passing over the shoulders and around the body under the arms. To the stem of its bulb was attached a rubber tube connecting with the chamber of a pneumatic registering pen, whose point traced the curve of respiration upon the surface of a horizontally revolving drum covered with smoked paper. During the later experiments a second pneumograph was added which recorded the character of the diaphragmatic respiration.

The features of the pulse and of blood distribution were recorded in one composite tracing given by an air plethysmograph. This consisted of a glass cylinder fifty centimeters long and ten in diameter, open at one end and at the other drawn to a neck, in which a cork, having a glass tube passing through it, was tightly fitted. Around the open end of the cylinder was stretched a rubber band twelve centimeters wide, which was cemented to the glass and formed an air-tight bandage upon the subject's arm when inserted in the cylinder. This cylinder was suspended by cords from the ceiling at such a height that the subject's arm, when inserted in it, might be in an easy position as he sat in the chair. This long radius from the point of support gave great flexibility in yielding to slight motor reactions on the part of the subject, those which normally occurred in the course of the experiment—as observation proved—having no appreciable effect upon the character of the pulse and volume curve. When the arm, as far as the elbow, had been placed in the glass chamber, the plethysmograph was connected by means of a rubber tube attached to that passing through the cork in its neck, with a registering pen similar to that used in recording the respiration.

The muscles selected for observation were those of the index finger of the right hand, which, during the early experiments, was placed in the holder of a Delabarre muscle recorder, and afterwards in an adaptation of this instrument, by which the direct extensile and contractile changes without the lateral movements were recorded. The right forearm rested upon a stand drawn up beside the subject, the wrist being supported by a cushion, the hand turned laterally, and the index finger, slightly flexed and free from interference or support by the others, inserted as far as the first joint in the muscle recorder.

The giving of the signals and the application of and relief from the various stimuli were recorded by the momentary deflections of a registering pen operated by pressure upon a rubber bulb held in the hand of the conductor of the experiment. There were thus traced upon the one drum at the same time five curves, two registering the respiration, a third the pulse and blood distribution, the fourth the muscle changes, and the fifth the giving of signals and application of the stimuli. The five points of the registering pens were aligned upon the face of the cylinder with each other and with the time recorder, so that being under the control of the operator, there was possible an exact knowledge of the correspondence between the phases of application, relief, etc., and the changes of the function recorders. The smoked paper records were subsequently fixed by dipping in an alcoholic solution of gum sandarac. The tracings were read with the aid of triangles and millimeter scales by reference to base lines parallel to the direction of rotation of the drum. Full notes were taken of each experiment, its conditions and the experience of the subject during its course.

Functional Changes during Perceptual Attention.

The subject sat in an easy position, with eyes closed and instruments adjusted. A period of thirty seconds was allowed to elapse during which he remained quiet, avoiding all movement and mental effort. At the close of this period a watch was opened and brought forwards to the subject's ear, until it was just possible for him, with considerable effort, to follow its ticking. To this faint, rhythmical sound his close voluntary attention was given during a second period of thirty seconds; and a third of similar length, in which the effects of relief and the return of functioning towards the normal type could be observed, closed the experiment. Silence was maintained through the three periods. At the close of each experiment the subject described his mental experiences, the degree, constancy, and mechanism of his attention, disturbances, and the like, so that the quality of the subjective condition represented by the record was known in each case. The following figures exhibit the character of the respiration during perceptual attention.

Average Length of the Respiratory Phases.

1. Normal.

	Inspiration.	Insp. Pause.	Expiration.	Exp. Pause.
A	.68 secs.	.33 secs.	1.08 secs.	1.51 secs.
B	.75 "	.47 "	1.41 "	1.16 "
C	1.35 "	1.08 "	2.59 "	2.55 "
D	1.21 "	.11 "	1.08 "	1.93 "
E	.77 "	.24 "	1.03 "	1.34 "
F	1.31 "	.27 "	2.07 "	1.71 "
G	.95 "	.32 "	1.31 "	1.14

	Total Respiration.	Depth.		Total Resp.	Depth.
A	3.59 secs.	14 mm.	E	3.38 secs.	17 mm.
B	3.81 "	11 "	F	5.35 "	24 "
C	7.45 "	36 "	G	3.62 "	27 "
D	4.34 "	40 "			

2. During Attention.

	Inspiration.	Insp. Pause.	Expiration.	Exp. Pause.
A	.83 secs.	.87 secs.	1.45 secs.	1.25 secs.
B	.73 "	.39 "	1.38 "	.93 "
C	1.12 "	.45 "	2.70 "	1.08 "
D	.74 "	.11 "	1.08 "	1.38 "
E	.69 "	.36 "	1.42 "	1.76 "
F	1.34 "	.22 "	2.02 "	2.09 "
G	.69 "	.57 "	.94 "	1.05 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	4.41 secs.	.19 mm.	E	4.27 secs.	.24 mm.
B	3.43 "	.15 "	F	5.58 "	.22 "
C	5.81 "	.13 "	G	3.28 "	.15 "
D	3.31 "	.28 "			

The characteristic changes accompanying attention to perceptual objects, as shown by these figures, are:

(1) A tendency to reduce the length of the inspiration. The respiration of sleep and of low mental activity in general has been found to be characterized by its long inspiration and short expiration. As the mental excitement rises the latter component increases, the former decreases. The same tendency appears here as attention succeeds inattention. In five subjects the decrease is an absolute one, sometimes of marked extent—*e. g.*, 1.21 to .74 secs.; in the remaining cases, (A) and (F), though the figures show an absolute increase, a comparison of the durations for the full respiration [3.59—4.41 secs.; 5.35—5.58 secs.] reveals the fact that there has been at the same time a relative decrease.

(2) There is a general increase in the relative length of the expiration. In four subjects this increase is also positive; in the fifth, in which an absolute decrease appears, there is at the same time a relative increase in length (1.41–3.81 secs.; 1.38–

3.43 secs). The sixth and seventh do not conform to this type. The time-relation of inspiration and expiration is characteristic of the state of mental activity. Respiration during sleep is marked by relatively slow inspiration and rapid expiration. In drowsiness and after a full meal the same predominance of the inspiration is noticeable. With the increase of cerebral excitement the inspiration grows rapid, direct and strong, the expiration slow and interrupted. The extreme forms are seen in the sudden inspiratory sob of weeping, followed by the prolonged expiration, broken by repeated suspensions of the breath; or in the similarly swift influx of air in laughter with its subsequent series of alternate suspensions and expulsions. Both the strong inspiration and the retardation of contraction during expiration point to an increased expenditure of energy as compared with the phenomena of sleep, where the innervation is sufficient only to inflate the lungs slowly, and where the contraction of the chest at its close is not interfered with by the contraction of the voluntary muscles.

(3) There is a general increase in the rapidity of the respiration. This is also a characteristic of heightened mental activity. The exceptions to it are noted under section (4).

(4) When the respiration decreases in rapidity the retardation is due not to a proportional increase in time of all the component phases, preserving the normal type, but to an abnormal suspension of the breath with the lungs inflated [A. .33—.87 secs.; E. .24—.36 secs.], to a prolongation of the expiration [A. 1.06—1.45 secs.; E. 1.03—1.42 secs.], or to an exaggeration of the respiratory pause [F 1.71—2.00 secs.], all of these indicating an inference with the regular periodic innervation of the organic muscles.

(5) There is a moderate tendency to superficiality of respiration. This is extremely marked in the case of three subjects. In three others it altogether fails to appear. Two of these are characterized also by slow respiration and retardation of the respiration.

(6) In general, the attitude of attention is characterized by disturbance of function. Every departure from the normal type is significant as indicative of an interference with the automatic

character of the respiration. The breathing during attention is marked by just such wide and frequent but irregular fluctuations. This will be made more apparent by the following table of the comparative variations in time and depth during the normal and experimental periods.

	Normal.		*Attention.*	
	Time.	Depth.	Time.	Depth.
A	1.01 secs.	13 min.	3.15 secs.	17 min.
B	.67 "	35 "	1.01 "	45 "
C	.45 "	6 "	.56 "	7 "
D	1.05 "	5 "	2.80 "	36 "
E	.82 "	9 "	2.68 "	25 "
F	.73 "	8 "	1.16 "	5 "
G	1.05 "	10 "	1.75 "	18 "

The changes in variability are more readily appreciable than those in average character. The variation in the length of individual respirations has increased to more than double the normal. This increase in variability is uniform; each individual record of every subject shows it. There is a similar increase in the extent of the variation in depth, amounting in extreme cases to seven times the normal, and failing to appear only in a single case, where the variations in normal and attention phases are as six to five.

Characteristics of the Pulse and Volume Curves.

As throughout the whole series of experiments the plethysmographic and sphygmographic records unite in one volumetric curve, an exact analysis of their separate character is frequently impossible, the change in strength of heart contraction and the secondary features of the pulse wave being obscured by simultaneous changes in the volume curve. A number of significant features, are, however, definitely determinable. There is, without exception, an immediate increase in the rapidity of the rhythm at the beginning of the attention period, succeeded by a more gradual and enduring decline. This slowing usually continues until a point below that of the preliminary period is reached, when there is again a gradual increase towards the normal. The following table shows the extent and duration of the changes for the various subjects concerned. The figures give the averages for successive periods of twelve seconds each.

	Normal.	*Attention.*	
A	72.5 per minute.	72.5 per minute.	70.0 per minute.
A	87.0 " "	90.5 " "	80.0 " "
A	72.5 " "	77.5 " "	75.0 " "
A	80.0 " "	82.5 " "	80.0 " "
B	75.0 " "	82.5 " "	77.5 " "
C	62.5 " "	67.5 " "	65.0 " "
C	62.5 " "	65.0 " "	55.0 " "
D	95.0 " "	102.5 " "	92.5 " "

The maximum increase is reached within the first ten seconds in the case of all subjects; and the whole acceleration is usually confined to a period of twenty-five seconds. In some cases the decline is more gradual and reaches the normal only towards the close of the experiment. With the greater number the retardation is more rapid and passes beyond the normal, in some cases to a greater extent than the primary acceleration rose above it.

There is, in the case of most subjects, an increase in the interference of the respiratory period with the volume curve, which tends to obscure the character of the pulse-beats. So far as has been observed, the effect of attention upon the strength of heart contraction is variable. With three of the subjects there is a reduction in the extent of the stroke, independently of the variations in the volume curve or the respiratory changes. In subject D there is an increase, together with a marked irregularity, in extent, and in a similar inconstancy in the rhythm of successive beats. This perhaps marks, in one of D's temperament, the presence of a rather strong emotional element due to nervous excitement.

Simultaneously with the primary acceleration of the pulse occurs a rapid and extensive fall in volume, reaching a minimum at the end of a period varying from six to ten seconds, followed either by a gradual and more continuous rise towards the normal line, or by a series of subsidiary waves, finally approximating to that which characterized the preliminary period. These wave-like and frequently rhythmical changes of volume find an apt interpretation in the hypothesis of fluctuation, each pulse of close and accentuated attention being followed by a period of distraction and relaxation; and the prevalent subsidence of the waves in the latter part of the experiment may indicate the gradual failing of attention through fatigue.

The effect of attention upon the interference of the respiratory period is variable and obscure. In some cases the rhythmical increase and diminution of volume is reinforced, even when the respiration grows more superficial; in others there is a reduction in the interference, especially if the breathing becomes more shallow. In others again no definable alteration in character appears.

Muscle Changes in Perceptual Attention.

These are typical and uniform. The changes consist, in general, of an exhibition of movement and tendency to relaxation of the muscles, indicating a lowering in the static tonicity of the muscular system. In the preliminary period the finger record is usually effected by the respiration, a slight extension of the arm accompanying each inspiratory elevation of the chest, and being followed by a corresponding contraction during expiration. These changes are uniformly reduced and frequently obliterated during the period of attention, though the respiration suffer no diminution in depth. The preliminary period is usually marked by a constant *subsultus tendinorum* as well as by more massive spasmodic contractions and expansions of the muscles. These are reduced and frequently disappear during the attention period. The tendency to relaxation does not always appear as a positive extension of the finger. In some experiments the finger will be found to remain stationary during attention; in some others a continued or intermittent contraction is manifested. But in every such case there is a *relative* relaxation. Where the preliminary period shows a tendency to contract—if strong, it continues, but in reduced degree,—if slight, it disappears or is replaced by moderate extension. Where the preliminary period shows no change, the passage to attention is marked by extension, slight in some cases, great in others; and where the first period is characterized by continuous extension it is immediately and strongly reinforced at the beginning of the experimental stage. These expansions and contractions during the earlier period are probably due to the fact of insufficient time having been allowed for the muscles to reach a state of equilibrium. But the effect of the passage to attention is seen as clearly in the altered direction of the curve as it is when the preliminary period is marked by rest.

A negative illustration of the muscular changes accompanying attention appears at the close of the period when the tremors, irregularity in contraction and expansion, respiratory influence, and general tendency to contraction again set in.

Functional Changes during Attention with a Strong Sensory Element.

In the preceding series the object of attention was a neutral one; there was nothing in the ticking of the watch which was *per se* interesting. The attention was wholly secondary and voluntary; the subject deliberately abstracted from all other objects and focused his attention upon this dull, monotonously-repeated sound.

There will evidently be a new element introduced into the mental complex if a stimulus be selected which besides the derived interest of voluntary attention, comes to the subject with a distinct of its own, one which by its unusual or pronounced sensory character arouses a certain emotional element and fixes the attention by its own power. Such a combination of voluntary and involuntary attention elements was sought by tracing upon the subject's cheek with the tip of a pencil a series of geometrical figures which the subject endeavored to discriminate and recognize by the sense of touch alone. The stimulus was novel; it involved a continuous sense stimulation apart from the volition of the subject; and with all the persons concerned in the experiments it seized and held the passive attention as simple touch sensation, apart from the character of the lines drawn. There was required in addition a close and constant effort to recognize the various forms which were one after another inscribed upon the skin. These experiments were conducted with five subjects with the following results:

Average Duration of the Phases of Respiration.

1. Normal.

	Inspiration.	Insp. Pause.	Expiration.	Exp. Pause.
A	.76 secs.	.22 secs.	.58 secs.	1.22 secs.
B	1.27 "	.31 "	1.40 "	1.46 "
C	.56 "	.17 "	1.03 "	1.17 "
D	1.08 "	.27 "	.72 "	.72 "
E	.63 "	.22 "	.58 "	.58 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	2.78 secs.	25 mm.	D.—	2.79 secs.	25 mm.
B	4.45 "	37 "	E.—	2.01 "	20 "
C	2.93 "	25 "			

2. During Attention.

	Inspiration.	Insp. Pause.	Expiration.	Exp. Pause.
A	.49 secs.	.36 secs.	.85 secs.	1.11 secs.
B	1.14 "	.36 "	1.48 "	1.90 "
C	.47 "	.56 "	1.34 "	.83 "
D	.94 "	.18 "	.94 "	.85 "
E	.49 "	.54 "	1.39 "	1.12 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	2.81 secs.	13 mm.	D.—	3.91 secs.	20 mm.
B	4.91 "	30 "	E.—	2.50 "	20 "
C	3.16 "	13 "			

The comparative changes here are of the same type as those which were found to characterize the passage from rest to purely voluntary attention; but they are more constant and of greater extent.

There is first a reduction in the relative length of the inspiration, and an increase in that of the expiration. But in both these components the change is more invariable than in the preceding series and of greater extent. In the present form of attention it is throughout positive as well as relative; the extent of the reduction, also, is greater in the present series than in the earlier one. In the case of the expiration, again, there is a more constant increase in duration, every subject showing both a relative and a positive increase, and this increase is of greater extent than in voluntary attention.

There is in every case a decrease in the rapidity of the rhythm. In all cases but one—E. 2.01–3.50—this increase is relatively slight. Voluntary attention, on the other hand, was characterized by an increase in the rapidity of the respiration.

The changes in the respiratory pauses are variable; in some objects the averages show an increase, in others a decrease appears. These figures are among the least significant of the record. What characterizes the curves of this composite attention is essentially departure from type, disturbance of function, which a system of averages may as readily tend to obliterate as to preserve. This feature of the breathing during attention appears more plainly by a comparison of the extent of the variations in respiration during the contrasted periods.

	Normal.		*Attention.*	
	Time.	Depth.	Time.	Depth.
A	1.10 secs.	10 mm.	3.40 secs.	22 mm.
B	.62 "	4 "	2.92 "	20 "
C	.45 "	2 "	.57 "	6 "
D	.22 "	3 "	.45 "	4 "
E	.45 "	4 "	2.90 "	9 "

The increase of variation both in the rapidity of the rhythm and in the extent of inflation of the period of attention over that of the preliminary period is evident. These fluctuations will also be found greater than those which accompanied voluntary attention. There is at the same time a uniform reduction of considerable extent in the depth of the breathing. The same general tendency is present also in the previous form of attention, but is found lacking with three of the subjects.

Pulse and Volume Changes.

Few changes in the character of the pulse are to be observed in these records. The apparent strength of stroke remains unaltered during the two periods. Slight irregularities in the interval between successive strokes appear in one or two instances, and with one subject there is a slight reduction in the strength of the stroke. In the rate of the pulse beats, before and after the beginning of the experiment, and at successive periods during its continuance, a new set of changes is met with varying from the normal of the preliminary period in a direction opposite to those which appeared in the previous series. With all the subjects who took part, the tracing of the figures upon the cheek is accompanied by a retardation in the rate of the pulse, usually immediate, but in some instances delayed for several seconds, followed by an increase again towards the normal. In some cases the diminishing retardation continued throughout the period of stimulation; in others an acceleration beyond the rate of the preliminary stage was reached; in one record a secondary wave of retardation appears. These changes are independent of the fluctuations which occur in the volume curve. They are altogether unexpected; the addition of the sensory stimulus, bringing with it a certain emotional tinge, might lead one to expect an increase of the acceleration which was found in the previous series instead of the retardation which actually obtains.

Muscle Changes.

The same muscle changes appear here which characterize the earlier form of attention. In all six subjects there is an alteration in the direction of the curve at the beginning of the attention period. These changes do not always present a positive relaxation of the hand. In those instances in which a gradual contraction continues throughout the previous period, the new direction appears as a diminution in the rate of contraction. When the contraction is slight the subsequent curve shows either a state of equilibrium, or a faint expansion. When a previous tendency to expansion exists it is appreciably reinforced from the beginning of the new attitude onward.

There is, also, in the preceding experiments a tendency to inhibition of movements manifested in the absence of slight irregularities in the curve, and the dampening of the *subsultus tendinorum*. The respiratory period is less marked during attention than in the preliminary period, occasionally disappearing. These muscle changes are not invariably present, and different records fail to show any change of condition in the passage from inattention to stimulated attention.

Functional Changes during Recall of Past Events.

The preceding experiments were concerned with objects of perceptual attention; the present and succeeding sections abstract from objects of sense, and have to do with reflection and more purely intellectual processes.

The method of conducting the experiments was simple. The subject sat as before with closed eyes, the instruments adjusted upon his body. After a preliminary period of inactivity, he was required to recall various groups of objects, such as the instruments which he had seen in a certain case, the substance of a late lecture, the experiences of a particular day in the past, and the like. At the close a final period of rest was given; and at the end of each experiment the observer made full notes of his subjective experiences.

In recall the changes in the character of the respiration are of the same general type as those found present in perceptual attention, but are more variable in direction, of slightly less extent, and present more individual variations. The quantitative relation of the phases is shown in the following tables:

1. Normal.

	Inspiration.	Insp. Pause.	Expiration.	Ex. Pause.
A	.76 secs.	.22 secs.	1.37 secs.	1.35 secs.
B	.72 "	.67 "	1.57 "	1.30 "
C	.76 "	.36 "	1.37 "	.90 "
D	1.12 "	.36 "	1.17 "	.40 "
E	1.26 "	.76 "	2.92 "	.63 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	3.70 secs.	29 mm.	D.—	3.05 secs.	52 mm.
B	4.26 "	23 "	E.—	5.57 "	37 "
C	3.39 "	30 "			

2. Recall.

	Inspiration.	Insp. Pause.	Expiration.	Exp. Pause.
A	.49 secs.	.27 secs.	1.08 secs.	1.03 sec.
B	.54 "	.18 "	1.80 "	.81 "
C	.30 "	.54 "	1.03 "	1.35 "
D	.63 "	.40 "	1.26 "	.54 "
E	.94 "	.63 "	1.71 "	.63 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	2.87 secs.	22 mm.	D	2.83 secs.	37 mm.
B	3.33 "	32 "	E	3.91 "	24 "
C	3.23 "	13 "			

Variations in Respiration.

	Normal.		*Recall.*	
	Time.	Depth.	Time.	Depth.
A	.45 secs.	6 mm.	.90 secs.	19 mm.
B	1.35 "	4 "	.45 "	9 "
C	.45 "	4 "	.70 "	9 "
D	.90 "	10 "	.22 "	6 "
E	1.35 "	4 "	.70 "	9 "

The general increase in irregularity of depth is evident here. The extent of this increase is magnified when it is remembered that there is at the same time a reduction in the average depth of respiration. It is probable that this irregularity is chiefly a physiologically originated phenomenon. The rapid superficial breathing which accompanies continued effort to recall, affords insufficient aëration of the blood; the series is broken in upon here and there by one or two fuller respirations stimulated by incipient asphyxiation. In some cases, however, an irregularity appears which is more closely related to the consciousness aspect of the experience.

The simple effort involved in all recall is expressed in the quickened shallow breathing, which increases in rapidity and superficiality with the difficulty involved in the process. The change here is rather in the direction of increased uniformity than of variation from it. When the objects of recall are not neutral, such as remembering a series of numbers or the instruments in a certain case, but are colored with a strong emotional element; wide irregularities in the character of the individual respiration are presented, similar to those which appear under strong sensory stimuli. The subject is, in a less intense degree, living over again the experiences which he is endeavoring to recall, and the disturbance of function which accompanied their original occurrence is partially reëstablished here.

There are therefore two elements in recall to be kept separate, as affecting the character of the bodily functions: (1) the effect of attention, the simple intellectual effort requisite to the recall of indifferent objects; (2) the effect of personal relation to the objects of recall.

The character of the functional change varies from individual to individual. In some cases, instead of the superficial respiration which characterizes most subjects, there is found a rapid, regular but more profound respiration than in the normal. With other subjects there is a great reduction in depth, the breathing at times being almost suspended.

While the degree of functional disturbance varies from subject to subject, within the individual record the variation increases with the effort requisite for recall. A sense of ease and freedom is accompanied by slight variation from the normal; with increasing difficulty the deflection of the curves becomes greater and greater. This is seen most clearly in those cases in which the emotional element is got rid of, and the changes are wholly due to the intellectual effort involved. This condition is approximated to in the following series, in which the subject was required to perform certain arithmetical calculations of varying complexity.

At present we may say that the effort to recall a series of past events is accompanied by a rapid superficial breathing, marked by a swift short inspiration and an interrupted and prolonged expiration; that with the decrease in depth there is a greater irregularity in the duration of successive respirations, in the depth of the breathing, and in the time relations of the component phases of the individual respirations; and that as the process of recall becomes more difficult and involves greater effort the extent of the variation from the normal increases.

Also, that while simple recall of indifferent objects is accompanied by a quick slight breathing which may be more uniform than the normal, the recall of things which involve a strong emotional element is characterized by irregularity of rate, depth and form, the nature and extent of the irregularities depending upon the emotional character of the objects recalled. The average rapidity of the respiration is likewise without exception increased; and there is an absence of the suspensions with full or deflated lungs, or in the midst of expiration, which frequently characterized the former mental attitude.

Characteristics of the Pulse and Volume Curves.

The type of change here seems to vary. The pulse-beat is in some cases at first increased, then slowly reduced to the normal towards the close of the experiment. With others there is an immediate slowing of the pulse rate gradually accelerating towards the normal again. These two types of change are present in the same individual at different times. In all cases in which an immediate increase in rate appears the process of recall was attended with difficulty. In those marked by a fall it was either easy or no mention was made of effort. The pulse rate frequently presents the form of a series of waves, alternately rising above and falling below the normal of the preliminary period. It is possible that these may indicate a series of fluctuations in the intensity of the attention and of the effort to recall.

In almost all cases the pulse stroke is shortened during recall. Frequently this change is immediate and definite, in some cases the reduction amounting to one-third or even one-half of the previous extent of stroke. There is usually with this a simultaneous reduction in the depth of the breathing, which, as a purely physiological phenomenon, is accompanied by such a reduction in the pulse stroke, but the change under these conditions is concomitant with a rapid rise in volume, and the reduction in pulse stroke is proportional to the extent of the volume increase. In the case of recall, however, the reduction is accompanied by a decrease in arterial tension, marked by a fall more or less rapid and extensive in the volume curve. It also occurs when the respiration is increased in depth instead of becoming more superficial.

The form of the pulse wave also undergoes alteration. The preliminary period is characterized by a full strong stroke, followed by an immediate and sharp fall towards the dicrotic crest, making an acute apex; in the period of recall the shorter stroke is succeeded by a slow delayed subsidence, the tracer dragging on towards the next stroke before a decided fall takes place giving a blunted form to the arterial wave.

The volume curve shows less tendency to typical forms of change than in the previous experiments. There is usually an immediate fall in volume at the beginning of the period, continuing from five to ten seconds, and followed by a more gradual rise, the two phases being repeated several times during the course of the period, usually with a decrease in the width of the variation towards the end.

These wide fluctuations in the volume curve during recall are the most constant factors which appear. Since the transition from the previous diffused mental state to the concentration of attention in recall is typically marked by a fall of greater or less extent in the arm volume, the occurrence of these repeated waves suggests a fluctuation in the degree of effort made, the attention coming and going in pulses.

The interference of the respiratory period increases during recall. In normal cases its influence increases and diminishes with the depth of the respiration; suspension of the breath causes it to disappear. Here, on the contrary, it grows more pronounced even when the breathing simultaneously grows more superficial.

As relief there is a slowing of the respiratory rhythm, an increase in the depth of the breathing and of the duration of the inspiration with a relative reduction in that of the expiration, an increase in the extent of the pulse stroke, a slowing of the pulse rhythm and a rise in the volume of the arm.

Muscle Changes during Recall.

The changes here are similar to those found present in the earlier series on perceptual attention. The transition from the preliminary effortless state to that of strenuous recall is marked by a change in the direction of the muscle curve. This is manifested as before in a reduction in the degree of contraction, a disappearance of it, or a substitution of expansion, in cases in which a contraction appears in the preliminary period, and a reinforcement of the tendency of expansion where such already existed. This change in the direction of the curve is very uniform. There is also a reduction in the greater irregularities which usually characterize the normal curve, and a disappearance or dampening of the *subsultus tendinorum*. The essential features of this curve, then, are a tendency to a relaxed condition of the muscles, and an absence of muscular excitement, marked by a quiet even curve without massive changes or tremors.

Functional Changes during Calculation.

The method of experimentation here is varied only by the substitution of a new form of stimulation. The subject was required to perform certain arithmetical calculations instead of recalling a series of past events. We may therefore proceed immediately to a consideration of the particular changes in function which present themselves.

1. Normal.

	Inspiration	Insp. Pause.	Expiration.	Exp. Pause.
A	.72 secs.	.22 secs.	1.39 secs.	1.35 secs.
B	.76 "	.31 "	1.24 "	1.12 "
C	.67 "	.40 "	1.17 "	1.35 "
D	1.26 "	... "	1.89 "	1.98 "
E	.67 "	.49 "	.85 "	1.11 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	3.68 secs.	29 mm.	D.—	5.13 secs.	81 mm.
B	3.43 "	34 "	E.—	3.12 "	15 "
C	3.59 "	20 "			

2. Calculation.

	Inspiration.	Insp. Pause.	Expiration.	Exp. Pause.
A	.49 secs.	.22 secs.	1.12 secs.	.90 secs.
B	.22 "	.31 "	1.12 "	.36 "
C	.67 "	.22 "	1.26 "	.37 "
D	.58 "	.13 "	1.62 "	1.89 "
E	.45 "	.45 "	.85 "	.85 "

	Total Resp.	Depth.		Total Resp.	Depth.
A	2.63 secs.	22 mm.	D.—	4.22 secs.	61 mm.
B	2.01 "	13 "	E.—	2.60 "	12 "
C	2.51 "	25 "			

There appears here, as in the previous experiments, an increase in the rapidity of the respiratory rhythm; but while in both series there is a reduction in the rate of the respiration, in the former the relation of the component phases was significantly altered, while here there is little variation from the tpye of normal unstimulated respiration. In one component, however, there is a great change, the respiratory pause is invariably shortened, usually to a great extent. In normal breathing it is frequently accentuated, the expiration being followed by a distinct period of quiescence before the succeeding inspiration. During calculation this pause is either lessened or altogether disappears; inspiration follows expiration with scarcely a break. This feature is significant. Exaggerated pauses are characteristic of one attitude of mind, diminished pauses of a typically different state. In any sudden surprise the breath remains suspended, inhibited sometimes for several moments, till the shock passes by. The same suspension appears in more exaggerated forms in fear and terror. In close attention—in the effort to catch a faint sound, for example—it is also a characteristic feature. These states of mind are marked by a general inhibition of function which extends to temporary cessation of the respiratory process, until the oppression of the lungs finds relief in renewed respiration. In all work—expenditure of effort—on the other hand, there is an increase of functional activity. The heart beats faster and stronger, the respiration grows deeper and more rapid, and the glandular secretions of the skin become more copious during muscular exertion. And the same change, in greater or less degree, accompanies increased intellectual activity. In exciting emotions the respiration is deep and rapid with lungs inflated, inspiration and expiration succeeding each other without pause.

In more purely intellectual activity, the breathing, which is usually more superficial as well as more rapid, is marked by an almost complete obliteration of the respiratory pause. This rapid, equable and slightly superficial respiration may be conducive to a more constant supply of blood to the brain, the sudden and great expansion of the lungs in deep respiration causing too great a fluctuation in the quantity supplied to its vascular tissues for continuous cerebral activity.

The uniformity of respiration during calculation will further appear from the following table of variations during the two periods:

	Normal.		*Calculation.*	
	Time.	Depth.	Time.	Depth.
A	.22 secs.	4 mm.	.66 secs.	19 mm.
B	1.10 "	17 "	.90 "	20 "
C	.70 "	7 "	.22 "	5 "
D	1.10 "	12 "	.70 "	30 "
E	.90 "	4 "	.70 "	6 "

The variation in depth is usually greater in the more highly stimulated conditions. This is probably due to two different causes. These are, first, the physiological one of periodically increased innervation from incipient asphyxiation, the more superficial respiration being insufficient for the needs of the system; and, second, the psychological one of fluctuation in the intensity of the effort required in calculation periods of close attention with rapid, regular, superficial respiration alternating with periods of relaxation indicated by the fuller breathing of relief. In some cases no such rhythmical series appears, the respiration growing continually more superficial as the calculation proceeds. This may indicate a continued attention with increasing effect, as both the shallowness of breathing and diminution of volume are found to bear close relation to the difficulty of reckoning involved in the problems given.

Pulse Volume Curves.

The beginning of calculation is in all cases accompanied by an acceleration of the pulse rate, sometimes of great extent, and continuing throughout the larger part of the period. The form of this acceleration varies greatly from individual to individual and from record to record.

The rise is sometimes immediate and rapid, succeeded either by a similarly sudden fall or by a sustained increase in rate. Sometimes the acceleration is slow, maintained during a considerable period, and falling again gradually towards the normal. In some cases the rise is developed for several seconds after calculation begins. The return to the normal is usually reached within a minute's calculation. In some cases the acceleration dies away before one-half the time of calculation has expired. At relief a fall below the normal appears with occasionally a secondary wave of acceleration. The following figures give the average rate before calculation and for successive periods of twelve seconds during calculation:

	Preliminary Period.	During Calculation.		
A	62.5 seconds.	67.5;	80.0;	85.0 secs.
B	72.5 "	72.5;	74.5;	75.0 "
C	77.5 "	77.5;	82.5;	85.0 "
"	75.0 "	82.5;	75.0;	—— "
D	57.5 "	65.0;	62.5;	62.5 "
"	65.5 "	70.0;	75.0;	75.0 "
E	60.0 "	65.0;	67.5;	65.0 "
"	75.0 "	77.5;	80.0;	75.0 "
F	50.0 "	55.0;	60.0;	60.0 "

An almost constant feature of the pulse during calculation is the reduction in height of stroke. This may be due either to a weaker ventricular contraction or to an increase in the arterial tension. The latter condition accompanies any voluntary reduction in depth or complete suspension of the respiration. The volumetric curve shows an immediate increase in curve volume, due to congestion of the blood in the smaller veins and arteries, and a reduction in the height of pulse wave—which may finally become obliterated—due to the continued increase in arterial tension. If then the respiration uniformly becomes more superficial during calculation, such a reduction of the pulse wave is to be expected; it becomes a secondary phenomenon, and, except as depending upon the primary change in respiration, is relatively insignificant.

But in these records it appears independently of the respiratory changes, occurring when the respiration is increased in depth as well as when there is no appreciable variation in it. Compare it again with the concomitant changes in the volume curve. This is found to alter as the character of the respiration changes, and as the volume changes the form of the pulse also undergoes alteration. But the changes which appear during calculation seem to be independent of the changes in the volume curve as well. A reduction in the height of the pulse wave is the physiological concomitant of increased arterial distension; but here it occurs simultaneously with its fall in arm-volume which normally marks the increased mental activity during calculation. It is present when there is no appreciable change in arm-volume, and it persists both in the rising and falling of the curve. The reduction of the pulse wave, therefore, since it appears even with increased respiration and a falling volume curve, both of which should tend to reinforce it, is a direct effect of the central change obtaining during calculation.

The volume changes are analogous to those of the preceding series. Decrease is more or less rapid and extensive, continuing for a variable period, and followed either by a continuous, gradual rise, or by a series of wave-like fluctuations in volume.

Muscle Curve.

The changes in the muscle curve are usually of slight extent and identical in type with those described in the preceding experiments upon recall. They are not invariable in direction nor so constant as in recall and perceptual attention. Occasionally there is an increase in the tremor of the muscles, occasionally also a greater irregularity in the form of the curve during calculation than in the preliminary period; and in one or two instances a slight tendency to contraction appears, or a previously existing contraction is reinforced.

In general, however, these changes are the same as in those of recall. Contraction and muscle tension are replaced by relaxation and extension of the fingers. This is shown also in the contraction which frequently appears again at the close of the period, the static muscle tension recovering its normal tone as soon as attention is drawn from the process of calculation. With this relaxation goes a diminution of muscular tremor and a reduction in the irregularities of the curve. This relaxed condition during close mental effort indicates a reduction in the degree of reflex stimulation throughout the organism, and inferentially a greater efficiency to the central nervous discharges. Tension represents expenditure of energy; there is a continual drainage of nervous force to the peripheral system when this is in a state of activity, and the lowering of this expenditure—which characterizes the types of activity here investigated—leaves free a wider margin of available energy for the central activity.

DISCUSSION AND REPORTS.

THE METAPHYSICAL METHOD IN ETHICS.[1]

In his preface Mr. D'Arcy defines his essential point of view and aim. It is to give *briefly* 'an account as well of the metaphysical basis as of the ethical superstructure' of conduct. Referring to Mr. Muirhead's Elements, Mr. Mackenzie's Manual, and my own Outlines of Ethics, he says of them that their ethical contents is much the same as that of his own work, 'but all three build without a foundation.' This foundation he takes to be Green's method and main results as reached in his Prolegomena to Ethics,[2] and he proposes to do in small space what Green did in a more extended way.

It should also be noted that Mr. D'Arcy declares his inability "to accept in its entirety the Hegelian[3] conception of the spiritual principle as presented" by Green. And as matter of fact, Mr. D'Arcy accepts the doctrine of Green only up to a certain point, and then supplements it by quite other considerations, derived, as a rule, from the real or supposed needs of man's religious consciousness, and sometimes from 'common sense.'

It is this effort, then, of Mr. D'Arcy to give the metaphysical

[1] *A short Study of Ethics*. D' Arcy. London and New York, Macmillan & Co., 1895–6.

[2] As silence is supposed to give consent, it may not be impertinent for me to say that while I have always recognized my own great indebtedness to Green, yet his metaphysical method seems to me far from affording any adequate basis for ethical doctrine; on the contrary, all the serious weaknesses in Green's specifically ethical discussions seem to me to flow from his metaphysical assumptions.

[3] Mr. D'Arcy seems to accept *in toto*, as does Professor James Seth, Professor Andrew Seth's identification of Green's doctrine with Hegel's. I never have been able to see any basis for this identification. Hegel protests continuously and consistently against the Kanto-Fichtean ethics, and Green's standpoint is essentially the latter. The logic of the identification of Hegel and Green seems to be: Each is 'unsound' as to the relation of the human and divine self, and, therefore, both teach the same doctrine.

foundations of ethical theory, which, affording the distinctive feature of his book, calls for especial attention.

The primary condition of all experience is the relation of the subject and object. The subject eludes our grasp, when approached by itself. The not-self or object is divided into an inner and an outer region, the former including sensations, emotions, thoughts, etc.; the latter contains all the things we know in the world around us. The inner experiences, of course, presuppose the thinking subject. The following course of reasoning shows that the outer region is also dependent. Every *thing* is constituted by relations. The world of things in space and time is simply a vast complex of relations. But it is 'of the very nature of a relation to have no existence, no meaning, except for a thinker.' A relation is a "unifying of the manifold, and is, therefore, an impossibility apart from a subject, which can pass from one member of the relation to the other, and combine both in a single apprehension." Hence "things exist only so far as they are due to the synthetic activity of the knowing subject." Morever, since the thing is always constituted by relations to everything else in the universe, it is really a 'cosmic object,' so that the self is the unifying principle in the whole cosmos of experience.

The self is thus a unifying principle, and it is also the ultimate principle of unity. It is not simply the correlative of object, for it can make itself its own object, being self-conscious. It is a real unit, not a logical principle of a unity.

So far the language and the method remind us of Green, although Green, I think, would hesitate at this extraordinary identification of the self with subject apart from object, and at the ruling out from the self of all sensations, emotions and thoughts. As the method is nominally derived from the Kantian, it is perhaps worth while to note that Kant urged not only the necessity of the synthetic activity of the subject, but equally urged that the subject could be conscious of itself and of its unity only through its synthetic activity upon the manifold. But Mr. D'Arcy knows a better way than that. This theory might lead to the doctrine of the correlativity of the subject and the cosmos of experience—which appears to be an objectionable doctrine, leading to Pantheism—and consequently having affirmed the synthetic activity of the self in the constitution of the objective world, Mr. D'Arcy affirms that since it is self-conscious, it can also abstract itself wholly from the world which it constitutes. As Mr. D'Arcy simply affirms this as given in the fact of self-consciousness, wholly apart from any examination of the nature or method of self-consciousness, I can only affirm from my standpoint that this way of giving 'foundations' for ethics seems to require more foundations for itself than it suceeeds in supplying.

Were the doctrine of the correlativity of subject and world affirmed, the self would obviously secure a certain universality; it would not be a merely particular self, if its essential being were found in the constituting of an objective world. But since Mr. D'Arcy holds that the subject exists in essential distinction from this constitutive work, and engages in it as it were only as by play, or as supererogation, the problem comes up: What sort of existence does the constituted world have? Is the universe a private possession of my own? Are we not committed to the doctrine of subjective idealism? Mr. D'Arcy implies, this would be the result if it were intended "to identify the cosmos of the individual experience with Nature. Nature must be accepted as a great fact, a mighty universe." Having thus secured from the simple 'common sense' affirmation (see p. 18) a world independent of the subject's consciousness, Mr. D'Arcy has also obtained a basis for the affirmation of an eternal self, free from all the pantheistic leanings of Green's doctrine. Since *our* world of natural things depends upon *our* synthetic activity, then surely this big world of Nature depends upon *its* constituting spirit—God.

I am forced to stop once more in my exposition to raise the question: What founds these foundations? Upon Green's doctrine—no matter what objections may be brought upon other grounds—there is one self and universe. There is no question of subjective idealism, because the subject is defined by reference to the permanent and objective work of constituting a universe; the particular individual knowing is a process of reproducing the eternal constitutive action. But this seems to Mr. D'Arcy pantheistic, and for reasons which he has not explained to the reader (save as indicated in deference to the opinions of Professor Seth and Mr. Balfour) pantheistic implications are to be avoided at all hazards, including those of logic. Hence this sudden break into a cosmos of my experience, and another bigger cosmos, with two spirits, the individual for my cosmos, God for the big one. Two questions can hardly be kept back. If we accept, because we cannot help believing it, the existence of this larger cosmos, it must also be remarked that common sense equally denies the dependence of *our* cosmos upon our subjective activity. Common sense is not particularly alarmed about the existence of the sun, moon and stars in the big cosmos, but objects with great vigor to making the sun, moon and stars which are individually known dependent upon our individual thinking power. I doubt very much if Mr. D'Arcy can satisfy the realist by handing over to him a world, however big, which is unknown, while allowing the subjective idealist complete proprietary rights in the cosmos of individual experience.

But it may be said this is quite unfair to Mr. D'Arcy. Does he not say that the "cosmos of experience must be recognized as identical with a part of the great cosmos of Nature?" This brings me to my second question: Why then is not the individual self-identical with God so far as the identity of worlds goes? How, indeed, do we know there is a bigger unknown world, save as a projection, an extension, out of our present experience? Is it our 'own' self,[1] or is it the absolute spirit which really constitutes our cosmos? If the former, how shall we account for its coincidence with the cosmos of the absolute subject, and for the continuity between the two, as the individual cosmos extends itself? How shall we account for this remarkable capacity on the part of a uniquely individual self to construct a world having its own objectivity and relative permanence? But if the latter, then the whole theory of the ultimate and irreducible distinction of the two selves breaks down.

This same method, viz: the following of the Kantian analysis of knowledge up to a certain point and then the contradiction of its logical conclusion in the interests of religion and common sense—appears in the discussion of volition and of the common good. Will is treated as self-determination, and as indeed, only the more explicit recognition of the constitutive process found in all knowledge. "Every act of self-determination, every volition, is a determination, not simply of one thing, but of the whole cosmos of experience. Self-determination must be world-determination." This principle of determination recognized from the standpoint of the whole is freedom; while necessity is the principle of the articulation of the parts. They are thus correlative and imply each other, instead of being contradictory. That is to say, each fact or event taken as particular is necessitated; but that it is determined at all and determined in relation to other facts is due to an act of self-determination on the part of the subject. (P. 29; pp. 39 and 49 also.)

[1] Nothing could exceed Mr. D'Arcy's conviction of the 'ultimateness' of the individual self. "Self is for every man unique and ultimate. The identification of the self in every man with God involves the identification of all human selves. But since each self is for itself unique and ultimate, this identification amounts to a denial of the essential nature of selfhood." P. 46.

Why the self and the world should not be correlatives, while self determination and world-determination, freedom and necessity, are correlatives, Mr. D'Arcy does not explain. It is difficult to see why one principle should hold for thought and another for volition; or why, if one is objected to on the ground of pantheistic tendencies, the other is not equally 'dangerous.' The pressure to make self-determination and world-determination correlatives is obvious. Without this cor relativity, self-determination would occur in a purely transcendental, and, so far as we are concerned, empty region; will would have nothing to say or to do with the details of conduct. But the demand for correlativity on the side of knowledge is certainly none the less real altho' not quite as obvious. What the self-consciousness is which is found neither in consciousness of objects, nor yet in sensations, thoughts or emotions, Mr. D'Arcy does not explain, and we have only his word for it that it is not formal and empty.

The contradiction is still more glaring when we deal with the question of the End or Good. Mr. D'Arcy having settled that the subject is purely individual—for it must not get too closely implicated with the divine self for fear of pantheism—is quite consistent in holding that the end of self is egoistic. "Will is by nature egoistic. . . . . No other individual can stand on a level with the self. . . . . Reason is essentially anti-social. . . . . Self, unless mastered by some superior principle, must wage unceasing war against all who would pretend to equal authority." (Pp. 58, 59; the same doctrine also on p. 124 and p. 147.) Hence every moral system independent of religious ideas breaks down. It cannot explain why a man should love his neighbor as himself; it cannot justify the idea of a common good.[1] On the same line of thought, Mr. D'Arcy questions whether society is really an organic whole, since the individual is so very individual, and refers to it as an 'amorphous mass of tissue' (p. 73).[2]

[1] Mr. D'Arcy seems a little hard on the individual self. In the first place, it must be purely individual and unique, since otherwise it will get mixed up in a most pantheistic fashion with God and other selves. On religious grounds, in other words, it is quite shut up in itself. Then the interests of religion being duly secured, the self is gravely rebuked for its self-centred and self-seeking nature, and assured to be greatly in need of the assistance of religion to give it an end common with that of others. It is a little hard, I repeat, to refuse and to demand at the same time participation with other selves to the individual self, and both in the name of religion.

[2] Mr. D'Arcy nevertheless holds that there is no other idea save that of organic unity, which can be applied to society, and yet that the truth is not fully represented in that idea (p. 74).

But on the other side, religion is going to help out the egoistic nature of the self. We cannot stop short, after all, with the unity of the self. In this case "God himself would be simply one unit in a multitude and isolated from his creatures. But it is impossible to end in a disconnected multitude." The mind is forced to suppose some principle of unity deeper than the unity of self-consciousness. There is in God a transcendent principle by which he forms the ultimate bond of union among the multitude of persons. The fact of the union of spirits must be assumed as the ultimate basis of all coherence, speculative and practical. (Pp. 47–8.) Hence the common good for all persons. "All persons are naturally exclusive *(i. e.*, they limit one another), yet are they one in God. Hence the good for the whole is the good for every separate member. The true good for every man is a common good and an absolute good." (P. 102, see also p. 124.) Man and God have a common end. The end of conduct is identified with the end of the universe (p. 126).

We have precisely the contradiction here between the isolated, egoistic end of the self, and the common end of the self through its transcendental union with others in God that we met before as regards the constitutive action of self in *our* cosmos, and of God in *the* cosmos, except that here it is most explicitly recognized that We must not exclude the working of the divine end from the constitution of the human end. Mr. D'Arcy might, indeed, attempt to bridge the gulf by holding that the natural self is wholly given to evil; and that only by supernatural grace, initiated wholly from without, does the natural self come to such social ends; but there are no traces of any such doctrine in him. He seems to hold that in the moral life as such there is the immanence of the common end through the union of all selves in God. Were it not that the contradiction obviously escaped Mr. D'Arcy himself, I should think it wholly unnecessary to point it out. As it is, I must be pardoned for saying that if there is one self, named the divine self, in which all selves are united in a common end which is also the goal of the evolution of the universe, then the doctrine regarding the isolated, exclusive character of each individual self must be radically modified. It certainly is not legitimate to insist on the purely individual character of the self from one point of view; and then, when different considerations are in view, insist upon the community of selves. That the two ends of the contradiction are both set up in the name of religion does not make it any the less a contradiction; although it may make one suspicious of the particular type of religion represented.

Thus far the tendency of our examination has been to make us question whether Mr. D'Arcy's metaphysical foundations do not of themselves require more grounding than any ordinary ethical theory is likely to call for. I shall take space for just one application of his metaphysical to his ethical doctrine, seen in the question of the end, with a view to determining whether the ethical superstructure stands any the more firmly for the foundation put under it.

The ultimate end is the idea of a social universe in which every person's capabilities shall receive their full realization, and in which every person's realization shall contribute to every other person's realization. It is impossible, however, to give any further definition of the ultimate end, because it is impossible to know what are the possibilities of selfhood (pp. 104–5). Whence it is a fair inference that the end though not formal in itself is purely formal for us. "It must be granted at once that the Ideal End, or Ultimate Good, is relative to a set of circumstances at present non-existent[1] (p. 107)."

Mr. D'Arcy then goes on to deal with the proximate end, this ultimate end being obviously useless for the immediate guidance of conduct. 'Every collocation of circumstances has its best.' 'The good is perfectly individualized.' 'It is no rigid standard.' 'Its unit is the concrete act.' (Pp. 108, 112 *Passim.*) In other words, the real end is always the content of some special act, performed with its own space and time considerations involved in it. This strikes me personally as excellent ethical doctrine; but what demand is there then for the ultimate goal furnished by metaphysics? How does that give foundation in any sense for the concrete ideals with which man is actually concerned? Mr. D'Arcy gives two answers, or two perhaps reducible to one: the thought of the far away goal helps us to read the special instance; and we judge by the *tendency* of the proximate to realize the ultimate end.

As to the first answer, it is of great advantage to the individual to be aware of what he is really about in a special case, and any principle, however formal and abstract, which aids him in doing this is justified thereby. But it is not the *remote* goal, but simply a larger view of the *present*, which thus helps one. It is the reference of an act to the present society which it maintains or furthers that helps one

[1] To which Mr. D'Arcy adds, "But this is a defect attaching to every ideal"—yes, to every ideal metaphysically established, but to *no* ideal psychologically, or socially, determined, because in the latter case the ideal always is a certain set of present circumstances viewed in certain new relations and therefore no more requiring reference to some ultimate goal of the universe as a whole than does a scientific discovery or an industrial invention.

see its true content; not its reference to a society distant an infinite length of time. So far is the conception of a perfectly realized community at the extreme goal of progress from helping us read the present that, on the contrary, we can only read, or put any meaning into, that conception by reference to the present. As to the other answer, that the present may be conceived as means, it simply removes all value from the present. If the present exists simply as one stage in bringing about an infinitely remote goal, it presents no imperative claims and affords no ends. Such a doctrine simply denies the doctrine that every collocation of circumstances has its *own* best. It makes rainbow chasing the essence of the doctrine of moral ideas. For my own part, I believe that an ethical doctrine with less 'foundations' under it is likely to go farther and last longer.

In discussing Mr. D'Arcy's book from this one standpoint of the relation of his metaphysical to his ethical theory, great injustice would be done Mr. D'Arcy if I failed to recognize his own acuteness, subtlety and frequent suggestiveness. No one can read the book without stimulation. Mr. D'Arcy's personal attitude and method as distinct from that of his philosophic position, is straightforward and ingenuous. But the use of religious presuppositions to direct philosophic doctrine, first this way, then that, seems to me essentially disingenuous. Let us either explicitly hold that philosophy has no distinct right to be, but is always a form of theological apologetics; or let us give it the same intellectual freedom that we now yield to mathematics and mechanics. Let us not, even unconsciously, give philosophy the appearance, without the substance, of an independent position. More specifically, the results of Mr. D'Arcy's investigations seem to me to give at least a negative support to the hypothesis that what ethical theory now needs is an adequate psychological and social method, not metaphysical one.

JOHN DEWEY.

UNIVERSITY OF CHICAGO.

INVESTIGATION OF CUTANEOUS SENSIBILITY.

In spite of the recent increase of our knowledge of that most general of our senses—cutaneous sensibility—the experiments hitherto made leave one difficulty only partially solved. This difficulty is, in the first instance, of a technical nature, but it occasions secondary disadvantages which in experiment prove greater or lesser sources of error. It consists in the accurate determining of the pressure-value to be produced on the portion of the skin under investigation. The importance of this factor in the investigation of cutaneous sensibility needs no further comment, for it is well known that the increased pressure, consequent on the deeper penetration of the instrument used, causes a larger area of skin to be affected. The æsthesiometers hitherto invented for the determination of degrees of pressure have not quite overcome this difficulty, for though serving to determine the pressure-value on portions of the body in a horizontal position, their application becomes difficult or impossible as soon as parts not adapting themselves to this easy posture are to be investigated; and in any case the abnormal attitudes exercise a disturbing influence on the results of the experiment. I should like therefore to direct attention to a method of investigating cutaneous sensibility, which, originating in physiological research, may become of importance in psychology. Prof. von Frey, of Leipzig, has described this method in *Berichte der mathemathisch-physischen Classe der Königl. Sächsischen Gesellschaft der Wissenschaften zu Leipzig* of July 2d, December 3d, 1894, and March 4th, 1895. It was a happy thought of the author to make use of the maximum stimulus-value of a hair of a certain length, this value being determined by the weight which the hair in curving just lifts on a pair of scales. For the different degrees of stimulation von Frey makes use of hairs of various sizes, bristles, horse-hair, beard-hair, women's hair, children's hair, cocoon-threads and glass-threads, also of similar hairs of different lengths. None of the stimulus hairs mentioned exceeds a length of 40 cm. Each single hair is stuck, by means of elastic glue, to a little wooden rod 8 cm. in length and perpendicular to its axis. The little rod serves as a handle during the experiments.

Having already written a detailed account of von Frey's interesting experiments for the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, to which I refer the reader, I need here only mention that von Frey designates the maximum value of a stimulus hair measured by the scales as its 'force' (Kraft), that the 'pressure' (Druck) to be determined by the hair is obtained through the division of the primary value by the microscopically-measured transverse section of the hair and that, according to von Frey's investigations, sense-points differing in quality and in liminal value, are to be found on the surface of the skin. These, designated by von Frey as pressure and pain points, represent a different liminal value on the different parts of the skin.

Having frequently worked with von Frey, subsequent developments of the researches are known to me and I may, therefore, add that careful study of the deformation-phenomena produced by pressure has caused him, with respect to the pressure-points, to alter the above mentioned designation of the value reduced to unity. The pressure is no longer determined by the quotient $\frac{\text{force}}{\text{surface}}$ but by the quotient $\frac{\text{force}}{\text{radius of surface}}$. The progress made in consequence of these experiments of von Frey is, I think, not only in the proving of difference in quality and intensity of the various points of the skin, but also in the possibility of obtaining exact liminal values, and psychological science in its investigations must, I think, take all these factors into account. These thoughts have occupied me since my first acquaintance with von Frey's work, and the subject, it seems to me, is worthy of further discussion. How far we must take into consideration, however, in psychological questions, the relative values given by von Frey or those absolute values designated by him as 'force' will depend on the individual cases with which the investigation has to deal, and according as one is able in each individual case to preserve one or the other component constant. But it is clear that the simplicity of the method permits of the performance of exact quantitative measuring experiments on all parts of the body as soon as a series of stimulus hairs has been determined on the chemical scales. It is also clear that, taking this principle as a basis, it would be easy to construct a simple, satisfactory aesthesiometer. This idea, in the interest of my own science in the first place I communicated to Prof. von Frey, who, after having developed it, has had two sorts of æsthesiometers constructed by the mechanician Zimmermann, of Leipzig. These deserve further notice on account of their practical usefulness. One is more adapted for clinical purposes, and will doubtless be of great service in the investigation of pathological cases. It consists of a small tube of about 5 mm. in diameter and about 10 cm. in length, in which a metal rod, graduated in millimeters, may be moved up and down. The stimulus hair is fixed to the free end of this rod with elastic glue, as already mentioned. According as the rod, with affixed stimulus hair, is moved into the tube, the hair is shortened and its pressure value immediately altered, the latter, of course, increasing with the shortening of the hair. If the hair has been measured on the crater according to its different lengths, the pressure value may be read on the graduated rod, since the transverse section remains constant. The second æsthesiometer by von Frey is constructed on the principle of earlier instruments. It differs from these only in having yielding stimulus hairs accurately gauged instead of hard points. Each hair is fastened into a capsule, which moves easily up and down a metal rod. The distance of the ends of the two stimulus hairs may thus be varied at will according to the experiment to be undertaken. The whole is fastened to a handle. It is needless to say that this instrument may be used with great ease in the investigation of every part of the body without necessitating any abnormal position, and I am not, I think, going too far in repeating that by von Frey's stimulus hair method former difficulties are surmounted.

The interest aroused of late in the investigation of skin sensation gives me hope that this short notice may direct attention to this method, the application of which will certainly not be fruitless. The arrangement of the stimulus hairs occasions some difficulty at first, but this is soon overcome by practice and more than compensated for by subsequent success. Von Frey's method has proven of great value in the treatment of diseases of the eye as also is other pathological cases.

I may add to the foregoing that when one wishes to mark certain skin-points for continued investigation 10 % nitrate of silver may be applied to the skin by means of a capillary tube the walls of which must not be too thin. Injury to the nervous end-organs can in this way scarcely be apprehended. This is as a rule von Frey's method of marking skin-points under examination. I often make use of a watery solution of methyloiolet which, as I have elsewhere mentioned (Wundt. *Philos. Studien*, Bd. 11 p. 137), dyes living tissues well and lastingly.

In conclusion I may remark that Prof von Frey intends to publish an account of his further investigations in the course of the year.

FRIEDRICH KIESOW.

LEIPZIG.

SUSPENSION OF THE SPATIAL CONSCIOUSNESS.

A recent note in this journal by Professor Hyslop on our localization in space induces me to record a somewhat similar but even more pronounced case of suspension of the power of localization. It is to be noted that the dream which in Dr. Hyslop's cases 'switched out' the ordinary date of localization was, unlike most dreams, accompanied by, perhaps caused by, hallucinations of vision, such as I have described in a recent number of the Journal of Comparative Neurology. The theory which seems to have been in the narrator's mind is that the existing mental picture forcibly displaced the memory image of the actual place occupied. The tactile and other sensations were not adequate to displace the vivid hallucinatory image. The question in this connection which seems of greatest interest is whether the mind, in its waking state, must (or at least always does) orientate itself, whether correctly or incorrectly. Everyone knows by unpleasant experience that the tendency to extend this orientation to correspond with the limits of our field of space-conception is very strong and, once formed, the orientation is exasperatingly persistent. I had suffered from the inconvenience of being 'turned round' in unfamiliar places for many years until a simple expedient permanently rid me of the habit. The remedy consisted in charging the mind to suspend judgment of direction until an intelligent one could be formed. After a short struggle this habit was formed and, although mistakes have occurred, they have been due in every case to imperfect or incorrect data and I have never been 'turned round' since.

But the instance which it is desired to record seems to show that the mind may be for a considerable time completely unorientated in both time and space. The experience referred to has occurred to me no more than three times and the period has in two cases been quite short, while in another the time was long enough to provide for a careful study of the state. It was some months after a return from a residence in Berlin lasting several months. Meanwhile the home had been removed from Cincinnati to Granville. Yet there had been a long period of quiet routine at the new home, and the unsettled feeling which an ocean journey always produces had long since worn off. I had been for some time studying dreams and had acquired the habit of collecting my thoughts and attentively observing states following the awakening. Under these circumstances I awoke near midnight from a quiet sleep without any dream content being immanent. The room was absolutely dark and quiet. I lay at ease and it dawned upon me that I had no notion of where I was. I turned over in my mind the various sleeping apartments in which I had slept. Was this the state room of a steamer? Evidently not, for there was neither noise nor jar. Was it one of the three bed rooms I recalled in Cincinnati, or was it perhaps in Berlin? I could not tell. What had I been doing the day before? I had not the faintest idea. The events of one period of the past seemed as vivid and 'present' as those of any other. For some reason the sequence of events seemed gone, though many isolated occurrences were clearly recalled. I lay some time waiting for the appearance of some associated chain, but none emerged. A momentary fear that I had been smitten with blindness was relieved by a faint glimmer from the window. I then made several slight movements but could still get no idea of the shape of the room or of the position of objects in it. The necessary link was at last afforded by a movement on the part of my companion and a few tactile coördinations without the aid of vision. The state impressed me like that of a disembodied mind, but there seems to have been no special vascular stasis at the periphery, though it is, of course, probable that some circulatory changes had occurred in the brain. Tactile sensations were as usual. It thus is evident that the mind may operate in an apparently normal way with full consciousness and yet the correlation of vestiges necessary to localization be wholly suppressed, though other spatial reproductions are unimpaired. It is also seen that the orientation does not depend on vision or any one sense, though visual elements predominate when the orientation is at last affected. As I have said, this is not an isolated case, though in the other instances some sense impression has completed the spatial rapport before the state could be calmly observed.

DENISON UNIVERSITY. C. L. HERRICK.

FOCAL AND MARGINAL CONSCIOUSNESS.

There seems to exist among a large number of recent psychological writers a strange confusion of ideas respecting one of the simplest and yet most fundamental distinctions in the science. I mean that between sense content and sensation (not the content of the sensation, which is a very different thing).

The content of sense at any given time is the sum of the affectations of the lower or primary æsthesodic centres. In the visual sphere, for example, it is the totality of the immediate central reactions corresponding to the retinal excitations. We may think of them as distributed in the homologous parts of the tectum, but it is probable that we should add the effects of certain optic reflexes with their æsthesodic reactions, and not improbable that it will be necessary to include modifications or accretions due to changes in the cortical visual area; however this may be, there is as yet no sensation—only sense content. Besides the contents of the higher senses there is the whole æsthesodic contingent from the cord, many of whose elements never are brought into consciousness except under exceptional conditions. Some of them are perhaps incompetent to enter sensation at all, except as a *quale* of some other sensation, because they have no localizable 'tag' suit-ing them to independent recognition or isolation. These are, however, just as really part of the sense content as are colors or pains.

Now it is evident to ordinary experience that, in many cases at least, the 'sensing' of a sense content *is an act*, not an occurrence. We fix a certain element; it is immaterial how we were impelled to the fixation of that particular element, the act is an expression of our spontaneity—a reaction of the subject. Many considerations justify us in supposing that an act of consciousness involves, on its neurological side, a reaction between the æsthesodic and the kinesodic system of the cortex. Only so can the intimate connection between perception and various forms of innervation be explained. Here is an attractive field which it is not possible to enter now. Probably most psychologists will agree that consciousness is an act, not a state, and that it is a pivotal act which takes place in the very focus of our being.

The unity of consciousness may be interpreted to mean that consciousness is only possible when the æsthesodic and kinesodic currents affect the equilibrium of the entire mechanism of consciousness. It seems possible to conceive of the situation as an instance of most complicated equilibrium where each element of the conscious mechanism contributes its tension to the balance of the whole. However this tension is affected, a conscious state may follow. It will be understood that on a purely dynamic theory there is no question of spatial unity, only of a common form of action.

Letting this crudely-expressed concept serve for present purposes, we are prepared to consider what takes place when any given content of sense is presented to the mechanism of consciousness. If it is a given color, for example, then the balance is disturbed in a certain characteristic way at the moment it is admitted. We perceive a color. If, instead of the color, a retinal picture of great complexity, say a landscape, is presented, the equilibrium is disturbed in a different way, though one which produces an instantaneous impression of as truly a simple sort as the other. It differs from the former in that this one is followed by the after-shower of innumerable vestigeal impressions from the optic and other associated areas which, each in turn, affect the equilibrium of the mechanism of consciousness. We insist that there must be in this ultimate mechanism of consciousness an absolute succession. A wave of consciousness in the sense in which it is postulated by James, and especially by Morgan, is inconsistent with any conceivable means of bringing sense impressions to consciousness. There are, it is true, in the sense content of vision audition and tactile sense, distinct apparatuses for producing focal and marginal impressions. These are associated with localization and are most important in their bearing on the development of ideas of space, but the difference between them is one of degree or kind, not of order or succession, and would afford the same result whether reported cotemporaneously or successively. Just, then, as the various intensities of sense impressions afford a basis for focal and marginal sense contents, so a perspective of vestiges may be presented to consciousness, but we believe it a false use of analogy to claim that there are in cotemporary consciousness both focal and marginal elements.

We do not conceive that consciousness is bound by the same limiitations as its intermediary mechanism, nor that it is proper to apply to it the predicates of succession or of time, but, in as much as we are concerned with the intermediary mechanism, the distinctions here insisted on seem to us important.[1]

C. L HERRICK.

DENISON UNIVERSITY.

NATURAL HISTORY OF THE CRIMINAL.

Permit me to remark, with reference to Dr. Hume's review of my *Naturgeschichte des Verbrechers* (p. 408, Vol. II. of the REVIEW), that its author seems not to possess sufficient knowledge of the German language to do full justice to a German author. Only on this supposition can I explain his remark that I 'have not only a very slight acquaintance with general psychology,' but that in my book 'there are references of contempt for those branches of study.' This latter judgment is absolutely erroneous; as to that, I refer only to the great number of books, on general psychology and psychology of ethics, which I have translated from the originals of H. Höffding, C. Lange, H. Ellis, C. Lombroso, E. Ferri, and others. Before expressing his feeling of deep disappointment with my chapter on 'Criminal Psychology,' Dr. Hume might have made reference to the preface, where I said, that I have been constrained to give only a short sketch of the fundamental problems of 'Criminal Psychology,' hoping to publish later my researches on murderers, vagabonds and cheaters.

Dr. Hume's imperfect knowledge reveals itself best in the manner in which he translates the title of the book: *Science of the Criminal.*

[1]The view that the higher orders of physical coördination are especially provided for in the cortex of the frontal lobes has received experimental support through the researches of Bainchi. See Brain: IV., 1895.

I have not the least idea of writing on the 'Science of the Criminal.' At the best it is possible to-day to give only the outlines of a 'Natural History of the Criminal,' and this is the title of my book.

BRIEG. DR. KURELLA.

THINKING, FEELING, DOING.

A review of my book in the last volume of THE PSYCHOLOGICAL REVIEW has come to my notice. It contains some statements that are quite misleading. I will pass over the injustice done to a book when its material and its form are criticised according to a standard with which it has absolutely no relation; it is a cheap and frequent method of exhibiting a young critic's superiority to judge a popular book as if it were intended to be a scientific treatise for the strictest specialists. I am, however, entitled to protest against the attempt to make it appear that my book is merely an adaptation of Wundt without proper credit. Your reviewer, for example, complains that, after stating that I am about to quote a few pages from Wundt, I put quotation marks around a couple paragraphs only. To any careful reader the text shows quite clearly that whereas the material is quoted from Wundt, there are minor changes and condensations in expression such as to render quotation marks not allowable except where used. At any rate, when an author expressly states that he is about to quote a few pages, it is but fair to take him at his word, whether he uses quotation marks or not. The critic again speaks of other quotations from Wundt without reference. These reduce to two paragraphs of pure matter of fact which were taken from Wundt, but which were scarcely entitled to a reference, as they consisted of the merest every day matters with no original thought. If the reader will only turn to the book itself he will find that I have given to Wundt and his books about all the credit a man can give.

Finally, the worst injustice of this attempt to make it appear that not enough credit is given to the master lies in the disregard of facts like the following: The preface speaks of Wundt as 'the greatest of psychologists;' the first chapter quotes him repeatedly; two other chapters contain special quotations; to the necessary rule of 'no references' an exception was made in favor of Wundt's *Vorlesungen;* the only footnote reference allowed in the book calls particular attention to a translation of Wundt; and finally the last chapter contains a biography of Wundt with a bibliography of his works and the brightest tribute to his genius that the author could think of. It seems to me that I could hardly have done more to express the obligations of my book and myself to him. In fact, I intended to make the book a popular tribute to his genius and an acknowledgment of my obligations to him. I am happy to say that the tribute has been accepted with the kindest expressions from the master, and I sincerely trust that the large sale of the book has carried the news of his fame into nearly every American household.

E. W. SCRIPTURE.

This acknowledgment to Wundt is both timely and honorable. The reviewer must have been very stupid, as well as young and superior, for apparently he failed to make intelligible the most important part of his criticism. Messrs. Creighton and Titchener are the gentlemen to whom above all others explanations and apologies are due. But their names do not appear here. Possibly they will feel, however, that Dr. Scripture's explanation is sufficiently luminous and inclusive to be satisfactory without any definite mention of them. And anyway, forbearance will be a necessary virtue. For the book by taking refuge in 'nearly every American household' has obviously outrun all possbility of successful pursuit.

JAMES R. ANGELL.

PSYCHOLOGICAL LITERATURE.

The Growth of the Brain: A Study of the Nervous System in Relation to Educatian. By H. H. DONALDSON, London, Walter Scott. New York, Charles Scribner's Sons. Pp. 365.

This book has been written with such complete appreciation of the requirements of the best scientific method, and the author has shown such untiring patience in collecting and analyzing all the facts which could be made useful, that he deserves to have ascribed to him that *besoin de la verité* which Louis speaks of as a so much rarer gift than the taste for scientific investigation, with which so many set out on their work. The only real criticism which we have to make is that the range of subjects is so great that the reader, unless well versed in the literature of neurology and anthropology, must find his progress slow and laborious. The style is clear, but the statements are, necessarily, concise and condensed. The very richness in data which makes the book so valuable, also makes it one of which it is difficult to give any adequate sketch within the short space of a review. As giving an idea of the wealth of material utilized, it may be noted that no less than sixty-four tables of figures are reproduced and studied.

How may we best hope so to modify the nervous systems of individuals and races that the work which they do will be more and more effective? This is the question for which Dr. Donaldson would be glad to find an answer, but he recognizes, more clearly than do the eager parents, teachers and physicians to whom the same problem presents itself, that before we can approach the solution we must learn to know under what laws the development of the nervous system normally goes on and what the conditions are that make the brain a better organ, independently of education.

Neither the brilliant achievements of formal education nor the progress that civilized races have made in their pursuit of knowledge are a sufficient warrant that a superior and better type of brain is being created. "Knowledge comes, for the hindrances to knowledge are in a large measure from without, but wisdom, as heretofore, continues to linger, and still to occupy its place as the rare performance of the balanced brain."

The first chapter contains a concise statement of the best biological researches in the study of growth. The whole of this is important, especially so is the reminder that, in the human nervous system at least, the production of new cells ceases some time before birth, and that the rate of growth, which, after birth, depends on the increase in size of cells, diminishes rapidly from birth onward (Minot and others). But though no new cells are formed after birth, the capacity for physiological development is not quite so fatally restricted as one might imagine, since there is always a reserve of nerve elements which do not, in the first instance, fully develop, but remain capable, to a certain extent, of subsequent change.

The life of any individual is practically a process of adaptation to surroundings, and this is marked at every point by a specialization of function, which leads eventually, when the power or adaptation becomes less, to impairment of coördinated activity and finally to death. It is not improbable that a law of this sort governs the life not only of individuals, but of species.

The special study of the growth of the brain is introduced by a brief but excellent analysis of the observations through which the laws of growth of the body as a whole have been ascertained, and then the relative growth of the different parts of the body is studied, with constant comparison of males and females. The writer's searching review of the various researches upon the weight of the brain and spinal cord at different ages, in the different sexes, and as related to size of the body and to intellectual eminence, will long be consulted as an impartial statement of the case, although, as he says, it is plain that the facts "contribute mainly to a healthy scepticism concerning the current interpretations of brain weight." It is impossible to judge by the scales alone about the intellectual capacity of a given person, or even whether he was healthy, criminal or insane. Where the weight falls below a certain minimal point, indeed, we are justified in assuming a defective mind, but here questions of structure come in which the author next proceeds to study.

It has already been pointed out that growth consists partly of cell multiplication, partly of an increase in the size of the cells. A careful estimate shows that at the end of the first twelve weeks of fœtal life the volume of the nervous system is about 2.25 cm. cm. By this time the number of nerve elements, or neuroblasts, has pretty much reached its limit, which is somewhere near three thousand million. The volume of the adult nervous system may be estimated as 1005 cm. cm., and, therefore, the average increase of size of each neuroblast is nearly five hundred times, though in fact the increase in the case of of many of them is many times as great as this.

"The determination of the number of neuroblasts occurs so early in the history of the individual, and under such uniform conditions, that it is very difficult to regard the environment as possessed of much power to cause variation in this respect, and for this reason, among members of the same race a high degree of constancy in this character is to be anticipated. The influence of the surrounding conditions becomes much more effective during the later stages of development that accompany the enlargements of the elements already formed, and it is during this period that adaptive modifications may occur." (p. 162.)

In the next chapter the following significant questions are asked and provisionally answered:

"1. By what means does the brain of the new-born attain the weight found in the adult, and decrease again during old age?"

The greatest factor in both the increase and the decrease is the gain and loss affecting the *medullary substance* which surrounds the processes of the nerve cell.

"2. Why do tall persons have heavier brains?"

This is probably due to increase in the size of the nervous and non-nervous elements arising from the greater cranial space allotted them for growth.

"3. What significance is to be attatched to the fact that the brain-weight is different in different races?" The provisional picture to be formed of the brains belonging to those races least capable mentally is that of one in which the number of cell elements is approximately similar to that in the most capable races; but many of these elements being but partially developed, the organization of the brain is less perfect, though the size is not thereby greatly reduced.

"4. What significance is to be attached to the difference in brain weight existing between men and women?" (and found, strangely enough, even among the defective classes.)

This difference must depend on the fact that the structural elements in the encephalon of the female are smaller than those in the male, and it is probable that, other things being equal, the larger cells have more stored up energy and permit of more complete organization.

The writer then gives a summary of the architecture and structure of the brain and cord which is full of interest. It hardly admits of analysis in a short review. In the course of it he refers to his own careful investigation of the brain of Laura Bridgman.

The chapters of most interest to the educator are those in which the physiological rhythms which characterize the nervous functions are dwelt upon at some length; then those which deal with fatigue and old age. The two final chapters are devoted to the study of education and to the statement of the 'wider view.' These deserve to be read in detail, and the reviewer will think his task sufficiently well performed if he has indicated on how wide a basis of positive data Dr. Donaldson's moderate but interesting practical conclusions are built up.

JAMES J. PUTNAM.

HARVARD MEDICAL SCHOOL.

Mental Development in the Child and the Race: Methods and Processes. JAMES MARK BALDWIN. New York, Macmillan & Co. 1895. (2d edition, 1895.) Pp. xvi+496.

Professor Baldwin's most recent book has already received much attention, and hardly needs introduction to the readers of this REVIEW. The volume is founded upon essays previously published; but in its wholeness it is an essentially new piece of work, which constitutes, so far, its author's most mature and original contribution to his science. It contains an uncommon union of decidedly special, empirical observations with comparatively recondite and very far-reaching evolutionary speculations. The present reviewer, as himself professionally disposed to the speculative, may very properly give his attention mainly to the latter aspect of the book, although well recognizing the high merits of the other aspect.

In its literary character this work, always as to all the details of the exposition pleasantly and stimulatingly written, is still in some of its most important features disappointingly obscure. Professor Baldwin's habit of referring to coming chapters for the explanation of the points that his present argument leaves unelucidated is too insistent, and has caused perplexity to more readers than one. Perhaps an author who deals especially with the phenomena of 'accommodation' may be doing well to enable the reader to make numerous subjective observations of the accommodation process while getting used to a novel and complex train of thought; but has not Professor Baldwin gone in this respect too far? To be sure he can be, and often is, so clear, especially as to the single sentence, illustration or argumentative point, that we are often most of all baffled in trying to make out why it is that just the connected whole, the unity, the total bearing of his reasoning, long escapes our close attention. Yet the result, when we get it, repays a good deal of trouble.

How does an organism come to make novel adjustments? How can new habits of a useful kind get formed? How can mind grow? What is the basis for the organization of experience, viewed as novel experience? In fine, how is 'accommodation' to be psychologically and biologically explained, in the individual and in the race? Here is the central problem about which Professor Baldwin's evolutionary speculations are grouped. Of old the organization of experience, as studied by the psychologists who followed in Locke's footsteps, and who developed the association psychology, meant primarily the grouping of the impressions and ideas, or of the Herbartian *Vorstellungen*, viewed as data received, retained and associated. That the experience of the mind influences conduct was regarded as a matter of relatively secondary import in the study of mental growth. But nowadays the psychologist is dissatisfied with confining his attention to these mental data, in so far as they merely come *to* the mind. One observes that the experience of a live creature is useful to the possessor *only* in so far as this experience influences the movements, organizes the conduct, calls forth or adapts the adjustments of the creature itself; and since Spencer's Psychology the problem of the organization of mental experience has been inseparable from the evolutionary problem regarding the acquisition of serviceable motor habits upon the basis of sensory stimulations. Every evolutionary psychologist attempts more or less elaborately and explicitly to trace the beginnings and the growth of mentally significant adaptations, and to correlate what we know of mental processes with such adaptations. In this field the well-known hypotheses relate, on the one hand, to the influence of natural selection upon the evolution of mentally significant capacities for motor adjustment, and, on the other hand, to the variously interpreted relations of pleasurable and painful stimulation to the modification of motor processses.

Professor Baldwin's contribution to this discussion may be briefly indicated, but cannot be quite fairly developed within the present limits. After devoting considerable attention (p. 180 sqq.) to an argument showing that the experience of the pleasurable or painful *results* of movements once made cannot be relied upon as a factor sufficient to explain the way whereby an organism not already provided with useful motor adjustments may acquire such adjustments, Professor Baldwin proceeds henceforth, in his speculations, upon the postulate that, in order to explain the origin of specific accommodations, *i. e.*, of definitely useful motor adjustments, "a theory of adaptation must have reference to the repetition of stimulations, fundamentally, not of movements" (p. 451). One must suppose, namely, that, in advance of all definite habits of motor adjustment, and in the absence of inherited tendencies to definite acts, a virgin organism (if we may use the phrase)—one standing at the outset of the evolutionary process—possesses just one, highly generalized, but essentially plastic motor tendency, whose origin (p. 203 *et passim*) one must refer to natural selection. This is the twofold tendency to expand in the presence of stimulations which exalt, and to contract in the presence of those stimuli which depress vitality. That such simple reactions to the presence of light, of food and of injurious objects exist and are universal amongst organisms of even the lowliest type is well known. The present theory supposes that the stimulations which cause expansion are pleasurable, and that those which cause contraction are painful. But now the expansion tendency is the representative of a vital 'excess,' an overflow of energy. From its nature it tends to lead the organism in question nearer to the source of the advantageous stimulation, and hereby it tends to produce a '*repetition*' of this stimulation, which again results in further excess, and in more movements of the same sort. This tendency to move so as to secure a repetition of the favorable stimulus involves, however, at every step, by reason of the very excess which is essential to the process, relatively novel movements. If these new movements, in so far as painful accidents do not check their appearance, tend to get fixed, as they do, in the form of habits, the organism, wherever it is exposed, thereafter, to new stimuli, will now be no longer virgin. For, in addition to its original and generalized tendency to expansion and contraction, it will henceforth have definite tendencies to certain movements. The nature of these movements, in view of their origin, and in view of the fact that all pain-giving or even useless accidental accompaniments of the excess process have tended to be excised by the original tendencies to draw back from the painful, and to emphasize the pleasurable stimuli, will be such that the newly acquired movements will be *apt to repeat stimuli of a certain type.* Henceforth the now trained organism will more and more tend to this type of 'circular reaction,' moving in the presence of certain types of stimuli so as to repeat or to enforce them; moving in the presence of other stimuli (*viz.* painful stimuli) so as to avoid repeating them. Upon this 'circular' type of reaction, as Professor Baldwin ingeniously insists, the remainder of the process of mental evolution is founded. This is the type to which, as readers of Professor Baldwin's remarkable paper in *Mind* and readers of this REVIEW well know, our author applies the general name imitation. Every new type of imitative or circular reaction once thus acquired becomes a basis for further modification or adaptation through the influence of new stimulations, whose effectiveness, in all pleasurable cases, will be ensured through the very existence of the repetition tendency itself. On high levels the circular reaction appears as the act of attention, whereby the effect of a given stimulation is, through repetition, so heightened as to ensure its effectiveness in causing accommodations. "In general" (p. 179) "the law of excess may be stated," says Professor Baldwin, "somewhat as follows: The accommodation of an organism to a new situation is secured, apart from happy accidents, by the continued or repeated action of that stimulation, and this repetition is secured, not by the selection beforehand of this stimulation, nor by its fortuitous occurrence alone, but by the proximate reinstatement of it by a discharge of the energies of the organism, concentrated as far as may be for the excessive stimulation of the organs most nearly fitted by former habit to get this stimulation again." Granted the repetition, and the accompanying excess, then the organism gets adapted 'by chance adjustments occurring among excessive diffused movements' (p. 198); since the process of repetition tends to favor these movements, so that ere long they become habits.

A crucial case for this theory of the acquisition of new fashions of movement is furnished by the phenomena which (p. 373) first attracted our author's personal attention to the considerations that now have taken form in his theory. These are the phenomena of the rise of volition in the child. Volition, our author insists, is a phenomenon, at the outset, of 'persistent imitation,' of the 'try-try-again' tendency of the child. In so far as an organism inherits tendencies which early, under the influence of pleasure-pain experiences, get welded, without deliberation, into even complex movements, such as are involved in holding the head erect (p. 390), Professor Baldwin does not consider these cases of volition. The acts that thus early get established may, by reason of the generally imitative character which all the organic responses to the environment must possess, appear, in children, as simple imitations. But these simple imitations, acts which, without deliberation, tend to reproduce given stimuli, are not yet voluntary. On the other hand, in the case of the 'persistent imitations,' the child has a model before it, and is first stimulated by this model to an act of more or less inaccurate involuntary imitation. Hereupon, however, the child is dissatisfied with the presented contrast that now appears between its model and this imperfect imitation.

The dissatisfaction gets expressed in an intensely attentive tendency to watch the objective model, and to repeat with variations the imitative act. The resulting process of trial and error may be a very extended one, the attention to this process may be long repeated, until at last the imitation comes to resemble the model enough to satisfy the child. This process constitutes, in Professor Baldwin's account, the first appearance of true volition, since here is an ideal, long attentively held before consciousness, and the gradual and persistent adjustment of means to ends.

These being, according to our author, the observed facts, it remains still to indicate the theory of the process of persistent imitation. Why this strained attention, this long pursuit of the ideal, and why—here is, of course, the more difficult question—why and how does this process of persistent variation of the first response to the model gradually tend to the establishment of acts which actually repeat the model more closely than the first act did?

Professor Baldwin's theory as to this matter is best stated on page 453: "In persistent imitation the first reaction is not repeated. Hence we must suppose the development of a function of coördination by which the two regions excited by *the original suggestion and the reaction first made coalesce in a common more voluminous and intense stimulation of the motor centre.* A movement is thus produced which, by reason of its greater mass and diffusion, includes more of the elements of the movement seen and copied. This is again reported by eye or ear, giving a new excitement, which is again coördinated with the original stimulation, and with the after-effects of the earlier imitations. The result is yet another motor stimulation or effort of still greater mass and diffusion, which includes yet more elements of the 'copy.' And so on, *until simply by its increased mass, including the motor excitement of attention itself*, by the greater range and variety of the motor elements thus enervated, in short, by the *excess discharge* the 'copy' is completely reproduced. This, it is evident, is just the principle of 'excess,' and it is very easy to find in it the origin of the attention. The attention is the mental function corresponding to the habitual motor coördination of the processes of heightened or 'excess' discharge."

In this conception, it will be noted, the general theory of excess, as stated above, is applied to the special case of volition, by the hypothesis that the being who possesses the power to acquire voluntary skill differs from beings lower in the scale by the presence, in his case, of centers of coördination where the continuation of the stimulus that produced the primary or simple imitation meets, later, with the resulting stimulus due to the perception of the imperfect copy. The result of this meeting is a new and more intense motor stimulation, involving at once attention and diffused new motor processes. That some of these new motor processes result in agreement with 'more elements' of the model is due simply to the fact that they *are* more numerous and diffuse than were the motor processes of the first imitation. And volition is now present, just because volition involves an element of persistent anticipation of a complex act that, when it comes, is to realize an ideal.

The natural question arises here, as in Professor Baldwin's other discussions of the results of the excess process, why it is that, when the successful imitation at last results from this process of excessive stimulation, the unnecessary or unfitting portions of the motor excess fall away. While the child is learning, in this persistent imitation, the essence of the process, according to the theory, is that the stimulation of the 'coördination-center,' through the combined sensory effects of the model and of the resulting imperfect efforts to imitate it, leads to excessively diffuse movements, *some* of which, by virtue of the mere diffusion, tend to produce results agreeing with the model. But since many of these diffuse movements of excess (such as kicking, tongue-movements, and the like incidents of the strain of learning) do *not* tend to make successful copies of the model, why do they later disappear and leave the successful imitative deed to become a settled habitual acquisition?

Professor Baldwin's response to this question is (p. 445, *cf.* p. 377) that "When muscular effort thus succeeds, by the simple fact of increased mass and diffusion of reaction, the useless elements fall away because they have no emphasis." Or, as p. 377 states the case, 'the useless elements fall away because they are useless.' It seems plain that considerations equally undeveloped govern our author wherever he speaks of that elimination of the useless or unadaptive elements of the excess-discharge which all grades of the process of accommodation, from the lowest up, appear to involve. Surely the very nature of the excess-discharge, in advance of definite adaptation, must be that it generally involves useless reactions quite as probably as useful reactions. The only apparent exception to this would be furnished by the primitive expansion movements noticed above. They, it may be said, inevitably involve a tendency to reinforce their stimulation, and to continue its presence, because the expanded organism will, as such, offer more surface to the source of stimulation. But as soon as one passes from this primitive state to the case of an organism having activities already complex, adaptation through the chance results of excess will apparently occur only in connection with the initiation of many unadaptive movements, which will need to be eliminated whenever the accommodation can become perfect. If one writhes or kicks in learning to draw, a positive theory is needed to account for the rapidity with which these unnecessary movements fall away after the occurrence of the successful imitation; or, even before that occurrence, since one must take theoretical account of the further fact that *such* excess-movements generally oppose the attainment of an accurate imitation, and must, therefore, in part, be eliminated *before* the first accurate imitation can occur.

Of course the elimination of painful and of positively unsatisfactory movement is used by Professor Baldwin as a coördinate factor in this process of the reduction of the excess to its due form (see p. 143). But this does not of itself explain the inhibition of such useless elements of the excess as are not directly felt to be in themselves unsatisfactory. Yet such elements might be not only present, but actually injurious to the imitation. An awkward man tries to acquire a new imitative art. He reacts to his model, and then observes the inadequacy of his first imitation. The perception of the incongruity excites his coördinating centers. The result is a new set of efforts, which may involve numerous excess-movements. Of these some will of themselves tend 'to include more elements' of the model. But some of them, perhaps most of them, will not only be superfluous, but will also actually stand in the way of the accomplishment of the desired aim. For, if the model is at once complex and definite, inhibition of the unnecessary will be an essential part, and, in most cases, a preliminary, of the first success. The immediate result will so far be that increased effort, in advance of inhibition, will mean failure. The 'more elements' of the right sort will be so mixed with 'more elements' which lead astray, that the total results will perhaps be no gain in accuracy. Now, if the awkward man can himself analyze his act and discover that the inhibition of certain superfluous elements would ensure success, *then*, but only then, will these superfluous acts become, by association, disagreeable to him, as hindering his ideal, and meaning failure. Thereupon the elimination of these elements will become easy to him. But surely a learner who can analyze the source of his own failure has already come to stand high, through previous success, in the imitative art. On the other hand, the really awkward man may easily be sensitive enough to be dissatisfied with his failure, and yet may be unable to analyze the cause of his failure. He makes, in one act of persistent imitation, superfluous efforts and useful efforts. Who is to tell him which of his efforts are the superfluous ones? What influence is, in advance of success, to overcome, to inhibit, the hindering elements of the excess-process? Their own disagreeableness as hindering elements. But it is for him, unless he is already skillful enough to analyze, only the total result whose failure is disagreeable. The superfluous parts, by themselves, cannot appear to him, separately, disagreeable enough to get inhibited, unless some preëstablished harmony makes them so. The awkward man will try and try again, with excess and failure constantly attendant upon his efforts. The more he strains, the more superfluous efforts will he make, until the whole process ceases in painful exhaustion. Here there will be no necessary tendency of excess to secure ultimate success.

Now this is no merely imaginary case. This is the process of failure in many instances of industrious awkwardness. This is what happens when we think vainly over our problems, and yet get no result. This is what happens to the socially awkward, who attempt social enterprises only to get more and more lost in the chaos of their own excessive efforts. This in particular is what happens in our personal relations to the people with whom, despite our best efforts, we 'cannot get on.' In trying to conform to their ways we attempt useless acts of conciliation, make ineffective chance remarks, complicate our relations through unnecessary explanations, and yet can never quite find out what it is that makes us go wrong. The excess reactions then, as such, need not involve useful *plus* merely superfluous reactions that will not positively hinder success. The excess reactions may, and often do, involve a union, that is for the striving learner unanalyzable, of useful and of positively hindering acts. The question here is what magic in advance of success is to ensure the inhibition of the elements of hindrance thus involved in the excess discharged?

But does one reply, with Professor Baldwin, that actual observation of the child's imitative successes shows, first the excess reactions, and then the inhibition of the superfluous elements? Hereupon one can but retort that the very problem of the acquisition of new habits is: How do these inhibitions of the superfluous elements take place? Does one say: Success is sometimes possible? The obvious retort is, What particular factor leads to success when the latter does occur? To this problem, so far as the present reviewer can see, Professor Baldwin has given very scant attention. Yet, unless this problem is definitely faced and solved, an appeal to the facts of excess, interesting as it is, must prove wholely inadequate to show how definite new habits can get formed. For, as a fact, whoever learns a new habit, either by persistent imitation, or by some less intelligent process, learns more numerous inhibitions than he does positive adjustments. This appears to be true low down in the animal scale as well as higher up, and the difficulty developed in the foregoing is one of a very general application. If excess is the beginning of novel adjustment, selection amongst the elements of the excessive reactions to interesting stimuli involves much more than the merely superior emphasis given to certain of these reactions by their pleasure-giving character, or even by their success as imitative reactions. Nor is the principle that the painful elements of the excess get eliminated by reason of their painfulness a sufficient account of how the needed inhibitions occur. For there remain to be accounted for the vast number of superfluous reactions which are not directly painful, but which are *indirectly* opposed to the definiteness and success of the new habit. The animal acquiring a novel skill in watching for prey must learn to suppress numerous signs of excitement which will indirectly hinder the success of its quest. How shall the principle of excess and selection work here? The excitement-phenomena will belong to the excess-wave. Whence will come the selection? From the animal's own intelligent observation of the hindrances that result from these superfluous acts? But it is the *origin* of just such intelligence that we are here tracing. No intelligence of this grade can exist unless definite successes have already given the animal a criterion for judging its own failures. The imitative animal must learn, and does learn, to be silent and hide when the others do so, to stand still and watch when the others do so, and in countless other ways to imitate inhibitory deeds and attitudes. But in the case of the imitation of inhibitions, how is the excess, merely as such, to contain 'more and more' elements that gradually conform to a model whose very essence is that its outward appearance involves a suppression of elements, the negative fact of the absense of certain groups of deeds. On the other hand, to explain all these inhibitions as due to the experience of the painful results of the acts suppressed is simply to abandon the region where a theory of imitation ought to have most scope, viz: the region of the imitation of inhibitions, or of acts in so far as they involve inhibitions. For, as pointed out, every complex positive act involves more inhibitions than it does positive activities.

Now, it is indeed true that Professor Baldwin has given some attention to the conditions of inhibition and of selective self control. But so far as the present reviewer is able to understand the very summary observations upon p. 473, our author appears to regard the problem of inhibition as altogether a secondary one. On p. 456 we do indeed find stated, as in several other passages, the 'problem of selection,' with some indication that the excess-function needs a selective accompaniment over and above the ones upon which our author lays most stress. And, as Professor Baldwin here adds: "In attention we have, undoubtedly, the one selective function of consciousness." One expects to find, accordingly, in the subsequent discussion of attention a genetic explanation of the obviously inhibitory character which forms so large an aspect of every attentive process. But what one finds is a valuable development of the doctrine of the positive motor elements of attention. At the end comes the passage of p. 473: "The theory of motor development now worked out throws much light also on the whole vexed question of muscular control—the regulation of movement in amount and direction, and its suppression, etc." There follow two or three sentences regarding the positive aspect of control, and then the words: "And negative control or inhibition represents, in general, the limitations which old organic ways of action impose upon our ways; the new must conform, if possible, to old organic 'copy.'" Surely, this means, if anything, that the presence of inhibition, at least where the latter is not a direct case of the results of painful stimulation, is due to the influence of old imitative functions already set in the organism. The present reviewer's difficulty is, however, that some sort of inhibitory process, not wholly due to directly painful stimulation, must be posited in order that the first important selections from any excess reactions should take place; that Professor Baldwin's discussion everywhere silently presupposes the presence of just such an inhibitory aspect of the whole selective process; that the dropping of the superfluous reactions, merely because they are not emphasized by success, is wholly insufficient to explain the actual selection upon which all new adaptation depends; that, as every teacher knows, some dropping of the superfluous is, in general, a necessary *preliminary* to success in novel adaptations; and that, therefore, in the absence of any teacher to do the inhibiting, the organism itself must contain the conditions for such inhibition of the superfluous; and that, in fine, without such primary inhibition, no theory of excess reactions can possibly explain the acquisition of definite new habits.

To conclude, then, the theory of the origin of imitation will be, in the present reviewer's opinion, whenever it comes, a theory of the origin of inhibition quite as much as a theory of excess functions. The presence and importance of the latter, the excess functions, Professor Baldwin has, indeed done well to recognize; but the theory as he leaves it is essentially incomplete, for the lack of any genuine explanation of the selective process everywhere presupposed by the whole discussion. Despite this essential gap in this theory, the volume before us is so full of ingenious observation and of courageous speculation, as to leave no enlightened reader in doubt of its author's power both to see and to think, and doubtless, ere long, to lead us further into the world where he has already done such admirable work. Agreeing fully, as the present writer does, with the prominence given in this book to the value of imitation for the whole of the higher mental processes, rejoiced as Prof. Baldwin's reviewer is to find in many pages doctrines as to the psychology both of imitation itself, and of the intelligence generally which he would have been glad, indeed, to have been able to express himself, one can only regret, in closing, that the foregoing comments have often been as negative as they have been. But it is by temporary disagreement that our common interests often find themselves in the end best furthered.

HARVARD UNIVERSITY. JOSIAH ROYCE.

Studies in the Evolutionary Psychology of Feeling. HIRAM M. STANLEY. London, Sonnenschein; New York, Macmillan. 1895. Pp. VIII+392. $2.25 net.

Mr. Stanley's book is, in my opinion, an interesting and important contribution to genetic psychology. It takes up the Spencerian formulation of the problem of mental development—the interpretation of the functions of the individual consciousness in the light of race-utility —and attempts to throw light on this question by the introspective method. As far as such a problem can be approached by such a method, Mr. Stanley approaches it; but he cannot, I think, discover in the adult mind a science of mental embryology. With this essential limitation of method—a limitation which is not accidental, but which Mr. Stanley defends—his results are rich in suggestiveness, and mark the author as entitled to a high place among contemporary authors in developmental psychology. This the more because his results are peculiarly his own, as his method necessarily makes them. With this general appreciation of the book, which I do not intend the criticisms which follow in any way to impair, I may set out a few points of the more essential results which the author reaches, and speak to them from my own point of view.

Mr. Stanley makes pain-consciousness primitive—what he calls 'pure pain.' It is accommodation agent through the 'will-effort' which it leads the animal to make in order to rid itself of the pain. Pleasure consciousness is a later state arising between want-pain and excess-pain. The derivative character of pleasure is argued at some length, but with arguments of an introspective character; although here as elsewhere Mr. Stanley deserts his method by appealing to the child consciousness and hints at biological facts. I think that all the points made can be met by facts from biology and child-psychology; but this is not necessary, since Mr. Stanley says in another place (28) in answer to points made by Mr. Marshall that he is not concerned to maintain this thesis and is quite willing to believe that pleasure and pain are equally primitive. This is generous, certainly, but it shows the essential weakness of the author's method. The point at issue here, I venture to think, is one of the most fundamental in all the theory of development. A number of Mr. Stanley's own later doctrines rest upon the probable truth of the claim that pain alone is primitive accommodation agent. And the admission made here that it is not, weakens the ground theory of the book all the way through.

The second element of Mr. Stanley's conception of the fundamental reaction, *i. e.*, 'will-effort,' finds no analysis or discussion that I can see anywhere in the work. It seems to be assumed along with pain as an ultimate characteristic of mental life. But even then we ought to have some notion of how it works to bring about the adaptations of the organism. This great defect is what I referred to above in defining Mr. Stanley's problem as the problem of race development. The parallel question of individual development—the ontogenetic question—seems not to have occurred to him. And yet, is not that just the question for which the introspective method is available? Here it seems to me Mr. Stanley shows a little want of touch with the discussions of current psychology—a sort of personal isolation, as it were. Why does he not bring in some reference to the recent discussions of motor phenomena, kinæsthetic doctrines of voluntary action, reduction of will-effort to a sensational basis, etc. Surely these theories are the most formidable opposites to the vague postulate of will-effort, which he fails even to define. The resource of child-psychology, which Mr. Stanley ranks next in importance to simple introspection, should give him an inkling of the need of settling this great problem. Spencer saw the necessity for a theory of the individual's adaptations—the more, perhaps, because of his Lamarckism in the doctrine of heredity; but there, in heredity, is another question the importance of which Mr. Stanley seems not to have appreciated. I do not mean these things as criticisms of a positive kind, in the face of Mr. Stanley's modest assurance that the book is only a series of studies. But yet when he uses phrases equivalent to 'will-effort' so freely, it can not fail to occur to the reader that it is a bridge of thin ice over these yawning caverns.

Again I think Mr. Stanley's free—I almost said indiscriminate—use of the principle of 'variations with natural selection' leads to little new truth. Cognition is a variation (6173) under which sensation, perception, memory, etc., are all variations. Attention, self-sense, and so on everywhere—all are variations. And then Mr. Stanley seems to think, that his problem is solved when he has pointed out some introspective or speculative utility which, in the mind of the psychologist, should justify this or that variation—*after the fact*. This gives a set of small unimportant problems which each one can settle for himself, as he thinks the facts 'most likely' were. But it is as if the biologist should say: The law of variations with utility solves the question of life; and for this organ or that, its utility assumed, its use was probably this or that. The biologist, on the contrary, goes to paleontology and morphology, and those are the fields where the real facts are found to justify the theory of evolution. The psychologist has his paleontology in the animals around him and his morphology in the nursery. And while, of course, we have immeasurable difficulties to deal with, yet the real emphasis is thus thrown on the problem of individual or ontogenetic development, where the actual utilities may be seen in operation. It is not a mere question of surmise as to this utility or that. I do not insist on this here because it is a matter of personal conviction which I have recently urged at length in my book on *Mental Development*. The principle of circular reaction which I became convinced was of the first importance in the development of the individual development seemed applicable then in race development as well. Whatever may be thought of such a particular sort of formulation, I am yet more than ever convinced, by this able book of Mr. Stanley's, that no mere introspective or descriptive surmises about race-utilities can take the place of some such principle of unity arrived at first by way of the ontogenetic problem.

This point of criticism holds, in my view, all the way through the book. The chapter on the 'self-feeling' is full of keen verbal distinctions, most of them true to introspection as matters of description, most of them requiring a general appeal to the law of variations, and many of them important for general psychology. But Mr. Stanley draws inferences for race-development on such grounds; and whether we agree with him or not depends largely upon whether we follow his distinctions and accept his definitions—and then what is the outcome? Why this: that so, and so, was probably the utility which the animal found in becoming self-conscious! But let us once turn to the field of morphology, the nursery, and enquire into the actual conditions under which the sense of personality arises, and I think one of the two most compelling and conspicuous factors in the whole group of phenomena, is just a factor which introspection has not revealed to Mr. Stanley at all—though even by that method I think he should have got glimpses of it—the fact, namely, that the sense of self—using the term in Mr. Stanley's sense 'as a reflection of experience upon itself'—'by which the individual becomes aware of its own activities as its own' (254)—comes by way of the progressive social consciousness. And if this be true would not the variation in the race series which the self-sense supposes (254) involve this differentia as well as that deduced from the direct interpretation of the private pleasures and pains of the organism? And so be a much later thing than his introspection suggests? This I do not mean to argue; but only to say that in the one case we are in the domain of live concrete facts, sufficiently objective to have positive verification; and moreover we are at a stage of the individual's development at which the elementary facts which we want to observe are likely to be found. To speak again of my personal views, I find with Professor Royce that the sense of self may be treated with some degree of explaining force by the principle of 'circular' or 'imitative' reaction, drawn from ontogenetic observations.

The chapter on attention is, from the point of view of the criticism made above, the most inadequate in the book. Mr. Stanley makes attention the great vehicle of 'will-effort;' thus throwing it in any case, I suppose, on the active side, the motor side, in the process of development. But as for 'will-effort,' so *a fortiori* for attention, we must ask: how does it work? What apparatus does it use? How does it effect organic or ideal accommodation? To these and the almost innumerable questions besides which come irresistibly up when one thinks of the attention genetically, Mr. Stanley has no answer, because he does not ask them. Certainly the bare phrase 'will-effort,' with its equivalents, is not at all illuminating.

I have left for the last the treatment of the Emotions, in many respects the most interesting and valuable parts of the book. This is so, I think, because in this field there are many introspective distinctions to be made, and also because the expressive characteristics of the grosser emotional qualities are so well differentiated objectively as to suggest interesting race utilities. Fear and anger are treated in detail with subtlety and profit. Fear is primitive emotion, and emotion is fundamentally 'pain at pain.' This formula means that emotional pain is due to revival of painful object with consciousness that it is painful (67), (98). This latter element is essential and constitutes the difference between emotion-pain and pure-pain. In this discussion Mr. Stanley lays all the emphasis on pain, none on pleasure, except to point out the contrast of the two qualities of emotion. The duality here, I suppose, is possible because emotion as revival-state does not occur until after pure-pain has differentiated itself into pain and pleasure states. And yet the organic evidence, to my mind, points the other way, namely, to the conclusion that the contrast of pleasurable with painful emotions points to the original presence of a distinction between pleasure and pain. Furthermore, I do not think that Mr. Stanley makes out his point that emotion-pain is 'pain at pain.' The consideration of the evolution of emotional attitudes in recent discussion has tended to show that less rather than more stress is to be laid upon the representative element in emotion; and more on the reflex element. The pain of emotion is largely immediate pain due to function of an hereditary kind. And even when the emotion is one learned by the animal in his own experience I think the pain of it is rather pain from the incipient revival of the reflex consequences of the cognition than from the cognition of 'pain-quality' in the object. So of pleasure, in emotion. As far as there is a new pain or pleasure of revival, it comes from direct accommodation to present experience of object. Of course, in our high reflective lives we have plenty of 'pain at pain.' But Mr. Stanley commits the psychologist's fallacy, I think, in reading the complex formula of 'pain at pain' down into the genetic origins of emotion states. It seems to me that the postulate of simple revival pain, either of direct stimulation or of a reflex kind, would do greater credit to the principle of natural selection and is altogether 'most likely.' The same considerations also apply to emotion-pleasure; we would have to have a formula calling for pleasure at pleasure. Why not say that the revival of cognition pleasure is not always necessary when the object is revived, but that the object-revival tends directly to stimulate the same pleasure that the cognition did?[1]

[1]Mr. Stanley admits a direct lack-pain (pain of unreality, or non-presence)

This requirement is so real, however, that it determines Mr. Stanley's account of desire. He argues for the old hedonistic view, coming now to lay all the emphasis on pleasure (193f). The avoidance of pain is, in the realm of desire, always the pursuit of pleasure.

The complexity that this gives may be pointed out. When a man desires to avoid a painful thing, what he does is this: he pictures the thing, the painfulness of the thing, has 'pain from the pain' of the thing, pictures pleasure from the removal of the 'pain from the pain' of the thing (or would it be the pleasure of the removal simply of the pain of the thing? The former, I think), and, finally, has 'pleasure from the pleasure' of the pictured removal of the 'pain from the pain.' This, to me, is the outcome, in sober truth, of the hedonistic theory when complicated by Mr. Stanley's theory of 'pain from pain' and 'pleasure from pleasure.'[2] And we must add to this the fact, as Mr. Stanley says, that the desire itself is painful. To take a concrete case. Suppose a child crying at the prospect of a cold bath and pleading to be let off. Does he picture the bath in revival, the pain of former baths also in revival, get pain from this pain, picture pleasure from the removal of this pain from the presented or revived pain, and then get sense of pleasure from this pleasure, to prompt his desire?—this last being the end which justifies the hedonistic postulate? Surely all this, or anything like it, is not there. The child has revived symbols of the bath-act, and reflex and associated pain states with them; these latter revive the associated shunning movement and speech tendencies, etc., and the consciousness of these latter is the desire. The *end is* the symbolic bath-act, pure and simple; that fills the child's consciousness up so full and its hedonic quality (not *recognized* mainly but *refelt*) is so utterly unbearable that he bursts out in the associated movements—in this case movements indicating negative, so to speak, rather than positive, desire.

In this difference from Mr. Stanley I have no intention of minimizing the factors involved nor of discounting the real complexity of these higher evolutionary products. It is possible—or, as Mr. Stanley says so often from his introspective points of view, it may be 'most likely' —that the process of genetic acquisition of desire has been more complex than the simple scheme which I have indicated. But we all

in lower organisms. Why should there not be a direct lack-pain at the higher representation level—pain of unreality of object without cognition of 'pleasurableness' of the lacking object?

[2] This on the view that desire is emotion (193); and I have not introduced certain other elements included in Mr. Stanley's scheme of eight factors (208).

recognize the abbreviating processes of evolution and expect the lapsing of links which a chronological order would seem to require; and, on the whole, it seems much simpler to make the original tendencies of action terminate on objects, clinging to the functional or 'index' view of pleasure-pain, and then to keep this object-consciousness forward all the way up the genetic scale. Then in the interpretation of the higher consciousness one may accept the outcome of the overwhelming current of criticism of hedonism. Certainly it is something to avoid the remarkable shifting of emphasis from pain in the original and lower stages, to pleasure in the higher, to which Mr. Stanley has to resort.

There are many interesting topics in the book on which it would be profitable to dwell; but I may only cite summarily certain special teachings of Mr. Stanley which are confirmatory or corrective of views of others, and important: 1. Pain is declared to be 'purely monitory (14)'; this I think contradicts Mr. Stanley's own view that pain is the direct stimulant to 'will-effort;' for if the latter, then the pain must be, as the author seems to teach elsewhere, index of benefit-from-stimulus, which is actual, not prospective only. It has a 'monitory' meaning also, of course. 2. The emphasis of the fact that all mental development is an achievement, 'never a given.' Everything is achieved by struggle, action, effort (23, 29, *et al.*). 3. Confusion arises from the use of the word 'feeling' in three senses: namely, as equal to 'consciousness,' as 'pure' pleasure and pain, and as qualitative emotion. 4. Confusing use of the expression 'quantity of consciousness' to mean area or *Umfang* (55). 5. Very interesting theory of the phylogenetic origin and value of 'unreality-feeling' (85). It is directly confirmed in the life of the infant, as I have argued elsewhere. 6. Unhappy use of the word 'representation' to include recognition (86). 7. Mr. Stanley makes the animal's going-out reactions—*i. e.*, for food, etc.—a late accomplishment, dependent on representation with recognition of object as pleasure-giving. Why is this necessary when the opposite—*i. e.*, the struggle away from the pain-giving object—is organic and primitive? The argument for the latter from natural selection will secure as well an immediate reaction for pleasure-giving stimulations. I have used the same argument for the primitive character of both sorts of reaction (*Mental Development*, p. 173f). 8. Mr. Stanley follows Spencer in making the utility of touch lie largely in the 'circular reaction' function which it exemplifies (193). Why does not natural selection secure this state of things more primitively, so that it is true earlier that 'the edible is no longer fortuitously hit upon?' 9. The doctrine that all attention is volitional and that all intensity quality in sensation is in its origin volitionally achieved, would be much better expressed by maintaining the current distinction between 'reflex' and voluntary attention, and then adopting some general term like the current 'motor-process' to express the active process of 'achieving' all the way through (228). The confusions into which Wundt has fallen in his doctrines of attention by this same procedure might be a warning against calling the struggle of the amœba away from pain-conditions 'volitional.' 10. Object and subject-cognition are 'coincident in their origin' (252); and since sensation is cognition, all sensation involves self-sense. Mr. Stanley here seems to confuse pleasure and pain values with sense of their value for a self. He is led into it by his doctrine (criticised above) that pleasure-pain is represented as conscious end. 11. The insistance that emotion is *genetically stimulant* to useful activities and not result of them is justified (360); but only on Mr. Stanley's view that emotion is intrinsically pleasure-pain. I can not see any way to avoid this claim that pleasure-pain-feeling is the dynamogenic factor all the way through. 12. Interesting discussion of play (364ff).

I have no space to speak of the author's interesting chapters on Æsthetic and Ethical Emotion. J. M. B.

ETHICAL.

Studies in Character. S. BRYANT. New York, Macmillan, 1894. ($1.50.)

Hedonistic Theories from Antippus to Spencer. JOHN WATSON. New York, Macmillan, 1895. ($1.75.)

Mrs. Bryant's Essays are grouped under the heads 'Ethical' and 'Educational.' None the less there is a decided unity of method and point of view running through all of them. The ethical essays carry educational implications throughout, and it is the ethical side of education which commands Mrs. Bryant's attention. It is to be hoped that the book will attain a wide reading in the educational community. It is a book that does not shock one's intellectual self-respect, which is more than can be said of many professedly pedagogical treatises; and it utilizes in an unobtrusive, but none the less effective, way very much that is best in current ethical and psychological writings. Mrs. Bryant is at home in what is being said and discovered in the vital places of current discussions—another mark of eminent distinction from much of what passes as pedagogical contributions. Systematic in outer form, being a collection of essays, the book is not; systematic in unity of conception and method the book is, much more so than many more pretentious treatises.

In dealing with such topics as 'My Duty to thy Neighbor,' 'Friendship,' 'Soundness of Intellect,' etc., one perhaps could be brilliant only at the expense of sanity, and original only by leaning towards eccentricity, and the originality of sincerity (which, as Mrs. Bryant quotes Carlyle is the real originality), Mrs. Bryant possesses. However this may be, there is a tendency at times to fall into a certain explicitness of classification and definition that makes long continued reading an impossibility. A few pages are suggestive; two or three chapters of it load one with the feeling of assisting in the laying out of the corpse of the moral universe. As Professor James has remarked about too much descriptive psychology there are many things which it is highly interesting to experience, but a little tedious to be reminded of in too much detail and with too explicit a touch after we have been through them. Perhaps only Aristotle at his best, and the French moral essayists with their capacity for unexpected epigram and their ability to flash upon the reader the ironical reverse of their own definitions, have ever been at home in this region or moral description.

As to the implied ethical doctrine of the book, it is upon the whole, the idealistic interpretation of the conception of self-realization, vitalized for educational purposes with considerable concrete psychology regarding the motor tendencies of ideas and concrete insight into individual temperaments and types. I cannot forbear from pointing out that while in her ethical doctrine Mrs. Bryant conceives the 'ideal' to be perfection located at a remote goal; for practical purposes, she, like all other perfectionists, gets down to approximate ideal, which is the right functioning of present powers, or the relating of conditions of a present situation. The same contradiction occurs when Mrs. Bryant is getting at ideals from a psychological standpoint. The theory implied in practice is so certain to be more adequate than theory set up as theory of practice.

There appears to me also to be a regrettable tendency in Mrs. Bryant to over-emphasize the personal or immediate, direct side of conduct—devotion to persons, whether one's self or somebody else, instead of devotion to work, to action and to persons, whether one's self or others, indirectly through their implications in activity. But so far as there is any concensus of ethical doctrine on this point, I suppose it is with Mrs. Bryant rather than with the reviewer; and, as the point is too big for discussion in a review, the matter must go as a personal regret and dissent. All this direct moral devotion to persons, I believe can end only in useless complications, weariness of flesh and spirit and contradictions between our aspirations and our accomplishments, both in theory and in practice.

Professor Watson publishes his criticism of hedonism 'as a needful supplement to the ethical part of his [my] Outlines of Philosophy.' His method of criticism is, as indicated in his title, historic. It is historical types, rather than actual historic continuity, however, which Mr. Watson deals with; his authors being Aristippus, Epicurus, Hobbes, Locke, Hume, Bentham, John Stuart Mill and Spencer; about one-fourth of the book being devoted to the last named.

After discussing the influence of the Sophists, Aristippus is considered as the type of naive and, in one sense, the only consistent hedonism—the seizure of the pleasure of the present moment. Professor Watson points out a psychological contradiction contained in the idea of seeking momentary pleasure; *seeking* for pleasure introduces struggle and pain; pleasure as pleasure comes and is enjoyed without being sought. The doctrine is also shown to involve an essential misreading of human nature, ignoring the simple fact of experience that men seek active ends in which undoubtedly they anticipate and find pleasure, rather than pleasure as such. Epicurus enlarges and, in an objective sense, rationalizes the momentary, transitive end of Aristippus in introducing the idea of the greatest pleasure on the whole as an end; but as Professor Watson points out, at the expense of hedonism, virtually substituting a state of contentment for the ideal of pleasure; and contentment, in turn, involves its own peculiar self-contradiction, since to make the attainment of individual contentment the ideal is to throw everything back upon individual temperament, and thus deify lawlessness. Hobbes generalizes the hedonistic conception still further; Aristippus simply ignored the state; Epicurus was for getting along with it with the least possible trouble; Hobbes will turn the whole social organization into a means of bringing pleasure to the individual.[1]

[1] While I hesitate to differ from Prof. Watson on a historical point, this statement as regards Hobbes seems doubtful. Perhaps Hobbes ought in logical consistency to have taken this view; but as matter of fact he seems to me to throw all the emphasis on the *substitution* of the end of the sovereign for that of the individual; and his whole political reasoning to be a back-handed way of saying

Locke represents a consistent inconsistency—a philosophy of compromise. His intentions are good; his performance poor. He intends to assert freedom, but he holds that the strongest uneasiness determines the will, and uneasiness is simply the desire for the pleasure that is strongest. He intends to uphold the objectivity of moral distinctions, and defines the good as that which is conformable to law; but when he states how law lays hold on the individual he falls back on the pleasures got by obedience and the pains suffered through disobedience. Hume is as uncompromising as Locke the reverse. Pleasure is the sole motive, and reason can never be a motive; its sole office is to serve the feelings. With Hume the hedonistic logic may be said to have become explicit and self-conscious. The self being only a bundle of feelings, there is naught but feeling to seek or avoid, or by which to seek or avoid.

With Hume the logical evolution of hedonism ceases; since him we have only recurrences to earlier types, or else its ennobling through the introduction of ideas non-hedonistic in character. Bentham in a way went back to Hobbes, only with great practical interest in social reform which lead him to introduce elements irreconcilable with hedonism, while Stuart Mill can be made consistent only by in-interpreting his practical views from the standpoint of an idealistic theory. The examination of Mr. Spencer takes up his ethical doctrine both in its hedonistic psychology, its evolutionary aspects and the relation of one of these to the other, with a view to showing that Mr. Spencer's general formula of evolution throws no light on moral conduct; that his psychology destroys the reality of obligation, and does not justify the transition from egoism to altruism; while the idea of a completed life and completed society held up as the goal from the side of evolution have no special coherence with the ideal of pleasure set up on the analytic side.

Philosophic exposition is at its best as to style in this book of Professor Watson's. I could with difficulty name another book which might at once command so thoroughly the respect of the specialist and receive comprehension by the layman as does this lucid, direct piece of exposition and criticism. It may be of service to teachers of ethics to point out that the expositions of the various authors, mainly

that *since* men live in society they must regard the social end before the individual end; and that *if* they lived in a state of nature, while each might then follow his own selfish end, yet such a state would be self-contradictory. In other words, Hobbes' psychology and his sociology contradict each other flagrantly, instead of the latter being an instrument as regards the former.

in the authors' own words, are well proportioned, condensed and accurate, and, in some cases, the best available substitutes for a perusal of the original texts, and in all cases a helpful accompaniment of such perusal.

The book seems to me to close the case, on the polemic side, as regards hedonism. Undoubtedly we shall go on having arguments both for and against hedonism, but the interest seems about done with. The rise of a new psychological method and of a new sociological point of view and body of facts have presented new problems and shifted the focus of attention. These indirect influences have probably done quite as much as more direct criticism in making hedonism a played-out standpoint. Just because Prof. Watson's book has accomplished its task so thoroughly, one lays it down with a feeling of what has not been accomplished, and of what constitutes the next task—the discussion of hedonism from the historic standpoint, in the evolutionary sense. We do not need longer to contend with hedonism as a present foe, and consequently we want to comprehend it more thoroughly as a manifestation—comprehend it not in terms of itself, but in terms of the social and intellectual conditions which have given birth to it, to see what it really means when so interpreted. From the historic evolutionary standpoint, there has been the same inner necessity, in the logic of growth, for the appearance of these hedonistic systems as there has been for that of any transcended animal or political form of life. What is that inner necessity?

JOHN DEWEY.

LESIONS OF THE CORTICAL NERVE CELL IN ALCOHOLISM.

Experimentelle Untersuchungen über die Veränderungen der Ganglienzellen bei der acuten Alcoholvergiftung. HEINRICH DEHIO, Centralbl. für Nervenheilk. u. Psychiat. V. 113–118.

Studies on the Lesions produced by the Action of certain Poisons on the Cortical Nerve Cell. I. Alcohol. HENRY J. BERKLEY. Brain, LXXII. 473–496.

The two articles above mentioned have made an attack upon one of the least worked fields of nervous pathology opened up by the advance of the last few years in the methods of preparing and staining nerve tissue. The results hardly admit comparison, for while both observers used rabbits for their experiments, and to a certain extent the same method of preparation of sections, Dr. Dehio's subjects were all those of extremely acute alcoholism, the animals having died within 1–36 hours after the first administration of the poison, while Dr. Berkley's had been treated for a much longer time (from five to twelve months), so that the alcoholism had become more or less chronic. What is also to be regretted for comparative purposes is that the German observer confined himself to one method of staining (Nissl's methylene blue), and his observations to Purkinje's cells in the cerebellum, while Dr. Berkley apparently made no observations on these cells with that method, though he employed the Nissl stain on cells of the hemispherical cortex.

Dehio's rabbits were treated in most cases with subcutaneous injections of 40 per cent. alcohol, the first injection being from 7–10 ccm. and the resulting intoxication kept up by injections of 5 ccm. whenever the animal showed signs of recovery. According to the time of intoxication before death resulted the total amount of alcohol administered varied from 20 to 25 ccm. To be brief, there were no changes noted in Purkinje's cells in cases where the ante-mortem intoxication had been very short. When that was longer he found that, instead of the normal finely meshed network of the cell body, the stained substance showed granules irregularly distributed, but of fairly uniform size, while the unstained substance had taken on a light bluish tone. The cell changes in some cases involved the whole cell, in others only a part, while the fine granule rows of the processes appeared always unaffected. The nucleii were unaltered. Even in extensive changes by no means all of the ganglion cells were affected; there were often whole rows of entirely normal cells, while between them lay singly or in groups the pathologically changed.

Dr. Berkley's experiments were much more complete and systematic and had what would seem the additional advantage for the investigation of the comparatively long period of alcoholism before death. The methods of staining which he used were both the Nissl and a modified Golgi-Cajal.

His results showed, besides certain vascular changes, modifications of nerve cells, as follows: By the Nissl method the nucleoli of many of the cortical cells appeared roughened and uneven, and in many cases enlarged and surrounded by a granular appearance. By the method of silver impregnation the principal lesions were distinct diminution in size of a great majority of all the cortical cells, certain swellings in the dendritic processes with roughening of some of the processes and sometimes of the cell bodies. All the layers of cortical cells seemed to partake of the degeneration to some extent. Owing to the small proportion of cells that are stained at any one time by the silver method, it was impossible to determine even approximately the proportion of normal to abnormal cells.

In the cerebellum Purkinje's cells showed distinct degenerations, but, as already remarked, the difference of method does not allow comparison between Berkley's preparations and those of Dehio. Dr. Berkley promises a subsequent paper upon the lesions in chronic alcoholism in the human subject which will be awaited with interest.

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SENILE DEMENTIA.

Ueber Dementia Senilis. Inauguraldissertation, von JEAN NOETZLI, med. pract. Aus der psychiatrischen Klinik des Herrn Prof. Forel in Zürich.

Dr. Noetzli submits 70 cases of senile dementia to a clinical and pathological analysis. Only those patients were selected who died between January 1, 1880, and December 31, 1891, and in whom an autopsy was made. The dissection of the brain was made by Prof. Forel himself in most cases, which secures a very welcome uniformity of the material for comparison. The method used is Meynert's.

N. makes first a rough classification based on the pathological findings. While the changes in the brain are purely degenerative and always accompanied by arteriosclerosis, in one class the patients die with a uniform degeneration of the brain without focal lesions; in the other classes there are moreover circumscribed lesions of a thrombotic or hæmorrhagic nature. This second group is again subdivided into:

A. Cases of Senile Dementia, the focal lesions of which are symptomatically completely latent, or in which focal symptoms appear in the course of the disease or towards the end.

B. Cases of Senile Dementia which set in with apoplexies or other focal symptoms.

Clinically, the cases with focal lesions are marked with rapidly progressing dementia and transitory melancholy or maniacal periods of excitement, while typical senile melancholia and senile 'mania persecutoria' belong almost exclusively to the group without focal lesions.

Etiologically, N. states with Forel that the heredity of a disposition to atheroma of the blood vessels may be more important than a heredity of mental diseases (Fürstner), but that the quiet forms of Senile Dementia seen in the poorhouses or asylums for old people would be cases without disposition to (positive) insanity, whereas those predisposed to insanity will show the symptoms of senile mania and senile 'mania persecutoria.' This is, however, not proven, because no special attention is given to the question in the records. Changes in the material life of the patients, especially physical infirmity and diseases, are very prominent elements, not only where they lead to senile hypochondria; psychical influences play decidedly a part in many of those predisposed to insanity, contrary to the view of Fürstner, who states that as a rule the victim of senile dementia is dull towards psychical impressions. Further, Forel makes a special group of Dementia alcoholico-senilis, which seems to be very frequent in Switzerland.

Symptomatology: 'Senile' psychoses are, as a rule, distinguished by a prodromal stage, with loss of memory, change in character, and slight intellectual and ethical defects.

Senile mania lacks the breadth of ideation, the acuteness of judgment, the wit and the flight of ideas of typical mania; it is rather loquacity with senseless, impulsive actions and confusion especially as to time and place. Emotions are superficial.

Senile melancholia: In his cases, N. finds no delusions of self-belittlement (on account of the weakness of ethical and moral processes); anxiety and fear are apt to cause raptus and unexpected attempts at suicide; hypochondriacal symptoms are prominent.

Senile 'mania persecutoria' or Verfolgungswahnsinn are very often based on hallucinations and most marked at night; these are, however, primordial deliria of persecution.

The alcoholico-senile Dementia shows strong manifestations of chronic alcoholism; its outset is relatively premature; at a very early stage the patients commit suddenly impulsive acts on their relations; hallucinations are frequent and characteristic for alcoholism, but the dementia modifies the whole symptom complex of alcoholism.

The 70 cases are classified as follows;

I. Senile psychoses without focal lesions 40
(a) Senile mania 6
(b) Senile melancholia.......................................10
(c) Senile 'mania persecutoria' 4
(d) Senile hypochondria 1
(e) Dementia alcoholico-senilis 6
(f) Typical simple senile dementia 13
II. Senile psychoses with focal lesions 30

A. Cases of senile dementia with focal lesions, which were latent or secondary—15:

(a) Senile mania	—
(b) Senile melancholia	3
(c) Senile mania persecutoria	2
(d) Dementia alcoholico-senilis	1
(e) Typical senile dementia	9

B. Cases of senile dementia setting in with apoplexies or with other focal symptoms—15:

(a) Senile mania	1
(b) Senile mania persecutoria	2
(c) Dementia alcoholico-senilis	2
(d) Typical senile dementia	10

The very important study of the brain weights cannot be given in detail here. The chief results are that the decrease of weight averages 200 grammes, the average weight in men being 1,190, in women 1,065 grammes. The brain mantle loses most in weight, but, contrary to the view of Meynert, the loss is not greater in the frontal lobe than in the occipital or temporal lobe. ADOLF MEYER.

WORCESTER, MASS.

HYPNOTISM.

Ueber Schlaf, Hypnose und Somnambulismus. MAX HIRSCH. Deutsche med. Wochenschrift. 5 Sept. 1895.

The above essay offers, according to the author, a partial solution of the question whether hypnotic and normal sleep are identical, or whether these two conditions must be considered as differing from each other. In his manual 'Suggestion und Hypnose' (Leipzig, 1893) the author expresses himself of the latter opinion; hypnosis he here considers merely as a sleep illusion. Influenced by new observations, he now announces a modification of this view. He could observe that 10 per cent. of all persons hypnotized by him fell at the first attempt into the deepest state of hypnosis, which term the author applies to that condition in which all hypnotic phenomena may easily be produced and with which loss of memory is always connected. The author further observed that the same persons likewise show a certain peculiarity with respect to their normal sleep. They can fall asleep when and where they wish. Rapport is also present in these persons during sleep. As the sleep of these persons completely resembles the condition of hypnosis, the author designates it as 'somnambulic sleep,' but he leaves it undecided whether this sleep is or is not a pathological condition, although in a number of people of neuropathic tendency it represents a species of sleep. Those somnambulists, however, in whom the representations during sleep develop into actions are designated as undoubtedly pathological. Although Hirsch still maintains the opinion that hypnosis is *in general* a sleep illusion, he designates, on the other hand, the hypnosis and sleep of somnambulists as identical. "Hypnosis is in them somnambulic sleep." The author explains these phenomena as arising from the different degrees of attention. In normal sleep the attention is, in his opinion, diffused over all centers of represention; in this condition it is incapable of concentration; in somnambulic sleep, on the contrary, the attention retains the power of diverting itself to single representations. Thus the attention of these persons need only be diverted towards the representation of sleep in order to cause them at once to fall asleep.

I should like in this place to make mention of the investigations by which Dr. Oscar Vogt has endeavored to decide the present question, and which he will shortly publish in the 'Zeitschrift für Hypnotismus, etc,' edited by him. So far as I am acquainted with these interesting researches I am enabled to communicate the following facts. In comparing the graphically fixed attendant phenomena of hypnotic and normal sleep Dr. Vogt comes to the conclusion that there are here many individual differences, but that these attendant phenomena are identical in one and the same individual. Vogt further states that in all normal sleep there is a certain stage which admits of rapport as in hypnosis and that, if this moment is not missed during the falling asleep of a person, he may be treated precisely as a hypnotized subject. Vogt adheres therefore to the school of Nancy and maintains, contrary to the view taken by M. Hirsch, the complete identity of natural and so-called hypnotic sleep.

LEIPZIG. FRIEDRICH KIESOW.

Criminelle hypnotische Suggestionen. DR. A. A. LIÉBAULT. Zeitschrift für Hypnotismus. Bd. III. Hefte 7, 8, 9.

The author advocates the possibility of hypnotic and post-hypnotic criminal suggestions by presenting facts of history, analogies between sleeping and waking states, and incidents of his own experience. Dr. Durand de Gros, who wrote 'Electro-dynamisme vital' (1855), believed himself able to transform moral character and wrote to the Spanish Court for permission to operate on Manuel Blanco, who, under the conviction that he was a wolf, killed six men and actually ate parts of the bodies. The middle ages produced numerous cases of this kind under the name 'Warwolfe.' They are men who reflect upon and admire one action or type of action till they finally merge their own personalities into a submission to it from which neither reflection nor volition can free them. A lunatic who believed himself a general and dressed in uniform might just as well have been a Warwolf. A little girl of nine or ten years was freed by hypnotic sittings from the illusion that she was a dog and from habits of lying by the door, barking at visitors, running on all fours. Later the girl's grandmother asked the author to free the girl's father from a low passion for his child. Still later a neighbor's daughter reported to her parents that the man had misconducted himself toward her. He was imprisoned for life. Author believes the father had already suggested to his own child that she was a pup.

Fixing the attention on the key-note of hypnotism, sleep, the fixed idea of rest, why may not one become subject to personal influences as completely as in cases of fascination where persons are often brought to violate conviction and habitual sentiment? Certain signs of sleep in waking state are physiological and pathological hallucinations, impulsive actions, fixed ideas, as that one cannot swallow something bitter, etc. Signs of wakefulness in sleep are writing poems, solving problems, recognition of approaching danger, consciousness of the passing of time, recognizing the stopping of a clock, etc. From 4 per cent. to 5 per cent. of subjects have by the author been brought to perform what would have been terrible crimes had they been real. Automatic sleep-suggestions have led to crimes in waking state in three cases. Author mentions Jacques Clément, who believed an angel commanded him, in sleep, to murder the King of France; also of Friedrich Staaps, who attempted the murder of Napoleon I., because convinced of a divine commission to do so. Liégeois and the author saw a subject choose a suggested watch instead of his real one for his own, proving that suggestion is real.

The author asserts that anyone in artificial sleep can be brought to perform any crime in which he is able to participate without waking in dreams. The author holds that only somnambulists—'and they are but few, as one sees'—are capable of carrying out these crimes. Those who fail to get criminal suggestions carried out choose subjects without premeditation. The author relates the case of somnambulist N. Dr. X. and the author suggested to N., in artificial sleep, that he would visit Herrn F. at his home on the next morning at 11:30, and in leaving the house would conceal two statuettes near the door under his mantle; after two days N. would regret his theft and return the pieces of art. Dr. X. repeated to N.: "and you will steal, do you hear? You will steal!" Later Herr F. related the event as having been caried out in every particular. Later still, N. was imprisoned for stealing an overcoat. On his person was found a note-book recording many small thefts, such as visiting cards. The author felt himself possibly to blame. Much later when N., who was at the time of the theft 17 or 18, had grown (N.'s father forbade it sooner) the author again hypnotized N., and learned that at the same time when the boy committed the theft which ended in imprisonment, Dr. X. had met him on the street, lead him into a café, hypnotized and commanded him to steal 'little things,' such as watches, gloves, money cases and probably visiting cards. GUY TAWNEY.

LEIPZIG.

VISION.

Note on the Analysis of Contrast-Colors by Viewing, through a reflecting tube, a Series of grey disks or rings, on colored surfaces. A. M. MAYER. Am. Jour. of Science. (4) I., 38–40. 1895.

Article (Vision) in Johnson's Universal Cyclopædia. W. LE CONTE STEVENS.

Untersuchungen zur Lehre vom Farbensinn. W. KOSTER. Arch. f. Ophth. 41 (4) 1–20.

Théorie de la Coleur. W. NICATI. Arch. d'Ophtal. XI., 1–44. 1895.

From the fact that a dark grey background is most effective when yellow contrast-color is to be produced, and a light grey when the color to be produced is green, Professor Mayer argued that a yellow-green contrast-color would change its tone with a change in the intensity of the background. He found this to be the case. Bits of violet paper were placed on thirteen different shades of grey; on the four lightest, the contrast-color was a greenish yellow; on the fifth, it was equally yellow and green; and on the darker papers it became greener and greener until at last it was a green almost devoid of yellow. But Professor Mayer considers that this *may* be due to the fact, noticed by Professor Rood, that some colors regularly change their tone on being mixed with larger and larger amounts of black.

Professor Stevens has given what must be considered as an admirably clear account of the principal phenomena of vision, when regard is had to the small space into which it had to be condensed. He makes short work of the bugbear of the inverted image, and he shows that the contest between the empirical and the nativistic school loses its importance in the light of evolution. Attention is given to some of the new facts of color-vision; but it is an inadvertence to say that the cones are sensitive to variations of color chiefly. The correct statement would be that *only the cones* are sensitive to variations of color; they must be extremely sensitive to variations of intensity in white light as well,—otherwise the fovea would not be the place with which we make out the minutest variations of line and shade in an intricate drawing. If the *cones only* give color, they do not give *color only*. Every new and adequate theory of vision must make provision for this fact; but, strange to say, it has been overlooked by no mean authorities.

I must protest also against saying that the physicists are satisfied with Helmholtz's theory of vision, with the implication that that is a fact of critical importance. The physicists have nothing to do with a theory as to what goes on in the retina and in the brain—that is beyond their province. It would be as much to the point if the chemists were to announce that they were perfectly satisfied with the corpuscular theory of light. As matter of fact, the objections to the theory of Helmholtz are exclusively objections from the side of sensation; as far as the physics of the question is concerned, there is nothing in the theory that anyone could take exception to. And when it is a matter of discussing light as a *sensation*, we do not so much say that the physicists are not in the habit of thinking about their sensations, pure and simple, as that they are not in the habit of reading up the discussion that is going on regarding sensation. Professor Cattell has said he best word that has been said about the Helmholtz theory when he said that it is both pre-evolutionary and pre-psychological; the arguments that hold good against it are not only arguments that appeal with especial force to the physiologist and the psychologist, but they are arguments that have been debated in Pflüger's *Archivs* and the *Zeitschrift für Psychologie u. Physiologie der Sinnesorgane* and other journals of that kind, which the physicists, overwhelmed as they are by their own journals, have no time to read. Even the critical facts, of late discovery, do not always reach them. Captain Abney, in his last book on *Color Vision*, says of a certain man, who had no variation of sensation throughout the entire spectrum, that it has been 'proved' that he sees green only. Now if a man has no other light sensation with which to compare his one sensation, it is absolutely impossible for us ever to find out what that one sensation is, unless by way of deduction from a theory which is taken as proved. But, fortunately for such cases as this, there have been instances of monocular total color-blindness, and from them it is known that the single sensation is a colorless sensation. Moreover, we have ourselves this monotone sensation in the periphery and throughout the retina in a faint light, and one must have a very strong preconceived affection for a theory to regard this sensation as green, though this, too, is a feat that has been accomplished. I have been told that there is one important university in this country in which the theories of Helmholtz and Hering have both been definitely given up, and particularly in the physical department.

Koster explains the fact that the fovea lags behind the periphery in sensitiveness to faint light by the fact that it is generally, on account of its position with respect to pupil and lens, more brightly lighted up, and hence, if I understand him, in a condition of greater exhaustion. He forgets that in a faint light also the fovea has the same advantage of position, and that the superiority of the periphery is on this account greater than a simple measurement gives evidence of. For Koster's eyes the difference would seem to be very slight; if this is so his eyes differ most remarkably from those of other observers who have measured the phenomenon. He uses the term periphery without any indication as to what part of it he is comparing with the fovea. The maximum sensitiveness is about 35° away from the fovea, and it is true that at distances remote from this the superiority is not extremely great, but at this distanee it is, for most eyes, as four to one, which is hardly to be called slight. He seems to have made no measurements.

Koster finds the Pürkinje phenomenon to persist, under certain conditions, in the fovea itself; this does not, however, disprove the belief that the visual purple is the principal factor in the adaptation which the rods undergo. The cones have a means of adaptation of their own in the varying length of their myoids under light and shade (Angelucci, van Genderen Stort), and also in the moving out and in of the pigment grains. This might also account for a superiority of the edge of the fovea over its centre, which Koster detects. That the adapted eye sees colors less well than the unadapted, Koster finds not to be the case. This agrees with my own observations; I found, in fact, that there is a distinct adaptation for color, though nothing like so much as for light, in the middle periphery.

Nicati uses the term color for the entire sensation produced by light, as painters speak of the color, sometimes, of a picture in black and white. By protochroism he means grey vision; by metachroism, partial color blindness; and by pleochroism, normal vision. He gives a theory which, he says, will seem at once to be plausible, and which will be confirmed by all the considerations which he will have mentioned at the end. In the rods and cones, he says, there is no differentiation such as could give rise to *three* colors, but in the central terminations of the bipolar cells, as described by Ramon Y. Cajal, we have just the separation into *three* layers, which we are in search of as a basis for a three-color theory. The chemical effect of light on the photopsine (the visual purple) is to disengage electricity; the different threads of the bi-polar cells have different electric resistance, and thus the electricity is conducted, according to its varying degrees of strength, by one or another of the sets of threads to the several layers of their terminal expansions. (But is not this a little like making a big door for a cat and a little door for a kitten? What prevents the strong current from going also through the path which is fitted to conduct the weak current?) The synoptoblasts are the large ganglia below the bi-polar cells, and their function is to restore equilibrium, after red has been seen, by sending down a discharge which results in green.

C. Ladd Franklin.

EXPERIMENTAL.

Ueber den Einfluss von Gesichts-Associationen auf die Raumwahrnehmungen der Haut. Miss M. F. Washburn. Philosophische Studien. Bd. xi. (1895), pp. 190–225.

The development of tactual space is undoubtedly influenced by vision and, though the assertion of this article that the fact has entirely escaped the notice of previous investigators, with the one exception of Weber, who mentions it only in a negative way, called for a correction in a note by the editor, yet a series of experiments such as Miss Washburn has made serves to emphasize an important truth. Results obtained from subjects who visualized but little, from others who are able at will to abstract from their otherwise vivid visual images, and finally from one blind subject are compared with normal results in which the images immediately arising when the skin is touched are allowed to play their usual part. In this way it is shown that Camerer's subjects in his experiments on the method of equivalents used visual images as a means of estimating the lengths of the two distances and that his results are to be interpreted in the light of this hypothesis. The well-known observations that the threshold on the extremities is shorter in the transverse than in the longitudinal axis is corroborated only in the case of good visualizers. For poor visualizers the difference does not appear, and for the blind subject the ordinary relation was completely reversed. These observations are to be explained on the ground that the narrower, transverse axis is more easily and clearly visualized. The rapid lowering of the threshold through training, as reported by Volkmann, does not appear when the influence of vision is eliminated. The author thinks the fact that the earlier investigations were carried out with open eyes accounts for the reduction of the threshold. The judgment of direction of continuous stimuli or of the relative direction of two points from one another is dependent to a large extent on visual images as shown by the fact that this judgment is most accurate in the care of good visualizers. In making these experiments the facts were noted that, in general, two points can be distinguished before their relative direction is correctly perceived and that the direction of continuous stimuli is judged better than that of an equal extent lying between the two points of the æsthæsiometer. CHAS. H. JUDD.

LEIPZIG.

On the Development of Visual Perception and Attention. HAROLD GRIFFING, Ph. D. American Journal of Psychology, Vol. VII., 227–236. Jan., 1896.

Several series of letters were exposed through an opening in a screen before a group of persons. In the first experiment six letters arranged so as to avoid suggesting words as much as possible were exposed at a time for $\frac{1}{10}$ second and for ten successive times. The intervals between the several exposures and the warning signals were varied and the observers were without knowledge when they might expect the signal. The observers were examined in groups of ten to thirty, representing all ages from seven to twenty years. The observers fixed their attention upon the opening in the screen until the warning signal was given, and after the exposure they wrote down the letters which they thought they saw. Tables are given showing the results arranged according to age and grade in school. From these "it is evident that the extensive threshold, or ability to receive and retain a number of simultaneous retinal impressions, is a function of individual growth, reaching its maximum only when the observer is fully developed." "Practice increases the extensive threshold * * *" "The tendency to guess seems to decrease with maturity." The intellectual capacity as judged by the teachers was compared with these results. The brightest pupils showed the highest averages, with some notable exceptions. Those pupils who marked high in attention generally excelled others. The girls showed no superiority over the boys. Better results were obtained when the exposure followed the warning signal by a long interval. Children may experience abnormal fatigue "without any marked effect upon the accuracy of perception." In recalling the letters we "see the given stimuli as a unit and then analyze this unit into its components." When only one letter was exposed the older pupils again excelled the younger; the results were respectively seven errors in 230 observations and twenty-eight errors in 160 observations. When six colors were used in the place of the six letters the results were apparently the same. When the exposure was made for a full second, there was a greater percentage of correct answers, the older pupils showing the higher percentages. "The extensive threshold does not measure the number of objects that can be simultaneously grasped by consciousness;" it "may depend upon the reproductive processes, and the analysis of the memory image," and "to some extent upon the attention." The 'capacity for attention' is to be distinguished from the powers of the attention.

The paper has great significance for psychology and for the practical teacher who has to do with the marking and promotion of pupils. The results are conclusive against the absolute value of a system of examinations. It is well to bear in mind in regard to the extensive threshold increasing with age that in our schools a process of selection is going on all the time. Many of the poorer pupils drop out before they reach the higher grades and the dull pupils are frequently cases of slow development; they show their brightness at a later period. The personal element enters into a teacher's estimation of pupils and this may explain some exceptions. In the last experiment we are not told whether different series of letters were used, or whether other pupils were experimented upon. Unconscious memory in the one case and practice in the other may play some part, as I have found in some of my experiments, to which the author refers in a note.

T. L. Bolton.

CONSCIOUSNESS.

Zur Kritik des Seelenbegriffs: einige Bemerkungen beim Studium der Wundt'schen Psychologie. ALLEN VANNÉRUS. Archiv für System. Philosophie, Bd. I. Heft 3,360–400. 1895.

Wundt rightly maintains, says his critic, that the subject of psychology, from the psychological point of view, exists in the activity of the psychical process and not as a substance lying back of it. But his opposition to the theory of a substantial soul rests on a restricted conception of substance, which need denote no more than the real ground for determinations not absolutely independent or be more than an abstractly conceived factor in a continuously changing whole. And his denial of the applicability of the conception to inner reality on the ground that the latter is reality at first hand, and therefore not constituted by a category which is its own product, rests on a mistake as to the facts. For only an actual content of consciousness is directly intuited, whereas other aspects of psychical reality, the fusion of sensations, for instance, can only be inferred. Wundt's emphasis of the process in mental life seriously threatens its real unity. But change without permanence is impossible. It is psychologically impossible because the relating activities of consciousness presuppose at least a relatively permanent subject, and because, without some constancy in the subject, not only would all mental states eventually pass into nothingness, but, except by a miracle, no mental state could ever arise. Logically, again, all activity implies a constant factor; otherwise reality is 'a hideous mystery of limitless possibilities.' Finally, the theory of parallelism requires an original psychical reality as the subject of the development of consciousness and the basis of its various modes. This original psychic basis of mental life is constant, not as a substance 'lying back' of experience, but in the sense that it is self-identical in its different functions. Wundt, however, makes the unity of the mental life consist in the connection of the psychical events themselves. But if these events are not functions of the same subject, how is such connection possible? We must postulate their creation *ex nihilo* and assume as many egos as states of consciousness. The truth about the soul is that it is a living, organic unity. The psychical life is a single undivided whole and itself the real unitary subject. This concrete living self consists in given ideas, feelings and volitions and the activity by which these functions are conditioned; the whole, however, is unified by a factor which in itself is the abstract ego and from the empirical point of view one side of that psychophysical substance in which Wundt finds the substrate and basis of the soul's unity.

Wundt's reply to this argumentation in the current number of the 'Philosophische Studien' (XII., 37 ff.) is to the effect that his critic has not sufficiently grasped the distinction between physical science and psychology, according to which the latter is science of experience as immediately given, whereas the standpoint of the former requires it to deal with objects constructed by thought. Consequently a physical hypothesis is tested by its utility, a psychological by fact, and the fact is that no other unity is found or required in the psychical life except that which exists in the connection of its processes. This is singularly unsympathetic and avoids the real issue. The real question is, Is there discoverable, whether by direct inspection or by reflection, in the movement of our subjective experience, any constant factor? Is the psychical life like a stream which simply flows on or is it a process of self-development? Sameness without change is asserted by nobody; change with the sense of sameness is a fact. Is the sameness predicated really there? That is the real question, as James puts it. Theories of 'actuality' and 'substantiality' are altogether subordinate, mere names. And the question is not to be set aside by the arbitrary distinction of hypothesis of fact and hypothesis of utility nor referred for answer to such irrelevant illustrations as Kant's elastic balls, which, if they were conscious, would be obliged to suppose, as we are, that they themselves were the subjects of experiences referred to the past but appropriated by the present self, whose identity with the past self would be, if illusion, then a necessary illusion.

H. N. GARDINER.

Le Moi des Mourants. V. EGGER. Revue Philosophique, XLI., 26–38. Jan. 1896.

Many persons who have survived an accident that seemed to be fatal report that at the time their whole past life came up before them. This experience, which is not, however, to be taken literally, M. Egger is disposed to connect, not with pathological exaltations of memory in epileptics, etc., but with quite normal phenomena. Noticing that children apparently do not have the experience, he refers to the aggregation of memories with which the ego is continually being constituted from youth to age, and which is particularly marked in the aged, and the fact that the civilized adult about to die and capable of reflection normally realizes his personality in a form vivid and significant. But with regard to their experiences we want more evidence. The author therefore suggests a systematic enquiry among persons who have faced what seemed to them certain death.

Arrested Mentation. G. FERRERO. The Monist, Vol. 6, 60–75. October, 1895.

It is a natural law of 'unconscious' reasoning, that is, reasoning not consciously guided by the ideals of strict logic (Aristotle and Mill), that it stops short in its explanation of phenomena with what is revealed directly to the senses and neglects factors which can only be discovered by reflection and comparison. This species of 'arrested mentation' explains various popular errors and suggests the possibility of a new science, positive logic, the study of the laws of human reason according to age, intellectual development and the state of civilization. Another more radical species involves the abolition of all observation. This is *a priori* reasoning. Mentation is here arrested because the deductive method merely draws a conclusion from a premise and consequently involves far fewer mental elements and less effort than the inductive.

In his grasp of the facts and the psychology of the so-called deductive method, historically considered, as well as in his general appreciation of logical process, the author seems to furnish a good illustration of his subject.

SMITH COLLEGE. H. N. GARDINER.

THE PSYCHOLOGY OF RHETORIC.

Figures of Rhetoric: A Psychological Study. GERTRUDE BUCK. No. I. of Contributions to Rhetorical Theory. Edited by F. N. SCOTT. University of Michigan. 1895.

This monograph endeavors to state in psychological terms the process by which rhetorical figures come into being. While a concept is seen to be the verification of two or more percepts, which verification, when complete, is expressed in language by a name, there are obviously sentences that express implicit rather than explicit relationships. These sentences, representing an incomplete verification, are 'figurative.' 'Radical' figures occur when the verifying relation between the two objects has not yet been constructed in the mind of the speaker or writer; 'poetical' figures when the relation is at last partially explicit in his consciousness. The ordinary classifications of rhetorical figures could be simplified by making merely two groups; those in which stress is laid respectively upon the stages of verification and of discrimination. This theory that a figure of speech is an organized complex of mental activities whose implicit verifying principle comes to be more or less explicit in the mind of the reader or hearer, is then applied to the problem of the nature of the pleasurable effect arising from the use of figurative language, and to the question of humor, the writer contending that the perception of the ludicrous and the recognition of a figure of speech are processes essentially one. A carefully compiled bibliography of figures is appended.

The Æsthetics of Words. L. A. SHERMAN. *The Northwestern Journal of Education.* Sept., Oct., Nov., Dec., 1895.

Professor Sherman's series of articles is thus far devoted to the exposition, largely by graphical methods, of the varying 'values' of words for artistic purposes. Words are signs of emotion as well as signs of ideas, and their emotional meaning cannot be told by logical definition. As a preparation for the study of poetry, pupils should be taught to distinguish between conceptual words, which occupy the mind more with knowing than with feeling, and experiential words, in which knowing is merged in feeling. These emotional and suggestive words, depending for their power upon reminiscences or sometimes upon anticipated experiences, stand thus in the closest relation to things. The æsthetics of things depends on the types or ideals of ultimate truth or beauty which they evolve or evince. Ideas, and words as the signs of ideas, satisfy us in proportion to their power to fulfil our types. Those words only should be called 'poetic' which stand for things absolute in their truth or beauty. Professor Sherman's theory of types will not be acceptable to all, but readers of his *Analytics of Literature* will welcome the interesting and acute discussion contained in these articles. BLISS PERRY.

PRINCETON.

NEW BOOKS.

Child and Childhood in Folk Thought. ALEXANDER FRANCIS CHAMBERLAIN. New York and London, Macmillan & Co. 1896. Pp. x+464. $3.00.

The Number Concept—Its Origin and Development. LEVI LEONARD CONANT. New York and London, Macmillan & Co. 1896. Pp. vi+218. $2.00.

La théorie platonicienne des sciences. ÉLIE HALÉVY. Paris, Alcan. Pp. xl+378.

Die Spiele der Thiere. KARL GROOS. Jena, Gustav Fischer. 1896. Pp. ix+359. M. 6.

Manueli di Semijotica delle Malattie mentali. E. MORSELLI. Vol. II. Esame psicologico degli alienati. With 77 illustrations and 13 tables. Milan, Vallardi. 1895. Pp. xviii+852. L. 15.

I Sogni e il Sonno nell' Isterismo e nella Epilessia. S. DE SANCTIS. Rome, Società Editrice Dante Alighieri. 1896. Pp. 216. L. 2.

The Theory of Social Forces. S. N. PATTEN. Supp. to Annals of the Amer. Acad. of Polit. and Social Science. Philadelphia, Amer. Acad. 1896. Pp. 151.

Evolution in Art. A. C. HADDON. London, Walter Scott; New York, Scribners. 1895. Pp. xviii+364. $1.25.

Studies in Childhood. JAMES SULLY. New York, D. Appleton & Co. 1896. Pp. viii+527.

Die Hauptpunkte der Hum'schen Erkenntnislehre. ERNST PETZHOLTZ. Berlin, Gustav Schade. 1895. Pp. 44.

Die Erkenntnisstheorien bei Leibniz und Kant. LOTHAR VOLZ. Rostock, Universitäts-Buchdruckerei. 1895. Pp. 70.

Movement. E. J. MAREY. New York, D. Appleton & Co. 1895. Pp. xv+322. $1.75.

Die Lehre von den spezifischen Sinnesenergien. R. WEINMANN. Hamburg and Leipzig, Voss. 1895. Pp. 96. M. 2.50.

NOTES.

THE THIRD INTERNATIONAL CONGRESS OF PSYCHOLOGY.

The opening of the Congress will take place on the morning of August 4th, 1896, in the great 'Aula' of the Royal University.

All psychologists and all educated persons who desire to further the progress of psychology and to foster personal relations among the students of psychology in different nations are invited to take part in the meetings of the Congress. Women will have the same rights as men.

Those who propose (1) to offer papers or addresses or (2) generally to take part in the Congress are requested to fill up the accompanying forms and to send them to the Secretary before the beginning of the Congress.

The subscription to be paid by those desiring to take part in the Congress is 15 M. On receipt of this sum a card will be sent to every member entitling him to attend all the meetings and to receive the Journal, *Tageblatt*, issued daily (with a register of the members) and one copy of the Report of the Congress. The card also admits to all festivities arranged in connection with the Congress and all special privileges granted to its members.

The *Tageblatt*, which will appear in four numbers, will serve to register the guests and contain information as to accommodation, the programme of the papers and addresses and social arrangements, the list of members and a short notice of the places of interest in Munich.

The languages used at the Congress may be German, French, English and Italian.

The Congress will perform its work in general and sectional meetings. The division of the sections will be arranged according to the papers and addresses which may be offered. The meetings take place at the Royal University.

The length of the papers or addresses of the sectional meetings is limited to 20 minutes. It is hoped that any member who takes part in the discussion will, to insure a correct report of his speech, give the chief points of it (on a form which will be provided) either during or at the close of the meeting.

Any psychologist who offers a paper or address is requested to send to the Secretary before the beginning of the Congress a short written abstract of its contents. These abstracts will be printed and distributed amongst the audience, so that the different languages used at the Congress may be better understood.

Those members of the local committee who are mentioned in the programme below will all give information as to their respective departments of work and also in connection with the inspection of scien tific institutes and demonstrations.

The Congress will meet in Sections as follows:

I. Psychophysiology.—Prof. Rüdinger, Prof. Graetz, Privatdocent Dr. Cremer will give all information concerning this part of the programme.

a. Anatomy and Physiology of the brain and of the sense-organs (somatic basis of psychical life).

Development of nerve-centres; theory of localization and of neurons, paths of association and structure of the brain.

Psychical functions of the central parts; reflexes, automatism, innervation, specific energies.

b. Psychophysics. Connection between physical and psychical processes; psychophysical methods; the law of Fechner. Physiology of the senses (muscular and cutaneous sensibility, audition, light-perception, audition colorée); psychical effects of certain agents (medicines). Reaction-times. Measurement of vegetative reactions (inspiration, pulse, muscle-fatigue).

II. Psychology of the Normal Individual.—Prof. Lipps, Privatdocent Dr. Cornelius, Dr. Weinmann will give all information concerning this part of the programme.

Scope, methods and resources of Psychology. Observation and experiment. Psychology of sensations. Sensation and idea, memory and reproduction. Laws of association, fusion of ideas. Consciousness and unconsciousness, attention, habit, expectation, exercise. Perception of space (by sight, by touch, by the other senses); consciousness of depth-dimension, optical illusions. Perception of time.

Theory of Knowledge. Imagination. Theory of feeling. Feeling and sensation. Sensuous, æsthetic, ethical and logical feeling. Emotions. Laws of feeling.—Theory of will. Feeling of willing and voluntary action. Expressive moments. Facts of ethics.—Self-consciousness. Development of personality. Individual differences.

Hypnotism, theory of suggestion, normal sleep, dreams.—Psychical automatism.—Suggestion in relation to pædagogics and criminality; pædagogical psychology.

III. Psychopathology.—Prof. Dr. Grashey, Dr. Frhr. v. Schrenck-Notzing, Edm. Parish will give all information on this part of the programme.

Heredity in Psychopathology; Statistics.—Can acquired qualities be transferred by inheritance?—Psychical relations (somatic and psychic heredity), phenomena of degeneration, psychopathic inferiority (insane temperament).—Genius and degeneration; moral and social importance of heredity.

Psychology in relation to criminality and jurisprudence.

Psychopathology of the sexual sensations.

Functional nerve disease (hysteria and epilepsy).

Alternating consciousness; psychical infection; the pathological side of hypnotism; pathological states of sleep.

Psychotherapy and suggestive treatment.

Cognate phenomena; mental suggestion, telepathy, transposition of senses; international statistics of hallucinations.

Hallucinations and illusions; imperative ideas, aphasia and similar pathological phenomena.

IV. Comparative Psychology.—Prof. Dr. Ranke, Dr. G. Hirth, Dr. Fogt will give all information in this department.

Moral statistics.

The psychical life of the child.

The psychical functions of animals.

Ethnographical and anthropological psychology.

Comparative psychology of languages; graphology.

Prof. Dr. Lipps, Georgenstrasse $18/_1$, is Committee of Reception, and Dr. Frhr. von Schrenck-Notzing, prakt. Arzt, Max Josephstr. $2/_1$, is General Secretary.

The International Committee of Organization is as follows;

President: Prof. Dr. Stumpf, member of the "Akademie der Wissenschaften," Berlin W., Nürnbergerstrasse 14; Vice-President: Prof. Dr. Lipps, München, Georgenstrasse $18/_1$. General Secretary: Dr. Frhr. von Schrenck-Notzing, prakt. Arzt, München, Max-Josephstrasse $2/_1$. Members of the committee: Prof. Bain, Aberdeen, Scotland. Prof. Baldwin, Princeton University, New Jersey, U. S. A. Prof. Bernheim, Nancy, hôpital civil, France. Prof. Delboeuf, Brussels, Belgium. Prof. H. H. Donaldson, Chicago, Ill., U. S. A. Prof. Ebbinghaus, Breslau, Germany. Prof. Ferrier, Cavendish Square, 34, London W., England. Prof. G. S. Fullerton, 116 Spruce street, Philadelphia, Pa., U. S. A. Prof. Stanley Hall, Clark University, Worcester, Mass., U. S. A. Prof. Hitzig, Halle, Germany. Prof. James, Cambridge, Mass., 95 Irving street, U. S. A. Prof. Lehmann, Kopenhagen, Hagelsgade 7, Denmark. Prof. Liégeois, Nancy, France. Prof. Lightner Witmer, University of Pennsylvania, Philadelphia, Pa., U. S. A. Prof. Mendelssohn, Petersburg, Möika 81, Russia. Prof. von Monakow, Zürich, Stadelhoferstr 10, Switzerland. Prof. Morselli, Genova, via Assarotti 46, Italia. Mr. F. W. H. Myers, Deckhampton House, Cambridge, England. Dr. Newbold, University of Pennsylvania, Philadelphia, Pa., U. S. A. Prof. Preyer, Villa Panorama, Wiesbaden, Germany. Prof. Richet, rue de l'Université 15, Paris, France. Prof. Schäfer, University College, Gower street, W. C. London, England. Prof. Sidgwick, Newnham College, Cambridge, England. Prof. Sully, Hampstead, N. W., East Heath Road, London, England. Dr. Ward, Selwyn Gardens, Cambridge, England.

THE *Année Psychologique* for 1895 will contain, in addition to analyses of psychological literature, and the Bibliography for 1895, prepared by Dr. Farrand and Prof. Warren for this REVIEW, the following original articles:

1. Ribot, Les caractères anormaux et morbides. 2. Binet et Courtier, Etude des vasomoteurs dans leur rapport avec l'ètat intellectuel, des émotions, etc. 3. Bourdon, Expériences sur les associations d'idées. 4. Flournoy, Temps de lecture et d'oubli. 5. Biervliet, Illusions de poids. 6. Forel, L'instinct des fourmis. 7. Nilliez, La mémoire des chiffres. 8. Henri, La localisation des sensations du toucher. 9. Binet, La peur chez les enfants. 10. Binet et Courtier, Recherches graphiques sur la musique. 11. Binet et Courtier, Appareils nouveaux pour la méthode graphique. 12. Passy, Revue générale sur les sensations olfactives. 13. Henri, Revue générale sur l'erreur probable. 14. Henri, Revue générale sur la mesure de la sensibilité tactile. 15. Binet et Henri, Revue générale sur la psychologie individuelle. 16. Azoulay, Revue générale sur les conclusions psychologique des derniers travaux sur la structure du système nerveux. 17. Binet, Revue génèrale sur les expériences de plethysmographie.

DR. JAMES WARD, of Cambridge, England, writes a private note to one of the editors (apropos of a reference to him in a book review), which has a certain historical interest as well as the purely personal one. He says: "In your excellent Review (Nov., 1895, p. 608) I am charged with 'virulence' and 'acridity' in criticizing the 'new psychology.' The words seem to me to be unfair and ill-chosen. It is odd that I who did my level best to get a psychophysical laboratory started here before there was a single such laboratory in existence—unless Wundt's then private laboratory is to count—should be counted the enemy of pyschophysics. The very first thing I ever wrote was a monograph on the Relation of Physiology to Psychology, and before 1880 I had spent two years in physiological laboratories. What I object to is psychophysics by men who are not psychologists."

THE psychological department of Cornell University has moved to Morrill Hall, where it is said to have nine rooms and 4,000 square feet of floor space. The Psychological Laboratory of the University of Nebraska has been moved into the first floor of the new library building and occupies a series of five rooms with a floor space of 3,000 square feet. In the new biological buildings which the University of Chicago will erect with a part of the $1,000,000 given by Miss Culver, ample provision will be made for the Psychological Laboratory. In the new Schemerhorn Hall of Natural Sciences to be erected for Columbia University at a cost of about $400,000, more than one-tenth of the building is allotted to psychology.

Dr. C. A. Strong, associate professor of psychology in the University of Chicago, has been elected lecturer on Psychology in Columbia University.

H. C. Warren, M. A., has been appointed assistant professor of Experimental Psychology in Princeton University.

Leopold Voss, Hamburg and Leipzig, has begun the publication of a new *Archiv* called *Kantstudien*, edited by Dr. Hans Vaihinger, of the University of Halle, with the coöperation of E. Adickes, E. Boutroux, Edw. Caird, C. Cantoni, J. E. Creighton, W. Dilthey, B. Erdmann, K. Fischer, M. Heinze, R. Reicke, A. Riehl and W. Windelband. The journal will treat not only Kant's contributions to philosophy, but also the general development of modern philosophy in its relations to Kant. Contributions (which may be in English) are invited by the editor.

A new Russian journal, a Review of Psychiatry, Neurology and Experimental Psychology, edited by Dr. Bekhteret, will hereafter be published monthly.

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THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

I. REACTION-TIME: A STUDY IN ATTENTION AND HABIT.

BY JAMES ROWLAND ANGELL AND ADDISON W. MOORE.

ASSISTED BY J. J. JEGI.

It is not without grounds that experimentation upon reaction-time has been called the *Lieblingsgegenstand* of experimental psychology. The facts appear so simple and the interpretation so illusive that ingenuity has seemed piqued anew each time the matter has been opened. The fact has had its interest recently augmented by Professor James Mark Baldwin's challenge of the results and explanations which have hitherto passed current stamped with the mark of the justly revered Leipzig school. It must often have occurred to many readers following the Leipzig explanation of the alleged fact that the time of the so-called 'motor form' of reaction is faster than the 'sensory' to ask, why a brain-reflex should be established in the former case and not in the latter, or why, if established, it should be so much less effective in reducing the time consumed by the reaction; why occasionally, despite the assertion that such persons did not possess the necessary *Anlage* and so could not be regarded, some persons proved unable to make any distinction whatever in the forms or even showed faster time in the sensory attitude. The fact that these and other questions were left open by the Wundtian explanations appeared to leave room for further investigations.

We set out with the general conception that from the evidence already in hand it was to be anticipated that each individual mind would, from influences already surrounding its growth, show itself possessed of certain coördinations which were customarily employed in the everyday business of life, and that these coördinations would afford pathways peculiarly pervious to rapid nervous discharges, *i. e.*, they would form paths of least resistance; whereas certain other coördinations would be either wholly lacking or much less practiced and much more difficult of employment, yielding when actually pressed into service much slower results. Working under this general conception we had reached results in our experimentation very similar to those of Professor Baldwin, and we were just ready to publish when his very notable article upon the subject appeared in this REVIEW of May, '95, showing essentially the same results as we had reached. Not only had Professor Baldwin anticipated our results up to that time; he had also anticipated almost completely our mode of procedure. This full acknowledgment of his priority is due him on every score. Although our time results have continued to confirm those of Professor Baldwin, yet as the investigation proceeded a standpoint of interpretation emerged, differing in some essential respects as much from Professor Baldwin's as from that of the Wundtian school. On the other hand, the interpretation here given on the basis of the interrelation of habit and attention seems to us to combine and reconcile some of the principal contentions of both sides of the 'type' discussion.[1] The explanation we have attempted is 'dynamo-genetic' rather than static as most interpretations appear to us very largely to have been.

The experiments were begun in March, 1895, and have continued, with a two months' summer intermission, up to the present time. While some differentiations have been made, both in the stimulus and the mode of response, the results of these are submitted not so much to establish the characteristics of these differentiations themselves as to furnish cumulative evidence of

[1] The entire discussion referred to includes an article by Prof. Titchener, of Cornell University, on 'Simple Reactions,' *Mind*, N. S., IV., 74–81, Prof. Baldwin's report in this REVIEW, May, 1895, a criticism of the report by Prof. Titchener in *Mind*, IV., 506–514, and Prof. Baldwin's rejoinder, *Mind*, January, 1896.

the general nature of the reaction, and perhaps point the way for more detailed research in the future.

The reagents of this series were the persons whose names appear at the head of the report. J., as indicated by the smaller number of his reactions in some of the series, came into the work recently; A. had taken part in no reactions for several years; M. and J. were entirely unpracticed.

The time was taken with the Hipp chronoscope. The clock was tested at each hour's work, and frequently twice during the hour, by a falling screen whose time was taken from a 1,000 V. König fork. The variable error of the machine for the whole series was .0004 sec.

We may hope to escape the recriminations generally hurled at all users of the Hipp chronoscope, inasmuch as the significance of the figures rests but little upon their time values. As will appear, the essential question is whether certain groups of reaction-time approach or recede from each other. As a matter of fact, however, the accuracy of our instrument permits much more stringent conclusions. But this is all we are immediately concerned in.

The experiments include responses by the hand, foot, and lips, to auditory and visual stimulations in both 'sensory' and 'motor' forms. In the auditory series a large number of the hand and foot reactions were further differentiated into those made in the light and those taken in the dark. The auditory stimulation was given in most of the series through a telephone. The visual stimulation was the movement, from a stationary position, of a black screen with a white center.

The hand responses were given by pressing downward with the first finger of the right hand. The foot reactions were made by downward pressure of the toe of the right foot, the foot being supported under the instep to prevent fatigue and complicating strain. The lip responses were given with a special key, the reaction being made by parting the lips.

Most of the visual series were not begun until after the auditory series were completed, and in the latter most of the lip and foot reactions were taken after the hand series had been finished. As the modes of response were the same in both the auditory and the visual series the reactions in the latter had the benefit of the practice secured in the former. This observation is of importance in considerations where the effects of practice are to be taken into account. It also accounts, in some measure, for the time of the visual series being faster than is usually reported. Indeed, if the mean variation had not in many of these cases remained so small, suspicion must surely have arisen as to whether the conditions desired were really being attained, but with small variations (6 to 15%) and clear distinctions in the sensory and motor forms, the figures appear trustworthy. Since the hand responses to sound were taken first, far outnumber the others, and were distributed over a much longer period of time, they are of most value in showing the development under practice.

While the reactions were not all taken at the same hour of day, each period of work was divided between the sensory and motor forms. So that, throughout the course as a whole, each sensory reaction is balanced by a motor under parallel conditions. The number of reactions taken under each mode of response, hand, foot and lips, was about equally distributed between the sensory and motor series. Beside the differentiations above mentioned, several minor ones, such as alterations in the intensity and location of the stimuli, changes in position of body, response with the left instead of the right hand, etc., were made in the course of the work. These variations, to which reference will be made later, simply served to emphasize the importance of habit as a factor in attention.

Effects of Light and Darkness.

Most of the reactions in the light were taken with the eyes resting upon the responding organ, save of course in the case of reactions to visual stimulus. In its external aspects this form of reaction corresponds to what Prof. Baldwin calls the 'visual motor,' as distinguished from the 'kinæsthetic motor' reaction. In the latter attention is focussed upon the thought of the movement, the responding organ not being seen. He further says:

[1] Psychol. Rev. II., pp. 261 ff.

"In subjects of the motor type the 'kinæsthetic motor' is shorter—the 'visual motor' time approximating the sensory reaction time." Taking Prof. Baldwin's definition of 'kinæsthetic motor' in terms of attention, as that form in which the attention is occupied with '*the thought of the movement*,' we found the same results for it, viz.: that in reagents of the 'motor type' it was the fastest form. But the external conditions of this location of attention we found just the reverse of those stated by Prof. Baldwin. Both A. and J. found that the attention could be more completely centered in the reacting organ when the latter was visible. When blindfolded or in a dark room, there was a tendency toward the sensory form; and the time of the reaction was between that of the motor in the light, and the sensory. A.'s and J.'s sensory form was also retarded in the dark. M.'s sensory form was faster in the dark. But the motor was slower and tended very strongly to pass over into the sensory.

Effects of Practice.

At the outset of the course, it was found in the hand reactions to sound that A. and J. returned the faster time for the motor reactions, and M. for the sensory. The first attempts to react in both ways showed in all the reagents—especially in the cases of M. and J., who were entirely unpracticed, very little difference, save an occasional big jump or total failure to react. This taken with the testimony of introspection showed that most of the reactions still came in the habitual way, the other form of reaction not yet having emerged. Then, as the new form began vaguely to define itself, there arose a large time difference between the two series, and a large mean variation in the series of the new form. At this stage, in the attempts at the new coördination, in spite of the reagent's best effort, attention frequently jumped back into the habitual form. With further practice, however, the confusion began to disappear, the new form coming out clear, with its time and mean variation diminishing. Meanwhile the time of the old form also kept diminishing, but did not make such rapid progress in reduction as did the new one. This continued until, at the close of some of the series, the difference between the two forms was inside the mean variation for each series with perhaps a slight final balance in favor of the motor as the faster time. Thus both A. and M., who had the longer practice in the auditory-hand series, and who started in the one motor, the other sensory, came out at the same point, relative to the two series, *i. e.*, both were a little faster in the motor form. J., who came into the work recently, does not show this outcome so clearly, though in some of his series the approach of the two times is clearly marked. Meanwhile the decreasing time and continued approximation of the times of the two series were accompanied in each series by an ever increasing degree of reflexness. At this stage any extraordinary effort to concentrate on either form resulted in a confused and lengthened reaction.

To sum up the steps in the development, we have (1) different habitual forms of attention at the outset; (2) a period of confusion and wide time difference in evolving the new form; (3) a subsequent reduction of absolute time and mean variations in both forms; (4) an approximation of the time values of the two forms; with (5) a final possibly shorter time for the motor form. In short, to generalize these steps, the conclusion, to which the whole series points, is that continued practice in the two modes of coördination with a constant stimulus, under constant conditions, results in two highly reflexive forms, not of widely different, but of about equal times values. This result receives further confirmation from Professor Baldwin's report of his own case[1] in which he says his reactions "have only changed in that the distinction between the sensory and motor time is less marked than it used to be, and this I explain as probably due to habit and practice, as my theory again seems at least not to contradict."

On the whole, the outcome seems to agree with some of the results on both sides of the 'type' discussion. It indicates with Professor Baldwin's results that "the ground of origin of types is to be found in education, which must necessarily apply to *single functions;*" that, as so defined, in the sensory 'type' the sensory form of the reaction may be shorter than the motor even after the latter has clearly emerged in consciousness. That

[1] *Mind*, January, '96, pp. 85 ff.

continued practice does not tend to widen the time difference at first manifest between the two forms, but on the contrary. On the other hand, it finds that when *both* forms have reached a high degree of reflexness, the motor form is probably somewhat faster, though not to the extent reported in the Leipzig results.

The following table shows the effects of practice in the time differences between the first and last thirds of each sensory and motor series. In all, except the hand series of A. and M., the

TABLE SHOWING THE RESULTS OF PRACTICE.

Sen. Organ.	Mot. Organ.	Focus of Atten.	A No.	A Time in σ. First Third.	A Time in σ. Last Third.	M No.	M Time in σ. First Third.	M Time in σ. Last Third.	J No.	J Time in σ. First Third.	J Time in σ. Last Third.
Ear.	Hand	Sen.	560	195	133	420	163	132	160	185	173
		Mot.	540	149	127	380	178	134	165	169	159
	Foot	Sen.	145	182	168	220	138	133	125	218	208
		Mot.	155	159	150	230	145	134	115	204	196
	Lips	Sen.	130	132	122	125	117	108	160	169	155
		Mot.	120	125	116	125	112	106	140	157	146
Eye.	Hand	Sen.	100	206	173	100	153	125	100	180	173
		Mot.	100	193	150	100	176	130	100	193	165
	Foot	Sen.	100	218	170	110	160	153	125	229	183
		Mot.	100	170	151	115	153	148	125	199	175
	Lips	Sen.	130	141	135	125	144	138	100	193	179
		Mot.	120	133	127	125	136	133	100	166	165

number of reactions is very inadequate to show anything like the full effects of practice. But even in the shorter series the drift is clearly indicated. In addition to the general results already pointed out, it further appears from the table, that for all three reagents, the ear-lip coördination is the fastest; also that there is less difference between the sensory and motor reactions at the outset of this series than at the beginning of the others. For A. and J., the reactions at the beginning of the eye-foot series, are the longest on the record, and show also the widest difference between the sensory and motor forms. A. shows at the beginning of all the series the faster time in the motor form; M.'s reactions are quite divided between the two forms showing the faster time in the sensory form in both hand series, and in the auditory-foot series, but is faster in the motor form for both lip series, and for the eye-foot series. J. is motor throughout except in the eye-hand series. A series of auditory and visual memory tests with nonsense syllables showed A. and J. quicker in the visual, and M. slightly faster in the auditory form.

INTERPRETATION.[1]

Taking the simple reaction as the type of voluntary action in general, and voluntary action as action under the direction of attention, it seemed that the key to any explanation adequate to all the facts, the individual peculiarities and the effects of practice, must be found in the functions of attention and habit in their relations to each other.

Not to go into too great detail, the process of attention in its essential outlines in, say, the auditory-hand reaction, appears something as follows: As the reagent receives his instructions for the reaction, he formulates in imagination what he is going to do. This formulation, the getting in mind what he is to do, is his attention to the act. Whatever may be the detail of imagery involved in this formulation, it involves primarily the coördination of two groups of incoming sensations, one from the ear, the other from the hand, started by the operator's descriptions. From this, two distinctions may be drawn: (1) As related to the act of attention, these two sensation groups are its stimuli; and each group is as much stimulus as the other —the sensations from the hand as much as those from the ear. The 'reaction' as meaning the whole act to be performed is not the mere response of the hand to the ear, but the act of attention in coördinating the incoming stimuli from *both* the hand and the ear. Concerning the 'sensory-motor' distinction it follows that, since the stimulus, *i. e.*, the material for the act, lies in

[1] Under this head we are indebted to Professors Dewey and G. H. Mead, for suggestions without which the following interpretation would not have been reached.

these incoming currents from both hand and ear, as related to the whole act, both 'forms' may be regarded as equally 'sensory' or equally 'motor.' (2) In relation to each other *inside* the act of attention, most discussions of the subject appear to make the ear process merely a stimulus to which the hand adjustment is merely a response. But the question arises, What holds the ear to its work? Why does the reagent maintain his listening attitude? It may be replied that it is 'because he is told to.' But he is not told to listen any more than he is told to move his hand. If the telling suffices in one case it should in the other. Moreover, he is not *merely* to listen, or even to listen just for the click, but to listen for the click *as a pressure signal.* It is this character of the click as a signal for pressure that keeps up the interest in it and the attention to it. (We are assuming here, of course, a case of sensory attention.) The hand therefore is stimulus as well as response to the ear, and the latter is response as well as stimulus to the hand. Each is both stimulus and response to the other. The distinction of stimulus and response is therefore not one of *content*, the stimulus being identified with the ear, the response with the hand, but one of *function*, and both offices belong equally to each organ. The reason the movement of the hand is so often treated as the *mere* response to the ear as its *mere* stimulus appears to be that the whole act, or 'reaction,' is identified with the movement of the hand. But the entire act is the act of attention in coördinating the two groups of stimuli coming from both hand and ear. To be sure, *in* the act of coördination there is, as we have seen, the interaction of the two elements as stimulus and response each to the other. But it must be kept in mind that this latter is a distinction falling *inside* the act, not *between* the hand movement considered as the act, and the sound considered as its external stimulus or 'cause.' In a word, the reagent reacts as much with his ear as he does with his hand.

With the reaction now interpreted as essentially constituted by the *act of coördinating* the ear-hand activities; with the distinction of 'stimulus' and 'response' interpreted as wholly functional, falling inside the act, the question still remains,—why, in the act of coördination, is attention occupied more immediately with one of these processes than with the other? This question, again, does not ask whether the attention shall be given to the sound *or* to the movement *as such*, but where in the total ear-hand process the focus of attention shall fall. This point, wherever it is, must be determined not by the solicitation of the point in itself considered, but by the demands of the *whole* act of coördination. Whether the attention be 'in the hand' or 'in the ear,' it is 'there' in order to bring off successfully the ear-hand adjustment.

But why is it 'in' one or the other? This leads to the consideration of another function or rather another phase of *the* function of attention, namely, its function as the adjustor, the mediator, of the tension between habitually established coördinations and new conditions under which they have to express themselves. An habitual process, such, *e. g.*, as walking, comes into consciousness as, *i. e.*, under attention, only when some new set of conditions, some obstacle, arises, adjustment to which lies outside the scope of the habit. Then only so much of the process comes into consciousness as needs readjustment to the new conditions. Habit is still left to do all it can, and in every voluntary act there is always something left for it to do. No matter with how minute a portion of a process attention may be occupied, it always will be found giving direction to a *group*, no matter how small, of already coördinated activities. Any attempt, therefore, to leave habit out of the account in voluntary action makes such action impossible. It would be affirming a process of adjustment with nothing to adjust. If attention, *as such*, then, is the process of mediating the tension between habit and new conditions, its focus must be where this tension is strongest, *i. e.*, where habit is least able to cope with the situation. The position of this point will depend upon the extent to which the different parts of the whole ear-hand process can be left to habit. If the ear element of the process, that is, the sound, be unfamiliar and the movement of the hand be familiar, the point of tension will fall 'in the ear' and *vice versa*. With the sound and movement both familiar or both unfamiliar, the balance between them will be determined by education, inherited structure, etc. Let it be noted again, that in leaving one phase more than the other to habit, the former is not left out of the act, nor out of the process of attention. In attention to the sound, the movement of the hand is present in the character of the sound as a pressure signal; and in attention to the movement, the sound is present in the very fact of its being a movement in response to the sound.

Concerning the process of 'shifting' the attention and the accompanying time variations it follows from the very nature of attention that it is only from an external point of view, the point of view of an observer, that we can speak of 'shifting' the attention in the *same* act. For the reagent a 'shift' of attention is a change in his act; it means a different process of coördination. For him, therefore, the sensory and motor forms of attention are not 'two forms' of attention for the *same* act; they are essentially two different acts; and the time question for him is, which of the two acts is the shorter. Regarding the act, however, in an objective way, we should expect the time of the reaction to be shortest when the attention is upon that part of the process which is least habitual, or in which habit encounters the most new conditions. When one who reacts spontaneously to sound in the sensory way attempts to transfer his attention to the hand, two things are involved: (1) leaving the ear adjustment for habit to take care of; (2) a breaking up, in attending to the hand process, of an already efficiently established coördination. This means that for the performance of the 'act,' regarded objectively, unnecessary work is being done. The focus of the attention upon the more habitual phase of the process means its resolution into elements. Now the moment these elements are called out as unit groups, they bring with them their own train of associated groups, all of which have to be inhibited. This increased and, from the objective view of the act, unnecessary complication, means, of course, an increase in time and mean variation, and accounts for the exceedingly 'artificial' feeling that accompanies the effort.[1]

[1] Here it may be remarked that if the statement of attention as the act of coördinating activities more or less habitual be correct, Professor Baldwin is entitled to say not only that focusing attention upon the more habitual phase of the act 'may' but that it *must* retard the act.

Indeed, whether the attempt succeeds at all, depends upon the extent to which the ear adjustment *can* be left to habit. If the sound be very strange the reagent finds he cannot attend to the hand until, as he says he 'gets used' to the sound, and this 'getting used' to the sound is, of course, the ear adjustment becoming habitual.

This explains why at the outset of the series M.'s reactions were all made in the sensory form. As the ear phase, however, grows more and more reflex the breaking up of the hand process becomes possible and the motor form of attention emerges. A precisely parallel process takes place in the development of the sensory form for a reagent spontaneously motor. Under practice the new form continues, of course, to grow more and more reflex, and its time and mean variation steadily decrease. In the case of M., for whom the new form was the motor, its time kept diminishing in the hand series until, at the close, it was a few sigma faster than the sensory. A similar development occurred in J.'s visual hand series. The sensory was faster at the beginning, the motor at the close. In all the series where the motor was faster at the beginning, it still remained so at the close, though not by nearly so large a margin as reported in the Leipzig tables. In saying that continued practice on both these forms rendered them 'more reflex,' we mean that when at the close the reactions were made in the fastest time it was with a much less amount of tension, and consequently less attention, than at first. Any extraordinary attempt, at this stage, to concentrate upon either form resulted in great irregularity and increase of time, for the reason, as already stated, that all there is for attention to do is to break up and reëstablish processes already unified under habit.

But why even the small margin in favor of the motor time at the close of a course of practice on the two forms? The reason usually given is that 'attention to the movement is the beginning of it.' But if the whole act is not the mere hand movement, but the coördination of ear *and* hand, it is difficult to see why the sensory form is not as much the beginning of the act as is the motor. If attention to the hand is the 'beginning of the hand movement' it is no less true that attention to the ear is the beginning of the sound. And this is not mere 'arm chair' psychologizing, for as a matter of fact, at the beginning of the series, nearly all M's premature responses occurred in the sensory form, and at the close of the series about as many occurred in one form as in the other. When the premature response came, it was always preceded by a feeling of great tension in the ear, and succeeded by a corresponding feeling of relief in the ear, and when in this case the true signal came it was frequently lost entirely by the reagent. Again, it is said, that in attention to the hand a much larger portion of the whole pathway of discharge is 'innervated' than in attention to the ear. This seems to assume that only that part of the total pathway is 'innervated' which is represented in the focus of attention. But if 'innervation' is necessary to movement, and the focus of attention is necessary to 'innervation,' how, *e. g.*, does walking go on when the focus of attention is elsewhere? The answer is, that we must interpret the focus of attention not as a point of innervation *merely*, but *as a point of conflicting innervations demanding adjustment*. It is not the need of innervation as such, but of *adjustment* of innervations, that determines the focus. But, even supposing that attention means increased innervation, there appears no reason why, assuming the nervous structure homogeneous, and the amount of innervation the same, its application should be more effective at one point than at another. As to the distribution of the innervation over a larger area, if the amount is the same, it must be correspondingly weakened at each point, and so nothing be gained.

It appears, then, that for an explanation of the fact that at the end of the course of practice, when both forms had became highly reflex, the motor form was little the faster, we must appeal again to the relation between Habit and Attention, still regarding, in the objective way, the sensory and motor reactions as 'two forms' of the 'same' act, this fact of the shorter motor time means, (1) in terms of Attention, that the stimulus presented by the ear adjustment affords less material for the continued exercise of attention than that presented by the hand; (2) in terms of Habit it means that the ear process becomes more rapidly and more completely habitual than that of the hand. It takes but a short time to 'get used' to even the strangest sound. After this the character of the sound is comparatively fixed. It cannot be changed through further adjustment. This appears due largely to the more stable character of the inherited ear mechanism. On the other hand, the phase of the stimulus presented by the hand is not nearly so fixed and stable. Here there is much more opportunity for continued variation, hence more ground for the continued exercise of attention. Applying what has already been said, we should, then, expect that act to be faster in which the focus of attention is upon the less stable phase of the hand element of the coördination rather than when it is more 'artificially' occupied in breaking up the more completely established ear adjustment. In a word the time question is not a case of a 'sensory' *vs.* a 'motor' reaction, but of *a sensori-motor less habitual vs. a sensori-motor more habitual.*

As stated at the outset, this interpretation in terms of Habit and Attention seems to us to combine elements from both sides of the 'type discussion.' On the Princeton side it would say: (1) that the 'type' of attention and its accompanying time are determined by the relation between the individual's stock of coördinations, inherited and acquired, already on hand, and the particular coördination required by the reaction; (2) that the 'sensory form' may still be the faster even after the 'motor form' has clearly emerged in consciousness. On the Leipzig side it would say that under practice, in both forms, upon the same coördination, the sensory phase passes more completely under the control of habit and thus leaves the faster time to the motor form.

II. A Study of Visual and Aural Memory Processes.

BY LOUIS GRANT WHITEHEAD.

Problem.

We have been concerned in the experiments here reported to determine the general validity of the Ebbinghaus-Müller-Schumann method of procedure when applied to the following problems, and to obtain as far as possible answers to the same.

(1) What is the relative quickness of the visual and the aural senses when employed in the memorizing of nonsense syllables constructed like those of the above mentioned authors? (2.) What is the relative power of retention for matter memorized visually compared with that memorized aurally? Or, put otherwise, what is the relative rate of forgetting for material memorized in the two ways? (3.) In what manner is the ease of learning anew matter once memorized—but now partially or wholly forgotten—affected by the fact of its being presented on the second occasion to a different sense from that to which it was originally presented? For example, out of six sets of syllables learned to-day from visual presentations, three will a week from to-day be presented and learned again in visual terms, the other three in auditory terms. Will the mental coördinations constantly occurring between aural and visual presentations of the linguistic type, and generally mediated in these cases by the motor activities incident to enunciation, manifest themselves as complete or not? That is to say, having memorized a certain amount of material from visual presentations, will it require less time a week later to memorize this anew from the auditory form of presentation than it did the first time in visual form; and will it require as little time as does the fresh memorizing of the same matter from visual presentation? What are the results when reverse conditions are employed and the original presentations are made in auditory form?

METHQD.

Very little need be said of the method. It was essentially that now familiar to every one by the work of the authors already cited. Nonsense syllables constituted by a vowel placed between two consonants were arranged in a series, containing in this case from seven to twelve syllables each. These separate syllables were presented at regular intervals, the interval being given by a metronome. Five seconds were allowed to elapse between each presentation of a series; and after the successful memorizing of the series three minutes intervened before the next series commenced. All the usual precautions were taken to prevent the forming of significant syllables, rhyme, assonance, etc. The test of success in the memorizing was the ability to repeat the lists aloud at the rate in which they were given, a feat which many subjects found exceedingly difficult, owing apparently to inability to make enunciatory movements synchronously with the outside standard.

We incline to think that this requirement, which has characterized the work of our predecessors, is unwise. We would insist on the subjects adhering to a fixed rate, but it does not appear that it is imperative to have this given from without, much less to have it identical with the rate of presentation. The rate at which the syllables are most easily learned has not proved to be the easiest rate at which to repeat them, and although separate tests are necessary to give an unequivocal answer to the problem, we certainly found many subjects much hampered by the necessity of speaking at an artificially fixed rate. It is not so much that subjects wish to speak slowly and reflect as they go, as that they object to a ready-made rate which they find a distracting and inhibiting element.

We may at this point properly say a word about the general subject of rate. Ebbinghaus used a rate so rapid as to seem to us objectionable on several accounts. We hit upon our own rate —58 beats per minute—after considerable experiment, as being that most suitable in its avoidance of rush and hurry, on the one hand, and drag and tedium, on the other. From a number of observations made during the progress of tests, however, we began to suspect that the rate which permitted the most rapid and satisfactory memorizing was essentially that of the pulse. Our evidence is insufficient to warrant us in speaking dogmatically, and the matter must be submitted to yet further test. But the indications have at times been quite striking. The rate 58 it will be observed, on which we decided, from considerations based upon the subjective feelings of confusion and tedium, is very near the pulse rate, though a trifle slow for most of our subjects.

The series presented visually were placed on a drum revolving in front of a screen, through a window in which the syllables became successively visible.

The aural presentations were made by reading aloud, the voice being kept as nearly uniform in intensity as possible. Rhythm in the reading was suppressed. The subject invariably supplied his own rhythmic interpretation of the series, and it seemed wiser to let this be the determinant than to introduce arbitrarily any special form, which with the series of different lengths would certainly produce different results, *e. g.*, with iambic meter the lists with an odd number of syllables would come out with a syllable left over. In any event, it was thought best to let the subject follow his native bent, so far as possible. In this respect our procedure is at variance with that of Ebbinghaus and Schumann-Müller. There is much to be said on their side. Though they found the memorizing to be more rapid under their conditions, yet granting this to be true, our method still seems to us more correct. The total time occupied in memorizing each series, as well as the number of repetitions necessary, was carefully noted. The importance of this last matter will be commented on later.

At present, and before proceeding to examine any of the results, let us consider a moment the exact conditions which constituted the test, in order the more intelligently to interpret the report which follows. One of two general conditions was always involved; the subject was either learning or giving at fixed intervals successive groups of letters forming unfamiliar syllables. Under both conditions the disposition of almost every person experimented upon was to turn the presentation immediately over into motor terms, through a subdued mental enunciation of the syllables, which often passed over into actual movements of the lips. Nor has this apparently depended in our subjects upon the prevalent type of mental imagery. The predominant imagery was, perhaps, visual; but nothing very marked showed itself, and in general the mental furniture appeared normal in its mixture of visual, auditory and motor elements. The admixture of motor activities was a trifle more noticeable, with visual than with auditory presentations. But this condition of affairs is not abnormal nor peculiar merely to this experiment. On the contrary, it is the common experience in practically all attempts to memorize verbatim. The tendency crops out with perfect naturalness and is exceedingly hard to inhibit. On the last account we have not tried to prevent it. But the result is that, instead of having a presentation material affording ground for direct comparison of visual and aural factors, we have in the majority of cases a material giving us visual motor factors to compare with auditory motor factors. As a test of the *practical* working peculiarities of the memory for aural as compared with visual elements, this consideration deserves no very great weight after it is once explicitly recognized, because, as noted above, the actual conditions of memorizing linguistic material are exactly the same. It does, however, exhibit a difficulty, which the method does not seem competent to surmount, into making a direct comparison of pure aural and pure visual forms of memory. In calling them forms of pure aural and pure visual memory what is meant is not of course forms which are disconnected from *any* motor connections, for that is absurd, but forms which, during the process of memorizing, are disconnected from any *enunciatory* motor activities, which same activities must of course enter into the test of the successful or unsuccessful attempt to memorize, this always being tried by the effort to repeat the syllables.

Criticisms have not been wanting upon the asserted homogeneity of such material as has been used. Indeed, Ebbinghaus himself dwells upon the matter and admits the justice of the criticism. It has been maintained upon various grounds, which we need not canvass, that nonsense syllables do not, as they ought for the purpose of memory tests, possess exactly equivalent tendencies to set up association processes. In this connection we may say that our observations have substantiated those of the authors heretofore mentioned. We found that certain lists of syllables, quite apart from any assignable reason, have shown themselves much harder to memorize, or much easier, as the case might be, than the great majority of lists. This general question takes on a new importance in connection with this special inquiry, where it comes up in a somewhat different form. Granted that nonsense syllables when carefully selected and arranged do furnish an essentially homogeneous subject-matter when presented to a single sense, it does not at all necessarily follow that the same material when presented to a different sense is homogeneous. For it will at once appear, in so much as one of the well recognized conditions of ready and successful memorizing is found in clear and moderately intense stimulation of the sense organ, that we must show the stimulations to the eye and the ear possess essentially equal intensity and clearness, and that the presentations made to each are of essentially equal subjective duration. Now there is only one condition which can be applied to eye and ear that can be admitted as in any measure equivalent in point of intensity and duration of stimulus. This is found in such a combination of the two as shall *just* furnish each sense with clear and unequivocal perceptions. But obviously the different characteristics of the eye and the ear render widely divergent the stimuli which, objectively measured, are just competent to render a clear perception. Furthermore, under the conditions of the experiment the perception of a clearly enunciated syllable requires less time than is necessary for a clear visual perception. In other words, the ear requires a quicker presentation than the eye to make the conditions equal. But the attempt to reduce very materially the time of the exposure of the visual presentation, in order to make it more nearly equivalent to the auditory presentation, results simply in the introduction of disturbing and fatiguing eye movements and after images.

Here again, however, as was remarked above, these same conditions are substantially those met with in actual every-day experience, and as a test of the mere practical aspect of the problem the method might possibly stand unimpeached. But where absolutely accurate information is sought the situation is as set forth. Any results gained in this way must stand ready for possible overthrow by some more satisfactory method.[1] Our own procedure has given us aural and visual presentations of the character mentioned, *i. e.*, as nearly just clear and readily perceptible as possible. The auditory presentations were given in a clear, crisp form of enunciation. The visual pres-

[1]We believe these difficulties might at least be minimized by using an iris photographic diaphragm to make the visual exposures. This would, however, require a somewhat complicated mechanism.

entations at the same rate were actually in clear sight about .5 of a second. We believe, therefore, that our results are fairly comparable among themselves, yet we believe also that for ideally accurate material it is necessary, if possible, to decide by preliminary experimentation the conditions which secure most nearly subjective equality of duration and intensity for the visual and aural stimuli.

It has been customary in handling the results from memory tests of this general character, to lay the chief emphasis upon the number of repetitions necessary for successful memorizing. There is, over against this, the possibility of employing the time occupied as a criterion. Ebbinghaus has discussed this matter in its purely theoretical and mathematical bearings, and upon the basis of his conclusions retained the number of repetitions as the standard of reference in cases where either the speed of memorizing or the permanency of the impression was considered. The conditions with him, however, as with Müller and Schumann, were sufficiently different from ours to warrant examination. In our tests the subject had been instructed to make no attempt to repeat the series until he felt he could do so successfully. Now as a matter of fact the actual number of attempts to repeat varies widely from time to time and with different subjects. It will be noticed in the tables subjoined that, other things being equal, the greater the number of repetitions necessary to memorize a list, the less is the number required to memorize it anew later on. This agrees with the results of Ebbinghaus. Again, the unsuccessful attempts to repeat a list often results in an apparent confusion which requires several extra repetitions to overcome. Furthermore, a half dozen repetitions with three unsuccessful attempts to repeat, may occupy as much time as eight repetitions with only one attempt at repeating, and that successful. During these attempts to repeat, the subject is of course giving himself both auditory and motor stimulations, which will be helpful or otherwise in somewhat the same degree in which they are accurate. It does not commend itself as wise to place any restrictions upon the subject's attempt to repeat, for this would introduce an artificiality into the method more obnoxious than anything yet touched upon. The subjective certainty of the subject is far from being a trustworthy criterion in any regard, for it not only happens that one is often quite sure he has repeated a list correctly when such is not the case, but the converse also occurs and one finds he can repeat correctly a list about which he felt no certainty at all. In view of this it appears that we must ascribe a certain weight at least to the time involved as well as to the mere number of repetitions. We have kept such time records, and an accurate stop-watch has shown itself very useful. The difficulty of fixing upon a convenient rate table, adequate to express the results in terms having reference at once to the number of repetitions and to the time consumed, is that we have no really reliable index as to how much influence should be ascribed to one factor as compared with the other. In fact, our experience leads us to the opinion that this interrelation between the time and the number of repetitions is one of considerable irregularity. The most desirable procedure which suggests itself as actually feasible is to eliminate all cases from the results where more than one attempt to repeat has occurred. This involves a much larger body of tests, for, of course, the subject ought not to know that such a method is to be pursued. Yet it would at least free the statistics from the ambiguity now under consideration.

We reach then the following conclusion concerning the adequacy of our method for answering the questions put at the beginning of this paper. The nonsense syllable method is competent for the solution of the problems proposed, but only within certain limits and under certain specific restrictions as to conditions, which we have already canvassed. The main objection which holds against it is the apparent impossibility of excluding enunciatory motor activities. We doubt whether any method which submits the eye and the ear to a test upon the peculiarities of the memory processes can hope to avoid the difficulty. It will come up indirectly through association processes, if it does not occur directly as in this case. Even Wolfe's experiments upon tone memory do not escape it entirely. In replying to the last of our three special questions we beg to emphasize that, in accordance with all we have heretofore said, we regard our results gained by this method as provisional. The experiments have extended over about nine months and consist of tests upon thirteen persons, six women and seven men. A considerable number of the experiments we have eliminated as untrustworthy from defects of one kind and another. We give results in terms both of time and repetition.

TABLE I.

WOMEN.					MEN.				
Subjects.	Visual present'n.		Aural present'n.		Subjects.	Visual present'n.		Aural present'n.	
	Av. time in min.	Av. repetit'ns.	Av. time in min.	Av. repetit'ns.		Av. time in min.	Av. repetit'ns.	Av. time in min.	Av. repetit'ns.
1	1.41	7.53	2.388	10.	7	2.153	7.06	1.52	6.06
2	2.435	9.8	3.	12.79	8	2.18	9.8	2.326	11.66
3	2.397	9.15	2.355	9.05	9	2.34	9.56	2.54	11.08
4	2.395	10.75	4.17	16.15	10	2.35	9.64	2.37	8.86
5	4.41	12.	4.42	16.2	11	2.45	11.4	2.477	12.1
6	3.11	14.8	4.338	19.93	12	2.15	11.46	2.54	13.4
					13	6.2	23.1	8.63	32.

The table takes the number of visual repetitions as a standard. Visual repetitions are arranged with reference to this in a progressive series, beginning with the lowest and ending with the highest number both for women and men. A comparison of the sexes is consequently easy. The general averages show that ten persons were visually quicker; two aurally and one doubtful. It must be borne in mind that, although in the large majority of our subjects the average for all the cases shows the visual processes faster, the auditory presentations were in many individual instances more rapidly memorized. For example, it may happen that a whole set containing seven syllables each will be memorized more rapidly from the auditory form of presentation, although the sets containing from nine to twelve syllables are learned more readily from visual presentations. This would seem to indicate that the formulations made by previous investigators of the relation between the length of the presentation and the speed of memorizing must be revised for the separate senses.

TABLE II.[1]

Subjects.	Visual Presentations.				Aural Presentations.			
	Time in min.		Repetitions.		Time in min.		Repetitions.	
	1st pres.	2d pres.	1st pres.	2d pres.	1st pres.	2d pres.	1st pres.	2d pres.
1	1.414	1.10	7.285	5.285	2.171	1.415	10.285	8.
2	2.10	1.40	10.16	6.83	2.43	1.35	13.33	6.5
3	2.288	1.266	10.55	5.66	2.40	1.566	9.77	6.66
4	1.57	1.15	8.	4.75	6.45	2.	23.75	5.5
5	3.512	2.212	11.25	8.	4.15	2.40	12.25	8.25
6	3.268	1.376	15.66	8.16	3.288	1.148	15.66	7.16
7	1.52	1.34	7.6	5.	1.12	1.35	4.8	5.4
8	2.38	1.33	11.5	6.5	2.293	1.14	11.166	5.5
9	2.10	1.35	8.33	6.66	3.	2.25	11.5	8.
10	2.318	1.517	9.428	6.21	2.121	1.492	7.714	6.07
11	2.	2.13	7.8	9.24	2.48	1.48	11.2	6.8
12	2.283	1.39	9.	7.33	3.045	1.50	13.5	8.5
13	7.233	3.116	24.66	11.	8.57	3.39	34.4	11.

Table II. shows by general averages the difference between the first and second presentation of the same series to the same sense. With two possible exceptions, the second presentations are quicker. The larger the number of first presentations, the greater the difference. A comparison of the two senses shows that this is true of each. In two cases only was the gain of one sense sufficient to overcome the lead of the other. Hence the conclusion must be that the relative power of retention, or the relative rate of forgetting is about the same. A mathematically exact statement of the relation would evidently show that individuals varied somewhat. Such a statement is now, however, beyond our purpose.

[1] In cases where the powers of retention were tested an interval of one week intervened between the first and the second presentation.

TABLE III.

Subjects.	Visual-Aural Presentations.				Aural-Visual Presentations.			
	Time in min.		Repetitions.		Time in min.		Repetitions.	
	Visual pres.	Aural pres.	Visual pres.	Aural pres.	Aural pres.	Visual pres.	Aural pres.	Visual pres.
1	1.328	1.357	6.428	7.282	1.45	1.10	8.714	5.142
2	2.50	2.075	13.75	10.	2.112	1.487	10.5	8.
3	2.466	2.233	9.833	8.166	2.15	1.116	9.5	4.833
4	2.518	1.484	11.437	4.937	3.464	1.488	14.237	5.437
5	3.412	2.50	12.187	9.687	4.309	2.49	13.375	8.875
6	2.177	1.562	12.25	10.25	3.13	1.497	14.25	9.
7	1.12	1.12	4.2	4.4	1.15	1.14	4.4	3.8
8	1.51	1.43	8.25	7.75	3.342	1.342	16.25	6.75
9	2.412	3.10	9.3	10.4	3.025	1.555	9.7	6.8
10	2.409	1.572	9.909	6.272	2.472	2.218	8.818	7.545
11	3.398	2.34	11.2	9.25	2.34	2.14	9.25	9.4
12	2.142	2.17	9.5	9.75	2.457	1.365	12.	6.5
13	5.15	2.525	20.75	11.	9.23	4.433	28.	16.

The third table shows by general averages, the difference between a presentation to one sense, and the same presentation repeated later on to another sense. Except for four possible cases, the presentation to a different sense is memorized more quickly. Hence it appears that internal mental coördinations have taken place. The gain measured by repetitions is approximately 26%.

To answer our original questions explicitly and seriatim, we may say: (1) of our thirteen subjects ten showed themselves able to memorize most rapidly from visual presentations and two from auditory, while one gave ambiguous results. This outcome is without much doubt to be correlated with the fact that so much of our memorizing, whether it occurs in the verbatim form, or merely as the assimilation of meaning, is brought about through visual processes. (2) Matter memorized aurally appears to be retained slightly better than that memorized visually. It requires less repetition by 32% to learn anew from visual presentations matter memorized visually a week previous, and less repetition by 40% for aural memorizing of the same kind. The difference is insignificant in view of the total number of cases.[1] It seems to be simply a special case illustrative of the general principle already mentioned that the greater the number of original repetitions the less the number necessary for learning anew.[2] (3) when visual presentations are memorized and then a week later submitted to the ear for learning, we find unmistakable evidence that mental coördination between the visual and auditory processes has occurred in large degree. When the order of procedure is reversed, the same thing holds true. This is shown by the saving in time and repetitions necessary to memorize the series anew. This saving varies with the order of procedure, the greatest saving occurring when the first presentation is made to that sense in which the memorizing proceeds most slowly. This must be remembered when the percentage of gain (26 %) under these conditions is compared with the percentage of gain in the cases where both the first and second presentations are made to the same sense (32 % and 40 %). It is quite possible that with longer practice than our subjects have had these percentages might be altered somewhat, but we believe nevertheless that they indicate in a reliable way the general relations of the processes investigated.

[1] An apparent discrepancy in the figures of the different tables arises from the fact that we have not been able to obtain exactly the same number of tests from all our subjects. Yet for the purposes we had in view it seemed desirable to employ a considerable number of persons. We should much have preferred a stricter uniformity, but were forced to content ourselves with what we could get. We do not believe our results are vitiated by this fact, although it would plainly render certain problems and conclusions impossible.

[2] This is additional substantiation of results attained by Miss Calkins, showing the preponderating influence of frequency over other factors affecting cases of association.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY (V.)

THE ÆSTHETICS OF SIMPLE FORMS. II. THE FUNCTIONS OF THE ELEMENTS.

BY EDGAR PIERCE.

At the same time as the experiments of our first article, those intended to determine the conditions for the preference of the golden section or the symmetrical arrangement, another series was conducted in which the same figure appeared in the vertical position. Here neither symmetry nor the golden section was chosen nor indeed any one proportion, but various associations determined the result. The testimony of the subjects obtained through introspection made it evident, however, that the movable lines were not placed utterly at random. The principle that governed most of the cases was that of stability. If the figure seemed firm it was more or less pleasing. The precise place where the line was placed was very often due to some specific association, as a column or bottle or vase. Now, when the whole figure is in a vertical position it is clear that the various lines of which it is composed bear a very different relation to the eyes and their movements from what they do when in the horizontal position. We have seen that when the figure is horizontal the eyes move horizontally, either connecting the ends of the lines or traveling across them from side to side. In the vertical position, however, the eyes move naturally up and down across the lines or connecting their ends. Now it seemed as if it might throw some light on the function of the eye-movements if, when the objects were in a horizontal position the eyes were made to move as they would for a vertical object. This is not at all difficult to accomplish, for if the subject lie on his side parallel to the floor with his head at the height of the horizontal object and his eyes opposite the center of the figure, the horizontal object will then be in precisely the same relation to his eyes as a vertical figure is when he is sitting in the normal position. It seemed as if it would be very interesting and instructive to compare the results obtained by experiments on the subject while sitting, or in the normal position, with those obtained when he was lying or in the abnormal position. Will the vertical eye-movements with a horizontal object cause the line to be placed as it is in a vertical object or as it is in a horizontal object, or will a compromise between the two be made? With a view of answering these questions and with the hope of throwing light on the more *general question as to the function of the eye-movements in relation to the æsthetic consciousness* the following experiments were undertaken with Messrs. Hart, Hylan, Gehring, Lough, James Pierce and the writer.

The instrument (the same as used in the former series) was so arranged that the lines were opposite and at the height of the eyes of the subject when sitting; a table was at hand on which the subject could lie on his side; it was of such a height that then also the eyes would be opposite the lines and at the same height. First a series of experiments was made in the normal position, the lines appearing first horizontal with the movable line at the subject's left, then vertical the movable line at the top, then at the right, then vertical again the movable line at the bottom. Then the subject was asked to take his place on the table and the same series was repeated. All the minor arrangements were the same as in the former experiments. There were six different combinations of forms used; three with a line 30 cm. long, 1.5 cm. wide in the center, two blue lines 12 cm. distant from the middle line, each 10 cm. long, 0.5 cm. wide. At one side a red line 20 cm. long, 1.5 cm. wide; then three with a central line 5 cm. long, 1.5 cm. wide, all the others being the same as before; the movable parts in both cases were a line 10 cm. long, 1.5 cm. wide, a square with sides 5 cm. long, and a star of 5 cm. diameter; all these forms were red.

In the statement of the results Hn. means that the figure was horizontal and the subject in the normal position, that is sitting in front of the board; Vab. means that the figure was vertical in relation to the floor; but that the subject was in the abnormal position, that is lying on the table in the manner described; thus in relation to the eyes the figure was the same as a real horizontal figure; Vn. means that the figure was vertical in relation to the floor and the subject in the normal position; the figures for the Vn. position are farther divided into the positions of the line at the top of the figure and at the bottom; Hab. is the horizontal position of the figure in relation to the floor, but, as the subject is in the abnormal position its relation to his eyes corresponds to that of the Vn.; that is to say when the movable point is to the left in the figure which is really horizontal it is in the same relation to the eyes of the subject in the abnormal position as the movable part in the real vertical position is when the subject is in the normal position; when the movable part is to the right, it of course follows that the movable part is in the same relation as one at the top of a real vertical figure when he is in the normal position. Thus the Hab. also is divided into the position for the line at the top and bottom in relation to the eyes.[1] The figures are the averages of 12 judgments for the apparent horizontal position and of 6 for the two apparent vertical positions; the figures give the distance of the movable lines from the blue side lines.

L.—Hn, 15.5; Vab, 16.0; Vn top, 10.9; bottom, 9.6: Hab right, 11.2; left, 10.1.

J. P.—Hn, 16.7; Vab, 16.2; Vn, top, 18.2; bottom, 15.5; Hab right, 17.1: left, 14.8.

Hy.—Hn, 17.6; Vab, 16.0; Vn, top, 16.2; bottom, 13.9; Hab, right, 17.0, left, 15.5.

Ha.—Hn, 16.5; Vab, 16.7; Vn, top, 15.7; bottom, 16.0; Hab, right, 15.2; left, 15.8.

G.—Hn, 20.3; V Ab, 19.7; Vn, top, 19.4; bottom, 20.8; Hab, right, 19.6; left, 20.7.

E P.—Hn, 16.9; Vab, 16.4; Vn, top, 14.4; bottom, 18.4; Hab, right, 15.9; left, 17.3.

The results of these experiments, except in one case, are, I think, perfectly clear. The horizontal normal and the vertical abnormal correspond; the vertical normal top corresponds with

[1] In Hab. right the movable lines are in the same relation to the eyes as in Vn. top; in Hab. left they are related to the eyes as in Vn. bottom.

the horizontal abnormal right, and the vertical normal bottom corresponds with the horizontal abnormal left.

The testimony of the subjects is very important in these experiments. In this series few direct questions were asked, for fear of influencing the results as all the subjects but myself were perfectly unaware of the ultimate purpose of the experiments; they agreed, however, in saying that in the abnormal position it seemed somewhat more difficult than in the normal to decide when they like the line best; that the eyes in the horizontal position would naturally move across the lines from left to right or the reverse, while in the vertical position they moved up and down. In the abnormal position it was said that there was sometimes a tendency to twist the eyes and head so as to bring the eyes into the same position in relation to the figure as in the normal position. When this tendency was checked, the figure seemed different from what it did when the tendency was allowed to work. If, however, the head were kept motionless any attempt to twist the eyes was uncomfortable and could not be long continued. As concerns myself, I feel sure that if in the abnormal position when the figure was really horizontal I moved my eyes as seemed natural, that is up and down in relation to the head, that the figure would then appear vertical. If I forced the eyes to twist, the figure appeared more doubtful, possibly more like the horizontal, but it was difficult to decide. Moreover, the subjects agreed that if they considered the relations of the board to the rest of the room when in the abnormal position that it influenced the result; they then tried to place the line as nearly as possible where they thought they should like it when they were in the normal position; if they abstracted from the rest of the room, and from their own position the figure was judged on its own merits, it was merely considered as a figure and was judged by the intrinsic value of the relations of its parts. On my part, I am sure that it was much more natural for me not to consider the relations of the board and of my own body to the floor, for this necessitated a very complex mental operation; I much preferred simply to ask myself what was the best position of the movable part in and for itself under the given conditions.

It is to be noted that there is no mention of the words horizontal or vertical in the above account of the subjective feelings of the subjects, except in my own case. This is due to two reasons, I believe. In the first place, I asked the subjects before doing any experiments to speak of the movable part in relation to the center as 'out from the center' or 'in near it;' never to use the terms right or left, or top or bottom. I did this so as to have the judgments as free from any associations as possible. Also I believe that in the abnormal position their attention had been so turned away from considering the vertical and horizontal relation that they did not then consider these relations at all, but merely placed the lines as they liked them best, without being fully conscious of the reasons. In fact, after finishing this series, I asked the subjects if they had thought about the figure as horizontal or vertical, and with only one exception they answered in the negative.

Hy.'s testimony was, however, somewhat different, as in fact was his whole attitude during the experiments. I noticed that in the normal position Hy. frequently held his head on one side; this, of course, altered the relation of his eyes to the board, a thing I wished particularly to avoid in this series. In spite of frequent cautions, I believe that even to the end this habit continued to operate to some extent. When he reclined on the table he was also tempted to raise his head a little, thus throwing it toward one shoulder and rendering the effect of the abnormal position less pure. I was able, however, to control the position of the head better in this case, as I could fix his head on his arms in the right position after he had lain down. Even then he moved sometimes, and he did not appear as much at his ease in the abnormal position as did the others. Hy. said that it was almost impossible for him not to consider the relation of the figure, and his body to the rest of the room, that he almost always reasoned out where he was and where the figure was, as it were. Still this was not always the case, and when he did not, the position of the movable line which he preferred seemed to him to be different. I requested Hy. to think as little as possible about the spacial relations.

The points in this series of experiments to which I wish especially to call attention are, then: that in the abnormal position with the figure vertical there is a strong tendency to place the movable line as it is placed in a horizontal figure when the subject is in the normal position, and the like is true for the horizontal abnormal; that this tendency with the majority of the subjects was not an explicitly conscious one—the subjects did not know that the abnormal vertical seemed like a normal horizontal, but as a matter of fact they did place the lines in the same position when the eye-movements were the same; that the subject Hy. was an exception in both the objective and subjective results.

The second series of experiments concerning the same problem resembled the first, except that the board was turned so that its diagonals were respectively perpendicular and parallel to the floor, thus making the figure appear in an oblique position. The movable part was shown first to the left at the top, then to the right at the top, then to the right at the bottom, then to the left at the bottom. The subjects were in both normal and abnormal positions as before. In this series I asked the subjects to pay especial attention in both normal and abnormal positions to whether the figure seemed more like a horizontal or more like a vertical figure. The results were as follows:

The figure was regarded by all the subjects together, when in the normal position and when the movable line was at the top to the left, 36% vertical, 6% horizontal and 58% neither. The movable line at the top to the right was apperceived 39% vertical, 8% horizontal, 53% neither; when the line was to the left at the bottom, 22% vertical, 45% horizontal, 33% neither. In the abnormal position when the line was to the left and at the top it seemed 6% vertical and 94% horizontal; when to the right at the top, 91% vertical, 3% horizontal, 6% neither; when at the right at the bottom, 3% vertical, 94% horizontal and 3% neither; when at the left at the bottom, 89% vertical and 11% horizontal.

This means that in the normal position the figure was much more generally regarded as an oblique figure, although it seemed slightly more like a vertical one when the movable part was at the top, more like a horizontal one when the movable part was at the bottom; the feeling was not strong, however. In the abnormal position, almost without exception, the positions where the line is to the left at the top and where it is to the right at the bottom seemed horizontal; when the line is to the right at the top it seemed vertical, the movable line at the top; when the line is to the left at the bottom the figure seemed vertical with the movable part at the bottom. So much for the subjective testimony; let us now see how the objective position of the movable line corresponds. The figures for each subject, indicating the position of the movable line as before, are as follows:

L.—Normal, left up, 16.5; right up, 15.5; right down, 16.2; left down, 17.2.

Abnormal, left up and right down apperceived as horizontal, 16.8; right up, 12.1; left down, 14.8.

J. P.—Normal, left up, 18.0; right up, 18.0; right down, 15.6; left down, 14.6.

Abnormal, left up and right down apperceived as horizontal, 15.6; right up, 18.0; left down, 16.6.

Hy.—Normal, left up, 15.4; right up, 16.1; right down, 13.4; left down, 13.1.

Abnormal, Left up and right down, apperceived as horizontal, 15.3; right up, 16.8; left down, 12.9.

Ha.—Normal, left up, 16.7; right up, 17.5; right down, 16.6; left down, 16.6.

Abnormal, left up and right down apperceived as horizontal, 16.6; right up, 15.9; left down, 17.6.

G.—Normal, left up, 18.2; right up, 17.5; right down, 17.1; left down, 18.2.

Abnormal left up and right down apperceived as horizontal, 18.0; right up, 18.5; left down, 16.6.

E. P.—Normal, left up, 15.7; right up, 16.5; right down, 16.9; left down, 18.0.

Abnormal, left up and right down apperceived as horizontal, 17.2; right up, 16.3; left down, 18.6.

Now if these figures are compared with those for the same subjects in the normal position with the board horizontal and vertical, it will be seen that they correspond with the subjective results we have already noted. In the normal position with the figure oblique, the table shows a slight tendency to place the line as in a vertical figure. This is not very strong, however, and many variations result. The tendency is strongest, however, for Hy., J. P. and G. There is also a tendency shown by the figures for E. P., Ha. and L., to place the line when it is at the bottom more nearly like in the horizontal figure. It is to be noted, however, that the figures show great variations from the normal horizontal as given in the former series. The figures for Hy. show the greatest approach to three of the former series. For the normal vertical he had, top, 16.2; bottom, 13.9. For the normal oblique, top, 15.7; bottom, 13.2.

The abnormal oblique shows a marked resemblance to those for the horizontal and vertical normal, the left top and right bottom corresponding to the horizontal, the right top with the vertical, the movable line up; the left down with the vertical, the movable part down. L., in one instance, the right up, is an exception; it seems probable that here some association influenced the result. Hy. also shows a considerable variation in his choice for the left up and right down, the figure for this being 15.7, while the normal horizonal was for him 17.6.

This is then a very strong tendency when the figure is oblique and the subject in the normal position to regard the figure as oblique, that is to say as neither horizontal nor vertical. Some subjects show a tendency, however, to regard it as *either* horizontal or vertical; in the abnormal position the figure is regarded almost universally as either horizontal or vertical according to the position; here the subjects practically agree. These results are in accord with both the testimony of the subjects and the objective results obtained. Hy. is different from the others in almost every case. It is also to be noticed that in all cases the general tendency of apperception determines the position of the movable line.

The explanation for the changes in these tendencies of apperception and for the resulting changes in the position of the movable line is undoubtedly to be found in the changes of the eye movements. Notice the perfect parallelism between the two.

In the horizontal normal position the eye movements are from one side of the head to the other—the figure is apperceived as horizontal; in the vertical position the eye movements are from the top of the head toward the chin, or the reverse—the figure is seen as vertical; in the abnormal position when the figure is really horizontal we see that there is a tendency to apperceive the figure as vertical; here we have the eye movements that go with a real vertical position; the reverse is true for the abnormal vertical. When the figure is oblique and the subject normal the eye movements are neither like those in the vertical nor like those in the horizontal positions; the figure is usually apperceived as oblique and is always different from either horizontal or vertical. In the abnormal oblique we have a peculiar case and one that seems contrary to our theory, for here with eye movements that are in reality neither horizontal nor vertical, yet the figure is apperceived with almost absolute certainty as either horizontal or vertical. To explain this we must make a rather detailed examination of the conditions which influence the apperception.

Reference to the testimony of the subjects shows that associations called forth by the position of the body might influence the apperception of the object. If while in the abnormal position they thought about the fact that they were lying on their side, and that the figure was parallel to the floor, the position in which they preferred the movable line was changed. Most of the subjects found it easier not to make this connection. Hy. however, found much difficulty in not doing this. The explanation is simple. In the abnormal position Hy. was not very comfortable; this means that sensations from his body were continually being forced on his attention; hence it was, of course, difficult for him to abstract from the position in which he found himself. The figures show that the effect of the abnormal position was less powerful with him than with the others. Associations, then, form other sources, as well as eye movements and their associations, can influence the apperception.

In the normal oblique position the eye movements aroused usually the idea of an oblique figure; this is probably due to their effect above, as is corroborated by the peculiar figures in the results of Hy., where the obliques were much more nearly like the horizontal and vertical than was the case with the other subjects. It will be remembered, however, that Hy., had a strong tendency to hold his head on one side. His eye movements, then, were never purely from side to side, or up and down, but were more nearly like those for an oblique position of the figure with the head normal. A change from horizontal to oblique was then, for him, not one of kind but only of degree as is shown by the figures. But in the normal oblique there was also a tendency to approach the horizontal and vertical with the other subjects, his I believe to be due to the fact that we are accustomed to see at times objects which are normally horizontal tipped to one side; these objects would cause oblique eye movements. It is natural then that these oblique eye movements should at times arouse associations with a horizontal or vertical figure.

We may now return to the abnormal oblique from which we started. Hy. it will be remembered for the 'left up' and 'right down' abnormal positions, upon which all the others showed great agreement, placed the line very excentrically. But we saw that the associations called forth by his body played an important part in his judgments. In the abnormal position then it seemed probable that he was more or less conscious of being in an abnormal position and that the board was really oblique. When such associations were aroused we have seen that a different position of the line was chosen by all. Hy. is no exception for the figure for the abnormal oblique position (which the others apperceived as horizontal) were 15.7, while the normal horizontal was 17.6. Moreover, we saw that there was nearly uniformity in the apperception of the oblique abnormal for the other subjects, only nine variations in one being recorded; of these Hy. had 5.

In the case of the rest of the subjects these associations from the position of the body were largely absent; they were moreover accustomed to place the line without considering the relation in space. Now in the normal position we saw that the general habits of the eyes determined the apperception, but that when these were ambiguous the apperception was ambiguous.

In the abnormal position where the line is up and to the left there is first the side movements of the eyes; this suggests a horizontal figure; there is also a vertical movement of the eyes which suggests a vertical figure; we should then expect an ambiguous result in the figure, but this is not the case. There is no doubt, however, that the eye movements most easily made in this case are side ones meaning a horizontal figure. Now the whole situation is very strange, and in order to make the necessary connections and apperceive the figure as oblique a very complicated process, as we have seen, would have to occur. But the subjects are trained to inhibit even much less difficult processes; it follows then that the eye movements which preponderate will determine the apperception immediately without more ado. Such we find to be the case for nearly every subject. When associations are aroused as with Hy. the result is very different.

In every case then it is perfectly clear that the eye movements and the intellectual associations determined a general way of apperceiving the object, although this tendency of apperception was not present to the individual consciousness unless attention was called to it. The line was in every case placed in accord with this general way of apperceiving, whether this was wholly conscious or not. This is proved not only by the general agreement, but also by the individual variations, and is corroborated by the subjective testimony.

There is then no doubt but that in these simple forms one function of the eye movements is to suggest the general way of apperceiving the object; they are not the only elements, as association from other sources may influence the result. But we have seen in our former paper on symmetry that eye movements and associations influence the proportions between the different parts of the figure, that is to say these elements fix the relations between the parts of the object. *It seems then as if the eye movements with the other elements suggested a given kind of apperception of the object*, which tendency need not be fully conscious, and also by laws of their own determined the objective relations necessary to complete this apperception. *When the objective conditions fulfill the suggestions aroused by it, then the object satisfies the æsthetic demands.*

We started out in our experiments to explain the æsthetic consciousness derived from simple forms. We found the first condition of these to be unity and variety. We found also that in the horizontal position where variety is given that the symmetrical arrangement was preferred for this gave unity. The objective condition for this unity varied with the content and involved sensational and intellectual elements.

In the vertical position we found other conditions. Stability was here the important unifying element; the objective conditions which produced stability were probably due to the same elements that produced symmetry. The elements that enter in to the unity of these forms are sensational and intellectual.

Why do we demand unity was the next question, and it seemed as if a study of the elements that constituted this unity might explain it. An examination of the different forms showed us that certain sensational and other elements determined whether we should regard the forms as horizontal or vertical, and that the specific position of the line always corresponded to the general tendency of apperception. *The desire then to make the objective conditions correspond with the subjective ones is what necessitates unity in our forms and is the one essential condition for the emergence of the æsthetic consciousness.*

But it will be seen that in our experiments something suggested in a general way has been just as necessary as the unity of the forms. This in itself necessitates a variety of elements, for one kind of elements is not rich enough to suggest such a general tendency of apperception. Thus unity and variety result from the fact that the æsthetic consciousness is the feeling resulting from a realization by the object of a tendency suggested by it. Any form then that by means of any elements suggests a general tendency which can be satisfied by the elements it contains, apperceived as a whole, may be beautiful. One more limitation is, however, necessary before we reach a true idea of the æsthetic consciousness. We saw in the horizontal position that symmetry was preferred, in the vertical usually the stable, but that associations often influenced the result. Thus, if one thinks of a vase, the lines are put so as to carry out the idea. It seemingly then makes no difference what the general tendency is as long as the object carries out this tendency. The essential thing is the fulfillment of a tendency of whatever sort for its own sake without involving any purpose. The æsthetic consciousness is, then, a state aroused by the objective fulfillment of a tendency regarded without reference to any ulterior end, and the function of the elements of the beautiful object is to suggest such tendency and at the same time to fulfill it.

A New Perimeter.

BY JAMES E. LOUGH.

Indirect vision is one of several problems of sight now under investigation in the Harvard Psychological Laboratory. For the purposes of this study the ordinary perimeter and campimeter have proven themselves almost useless. In these instruments the eye looks at a stationary point, whilst the stimulating object changes its place on the graduated arc. This change of place alters the objective illumination, etc., of the object, so that the effect of fine changes in its intensity, size, etc., cannot be accurately studied. This difficulty has been overcome in the instrument here described by reversing the usual order of things and making the fixation point movable while the stimulus is the stationary part of the apparatus. By this arrangment the operator is given absolute control over the variations of the stimulus.

A description of this instrument is published now before any exact results can be reported, in the hope that it may prove helpful to others engaged in this same line of investigation.

I.

Figure 1 shows the ground plan, a semi-cylinder of blackened brass. A, 30 cm. high, with a radius of 30 cm., is supported by a base board 60 cm. x 40 cm. and by back and side boards B and F. In the middle of A and extending through B is a window, W, 10 cm. x 10 cm. This opening may be filled

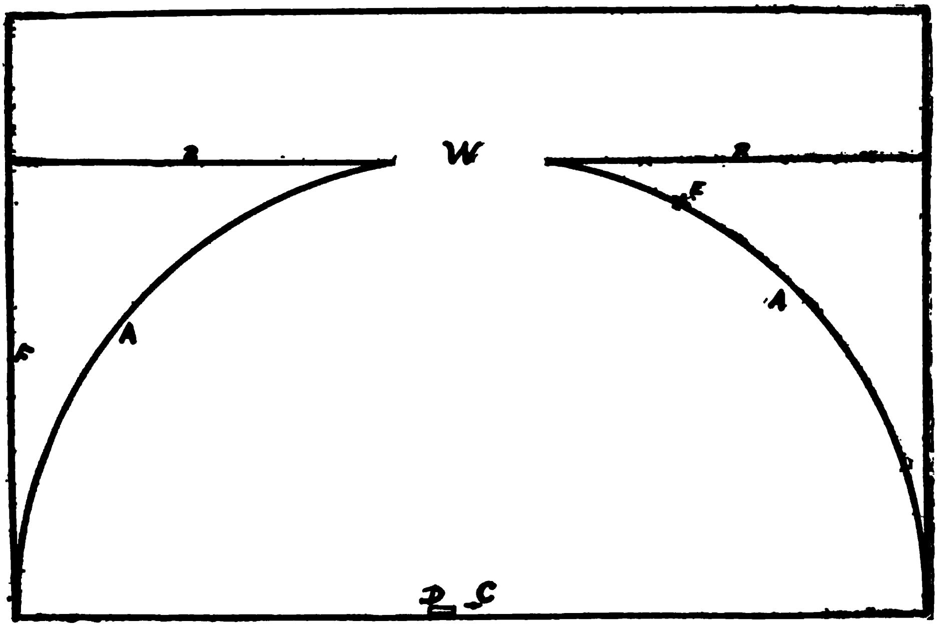

FIG. 1.

by the various contrivances described later. E is the point of fixation which may be moved to any position on A. The angle W C E can be read from a scale on A. The eye piece C retains the eye exactly at the center of the cylinder, but as it rotates freely upon its axis the eye may always fixate E, throwing W into indirect vision. The chin rest D will give the head a firm support.

II.

Figure 2 fills the window W during the investigation of the various retinal parts. It consists of a sheet of blackened brass bearing a circle of brass, pivoted at X. The slit O S is ½ mm. wide and except for ½ mm. at O is covered by the circle N. This circle contains a series of holes ½ mm. in diameter placed ½ mm. apart upon the line of an archimedean spiral.

When this shutter (shown in Fig. 2) is placed in the window W, and a lamp back of it, the eye at C will always see one point of light at O, while a second point will also be visible whenever one of the holes in the circle coincides with the slit O S. The rotation of the circle about X will vary the distance between these two points. This distance can be easily read to $\frac{1}{8}$ mm. (2′) upon the scale P. The intensity of the stimulating light is easily regulated by the distance of the lamp, while the quality of the light may be varied by the use of gelatine sheets.

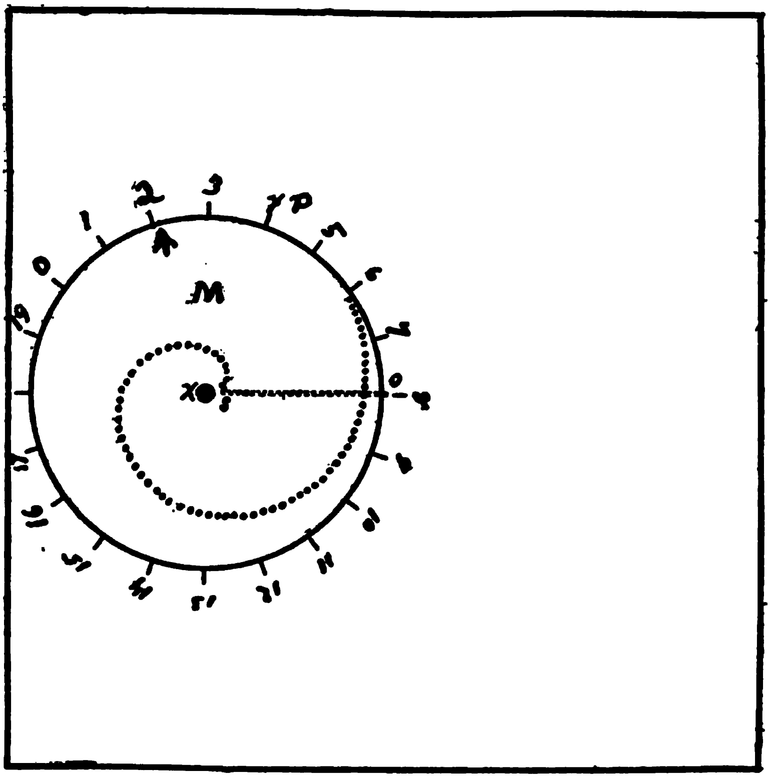

FIG. 2.

By this apparatus, therefore, the vertical and horizontal distance at which two points of light stimulating the retina appear as one (the retinal unit) may be obtained for all portions of the retina. And the influence of the intensity and the quality of light upon the retinal units may be determined.

Another shutter may be placed in the window W, having an opening at the center, the size of which is controlled by an iris diaphragm. By means of a lamp and gelatine sheets a light stimulus of any quality, intensity or size may be made to excite any portion of the retina. Such experiments quickly demonstrate that the 'color fields' depend entirely upon the size and intensity of the stimulating color.

This instrument facilitates the study of two points of different color within one retinal unit; of the threshold for colors; of the perception of differences in quality, in intensity and in position for all portions of the retina. Reports of these and of other investigations will be published as they are completed.

THE ACCURACY OF RECOLLECTION AND OBSERVATION.

BY FREDERICK E. BOLTON.

University of Wisconsin.

The following observations and discussion are offered as a further contribution to the line of study suggested by Prof. Cattell's article on 'Measurement of the Accuracy of Recollection' which appeared in *Science*, Dec. 6, 1895, and in which the author intimated that a fruitful field of psychological research might be opened up by comparison of results obtained from classes of persons differing in certain specified characteristics.

A series of questions, similar to those given by Prof. Cattell, together with several others, was assigned by Prof. Jastrow to his psychology class in the University of Wisconsin. The class consisted of juniors and seniors, 92 in number, 26 being women. From the results obtained it is possible to make comparisons of the U. W. students and Columbia students as classes, and also to compare the records of students in different courses, of men and women, and of classes made upon basis of college standings. To ascertain the degree of confidence, in all answers the students were requested to mark their answers '*c*' if *very* confident, 'c' if confident, 'm' if *moderately* sure, 'D' if doubtful, and '*D*' if *very* doubtful.

The question given to determine the reliability of recollection was the same as the one proposed at Columbia; viz: "What was the weather a week ago to-day?" The answers showed great divergence. Out of the 92 that answered, there were 56 that said 'cold,' 32 'warm,' 36 'clear' or 'fair,' 37 'stormy,' and 21 indicated that rain fell while 21 said it snowed. (Many gave double answers, as 'cold' and 'snowy.') On the day in question it was very moist in the morning, sprinkling a little, while later in the day it turned to rain and sleet. The temperature varied from a little above freezing in the morning to a little below at night. Perhaps little weight could be attached to those answers classed as cold, inasmuch as standards of cold vary so greatly. Also, the most natural assumption would be that it was cold on any December day. While the answers 'stormy' only just exceed those of 'clear' or 'fair' yet it is worthy of note that 16 out of 18 of those who indicated that they were very confident were correct in their answers. The distribution of confidence in the 37 correct answers was as follows: 16 *c*, 4 c, 9 m, 6 D, 2 *D*. Out of the remaining 55 answers only 2 were *c* and 14 c, thus indicating that the degree of confidence is of great weight in this case.

On comparison of the records of the 26 women of the class with those of an equal number of men (selected by lot) it was found that 14 women gave answers substantially correct, while only 5 of the men's answers were correct. The degree of confidence shown is also significant. The women's answers were marked as follows: 13 *c*, 4 c, 6 m, 3 D. The men's: 1 *c*, 10 c, 7 m, 3 D, 1 *D*, 4 unexpressed. Assigning the following scale of marking to the answers, *c*=5, c=4, m=3, D=2, *D*=1, we should have as the women's index of confidence 4.04 and the men's 3.32. The above results, both as to accuracy and confidence seem to suggest (unless the result is accidental) that women are better observers or 'recollectors' of the weather than men.

The answers given to the question relating to the direction in which apple seeds point were 42 'toward the stem,' 31 'away,' 10 'toward the center,' 3 indefinite. Thus the right answers, although not a majority of *all* the answers, include 4–3 as many as any other class. Of the 42 correct answers, 3 were marked *c*, 10 c, 17 m, and 12 D. Of the 26 women 12 were correct and of the same number of men 13 were right. The degree of confidence in the correct answers of the women, however, considerably exceeds that of the men. In the former the answers were distributed as follows: 3 *c*, 3 c, 2 m, 3 D, 1 *D*. In the latter: 2 c, 7 m, 4 D. Marking on the same scale as above, the index of confidence for all the women's answers is 3.33, for the men 2.97. Comparing the answers of the entire class with the Columbia records, we find exactly the same proportion of correct answers. There is some variation in the wrong answers. A comparison on the basis of confidence cannot be made, because the results from Columbia were not given on that basis.

The question asking the relative date of Luther's and Michael Angelo's birth received an equal number of answers giving each the precedence. But the average of all the answers assigned the earlier date by 6.6 years to Michael Angelo, giving a constant error of—1.4 years with an average departure of 52 years from the correct date. The Columbia records showed a constant error of+4 years and an average error of 54 years. In this again the records of the women were more nearly correct, they having placed the birth of Michael Angelo 11.1 years before that of Luther, while the men's average showed that Luther was born the earlier by 9.1 years, or 17 years from the correct date.

The next two questions were: "In what year did Victor Hugo die? Chas. Dickens?" The average of the class placed Hugo's death 1851 (true date 1885), and Dicken's 1862 (true date 1870). In the first the average departure from the true date was 35 years and in the second 17 years. This gives the Columbia students the nearer average estimate by 22 years, and an average error of 22 years less than the Wisconsin students. In these two questions the men came much nearer the correct date than the women, placing Hugo's death in 1860 and Dicken's 1865. The women's averages indicated that Hugo's death occurred in 1847, 38 years from the correct date, while Dicken's death was placed in 1860, 10 years from the true date. The average of the entire class came considerably nearer to the correct date of Dicken's death than Hugo's, which is perhaps due to the apparently closer relationship of Dickens to us. Several of the answers showed great deviation, as at Columbia, from the correct ones. Hugo's death was placed as early as 1735 by one and as late as 1890 by several. Of the entire number 3 were right concerning Hugo's death; one of the 3 was c, one D and the other *D;* 6 were right concerning Dickens; of these 1 was c, 3 D and 2 *D*. The degree of confidence based upon the scale of marking is about equal in the two cases, 1.84 for Dickens, and 1.82 for Hugo.

To determine the average accuracy in estimating weight, distance and time, Prof. Jastrow gave similar questions to those given by Prof. Cattell, as follows:

I. (*a*) Estimate in feet the distance from one college building, "A," to a second one, "B." (*b*) The distance from building "A" to a third one, "C." (*c*) The distance from building "B" to building "C," (the three buildings being situated at the vertices of a familiar triangle on the campus).

II. Estimate in seconds the time required in walking from "B" to "C." (All had repeatedly walked the given distance).

III. Estimate in ounces the weight of James' Psychology (Briefer Course).

The results obtained are tabulated for convenient reference in the following form:

Estimation of	Actual Magnitude.	Average Estimate.	Constant Error.	Average Error.	Medium Estimate.	By Whom.
Ounces.	24	20.5 22.8 19.8 (17)	—3.5 —1.8 —4.2 (—7)	8 8.8 8.8 (8)	19.6 20 18 (16)	Entire Class. Men. Women. Columbia.
Feet from "A" to "B."	810	580 606 455	—230 —204 —355	306.7 224 462	575 600 350	Entire Class. Men. Women.
Feet from "A" to "C."	750	508 546 402	—242 —204 —348	229.9 216 416	500.5 510 300	Entire Class. Men. Women.
Feet from "B" to "C."	450	276 296 261	—174 —154 —189	216.6 186 333	245 262.5 210	Entire Class. Men. Women.
Seconds from "A" to "B."	160	182 177 187	+22 +17 +27	48 62 44	182 180 180	Entire Class. Men. Women.
Seconds.	35	66	+31	40	60	Columbia.

From these tables it will be seen that the average estimates relating to weight and time were more nearly accurate than those obtained at Columbia. Instead of a difference of over ¼ of the weight of the book as at Columbia ours differed only about 1–7. The number of seconds instead of differing by 90 % was correct within 15 %. The actual magnitudes compared in the time estimates not being the same, the comparison must be only a general one; the numerical constant error at Columbia when 35 seconds was the actual magnitude, was greater than at Wisconsin with 160 seconds as the actual magnitude. The average error in the weight estimate is the same in both records, but the Wisconsin median estimate is considerably closer. There is some difference between the men's and women's records; in each of the above the men being more nearly correct.

Our results relating to distance show a much greater constant error than those obtained by Prof. Cattell. Ours show positive constant errors of over 30 % while his show negative errors of only 15 %. The most interesting point, however, is in the direction of the constant errors. In the Columbia results "there was a marked tendency to underestimate weight and to overestimate time. Length was overestimated, but to a less degree." At Wisconsin the errors were in the same direction for time and weight estimates as at Columbia, though of considerably smaller degree. The average errors in the distance estimates were smaller than at Columbia, ours being less than 40 %, except in the 3d case, while theirs is nearly 50 %. The average of the U. W. distance estimates is 71.5 % of the actual, while at Columbia it is 115 %. We find that the greater the distance the nearer correct the actual estimate is. The distance 450 ft., which is nearest to the Columbia distance was estimated with the least degree of accuracy, and diverges most from the Columbia results. Should a few exceptional results be eliminated, the average errors would be very small. One person gave the distance from 'A' to 'B' 2000 ft., nearly half a mile, and another recorded it 45 ft., less than three rods. These extremes are found in the women's records. An examination of the tables reveals the fact that the men's average estimates are much more nearly correct and their average errors much smaller than the women's.

The records of the women on distance show a very small degree of confidence in their answers, the sign 'c' occurring but once. A large majority expressed themselves as doubtful. Among the answers of the men there was a considerable number who were 'c' and 'm.' From these comparisons we should judge that in quantitative estimations of measurement that men are more accurate than women, and that their index of confidence is higher. The following diagrams show the distribution of answers to the weight estimate and a comparison of the actual and estimated distances.

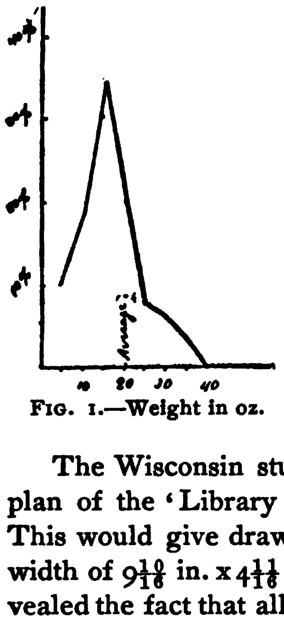

FIG. 1.—Weight in oz.

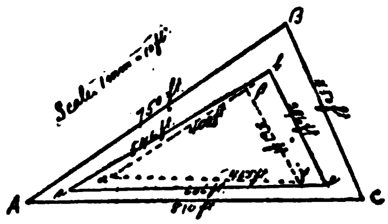

FIG. 2.

Triangle A B C represents actual distances.

Triangle a b c represents men's estimated distances.

Triangle α β γ represents women's estimated distances.

The Wisconsin students were asked to draw a ground floor plan of the 'Library Hall' on a scale of $\frac{1}{16}$ inch to the foot. This would give drawings, if accurate, of extreme length and width of $9\frac{10}{16}$ in. x $4\frac{11}{16}$ in. A measurement of the drawings revealed the fact that all had considerably underestimated the size. The size of the paper on which the drawings were made was 8 in. x 10 in. The drawings averaged $3\frac{1}{2}$ in. x 6 in., or indicated that the building was about 55 ft. x 95 ft., instead of 78 ft. x 155 ft. In these records we see clearly the same tendency to *under*-estimate distance. Many of the drawings exhibited all the characteristic features of the correct plan, and would give a tolerably correct impression of the building. A composite drawing made from the collection would show the plan to quite a degree of exactness. Only five drawings were too large, and these only slightly.

Another task to test memory and observation was to draw a print of a dog's foot as it appears in the snow. The drawings present a great variety, and not a very correct impression of what was intended could be gained from most of the drawings, taken separately. It is possible that a composite drawing made from the collection would exhibit the most prominent characteristics. A classification, made on the basis of number of toes, gave the following results: 3, two toes; 16, three toes; 44, four toes; 22, five toes; 1, six toes; 6, no toes at all, the foot being one solid piece with slight lobes. *Fac-similes* of a few drawings are appended.

FIG. 3.—*Fac-similes* of drawings of dog's foot print.

The last question was (a) tell the number of steps in a familiar stairway 'L' and (b) the number of steps in another stairway 'S.' The results obtained are given in the following table.

Estimation of.	Actual Numbers.	Average Estimate.	Constant Error.	Average Error.	Median Estimate.	By Whom.
Steps in "L."	6	5.43 5.5 5.2	—.57 —.5 —.8	1.3 1.27 1.44	5.28 5.8 5.4	Entire Class. Men. Women.
Step in "S."	14	9.8 10.8 9.1	—4.2 —3.2 —4.9	4.4 4.2 5.	9.75 10. 8.75	Entire Class. Men. Women.

Curves are added below which represent the distribution of answers to the last question.

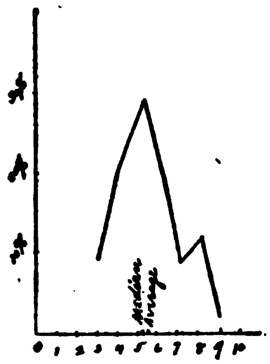

FIG. 4.—Steps in 'L.'

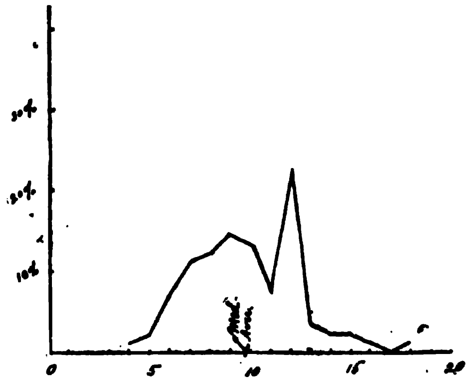

FIG. 5.—Steps in 'S.'

A study was made of the entire records on the basis of the course in college and also by classifying according to college standing. No definite results could be secured on the last named basis inasmuch as the standards of marking are so purely conventional with each different instructor that no safe working basis of comparison is available. The results most nearly correct seem to be found in the records of the class with lowest standings, which is probably due to the fact that the women, whose records show greater errors than the men's are found, the great part, in the class with highest standings.

The comparison of 'general science' students with 'ancient classical' students shows that on the whole the former are more nearly correct in their estimates. In these the number of women is about equal in each. In the three distance estimates the 'general science' student's estimate averaged 71.5 % of the actual while the 'ancient classical' student's estimate was only 51.5 % of the actual. The average of the science students placed Dicken's death in 1868, the classical in 1856. Hugo's death was given 1854 by the science students and 1839 by the classical students. The answers relating to the weather were slightly nearer to the correct and the one relating to the apple seeds much more generally correct in the answers of the science students. In two cases the classical students' answers showed a better though only slightly better average, than the science students.

A general study of the distribution of confidence in the several answers is interesting and suggestive. Comparing first the average confidence in the several answers, as expressed in the scale of marking used above, we find a high degree of confidence in the answers relating to weather (3.33); in the estimate of time (3.28); and in the question relating to the direction of the apple seeds. A second group, in which the confidence has an intermediate value, consists of the two questions relating to the number of steps (av. confidence, 2.52 and 2.92); of the three estimates of distance (2.49, 2.56, 2.54), and the estimate of weight (2.29). The third group, with the low confidence, comprises the historical group, relating to the death of Hugo, M. Angelo and Luther (av. confidence, 1.82, 1.84, 1.79). It thus appears that the smallest degree of confidence attaches to those questions that depend upon memory alone, the highest degree to those depending mainly upon observations (with a slight memory factor), while an intermediate degree of confidence attaches to those questions involving in addition to memory and observation, a process of estimation.

It would be interesting to compare the general correctness of the answers with their confidence, but the nature of the answers prevents such comparison, except in a few cases. We can compare the various estimates of number, weight and time. We thus find that the two questions most correctly answered are the estimate of time and the number of steps in a certain flight, and these are also those that have the highest confidence in the group. The estimate of weight is, however, somewhat of an exception to this relation, as the answers are good, but expressed with little confidence, while in the other estimates we have an amount of correctness as well as of confidence.

We may finally compare the general distribution of confidence in the whole group of answers, as below:

	Very Doubtful.	Doubtful.	Moderately Sure.	Confident.	Very Confident.	Average Confidence.
Total . .	15.7	31.9	36.0	12.8	3.6	2.57
Men. . .	11.7	34.0	36.7	14.0	3.6	2.77
Women. .	18.4	33.4	26.1	14.4	7.7	2.56

The numbers in the table express percentages of occurrence. It is observed that there are relatively more doubtful and very doubtful than confident and very confident answers. We observe, also, that, while the men tend to use the moderate confidence more than the women, the women use the extremely confident and extremely doubtful marks more than the men. This feminine tendency is due to the extreme confidence in the question regarding the weather, and to their extreme doubt regarding the questions of date.

I desire to acknowledge many helpful suggestions from Prof. Jastrow in the arrangement and interpretation of these results.

DISCUSSION AND REPORTS.

CONSCIOUSNESS AND EVOLUTION.[1]

MR. PRESIDENT AND GENTLEMEN: This conference between those who look upon many of the same phenomena from two points of view, the biological and the psychological, seems to me significant and promising. I think it is one of several indications that in general the devotees of the different particular sciences are coming more clearly to recognize the community of truth and interest which makes them dependent upon each other; and that this recognition is producing more of the spirit of appreciation and of sympathy among them all. It is to be hoped that the day of the *mere* specialist is waning. It may reasonably be believed that the day is dawning when a broad culture, a genial attitude and a firm grasp upon the unities of nature and of life will characterize the various departments of human knowledge.

The peculiarly close relations between biology and psychology are easily made apparent. I think that biologists are destined to make increasingly intelligent and emphatic the acknowledgment that they cannot understand or explain the phenomena of living animal forms (and, perhaps, not those of living plant forms) without appealing to the science of psychical phenomena. And since all science of psychical phenomena must forever take its rise from and return, after its attempted excursions into the fields of comparative psychology, again to the science of human consciousness, biology must always owe much to human psychology. On the other hand, every progressive student of psychology is entirely ready to recognize a constant and growing obligation on the part of his science to modern biology. Indeed, just now many psychologists are in danger of becoming too timid and—if I may be pardoned the word—even servile in their attitude towards the physical and natural sciences. It would seem that they often prejudice the facts of their own science, and reject the most convenient and satisfactory theoretical explanations of the facts by being more dogmatic

[1] Discussion before the American Psychological Association, Philadelphia, 1895.

about the validity and universal application of so-called 'natural laws' than are the physicists and biologists themselves. Witness the hasty and excessive confidence of many psychologists in the principle of causation, as conceived of after the pattern of physics and carried in again upon the sphere of mental life in discussing the phenomena of will; or the gingerly way in which the facts and laws of consciousness in its relation to brain states are discussed, whenever the shadow of the very dubious principle of the conservation and correlation of energy is thrown over this problem. It has been my experience that, on the whole, psychologists are much more inclined to dogmatism over many alleged physical principles than are the most candid and thoughtful students of physics and biology.

Without criticising or dissenting from Professor James' threefold division of the problem of consciousness and evolution, it seems to me that we may regard this problem from two points of view. If we take one of these points of view we look backward and ask ourselves as to the origin of consciousness, and as to the possibility of explaining it by considerations which the student of biology is able to present and to verify. If we take the other point of view we look from it in the forward direction; and then we ask ourselves as to the part which consciousness itself ever plays—has played and will continue to play—in the evolution of animal organisms. Our first question is: How far does the evolution of organisms, histologically and physiologically considered, enable us to give the history and the explanation of the rise and development of consciousness? Our other question is: How far does consciousness, having once got established, so to speak, influence—quicken, accelerate, retard and mark out into definite lines—the development of organisms?

The first of these two questions we may consider either in the more purely historical and descriptive way, or in the more profoundly philosophical way. And it is difficult, in all thorough discussion of the subject, to separate between the two. But a few words upon each of these ways of consideration, or sets of considerations, may not be out of place here.

It must be admitted with gladness and thanksgiving that the modern doctrine of biological evolution has drawn a most interesting and instructive picture of how the different forms of animal life might have succeeded each other, and of the relations, whether to each other by physical generation or to their total environment, under which they have appeared in succession, been modified, and disappeared, giving place to other forms. But it may well be questioned how far all this puts us in possession of the descriptive history, not to say the scientific explanation, of the rise and development of consciousness. For, in the first place, we are still almost wholly in the dark as to precisely where, in the series which evolution presents, consciousness in fact had its rise. Was it with those most elementary living forms which expert biologists hesitate to assign either to the animal kingdom or to the field of plant life? And, if so, shall we go on with Fechner to assume 'souls' as belonging to all the plants; or even with Clifford, to distribute our 'soul stuff' as widely and generously as Nature herself seems to have distributed the 'stuff' out of which things are made?

It seems to me that the most significant truth which biology is about to establish in such connection is this: The more careful and patient study of the micro-organisms with the higher powers of the microscope shows that an unexpectedly high development and complex exercise of psychic functions needs to be assumed to account for their behavior. Where, then, and how 'low down' shall be placed the rise of consciousness in the so-called scale of animal life?

But, even if we could find in biological evolution any answer to the question just raised, and also any answer to the inquiry for a trustworthy descriptive history of the development of conscious life as connected with organisms, all this would not give us a valid explanation of conscious phenomena. For, as is admitted by all when brought face to face with the problem, consciousness is *per se*—if I may so speak—a phenomenon of a totally different order from those phenomena with which histology and physiology deal. It appears, indeed, quite as hopeless a task for our imagination, to ask it to conceive how the simplest and lowest form of consciousness can arise out of the unconscious as to conceive the denial of the scholastic maxim: *Ex nihilo nil fit.* If we had our two parallel sciences complete—comparative anatomy and physiology in one line and comparative psychology in another—we should still exhaust all our wisdom with the sentence: Just at this time, it would appear, the *fiat* went forth: 'Let there be consciousness, and consciousness was.'

I will not attempt to take the question as to the relations between consciousness and the evolution of material forms out into the broader fields of general metaphysical philosophy. It seems to me, however, the history of speculation has sufficiently shown that all theories which make consciousness ultimately dependent upon the evolution of unconscious forms of existence succeed only by smuggling into their explanations everything which the very essentials of the theories require them to leave out. I will only call attention to one important truth in the theory of knowledge. It is impossible to have any science whatever without basing it upon a system of metaphysical postulates and metaphysical conceptions. But all these conceptions are themselves only products or processes in consciousness; and all the postulates are only the assumptions, the natural or acquired 'faiths' of human consciousness. If, then, whatever may be thought of the chronological position which human consciousness occupies in relation to the development of organisms, you do away with the logical *a priority* and the ontological value of consciousness, as rational thinking, as willing, as knowing, you remove all science. In the macrocosm it would appear that there is no escape from the position; being—so far as being can be known, or thought by us—is dependent for its genesis and evolution on some consciousness.

As to the other most interesting and important problem, namely, the dependence of the evolution of specific animal organisms upon the conscious psychoses of the animals themselves, it seems to me our trustworthy evidence of an experiential sort is much greater. I was not a little delighted at the main position which Professor Cope took in his address. But I believe that biologists will be compelled to go even further than he appears to, at present, in valuing the influence of consciousness upon the evolution of organisms. To speak in popular and figurative phrase, the psychical characteristies and psychical activities of every species of animal is an active and authoritative factor in the excitement and direction of organic changes in the individual. The activities of even the lower forms of animal life are within indefinite but really existing limitations determined by the mental representations, the passions, the conscious wants, desires and volitions of the animal. These forms are not in their individual development, *mere* molecular mechanisms.

I think that most biologists have quite failed sufficiently to reflect upon the significance of much of the terminology which they employ. How much of it is taken from our own conscious life, our psychical experience! Strip it of the more obvious meaning which it seems to have as applied to this life and to this experience, and how difficult it becomes to give it any meaning, whatever, which shall make our theory of evolution much more than a ceaseless, unprogressive repetition of the facts. Some years ago, when discussing this subject with a class of graduate students, a member of the class who had taught for years in a large high school expressed his astonishment as he once beheld an amœba and a fresh-water hydra, after preliminary exhi bitions of rage and cunning, come to a pitched battle with each other which ended in the hydra taking the entire insides out of the amœba. Here was indeed 'a struggle for existence' with a vengeance!

For myself, I do not propose to be deterred by doubtful principles of physics, from the most obvious inference that the animals, including the micro-organisms, have a true psychic existence; and that this psychic existence is a force, and an important force, for the preservation or destruction of the species. Only the settlement by biology of the disputed question as to the limits of heredity can decide how much psychic forces count for in the modification and direction of the physical evolution of species. Without emotion and what we call instinct to act as *veræ causæ* in the evolution of their organisms, the world of animal forms would be a system of pale shadows, moved by toy-like mechanism, compared with the exceedingly interesting and dreadfully earnest thing which it now is.

It is here, of course, however, that comparative psychology and biology came so close to each other; indeed, seem to run together. And comparative psychology—as the very term signifies—cannot be cultivated without knowledge of human psychology. Here, therefore, I am brought around again to the remark with which I started. Such a conference as this is significant of the unity of interest that maintains itself among the sciences; and it is promising of a more warm sympathy and a more helpful intercourse between them.

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CONSCIOUSNESS AND EVOLUTION.[1]

The addresses to which we have already listened by Professors James and Cope have raised so many interesting questions, and the various aspects of the general problem have been so clearly formulated, that I shall confine myself to a few remarks upon the positions which these speakers have taken.

Professor Cope's position on the place of consciousness in evolution seems in the main the true one, as far as the question of fact is concerned. I agree with him that no adequate theory of the development of organic nature can be formulated without taking conscious states into account. The fact of adaptation requires on the part of the individual organism something equivalent to what we call consciousness

[1]Discussion (revised) before the Amer. Psychol. Assoc., at Philadelphia, Dec. 28, 1895.

in ourselves. But I do not think that the need of recognizing consciousness in connection with organic functions leads at all necessarily to the view that conciousness is a *causa vera* whose modes of action do not have physiological parallel processes in the brain and nerves. The alternatives are not really two only, automatism—a theory of mechanical causation of all movement, with the inference that consciousness is a by-product of no importance, and this *vera causa* view which makes consciousness a new force injected into the activities of the brain. There is another way of looking at the question to which I return below.

With Professor Cope's view that the recognition of consciousness as a factor in evolution requires a Neo-Lamarckian theory of heredity I am not at all in accord. I have recently discussed the question apropos of Professor Cope's views in *Science* (Aug. 23, 1895). Instead of finding with Professor Cope that the emphasis of conscious function in evolution makes it necessary to recognize the Lamarckian factor, I think the facts point just the other way. As soon as there is much development of mind, the gregarious or social life begins; and in it we have a new way of transmitting the acquisitions of one generation to another, which tends to supersede the action—if it exists—of natural heredity in such transmission. This transmission by 'Social Heredity' (as we may call the individual's process of learning from society by imitation, instruction, etc.,) is so universal a fact with vertebrates that we may, it seems to me, say at once that the arguments for Neo-Lamarckism drawn by Mr. Spencer and others from the phenomena of human progress, at least, are completely neutralized by them. And there are facts which should show that the same state of things descend below man.

It is very probable, as far as the early life of the child may be taken as indicating the factors of evolution, that the main function of consciousness is to enable him to learn things which natural heredity fails to transmit; and with the child the fact that consciousness is the essential means of all his learning is correlated with the other fact that the child is the very creature for which natural heredity gives few independent functions. It is in this field only that I venture to speak with assurance; but the recognition of this influence has been reached by Weismann, Morgan and others on the purely biological side.

The instinctive equipment of the lower animals is replaced by the plasticity necessary for learning by consciousness. So it seems to me that the evidence points to some inverse ratio between the importance of consciousness as factor in development and the need of the inheritance of acquired characters as factor in development. This presumptive argument may be supplemented, I think, with positive refutations of the considerations which Professor Cope, Romanes and others present for the view that the transmission of functions secured by consciousness requires the Lamarckian factor.[1]

The examination of the biological evidence just cited by Mr. Cope in support of Neo-Lamarckism I am not competent to make; but there is present another distinguished biologist, Prof. Minot, from whom I hope we may hear.

There is one omission in Professor James' excellent division of our topic into its members—an omission whose importance may justify my bringing up a phase of the general question to which I think too much importance can hardly be attached. It is, in biological phrase, the *ontogenetic* question, the examination of development of consciousness in the individual, with a view to the generalization of results and their application to race-development. Professor Cope's emphasis on consciousness rests here, and it is well placed. In the life history of the organism we have the problem of development actually in a measure solved before us. The biologist recognizes this in his emphasis on embryology and also to a degree in his paleontology. But the psychologist has not realized the weapon he has both for biological and for psychological use in the mental development of the child. Moreover the biologist no less than the psychologist must needs resort to this field of investigation if he would finally settle the function of consciousness in evolution. The fossils tell nothing of any such factor as consciousness. Nor does the embryo. So, as difficult as the ontogenetic question is, it is one of the really hopeful fields on both sides. I may be allowed, therefore, to give a brief summary of certain results reached by this method in my own work; especially since it will set out more fully, even in its defects and inadequacies, the general bearing of this problem.

That there is some general principle running through all the conscious adaptations of movement which the individual creature makes is indicated by the very unity of the organism itself. The principle of Habit must be recognized in some general way which will allow the organism to do new things without utterly undoing what it has already acquired. This means that old habits must be substantially preserved *in the new functions;* that all new functions must be

[1] See my articles on *Heredity and Instinct*, *Science*, March 20 and April 10, '96; Prof. Cope's reply and my further note may be found in the *Amer. Naturalist*, April and May, '96.

reached by gradual modifications. And we will all go further and say, I think, that the only way that these modifications can be got at all is through some sort of interaction of the organism with its environment. Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult—by the selection of fit movements from excessively produced movements, *i. e.*, from *movement variations.* So granting this, we now have the further question: How do these movement variations come to be produced *when and where they are needed?*[1] And with it, the question: How does the organism *keep those movements going* which are thus selected, and *suppress* those which are not selected?

Now these two questions are the ones which the biologists fail to answer. And the force of the facts leads to the hypotheses of 'conscious force' of Cope, 'self-development' of Henslow, and 'directive tendency' or 'determinate variation' of the American school—all aspects of the new vitalism which just these questions and the facts which they rest upon are now forcing to the front. Have we anything definite, drawn from the study of the individual on the psychological side, to substitute for these confessedly vague biological phrases? Spencer gave an answer in a general way long ago to the second of these questions, by saying that in consciousness the function of pleasure and pain is just to keep some actions or movements going and to suppress others. The evidence of this seems to me to be coextensive, actually, with the range of conscious experience, however we may be disposed to define the physiological processes which are involved in pleasure and pain. Actions which secure pleasurable conditions to the organism are determined by the pleasure to be repeated, and so to secure the continuance of the pleasurable conditions; and actions which get the organism into pain are by the very fact of pain suppressed.

But as soon as we enquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested to the first question also, *i. e.*, the question as to how the organism comes to make the kind and sort of movements which the environment calls for

[1] This is just the question that Weismann seeks to answer (in respect to the supply of variations in forms which the paleontologists require), with his doctrine of 'Germinal Selection' (*Monist*, Jan., 1896). Why are not such applications of the principle of natural selection to variations *in the parts and functions of the single organism* just as reasonable and legitimate as is the application of it to variations in separate organisms?

—the movement-variations when and where they are required. The pleasure or pain produced by a stimulus—and by a movement also, for the utility of movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose: this is disputed no less by the infant's movements than by the actions of unicellular creatures. There are characteristic differences in vital movements wherever we find them. Even if Mr. Spencer's undifferentiated protoplasmic movements had existed, natural selection would very soon have put an end to it. There is a characteristic antithesis between movements always. Healthy, overflowing, favorable, outreaching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which a theory of the selection of movements from overproduced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give excess movements, from which new adaptations are selected; and that decreased vitality represented by pain should to the reverse—draw off energy and suppress movement.

If, therefore, we say that here is a type of reaction which all vitality shows, we may give it a general descriptive name, *i. e.*, the 'Circular Reaction,' in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. The term 'circular' is used to emphasize the way such a reaction tends to keep itself going, over and over, by reproducing the conditions of its own stimulation. It represents habit, since it tends to keep up old movements; but it secures new adaptations, since it provides for the overproduction of movement-variations for the operation of selection. This kind of selection, since it requires the direct coöperation of the organism itself, I have called 'Organic Selection.' It might be called 'motor' or even 'psychic' selection, since the part of consciousness, in the form of pleasure and pain, and later on experience generally, intelligence, etc., is so prominent.[1]

[1] See Chap. VII. on 'The Theory of Development' in my *Mental Development in the Child and the Race* (2d ed., 1895). I have prepared a new chapter (XVI.) for the German and French editions of this work, incorporating the positions which this view of ontogenetic development leads to in respect to heredity, as suggested in the article referred to in *Science*. It will appear as an article in the *American Naturalist* for June, 1896. It secures determinate variations in phylogeny, without the inheritance of acquired characters.

This is a psychological attempt to discover the method of the individual's adaptations; it has detailed applications in the field of higher mental process, where imitation, volition, etc., give direct exemplifications of the circular type of reaction. But if the truth of it be allowed by the biologist for the individual's development, it follows from the doctrine of recapitulation that this type function shall run through all life. This would mean that something analogous to consciousness (as pleasure and pain, etc.,) is coextensive with life, and that the vital process itself shows a fundamental difference in movements—analogous to the difference between pleasure-incited and pain-incited movements. The biologist may say that this is too special—this difference of reaction—to be fundamental; so it may be. But then so is life special, very special!

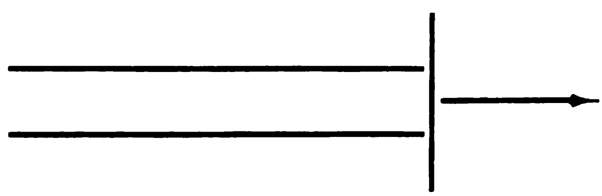
Whatever we may say to such particular conclusions, they illustrate one of the topics which should be discussed by anyone, biologist or psychologist, who wants to find all the factors of evolution. There are some factors revealed in ontogenesis which do not appear in the current theories of phylogenetic evolution. Indeed, so far beside the mark are the biologists who are discussing heredity to-day that they generally omit—except when they hit at each other—the two factors which the psychologist has to recognize; Social Heredity, for the transmission of socially-acquired characters, and Organic Selection, for the accommodations of the individual organism, and through them of 'determinate variations' in phylogeny.

Indeed, I do not see how either theory of heredity can get along without this appeal to ontogenesis. For if we agree in denying the inheritance of acquired characters, thus throwing the emphasis on variations, still it is only by the interpretation of ontogenic processes and characters that any general theory of variations can be reached. Either experience causes the variations, as one theory of heredity holds; or it exemplifies them, as the other theory holds; in either case, it is the only sphere of fact to which appeal can be made if we would understand them. So why do biologists speculate so long and so loud on the question of the mode of transmission, when the question of the mode of acquisition is so generally neglected by them?

The only additional point which I may claim a little time to speak of is that to which Professor James referred in describing the current doctrines of the relation of mind and body. He described the view that consciousness does not in any way interfere with the activities of the brain, as the 'automaton theory,' and spoke as if in his mind a real automatism—a view which considered the brain processes as the sufficient statement of the causes of all voluntary movement—was the outcome of any denial of causal energy in consciousness. In other words that there is no alternative to what is called the epi-phenomenon theory of consciousness except a theory holding that the law of conservation of physical energy is violated in voluntary movement.

Now this reduction of the possible views to two is, in my view, unnecessary and, indeed, impossible. In speaking of the antecedents of a voluntary movement we have to consider the entire group of phenomenal events which are always there when voluntary movement takes place; and among the phenomena really there the conscious state called volition is really there. To say that the same movement could take place without this state of consciousness is to say that a lesser group of phenomenal antecedents occurs in some cases and a larger group in other cases of the same event. Why not go to the other extreme, and say that the brain is not necessary to voluntary movement, since volition could bring about the movement without using the nervous processes to do it with? In his posthumous book on *Matter and Monism*, the late Mr. Romanes brings out this inadequacy of the automaton view, using the figure of an electro-magnet, which attracts iron filings only when it is magnetized by the current of electricity. Whatever the electricity be, the magnet is a magnet only when it attracts iron filings; to say that it might do as much without the electricity would be to deny that it is a magnet; and the proof is found in the fact simply that it does not attract iron-filings when the current is not there. So the brain is not a brain when consciousness is not there; it could not produce voluntary movement, simply because, as a matter of fact, it does not. So consciousness does not, on the other hand, produce movement without a brain. The whole difficulty seems to lie, I think, in an illegitimate use of the word 'causation.' Professor Ladd seems to me to be correct in holding that such a conception as physical causation can not be applied beyond the sphere of things in which it has become the explaining principle, *i. e.*, in the objective, external world of things. The moment we ask questions concerning a group of phenomena which include more than these things, that moment we are liable to some new statement of the law of change in the group as a whole. Such a statement is the *third alternative* in this case; and it is the problem of the metaphysics of experience to find the category, or the most general principles of experience as a whole, both objective and subjective. This I do not care to discuss, but I am far from thinking that the automaton or epi-phenomenon man can argue his case with much force in this higher court of appeal.

The other extreme is represented by those writers who think that the revision of the law of causation can be made in the sphere of objective phenomenal action represented by the brain; and so claim that there is a violation of the principle of conservation of energy in a voluntary movement, an actual efficiency of some kind in consciousness itself for producing physical effects. This is as illegitimate as the other view—is it not? It seems to deny the results of all objective empirical science and so to sweep away the statements of law (on one side) on which the higher interpretation of the group of phenomena as a whole must be based. And it does it in favor of an equally empirical statement of law on the other side. I do not see how any result for the more complex system of events can be reached if we deny the only principles which we have in the partial groups. To do so is to attempt to interpret the objective in terms of the subjective factor in the entire group; and we reach by so doing a result which is just as partial as that which the epi-phenomenon man reaches in his mechanical explanation. Lotze made the same mistake long ago, but his hesitations on the subject showed that he appreciated the difficulty. I agree with these writers in the claim that the mechanical view of causation can not be used as an adequate explaining principle of the whole personality of man; but for reasons of much the same kind it seems equally true that as long as we are talking of events of the external kind, *i. e.*, of brain processes, we can not deny what we know of these events as such.



The general state of the problem may be shown by the accompanying diagram, which will at any rate serve the modest purpose of indicating the alternatives. The line above, of the two parallels, may represent the statements on the psychological side which, on the theory of parallelism, mental science has a right to make; the lower of the parallels, the corresponding series of statements made by physics and natural science, includes the chemistry and physiology of the brain. Where they stop an upright line may be drawn to indicate the setting of the problem of interpretation in which both the other series of statements claim to be true; and the further line to the right then gives the phenomena and statements of them which we have to deal with when we come to consider man as a whole. Now my point is that we can neither deny either of the parallel lines in dealing with the phenomena of the single line to the right, nor can we take either of them as a sufficient statement of the farther problem which the line to the right proposes. To take the line representing the mechanical principles of nature and extend it alone beyond the upright is to throw out of nature the whole series of phenomena which belong in the upper parallel line and are not capable of statement in mechanical terms. And to extend the upper line alone beyond the upright is to allow that mechanical principles break down in their own sphere.

As to the interpretation of the single line to the right, it may always remain the problem that it now is. The best we can do is to get points of view regarding it; and the main progress of philosophy seems to me to be in getting an adequate sense of the conditions of the problem itself. From the more humble side of psychology, I think the growth of consciousness itself may teach us how the problem comes to be set in the form of seemingly irreconcilable antinomies. The person grows both in body and mind, and this growth has to have two sides, the side facing toward the direction from which; the 'retrospective reference,' and the side facing the direction toward which, the 'prospective reference' of growth and the consciousness of growth. The positive sciences have by their very nature to face backwards, to look retrospectively, to be 'descriptive,' as the term is used by Professor Royce—these give the lower of our parallel lines. The moral sciences, so-called, on the other hand, deal with judgments, appreciations, organizations, expectations, and so represent the other, the 'prospective' mental attitude and its corresponding aspects of reality. This gives character largely to the upper one of our parallel lines. But to get a construction of the further line, the one to the right, is to ask for both these points of view at once—to stand at both ends of the line—at a point where description takes the place of prophecy and where reality has nothing further to add to thought. I believe for myself that the best evidence looking to the attainment of this double point of view is found just in the fact that we are able to compass both of these functions in a measure at once; and that in our own self-consciousness we have an inkling of what that ultimate point of view is like.[1] I do not mean to bring up points in philosophy;

[1] I may refer to the extended use made of this general antithesis in my paper in this REVIEW for November, 1895, and to the philosophical considerations based on it by Mr. W. M. Urban in the number of January, 1896.

but it is to me the very essence of such a contention in philosophy that it is a comprehension of both aspects of phenomenal reality and not the violation or denial of either of them. J. MARK BALDWIN.

PRINCETON.

PAIN NERVES.

That specific nerves of pain have at last been established with a certainty fully equal to that for any of the other dermal nerves is an event, for psychology, of the first magnitude. Considering the rôle that traditional pain-pleasure dogmas have played in fundamental conceptions of mind, in ethical theories, and in philosophic deductions, it is perhaps not too much to say that this event is one of the most important determinations happening within the epoch of Modern Psychology.

I refer to the demonstration of pain-nerves through clinical evidence by Dr. Henry Head, of University College Hospital.[1] To many the revolution in conceptions which this work must necessitate will cause bewilderment, and perhaps also a lingering skepticism. For it was but a few months ago that Dr. Strong presented to the public his reports[2]—which from their grave judicial tone had quite the appearance of being official—assuring us that according to his summary of the evidence the existence of special pain-nerves was '*more* than doubtful;' which, of course, from this accurate writer could alone mean that they were no longer possible. Yet at the very time of Dr. Strong's writing (1895) the magnificent report of Dr. Head, which must set this dispute at rest forever, had been nearly two years in print in the official journal of Neurology for the English Language, and had been twice read in public the year previous (1892).

The proof which Dr. Head's work offers for separate pain-nerves rests on clinical demonstration that the skin of the body is divided into definite zones of nerve-supply for pain, which zones do not correspond to the zones of nerve-distribution for touch. These zones for pain are coextensive with those for heat, cold and trophic nerves, and all of these four kinds of nerves (pain, heat, cold and trophic) supplying any given zone have common origin in a *single* corresponding segment of the cord. In other words, each segment of the cord has its own zone of distribution for these four kinds of nerves. These zones are sharply

[1] Disturbances of Sensation with especial reference to the Pain of Visceral Disease. By Henry Head, M. A., M. D. *Brain*, 1893, p. 1, and 1894, p. 339.

[2] PSY, REV., March, 1895, p. 44, July, 1895, and January, 1896.

separate, do not overlap, and do not correspond to the zones of distribution of the touch-nerves. As is well known, the distribution of the touch-nerves had been previously traced with great accuracy from the posterior roots, where they are gathered from *several* segments of the cord, to peripheral zones, which markedly overlap or interlace for the respective nerve-roots. As a consequence of these facts: (a) that the zones of distribution for pain, heat, and trophic nerves cover markedly different fixed areas of the skin from the zones of distribution of of the touch-nerves; (b) that the former zones do not overlap one another, while the touch-zones do overlap one another; and (c) that the pain, heat, cold, trophic zones are each supplied by nerves having origin in a single segment of the cord, while the touch-zones are supplied by nerves having origin in several segments—from these facts results follow which demonstrate the existence of separate nerves for touch, pain, heat and cold-sensations with something very near to certainty.

No less significant, as the title to Dr. Head's papers suggest, is the relation of these peripheral pain-zones to the distribution of nerves in the viscera. In a word, the different viscera are supplied with nerves from definite segments of the cord. As a consequence, disturbances in the different viscera cause excitations to pass along these nerves to their respective segments in the cord; produce hyperalgesia for all the pain-nerves having origin in the segments so affected; and their pain-sensations become 'referred' or reflected to the dermal pain-zones corresponding to their segments. A large part of the papers are taken up with demonstration of the zones of 'dermal tenderness,' *i. e.*, painfulness, which are exhibited in various visceral disorders.

It would be inadmissible here to give even enumeration to the long list of visceral, spinal and dermal disorders which Dr. Head marshals into line with his remarkable discovery. Suffice it to say that separate nerves of pain are placed beyond reasonable doubt, and the multitude of heretofore inexplicable cases of the loss or the exhaltation of any one of the functions of touch, heat, cold, touch-pains, heat-pains and cold-pains, or of any sort of partial combination of these independently from the remainder (such as were quoted by Dr. Strong *against* pain-nerves), receive explanation upon the basis of separate nerve-fibres for each of the six separate kinds of sensations.

This much being determined three lines of investigation remain to be cleared up before the subject of pain-nerves shall be complete. These have reference to the *end-organs*, and modes of stimulation for the different sources of pain (mechanical, chemical, thermal). The mode by which pain is *conducted through the cord.* And the *cortical localization* of pain.

Regarding the first of these, we are perfectly in the dark as to the ultimate relations of stimulus, end-organ and nerve-impulse for *all* sensory nerves. It is not surprising, therefore, that the discovery of Dr. Head leaves us as ignorant of the means by which mechanical pressure, heat and cold respectively affect the pain-fibres as we are of how they respectively affect the touch, the heat and the cold-fibres. Since, however, it is now certain that the nerves of pain are separate from those of touch and from those of heat and cold, it is evident that there is identically the same grounds for expecting different end-organs as between touch-pains and temperature-pains as for expecting specific end apparatus for any kind of sensory fibres. It may be that light acts directly on the optic nerves, and temperature directly on all sorts of temperature-nerves. If so we should not require three different 'sets' of pain-nerves for the three different mode of pain stimulates, *i. e.*, pressure, heat and cold. Right or wrong, however, the prejudice of science at present runs in favor of specific end organs for most if not all of our sensations and so strongly for sensations of heat and of cold; and now, knowing that the distribution of pain-nerves coincides with that of the heat and cold-nerves, and does not coincide with that of touch, it seems more necessary than ever to expect different end-organs for heat-pains, and cold-pains (though these may be identical with the end-organs for heat and cold-sensations) in order to explain the cases cited by Dr. Strong of hyperalgesia to temperature in the midst of analgesia to mechanical pressure, *i. e.*, to explain the very cases on which, apparently, he rests his entire opinion. Under this head also, in order to clear the field of a confusion, as it seem to me, quite unnecessarily raised by Dr. Strong, I must humbly decline his flattering imputation of superior erudition on this subject, and declare that I know of no literature, certainly none of my own writing, which has ever in the remotest way suggested 'three distinct sets of pain-nerves' if by 'sets' is implied any requirements for additional 'sets' for 'muscular pains, colics, toothaches, etc.' Of course Dr. Strong's suggestion to this effect is graceful from the literary standpoint, and entertaining to 'the galleries,' but was it worth while deliberately to mislead for the sake of being facetious regarding a matter of scientific probability that now turns out to be next door to a certainty? If it prove true that there be different end-organs for heat-pains and cold-pains, still no sober man would speak of separate 'kinds' of pain-*nerves* for this reason, any more than he would speak of different 'kinds' of touch-nerves for the reason that certain touch-nerves have apparently free endings while others have different kinds of touch-corpuscles.

The second line of investigation concerns the mode of transmission of pain-impulses in the cord. And here again I must beg Dr. Strong patiently to extend his courtesy toward me, while I make plain wherein Prof. Wundt's theory of this subject does require a more complicated mode of transmission than is necessary or likely. In the first place, Dr. Strong jumps quite unwarrantably to the conclusion that if there be separate pain-fibres from the periphery to the cord, then these must continue through the cord. I, for one, hold it to be probable that such is not the case for the entire cord, though it is likely to hold good for the single segment into which the pain-nerves enter. For the greater portion of the cord it is probably true that the pain-impulses are transmitted from segment to segment rather than by continuous paths throughout, and the reason for this, when fully explained, is likely to prove one of the most instructive evidences of nerve-evolution in the range of anatomy. That there should be separate pain-paths for touch-pains, heat-pains and cold-pains from the periphery to the cord, and a single common path for pain thence onward to the brain, is, however, far and away a simpler requirement than a 'shunt' arrangement in the cord attached to common paths for pain and other sensations between the cord and the periphery, as Prof. Wundt proposes and Dr. Strong accepts. It would require a wonderful distribution of 'lesions' indeed, for the various phenomena falling under Dr. Head's list of disorders, to explain them on Dr. Strong's plan. And the simplicity of the conduction without 'shunts' is so obvious above that of conduction with shunts that Dr. Strong, I trust, will now feel relieved from all embarassment against undue prodigality of Nature, without further comment.

The third line of investigation, that of cortical localization of pain is of no less importance than the others and is receiving considerable attention among scientists, which is sure to be greatly stimulated by Dr. Head's discovery.

Incidentally, I may remark that Dr. Head's papers make it doubtful if the viscera are capable of sending any impulses to the cortex save through the common pain-path of the cord, the vagus, and the paths of the sympathetic system; and from the close alliance of these sources, it seems likely that the viscera are capable of no direct sensory response save one of pain; all of which is in accord with the summary of experimental and clinical evidence already cited by Foster on this point. In face of this it seems more obscure than ever how holders of the James-Lange Theory of Emotions are to explain emotions of *joy* from visceral reverberations capable alone of direct *pain*-responses. And in proportion as the James-Lange theory goes down, by reason of the evidence from Dr. Head's remarkable paper, will the Instinct-Innervation Theory of Emotions, which I presented in the Philosophical Review (September, 1895), become more plainly true.

In addition to this major evidence for pain-nerves I must mention as also apparently overlooked by Dr. Strong, the papers of Dr. von Frey, of Leipzig, which may claim independently to have demonstrated the existence of pain-nerves[1]. They throw much less light on pain-distribution and pain-conduction in the cord, and I have therefore confined myself to a report of Dr. Head's work. The existence of specific pain-nerves, however, now stands upon abundant evidence sufficiently independent in source and sure in substantiation to convert the most fastidious from the time-honored superstitions.

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THE RELATION BETWEEN PSYCHOLOGY AND LOGIC.

The influence of new tendencies in psychology is becoming more and more visible in the field of logic. One need only turn a few pages in the more recent books on logic to mark what a transformation has taken place since the days when Kant could say that the science had come from Aristotle's hands practically a finished work. Evidently, the Aristotelian Logic is now generally refused recognition as a completed science, for dissatisfaction is shown in various ways; one and another Aristotelian distinction is neglected, and it is pointed out that the complicated facts of actual judgment and argument cannot be cramped into the narrow mould of the ancients. The widened knowledge we have of the diversity of thought, of individual differences, of new methods of procedure in the modern sciences, of peculiarities of thought brought to light by a comparative study of languages, this new material, it is said, requires a recasting of the older logic. The older logic, they tell us, was based on an older psychology, and, as a consequence, no longer fits the known facts. For logic must

[1] Two of these papers also appear to have been in print a year previously to Dr. Strong's research. Berichten d. math. phys. Classe d. Königl. Sach. Gesellschaft der Wissenschaften zu Leipzig. 1894, pp. 185 and 283; 1895, p. 166.

be readjusted to the results of modern psychology; or better, logic is actually a department of psychology, and must grow with the growth of this.

But, as we might expect, psychology itself has been effected by the change it is producing in logic. If logic is the psychology of judgment and reasoning, the consideration of these processes may be omitted from psychology taken in the narrower sense. Only when we seek to state some general theory to cover all mental complexes need we glance over the whole field, and point out that our explanatory principle holds wherever we go; in such cases judgment and reasoning get some attention, even in strict psychology. But we find a strong tendency to turn over all the manifold details of these processes to logic. The number of pages given to judgment and reasoning in the books on psychology becomes less and less, though the books themselves grow ever larger. Of course, the rise of the experimental side in psychology has had much to do with this change of proportion; the new methods have found readier application in the field of sensation and perception, and consequently have swelled the corresponding chapters with a mass of new material. But, apart from this, there is doubtless a conscious withdrawal from the field of judgment and reasoning, on the ground that the matter is already dealt with in a psychological way in logic. Logic has become more and more a psychology of judgment and reasoning, while psychology in the exact sense is more and more restricted to the less complete processes of mind.

To many this will seem a happy division of labor. Psychology, they will say, can become a more exhaustive account of the other functions of the mind if it is relieved of a special treatment of judgment and reasoning, while the latter will receive more thorough treatment when marked off as the matter of a special science. It would, therefore, seem an advantage to both sciences to adopt such a basis of distinction.

If it were merely a question of nomenclature or of division of labor there certainly would be no objection to this. But the apparent advantages of this settlement should not close our eyes to the theoretical error upon which it is based. The proposed division really interferes with the proper work of each of these sciences.

In the first place, the rigidly psychological treatment of judgment and reasoning is endangered when turned over to the care of those whose main interest is in the logical aspect of the case. And, on the other hand, the problems of logic suffer violence when once we begin to treat them as purely psychological problems. For the problems arising in the two sciences are vitally different, and are to be solved by different methods. The answer to the psychological questions regarding judgment and reasoning is not in itself an answer to the logical questions involved; the very kernel of the logical problem will have been left untouched. Nor is the solution of the strictly logical problem any solution of the psychological problem in the case. Logic and psychology deal with the same materials, within certain limits; but in working up the materials there is in each of these sciences a different end in view, and a different method of procedure. The findings of both are necessary to the body of knowledge; consequently it is idle, and only brings confusion, when we try to substitute the results of the one for those of the other. But when we see that each, though a work in the same field, is a different and indispensable work, such an attempt at substitution will no longer be made. There cannot then be any clash between these two different interests.

The divergent aims of the two sciences may be succinctly expressed, perhaps, as follows: Psychology is an effort to state the *natural causes* of the various mental occurrences. Analysis, classification, and even description, all of which the history of psychology shows to have played such important parts, we must view as but means to the great end, which is explanation. Under what causal circumstances, we ask, does such or such a mental fact arise? Under what circumstances does the experience undergo change? What are the conditions that cause its disappearance? The main question is entirely regarding matters of fact: What is the actual causal order or connection in the mental life?

Logic, on the other hand, is not a search for the causes of mental occurrences, but, rather, an attempt to develop a *principle of criticism.* In logic, we assume the facts of reasoning, and proceed, not to explain, in the scientific sense, but to set forth the abstract marks which distinguish the consistent from the inconsistent. What relations must there be among the premises, and what between the premises and the conclusion—such are the questions asked—if the conclusion is to be justified by the premises? What sort of procedure is required if the procedure is to justify the outcome? Strictly speaking, in logic we ask not a word as to what the causes are that actually produce conclusions; nor as to what the various influences are that give to some mental facts one character, and to others another. The *marks* of the one character or of the other are set forth in logic, but the marks of a given character are not the *causes* of that character. It does not fall within the province of logic to ask what the scientific explanation of fallacy is, or what a similar explanation of consistent reasoning is.

The moment we ask such questions we turn aside from the proper and vital problem of logic, and for the time become interested in psychology; for the questions mentioned are questions of psychology. The problem of logic is to present in full the system of inner relations by which consistent, cogent, 'logical' thinking is distinguished from the loose, fallacious, 'illogical' sort.

It is possible, however, by a little effort to state the problem of logic so that it will seem to fall within the general limits of pscyhology. We might say, for instance, that psychology is the search for the conditions of mental occurrences in general, while logic is the search for the conditions of the particular occurrence called reasoning. But if it were meant by this that logic deals with reasoning in exactly the same way that psychology deals with the other mental occurrences, then we should have no science that presents the detailed criteria by which logical reasoning is recognized and distinguished from illogical reasoning; and yet such a science is necessary, and is historically to be identified with logic. For psychology does not supply the need here. The grounds for the distinction between good reasoning and bad reasoning, in the logical sense, psychology would have to accept from without, just as it must accept from without the bases for the distinction between a moral state of consciousness and an immoral. Psychology, after such acceptance, may go on to investigate the psychological differences, if there be any, that accompany these distinctions; that is to say, we may ask whether an illogical conclusion has any psychological difference from a logical one, or what are the psychological causes of morality or of immorality. But psychology must always presuppose the system of criteria by which such distinctions are made, and for the sake of exactness must require that these criteria be presented with scientific elaboration. The statement of the *psychological* causes of moral action would not be ethics, nor would the statement of the *psychological* causes of correct reasoning be logic. Instead of logic, we should have under psychology a presentation of the various influences that permit the correct reasoner to thread his way past all the possibilities of fallacy, and to land safe on the right conclusion. But, as I have said, we could only make such an investigation after we knew how to recognize correct reasoning; and if the recognition is to be anything more than haphazard and naïve, there must first have been developed a system of logic. For we should need some test of the various relations which constitute the evidences that the consistent is consistent; that is to say, we should need a critical decision as to what the requirements of consistency are. What are the postulates, we must always ask, of this ideal that we call logical unity?

But since, in answering such a question, we do (in a way) give the internal web and woof of the phenomena called consistent reasoning, we do seem for the time to be working, though not in explanatory psychology, at least in descriptive; and thus logic would appear to be a part of an auxiliary subdivision of psychology, grant as much as one may that description is subordinate in importance to explanation. Yet a little reflection will bring out a wide difference even here. For, in carrying out a descriptive psychology of judgment and reasoning, we should inevitably get interested in *all* phases of these facts—in the possible changes in the distinctness or the intensity of the *Vorstellungen* as the processes developed; in the tone of feeling accompanying the movement, and changing, say, with different rates or arrangements; in the time aspects of the mental act and its parts, and in the order of succession of the parts. For the purposes of logic, however, we are indifferent to all these things. And rightly so, because they have no bearing on the problem in hand. So far as the logical worth of a proposition or of a train of reasoning is concerned, it makes no difference whether the filling of the mental presentations is auditory or visual; whether the presentations are more intense or less so; whether the process is accompanied by this feeling or by that, or by no feeling at all; whether a given part occupies more time or less time than certain others; or, finally, whether the conclusion comes first, last or in the middle. In logic we can afford to neglect all these as irrelevant to the work in hand, and actually do neglect them without loss. But we cannot complete the work of descriptive psychology without attending to them all. In logic we go far enough with psychology to get materials for the special criteria desired; but in our choice of what we will attend to, we bring out clearly how different the aim of the one science is from that of the other.

The attempt to state the problem of logic so as to make it fall within the field of psychology would end, then, in missing the very heart of the logical problem. And if we should undertake enlarging the bounds of psychology so as to include the logical problem we should bring into psychology a discordant element and destroy the unity of its aim.

To make it perhaps clearer how essentially different the interests of the two sciences are, one need only recall the actual details that each science respectively admits as pertinent to its purpose. It will then be seen that certain combinations perfectly admissible in psychology are not so in logic; and, on the other hand, what we might judge to be relevant from the standpoint of logic we should condemn from the standpoint of psychology. In logic, for instance, it is truly said that every judgment presupposes sufficient premises, and that given premises necessarily lead to a conclusion just such and so. But in psychology it is just as truly said that in living experience we make judgments without any premises whatever, to say nothing of adequate ones, and that premises which in logic lead to the conclusion 'Caius is mortal,' in Q's process of consciousness lead to no conclusion at all, or possibly to the conclusion that 'Caius isn't mortal.' From the point of view of psychology all such experiences are as interesting and respectable as the logically faultless are, and we assume that they are completely explicable were the constitution of mind once fully understood.

On the other hand, according to logic, certain psychic collocations are declared to present an intimate and faultless unity, when, according to psychology, they utterly lack connection. We may suppose, for example, the case of three judgments in the form of a logically valid syllogism, which occur in a certain person's consciousness in the temporal order of (1) major premise; (2) minor premise, and (3) conclusion. From the standpoint of logic we should see in this an example of perfect conformity to law should hold that the premises *led* to the conclusion and that the conclusion was *grounded* on the premises. But, psychologically, there may have been an utter absence of causal connection between premises and conclusion; other factors, we may suppose, called up the conclusion at the happy moment; somebody whispered it in the person's ear or the sight of a book brought back the judgment in isolation from yesterday's reverie. We should then have two causal trains of activity, to be represented, perhaps, by the two columns below, in which each item is caused by the item immediately above it, the different levels representing differences of time.

a	
c	
d	
major premise	
minor premise	t
	conclusion
	v
	w

The two trains are here so timed that the conclusion comes in the second just at the very moment when it would have come if causally included in the other series, yet its cause does not lie there. But this fact would be no objection to the syllogism from a logical point of view; the syllogism, as a *syllogism*, is not concerned with such a fact. To the syllogism the three judgments are simply in the peculiar relation which the logical standards require. But from the point of view of psychology, we should have to declare that in this case neither the temporal sequence nor the conformity to the logical norm is sufficient to meet the special requirements; the absence of causal connections, in the sense meant by natural science, is fatal. Consequently, when examining these items with the interests of a psychologist, we should note a much closer connection between 'conclusion' and 't;' while, for logical purposes, the line of combination runs from 'conclusion' directly back to 'minor premise' and 'major premise,' leaving out the natural cause of 'conclusion' (namely, 't') as of no interest.

The two sciences thus present different and distinct standards of worth. For logic those combinations are good, the parts of which are *related in accordance with what we call logical norms.* For psychology those combinations are good, the parts of which are *causally connected.* As said above, the whole machinery of psychology is contrived for the purpose of *explanation;* while the aim of logic is to present *a critical canon.* In psychology the question is, What has produced the given facts? In logic it is, rather, Are the facts a justifiable combination, and why?

There is hardly any need of saying that each of these sciences has a right to its own special aim. The work of psychology does not make useless or superfluous the work of logic, nor can we substitute the results of logic for those of psychology. We need a *psychology* of judgment and reasoning, and also a *logic* of these processes, each science existing without prejudice to the other. For when we have decided what causal relation exist among mental occurrences we have settled nothing as to what forms of combination satisfy our logical needs. And, on the other hand, the decision that such and such combinations are *logically* necessary (*i. e.*, exist *de jure*), of course settles nothing as to what the combinations are *de facto*, nor as to the causes of these combinations.

Simple as this truth is, it is not always borne in mind by those who write on these sciences. There is frequent evidence of hazy or ill-observed boundaries; as when psychologists incline to leave part of their subject untouched, or when logicians alone are found treating of certain problems that are really psychological. Certainly there is no absolute objection to inserting in books on logic much that by nice distinction belongs to another science; this is a matter of expediency, and not to be decided by rigid definition. It would be well, however, always to make clear when we are within the strict domains of the science, and when we are digressing into attractive neighboring fields. Questions, for instance, as to the genetic relation between judgment and concept—whether judgments are developed out of concepts, or the reverse; as to the temporal order of premises and conclusion; as to whether we actually quantify the predicate (this is carefully to be distinguished from the question as to the logical importance of such a quantification, which is a question of logic)—such questions are usually discussed exclusively in the logics, and yet they are in fact psychological problems, and are to be settled, if at all, by the methods of psychology.

It would seem, then, to be in the interest of better logic and of better psychology to have more definite bounds set up between them. For many a psychological problem fails to get proper psychological treatment because, by reason of defective definition, it seems to be merely a logical problem; and many of the foundation-truths of logic, for a parallel reason, have appeared to lack validity because shown not to be psychological laws. Such errors would, of course, be impossible were the real basis of distinction between the two sciences once clearly seen and settled. GEORGE M. STRATTON.

LEIPZIG.

THE TESTIMONY OF HEART DISEASE TO THE SENSORY FACIES OF THE EMOTIONS.

What in ordinary parlance, as indeed in most psychological discussions, is termed emotion, is in reality a very complex activity. It is perhaps only in pathological states that the elements are analyzed by the falling out or suppression of certain elements. This analysis may be made in the case of fear. As a rule in the normal state, we have in fear a very vivid and attention-compelling concept of the fearful object, together with a more or less distinct representation of the fate of which we are apprehensive.

Perhaps in the majority of cases these elements usurp the prominent place in the complex, yet it is evident that neither of them is fear or emotion of any kind. We also usually have a more or less definite, if only implicate judgment of the reason for fear, but this is, of course, no more fear than the judgment that if our body is unsupported it will fall.

A vivid reproduction or imagination of an event of a disastrous kind is quite sufficient, in my own case, to produce the physical and mental symptoms of fear. These symptoms are, in the one case, sundry muscular contractions of a spasmodic nature, which may have a more or less distant relation to the fearful event, or the still more distant associational connection seen in expression of emotion of displeasure, or the wholly unassociated innervation of excited attention. Most writers make too little of this class of effects of emotional impulse, *i. e.*, of the state of general innervation in suspense, which has no association with any special purposive act, but which is a sort of preliminary tension (Spannung) preparatory to any possible impulse. In the other or mental domain these symptoms are obscure, that is unlocalized (but not therefore weak) sensations of innervations, but more particularly of vascular disturbance.

Entirely secondary, but often appearing more conspicuous, because localizable, are peripheral sensations. It may be shown that the real core of the fear is in the sensations of vascular change. It is perhaps idle to inquire whether the source of the disturbance is in the vascular change or whether it arises in the medulla, where its nervous center is situated. When an object of apprehension is imaged to consciousness it is certain that the vaso-motor center is affected and those circulatory changes characteristic of fear are produced. If this be not the case I may still view the serried ranks approaching and hear the horrid din of battle and may be fully conscious that any minute may stretch me on the ground mutilated beyond recognition, like a comrade at my feet, but I still have no fear as I calmly serve the gun. On the other hand, as I tread my way through the dense forest and suddenly find myself face to face with a little green snake which I have often handled with impunity—nay with pleasure, every drop of blood seems to stagnate in the heart, and I am a prey to unreasoning and unreasonable fear. It is, however, the pathological states of heart disease that are most conclusive. The irritable heart of neurasthenia affords proof of the connection of the sensation of fear with irregularities of the circulation. Thus, after a fatiguing day one falls asleep and rests quietly for several hours, then on awakening feels no pain or inconvenience of any kind, but soon finds his being suffused with what may be called a disassociated sense of fear or anxiety. One seeks for some reason for it in vain. In the earlier instances this disassociated fear soon affects its association with some concept of menacing content, such as that of a previous hemorrhage or the like, or perhaps of some external event, and one is easily persuaded that it was this concept which had, unknown to him, produced the fear. Directly the heart begins to throb and palpitate and the paroxism runs its course, after which the fear disappears. After a time, one comes to recognize the meaning of the feeling of apprehension and, knowing its relative insignificance, calmly analyzes the state as he awaits its culmination. "There is nothing to fear—I shall be all right in ten minutes—there is no pain," etc., but all the while the fear is there. If one succeeds in preventing the erroneous association he escapes the secondary reflex effects of the frightful concept, but the fear remains and only passes away with the paroxism. Anyone who has had this experience can have no doubt of the sensational nature and vasomoter occasion of fears.

The reader may recall the experiments of Mosso which showed that even slight irritations of the skin or sense organs produce contractions of the peripheral vessels, while in painful emotion the vasomotor changes were excessive, and were accompanied by changes in the respiration and muscular tension. Laehr[1] considers that the vascular center controls painful emotion as the cortex serves for the intellect. If the cerebrum has an excitation adapted to produce painful emotion, part of the reaction passes to the vascular center and part to the appropriate muscle centers. If the cerebral action is shunted out in any way, the reaction on the vascular center may be the more intense. He considers that the painful emotions have a transitory value only in the phylogeny and will disappear in the progress of a normal evolution.

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[1] Die Angst., *Berliner Klinik*, 58.

PSYCHOLOGICAL LITERATURE.

Outlines of Psychology. OSWALD KÜLPE. Translated by E. B. Titchener. London, Sonnenschein; New York, Macmillan. 1895. Pp. 462.

The translation of Külpe's *Grundriss der Psychologie* into English calls for some further notice of this already much-reviewed book. Several circumstances have combined to give unusual prominence to this work, of which its real merit and originality are certainly the first. But any writer so well known as Külpe, who, in these days of monographs and special researches, has the courage and the scholarship to venture a general text-book in psychology based upon experimental data, may be sure that his book will receive attention. This work is characterized by thorough and logical treatment of every subject which it undertakes. It is free from any evidence of hasty or superficial work. It summarizes a large amount of experimental research (chiefly German however), and thus becomes an indispensable handbook for psychologists. It is marked also by decided originality both in the division of the subject and in the treatment of special topics. The latter, however, is hardly a merit in a book of this kind. It seems to have been thought by many that this work would serve as a general text-book for classes in psychology, a hope encouraged no doubt by the title, Outlines of Psychology, Based upon the Results of Experimental Investigation. The author's wide departures from beaten tracks are interesting as contributions to psychological literature, but are somewhat too radical to permit the book to be generally used in the above capacity. The same result must follow from the relatively too-exhaustive treatment of some subjects, such as the psychophysical methods and the fusion of tones, to the omission or partial treatment of others, such as habit, instinct, judgment and reasoning. The author appears to have labored so long over fundamental elements and processes that he forgets to make mention of those finished mental products that the average student demands some account of.

One of the best features of the work is the author's clear and simple analysis and classification of the elements of consciousness. There are but three classes, peripherally excited sensations, centrally excited sensations and feelings, or, as we should say, sensations, images and feelings. The prominence given to memory, under the head of centrally excited sensations, and the clear and exhaustive treatment of the problems of reproduction, recognition and association, are most satisfactory. Equally commendable to my mind is his practical suppression of the will as representing any kind of simple conscious content. May one hope that this is the beginning of the end of this common source of confusion and mystery? So called elementary will is resolved by the author partly into certain *feelings* of effort and partly into certain tendinous and articular *sensations*.

The section on the feelings seems rather barren in contrast with that on memory. Feelings are not attributes of sensations, nor functions of sensations, but independent conscious processes. They are not classifiable except as pleasant and unpleasant. They have, like sensations, the attributes of quality, intensity and duration. But their qualities are only two, pleasant and unpleasant. They are investigated by two methods, the serial (Reihenmethode) and the method of expression. The former consists in observing what changes of feeling follow systematic changes of stimulus; the latter consists in observing the effects of feeling in producing changes of pulse, respiration, voluntary movements and changes in the volume of a limb, recorded by the sphygmograph, pneumatograph, dynamometer and plethysmograph respectively. A few results of experiments with these instruments are given, and they furnish, as the author says, about all the experimental material we have for the treatment of the feelings. But these researches suggest to the author the hypothesis that pleasantness is the accompaniment of increased excitability of the cerebral cortex, and unpleasantness the accompaniment of a diminution of the same, but whether this is to be finally reduced, with Meynert, to increased and decreased blood supply and metabolism in the nervous elements, or, with Wundt, to the form of reaction of a special apperception center upon sensory excitations, the author is unable to decide. At any rate, pleasantness and unpleasantness are to be explained by central nervous processes, and not in the common way as mere accompaniments of healthful and harmful stimuli, nor as due to the state of nutrition in the nerves.

The important subject of pain is practically omitted. Two short paragraphs in different places are devoted to it, which do not agree with each other, and one of which, at the author's suggestion, has been rewritten by the translator. In neither text is the author's theory of pain clear. He does not, however, accept special pain nerves and distinguishes pain from its accompanying 'feeling' quality, which leads him in discussing cutaneous sensations to the statement that 'pain is decidedly unpleasant.' Fifty pages are devoted to the feelings, occupied principally with classification, analysis, discussion, criticism and hypothesis, while of these not more than four or five pages are devoted to the actual results of experimental investigation, the latter in fact being almost limited to some experiments with the sphygmograph, etc., and to a few experiments upon the æsthetic results of the division of lines.

The author's position on a few special points may be noticed. On the relation of mind and body he maintains as psychologist the psychophysical 'parallelism' of Wundt. The relations of mind and body are not temporally determined, *i. e.*, causal. But the author's parallelism turns out to be only a half-hearted one, for mind and body are so related that any change in the one 'expresses itself' by a change in the other. In his more recent *Einleitung iu die Philosophie*, where fuller treatment of this subject is permitted, the author bravely takes the dualistic position. Concerning the question of space perception, the author affirms that the origin of the space idea does not belong to psychology. Extension is an attribute of sensation and space is thus an original datum. Any theory which would derive the space idea from experience is impracticable. There is clear and detailed discussion of all the problems of sensations of sight and visual perception. The author criticises the color-sensation theory both of Helmholtz and Hering and regards the theory of Wundt as the most satisfactory. In discussing the intensity of sensations he rules out visual sensations entirely. In sight mere increase of stimulus produces *qualitatively* different sensations of brightness. The author discusses the psychophysical, physiological and psychological interpretations of Weber's law, rejecting the first and not deciding between the other two. Külpe's work is throughout analytical and critical and is not at all an outline of psychology based upon experimental research. But the analysis is careful and the criticism keen. To be sure the criticism is rather of the crushing kind, but the reader soon learns that it is not so annihilating as it first appears, for instance, where he majestically sweeps aside the common dictum that there is nothing in the memory which was not first given in sensation, only to arrive in the end at the not-original conclusion that memory images differ from sensations mainly in intensity and that they contain no qualities not found among the latter.

As regards the translation, Professor Titchener deserves the thanks of English readers for giving them a good idiomatic version of this valuable book. A translation is always easy to criticise, and this one is not free from faults. The rendering is very free, so that in some cases the author's meaning is changed somewhat, and occasionally what is clear in the original is confused in the translation. The translator has made a special study of the English equivalents of German psychological terms and his choices are for the most part good. But his preference for Latin forms produces a somewhat dry and scholastic effect, which makes the English less attractive than it would be with more Saxon forms. 'Colligation' for 'Verknüpfung,' 'limen,' for 'Schwelle,' 'replica' for 'Wiedergabe,' 'limits of stimulability' for 'Reizgrenzen,' 'multeity' for 'Vielheit,' 'modal sensitivity' for 'Sinnesempfindlichkeit' and 'memorial image' for 'Erinnerungsbild' are examples. 'Local signature' for what the translator calls the 'collective' use of 'Localzeichen' is perhaps the worst, although it seems a pity to translate the expression 'Schwelle' by the word 'limen,' when we have a perfect English equivalent.

In its mechanical aspect this book is a sad commentary on English and American book-making as compared with German. The German book is compact, well bound, clearly printed on cream paper, and lies open at any page upon your table. The English book is spongy, loose jointed, printed upon glaring white paper with typographical errors, and yet refuses to be read unless held open by brute force.

UNIVERSITY OF IOWA. G. T. W. PATRICK.

The Number Concept: Its Origin and Development. New York and London. Macmillan & Co., 1896. ($2.00.) By LEVI L. CONANT, PH. D.

Only one exception can be taken to this book—as to its title. The book is not upon the origin of the number concept nor yet upon its development. The book deals with primitive methods of counting and with modes of expressing or registering the results of such counting. The true title would be: 'Numeral Systems (or Number Words), Their Origins and Various Forms.' Since the work actually undertaken is thoroughly and accurately carried out, this matter of title is, perhaps, of little account; yet one who approaches the book expecting to have light thrown upon the psychology of the numerical idea will be struck by the discrepancy between the title and the contents.

This discrepancy is worth insisting upon, because there is possible a psychological inquiry upon an anthropological basis which would agree with the title. The author insists (on pages 2–4) that the question of the origin of number is outside the limits of inquiry, with his title page still staring him in the face! "Philosophers have endeavored to establish certain propositions concerning this subject, but, as might have been expected, have failed to reach any common ground." The context shows that Dr. Conant understands by this subject the old controversy as to whether numerical judgments are *a priori* or the result of experience. He is quite right in ruling out this topic from an anthropological investigation, and confining himself to the simple statement that all primitive societies reveal that they have some, however crude, sense of number. But this is not the point from which the psychologist is interested in the problem. The sense of number is a historical, an evolutionary development. It arises in the race and in the individual. The psychological (and the pedagogical) problem is: Under what circumstances, in response to what stimuli or needs, in what psychical context, does this sense arise? It would be impossible to say, in advance, just how much light anthropological investigation would throw upon this problem; but it may safely be said that it will throw some light; and it is a pity that Dr. Conant, through confusing the metaphysical and the psychological problems of origin, should not have contributed what his learning and thorough research fit him to contribute. The book would then have been as useful to the psychologist as it now is to the philologist.

The following points of psychological interest may be gleaned from the philological data: 1. The numerical systems are *rythmical*. The count proceeds up to a certain point (sometimes only 2; sometimes 3, joints of a finger; sometimes 5, fingers of one hand; sometimes 10, both hands; sometimes 20, fingers and toes; then a knot is tied, a notch cut, etc., and the count repeated. With further developments, compound words are formed, making it possible to dispense, more or less, with the notch or knot, a definite base of reference being formed. 2. While the origin of many number names is from the fingers, many denote *activities* performed upon the fingers. For example, 1 may mean 'used to start with,' or 'the end is bent.' 3. The rhythms of the system show reference *ahead* and also *backwards*. For example, 9 may mean 'almost done,' 'that which has not its 10,' 'there is still one more,' 'hand next to complete,' 'keep back one finger,' etc. The reference to the starting point, however, is much more common. 9 will more often mean '4 of the *other* hand,' or 'hand with 4' or 'end and 4.' It is undoubtedly true, as Dr. Conant remarks (p. 72,) that the savage does not discriminate the numerical idea from the concrete image of fingers or whatever with which it is bound up, *i. e.*, does not consciously abstract. But it is equally true that this continual thought of reference forwards or backwards in the larger number, is, psychologically considered, an abstracting movement. When, for instance, in the Zuni scale, 3 means 'the equally dividing finger,' instead of simply the biggest finger, it must be acknowledged that abstraction is pretty well along. While it is not true to the same extent of the verbal form in which 6 means '1 on the other,' still the element of relation is obviously prominent in the latter. While a careful study of the actual circumstances under which savages use number would be necessary to justify the statement that the ratio element in number early comes to consciousness, the philological material collected by Dr. Conant points in that direction. 4. The fact that the "student is struck with the prevalence of the dual number" in the grammatical structure of the earlier languages is an important fact. Mind first dichotomizes the universe; the world is 'this and that,' 'this' and 'the other one.' Observations which I have made on such small children as have come within my scope bear out this principle for the individual. There was not, at first (with these children at least), a plural number, but conscious selection or preference. 2 denoted not a couple, but a contrast, something left out or ruled out. 2 was not used in an aggregative or enumerative sense until an effort was made also to recognize aggregates larger than 2, which at first (agreeing here also with the philological record) took the form of 'a lot'—many. I cannot, however, agree with Dr. Conant that the difficulty which the savage met in attempting 'to pass beyond 2, and to count 3, 4, 5, is, of course, but slight.' On the contrary, it seems to me the *essential* difficulty, marking a distinct advance in consciousness. It is one thing to mark off the mental universe into this and not this; it is quite another to assume the attitude of *ordering* things within the universe, and this is what occurs when numbers develop into a row or sequence. At all events, in the observation of children just referred to, I found that the attaching of any meaning to 3 was a much later accomplishment (often a year intervening) than in the case of 2; and that when the idea of 3 was grasped there was no difficulty in getting the child to count intelligently to 10; thus indicating that the idea of 3 is not simply cumulative, but marks a different psychical attitude. Till a child can grasp the idea of 3, numbers like 3, 4, 5, etc., are taken by him to be the absolute names of certain individuals

An incidental psychological contribution, which will not fail of catching the attention of those psychologists and sociologists who are dwelling upon the importance of imitation, is found on p. 11. Experiments were made upon five different primary rooms in Worcester, Mass., to determine the 'natural' place of beginning in counting off on the finger. In two cases the teacher allowed one child to count while the other children watched. In both cases every other child followed exactly the example of the leader.

It is to be hoped that Dr. Conant, or some other equally competent student, will supplement this book with another, in which the anthropological data concerning the circumstances and motives with relation to which savages count will be collected so as to extend and to justify the philological data and conclusions; and will also take up the matter of systems of *measurement*, upon both a philological and anthropological basis. In this case the contributions to psychology will be direct and not simply incidental. JOHN DEWEY.

UNIVERSITY OF CHICAGO.

Die Spiele der Thiere. KARL GROOS. Jena, Gustav Fischer. 1896. Pp. xvi+359.

When it is learned that the above is a volume of 340 pages, exclusive of an excellent index, it will at once be plain that the treatment of the subject is of the most thorough kind.

The book is well printed on good paper and with a type that encourages one to keep on when once he has begun the reading—a very important matter in a work which is, after all, of special rather than general interest. In the introduction a succinct statement of the author's entire position is given. The work is rendered valuable for reference by reason of a very full bibliography.

The subjects of the different chapters are as follows:

I. Consideration of the theory that play is an expression of excess or overflow of energy.

II. Play and Instinct.

III. Forms of Play among Animals, which is continued in a fourth chapter as 'Die Liebesspiele,' the two together making over 200 pages of matter.

V. The Psychology of Animal Play.

The author gives the most ample evidence of familiarity with the literature that bears on his subject, whether directly or indirectly, and well-known American writers on psychology are quoted again and again, some of the citations indicating that the writer appreciates not only their matter but their style, as when he says: "James hat vollkommen Recht, wenn er z. B. bei der brütenden Henne keine weiteren Erfahrungen und psychischen Vorgänge annimmt als das gefühl, das eben ein solches Ei, 'the never-to-be-too-much-sat-upon object' ist."

No doubt Professor Baldwin's work on mental development would have proved to the author a mine to be well worked had it appeared in time. However, it will serve a still better purpose in connection with that second treatise Dr. Groos promises, 'Die Spiele der Menschen.'

Briefly as to the author's views: Animal psychology has been wrongly regarded as a sort of mere amusement in consequence of which the subject has suffered. It has also been lacking in aims and methods. It would be well to consider what in men is animal (thierisch) if we go no further, and this implies a close study of animals. The author then sets forth his views as to the qualifications of the man who would make a thoroughly successful study of animals, and they so perfectly agree with my own that I will quote the passage in the hope that the editor may not throw it out for being too lengthy.

"Der Verfasser einer Psychologie der thierischen Spiele müsste eigentlich nicht nur zwei, sondern mehrere Seelen in seiner Brust beherbergen. Er müsste mit einer allgemeinen psychologischen, physiologischen und biologischen Vorbildung die Erfahrungen eines Weltreisenden, die Kenntnisse eines Thiergarten-Directors und die Erinnerungen eines wahrheitsliebenden Oberförsters vereinigen. Und auch dann würde er schwerlich sein Werk zu einem befriedigenden Abschluss führen können, wenn er nicht zugleich mit den Bestrebungen der modernen Aesthetik vertraut wäre. Ja gerade diesen letzten Punkt halte ich für so wesentlich, dass ich behaupten möchte: nur ein Aesthetiker kann die Psychologie des Spiels schreiben."

Dr. Groos does not reject the 'overflow of energy' theory originating with Schiller and expanded by Spencer, but considers it inadequate.

Play is a development and preparation for the use or expression of certain instincts. Without this preparation the 'blind might' of instinct would often be unavailing.

The author's work is saturated with the doctrines of evolution, of which he makes abundant use and with critical discrimination.

Bearing in mind the all pervading nature of the sexual instinct, he endeavors to prove that play is essential for the successful attainment of the the objects of this instinct, especially on account of the instinctive shyness of the females.

Dr. Groos thinks he has discovered a new 'principle' as described above, necessary to complete and correct those of Wallace, Weismann, Galton, Spencer and Darwin. He does not believe that the females select the males, but that the peculiar forms and colors of the males tend to diminish the shyness of the female, so that with the addition of his own principle that great end of nature, the propagation of species, is accomplished. Through these two principles, attraction of females by the forms, colors, etc., of males, and that behavior for which play is a preparation, suitable matings result.

The principle of special interest in psychology in this connection, and especially for Æsthetics, is 'der Scheinthätigkeit oder der bewussten Selbstäuschung.'

Whether one agrees wholly with the writer of this book or not, he will get many tidbits by the way and is likely to feel more than ever the force of the well-known saying of Bacon, 'Reading maketh a full man.' WESLEY MILLS.

MONTREAL.

THE PSYCHOLOGY OF ART.

Ueber den psychologischen Ursprung der Poesie und Kunst. M. J. MONRAD. Archiv f. system. Philosophie, Bd. I. 347–362. 1895.

In the fourth chapter of the 'Poetics' Aristotle refers the psychological origin of Poetry to two sources, the impulse to and delight in imitation and the impulse to and delight in rhythm and harmony. Connecting the first with the theoretic impulse—we first learn by imitation—he finds an essential element in æsthetic enjoyment to be the pleasure of recognition. Certain persons in whom the imitative impulse was stronger than the rest began with imitations which expressed the temporary interest of the occasion, and these, perfected by practice, gradually became a developed art.

The essay of Herr Monrad is an elaboration and application to art generally of the above Aristotelian theses respecting the origin and development of poetry. Aristotle's remark about learning beginning with imitation receives a deeper significance than its author probably intended in the observation that in learning there is an inwardizing, a spiritual reproduction, of the object. Such idealization is a characteristic factor in human imitation, and enables it to rise above the brute stage at which it begins and where it approaches the reflex type, and to become an imitation, not merely of movements, but of objects. Connected with this ideal reproduction, this imitation in the subject, is the impulse to objectification, for which the twofold reason is assigned that it is necessary both to define the image and to communicate it. The psychological *motifs* which are the vehicles of these reasons are not stated, unless the remark referring to the latter of them be regarded as such, namely, that the person, as of essentially a social nature, wills to express himself and to see himself mirrored in his work and in the recognition of others. The result of this expression is to bring out the essential and universal import of the image by placing it where it can be modified by the similar expressions of other persons. Thus every man is a born artist. Only the exceptionally gifted, however, succeed in giving an adequately universal expression to their idea. Besides the pleasure of recognition, mentioned by Aristotle, delight in the work of art is connected with its freedom from all practical interests as being only image, form, and not reality, and with the fact that it is a form produced freely from the spirit and bearing its stamp. It is also connected with the formal elements of technical superiority, and, intrinsically, of rhythm and harmony, these terms being used in a more comprehensive sense than Aristotle's. The essay closes with a strong characterization of that 'Afterkunst' which passes under the name of Realism, the function of true art being, in the author's view, to give such expression to the things of the spirit that the spirit may recognize and rejoice in its own ideality.

SMITH COLLEGE. H. N. GARDINER.

Sex and Art. COLIN A. SCOTT, Fellow in Psychology, Clark University. The American Journal of Psychology, VII. 153–226. Jan. 1896.

Mr. Scott undertakes to bring a vast mass of heterogeneous phenomena under a few relatively simple concepts. Erethism, he holds, is the foundation of all sexual phenomena. At first general, it is soon found chiefly in organs specialized for the performance of the sexual function, to which the balance of the organism is brought into relation by the law of radiation: "Starting from the act of copulation, the sexual instinct tends to widen and become more complicated, until the whole of the organism is involved in its activity." With progressing sexual differentiation sexual selection comes to view. It is effected chiefly by means of combat and courting, out of which have sprung the emotions of fear and anger, shame, coyness, probably the parental instincts, and the æsthetic sense. Out of the last named sprang in turn the instinct for ornamentation, and this, conjoined with the sense of shame, has given rise to clothing. From the symbolism and fetichism so characteristic of savage races, which is at bottom a tendency to attach the complex and subjective to things concrete and objective and which lies at the foundation of all our conceptual and symbolic thought, there springs phallicism, in which the vague emotions grouped as above described about the sexual function become attached to a definite symbol, associated with cosmological notions and give rise to religion. The relation of the primitive sex instinct to the more complex instincts and emotions its derivatives may be expressed in physiological terms by the relation of a simple reflex arc and its ganglion cell to complex systems of similar arcs and cells, which may stimulate or inhibit the functioning of the primitive and may even become dissociated from the primitive and function independently.

Such are the main outlines of Mr. Scott's inductions. The balance of his paper illustrates these conceptions in sundry connections. He sketches the general features and laws of courting, bringing to view the katabolic tendencies of the male as opposed to the anabolic tendencies of the female, analyses the phenomena of degeneration and perversion, describes the state of ecstasy, which "as involving an emotional condition accompanying the operation of the phantasy, is a connecting link between art and sex, " and, finally, æsthetics. He concludes that the higher derivatives of the sexual instinct are not only excitants of their primitive, but also inhibitants of and substitutes for it, and urges that "what we need at present is a modern phallicism, a religious and artistic spirit that goes out to meet the sexual instinct and is able to find in it the center of evolution, the heart and soul of the world, the holy of holies to all right feeling men." In education we should seize upon the artistic sense at the outset and seek to develop it to the utmost. "Love, in its best development in a continued married life, gives us the pulse of this (the artistic) movement, (and) the ennobling ecstasies of poetry, music, painting and the enthusiasms generally, are at the same time an outcome of and a substitution for this happiness."

This is a most interesting and suggestive paper, showing on every page the author's acute psychological insight and mastery of his material. Yet, as is perhaps inevitable in so wide and new a field, its defects are many. In reading it the suspicion constantly arises that generalizations so comprehensive may include or perhaps be based upon mere superficial resemblances, and the fear is increased by the lack of any attempt on the part of the author to criticise or define such concepts as those of erethism, art, religion, etc. The psychological scheme which Mr. Scott propounds raises a similar doubt. If it be regarded as an attempt to express the facts of observation in symbolic form—a series of physiological fetiches—no one will object to it, although the same end would be as well or better attained by a notation similar to that of algebra or chemistry. But if we are to conceive it as representative of what goes on in the nervous system, it must be regarded as purely mythical.

That the primitive sexual passion has been a leading factor in the evolution of the artistic sense, and has been concerned in that of the religious spirit, scarcely admits of question, and the positive side of Mr. Scott's paper is a permanent and valuable contribution to our knowledge of the subject. But his treatment seems to the present writer essentially defective from the theoretical point of view, and from the practical entirely misleading. Fear has been evolved in the presence of dangers of all kinds, and not merely of those arising from rival aspirants for the favor of the female. The primary factor in the evolution of religion is to be found, not in the sex instinct, but in the use of a belief in the continued existence of deceased parents and chieftains as a fetich for the repressive influence of the community. In its later forms religion has entirely cut loose, and, as I believe, finally and forever cut loose from its early association with the sex passion, and Mr. Scott's plea for a 'Modern Phallicism,' even in the refined sense in which he uses the expression, is little short of grotesque. In the evolution of the æsthetic emotions the sexual feelings have probably played the leading part, yet non-sexual utilities have had much to do with their fashioning. And while one cannot but support Mr. Scott's plea for the wider recognition of the æsthetic emotions in education, the facts which he alleges do not afford a sufficient warrant for it. The æsthetic sense can, and sometimes does, act as an inhibitant of or substitute for the grosser sex passion, but we have no reason for believing that with increase in æsthetic sensibility we should see a diminution of sexual excesses. The history of Greece and Rome, of modern France, Italy and Spain, and of the cultured and leisured classes in every community, shows conclusively that there is at least no inconsistency between high artistic development and gross sexual laxity. Mr. Scott duly recognizes the fact that the æsthetic sense serves to awaken as well as control the sex passion, but he fails in his practical deductions to give due weight to that recognition. Further, he makes no mention of the regulative function—the so-called moral sense—which has been evolved expressly for the control of the animal passions in the interests of individuals as well as of society.

However, it may well be that such sins of omission and exaggeration are inseparable from any attempt to deal in narrow limits with such raw material, and as an earnest attempt at original construction Mr. Scott's paper will meet with a cordial welcome.

UNIVERSITY OF PENNSYLVANIA. W. ROMAINE NEWBOLD.

Tempérament et caractère, selon les individus, les sexes et les races. ALF. FOUILLÉE. Paris, Alcan. Pp. xx+278.

"General Psychology," says Ribot, "studies exclusively abstract laws, whereas the psychology of character studies types produced by a particular combination of general laws and classifying individuals." The character of an individual is his average mode of feeling, thinking and acting. It is the result of a series of causes, the first of which is race, the second the fundamental difference between the sexes (the significance of which is not only biological but also psychological), and the last a product of the individual constitution and temperament. Thus is formed the innate character, the present manifestation of a long series of evolutionary changes. But the innate character is only a starting point for new developments; it is passive under exterior influences, but active through the reaction of the intelligence and of the will. It is indeed these personal reactions that constitute *par excellence* the character properly so-called by assimilation with the innate temperament and constitution. Hence the influence of education and culture.

Making these principles his starting point and drawing some of his observations and theories from those who had previously contributed to the new science of character, M. Fouillée studies:

I. *Physical and moral temperaments*, which result from the uature of changes in the body. There are two types, the reflective and the active, with their subdivisions. So much may be said from the standpoint of the body (which may be modified as age advances), but it must not be forgotten that character is affected by the will as well as by the organism, and that the physical constitution, according as it is well or ill directed, affects the moral order for good or evil. Following Descartes, Pascal, Rousseau and Biron, M. Fouillée considers it essential that a place should be given to ethics in the reflective and emotional life, which act not by virtue of abstract precepts, but by a concrete influence on the being.

II. *Character and intelligence.* The type of character is a result of the mutual relations of the three important psychic functions inseparable from the will, sensation, emotion and desire. A character is well balanced when no one of these factors predominates, otherwise according to the element that predominates it becomes sensuous, intellectual or volitional.

III. *Temperament and character according to sex.* Following M. A. Sabatier the fundamental features of the female character are said to be concentration, unification, cohesion. The elements of the masculine character are on the contrary separation and production; the one collects, the other gives out; the one is dependent on and the other independent of surroundings. From these fundamental differences result the dissimilarity of the male and female character.

IV. *Character of the human races.* The general character of a race is the result of selection under given conditions. Those races which have degenerated from a moral or physical point of view may be regenerated in two ways, the one psychological and the other physiological—education and intermarriage. In conclusion, M. Fouillée remarks that the period in which the physical and moral differences between races were large, while the differences between individuals were small, belongs to the past. This state of things is reversed today, but we are approaching a third period, in which differences between individuals will again decrease without lessening the similarity of races, and thus will arise the true type of man.

In the course of the work the writer discusses different classifications of character, especially those of Ribot and Paulhan, and emphasizes the value of these researches. "From a practical point of view," says M. Fouillée, "the science of character would be of undoubted usefulness to the moralist and teacher. As it is indispensable to the student of hygiene to recognize different physical temperaments that he may adapt general prescriptions to individual constitutions, so must the moralist adapt his precepts to different moral temperaments. It would be absurd to conclude that what is successful in one case would produce like effects in another, as Kingsley did in preaching that all should find happiness in the study of marine animals. The educator cannot apply the same rules in dealing with different children; severity will influence some, affection will influence others. To fear is necessary for some, to love is best for others. We do not indeed go so far as to say with M. Stewart that classes in a school should be divided into four parts in order to group together children of the same character and apply to them special methods. But it is certain that educators ignore too much the physiology of character, as they ignore the hygiene of intellectual work. If the first educators, the parents, understood the intimate relations of physical and moral temperaments they would begin to decipher the nature of their children from the very first and would learn better and better to appreciate their aptitudes."

Let us hope that the appeal will receive the attention of educators.

JEAN PHILIPPE.

PARIS.

Die moderne Physiologische Psychologie in Deutschland. DR. W. HEINRICH. Zürich, E. Speidel, 1895. Pp. iv+235.

The scope of this book will be best indicated by referring to the author's explanation of how he came to write it. His first and final aim is to attempt the construction of a new theory of attention. On the way to the accomplishment of this end he had to review preceding psychological theories which bore special relation to this problem. This volume contains the result of these studies, the presentation of the author's own theory being postponed.

The book has, on the whole, an unsatisfactory character. It contains criticisms and discussions which, however acute and however important in themselves and in the development of the author's studies, have no close bearing on the problem of attention, or, at any rate, lack the unification and justification which might have been given to them if we had been presented with positive constructive conceptions. On the other hand, the book is too brief and the selection of topics for discussion is too one-sided to allow us to take it as a history of physiological psychology.

Dr. Heinrich states in the preface that the only objective criterion for theories of attention is to be found in the law of psycho-physical parallelism; "*es ist nun zu entscheiden in wie fern die Psychologen, die ja alle den psychophysischen Parallelismus theoretisch anerkennen ihm auch praktisch treu geblieben sind.*" Our author, at least, is true to his principle; there are, however, hardly any psychologists who are not found to be unfaithful. In truth, if a psychologist sets himself faithfully to interpret facts, he will hardly fail to come into conflict with a dogmatically assumed *Gesetz*.

Dr. Heinrich finds that Külpe's recent work on psychology "while it may perhaps be of some use as a text book," . . . "is of no scientific value." The mere statement of this judgment serves as well as anything could do for a criticism of Heinrich's treatment. That Külpe should profess to investigate only *Bewusstseins-Erscheinungen* and should yet study attention along with other factors as a *Zustand des Bewusstseins* is found to be inexplicable and at least to involve a contradiction. One may admit that Prof. Külpe might have made more clear what he means by *Zustand*, but that there is a contradiction involved seems by no means to follow.

Evidently it is Avenarius whose philosophy is to give an adequate solution of the problem of attention, and we are presented with an account of certain general conceptions which, according to Avenarius and our author, should dominate psychological investigation. I cannot see that Dr. Heinrich's approval of Avenarius in this respect is more justifiable than his condemnation of Külpe.

It may be noted that Dr. Heinrich refers only to German psychologists. This would be more natural if he were studying attention only in its psycho-physical aspect; it must be considered as a defect when we remember that his aim is far wider and that his professed object is to construct a general theory of attention.

W. G. Smith.

Smith College.

Zur Psychophysik der Gesichtsempfindungen. G. E. Müller. Ztsch. f. Psychologie und Physiologie der Sinnesorgane X., 1–83.

Besides the axioms which it has in common with physics and chemistry, psychophysics assumes five which are peculiar to itself. They are:

(1) Some material process (a so-called psychophysical process) is at the basis of every state of consciousness.

(2) To the equality, similarity, difference of a sensation (we shall not here discuss other states of consciousness, though the same axioms hold for all), correspond respectively an equality, similarity, difference of the underlying psychophysical process and conversely.

(3) If the changes which a sensation runs through have the same direction, or if the differences between a series of sensations have the same direction, the like will be the case in regard to the corresponding psychophysical process; if a sensation is variable in *n* directions, so also is its psychophysical process.

(4) The directions in which a sensation can be varied are of different kinds. If a given direction is towards zero (that is, if the sensation, by continuous change in the same direction, finally vanishes), we say that the sensation is suffering a diminution of *intensity* (and if the change is the exact reverse of this, we speak of an increase of intensity). Among the different directions which lead to zero, that

one is of peculiar importance which leads to zero by the shortest route (that is, by passing through the smallest number of perceptible intervals). In this direction, or in its opposite, the change is said to be a change of *pure intensity.* In any other course towards zero the change is said to be one of mixed intensity and quality. A change is purely qualitative when it leads neither towards nor from the zero point of sensation.

The fifth axiom will be stated later.

From the fourth axiom it follows that to every qualitative change of sensation there corresponds a qualitative change in the psychophysical process, and conversely; and that to an increase or a diminution of the one corresponds an increase or a diminution of the other.

These axioms may be summed up in the general doctrine of a *psychophysical parallelism.* But the history of the doctrine shows too conclusively the necessity of setting out in detail the elements of which it is composed. The definition of psychophysical process is such a material process in the brain as is accompanied by sensation (or other condition of consciousness). This is the converse of axiom 1.

The above axioms have been stated with more or less clearness by Lotze, Fechner, Mach and Hering. But when Hering says that a given sensation of gray may be caused by psychophysical processes very different in amount, provided the *proportion* of the white process to the black process remains the same, he misinterprets the principle of parallelism. If this were so, the white process and the black process might become excessively small in amount without producing any change in the sensation, and upon their finally vanishing, the sensation would cease suddenly without having previously suffered any diminution of intensity. This is in complete contradiction with the principle of continuity; it also is in disaccord with our fourth axiom, and with experience in the corresponding region of sound. A principle object of this paper is to modify Hering's theory of antagonistic colors in such a way that it can dispense with this assumption.

Since all sensations vary in intensity, the psychophysical process which underlies them must in every instance have a corresponding variation. Shall we suppose that this variation is a variation in strength (to be measured by the energy, the velocity, the acceleration or some other function of moving particles), or a variation in extension (that is, in the *number* of particles which take part in the motion)? A full discussion of this question leads to the conclusion that Fechner's view is best, that subjective intensity must depend upon *both* of these factors, and that it is not at present possible to distinguish between them in consciousness. The *naïf* view which regards the feeling of the extension of a colored surface as dependent upon the extension of its image upon the retina, and then the connection of the retinal elements with a definitely extended portion of the cortex, could only be supported if it had been shown that in all the senses (whose cortical connections have also, of course, a definite extent) the same relation holds between extent in space of cortical elements, and an attributed extension of the sensation. And what sensation element should we attribute to the extension of the cortical process in the third dimension? As regards Lotze's theory of the local sign, it is to be remarked that no difference in *sensation* is necessary as its basis; it is sufficient to point out that different retinal elements must, on discharging in the brain, form part of different association tracts, and hence be the basis of different *ideas* of locality, whether they produce different sensations or not. For Lotze the association was a purely spiritual process; we take now a more material view of it.

The psychophysical processes may be either simple or mixed; a simple process is either really such, or is never separated in our experience into parts, and is never composed of its parts mixed in different proportions. The sensation corresponding to this is a *pure* sensation, but a mixed sensation is not such in the sense that it can be looked upon as a complex of several distinct *sensations*. If μ is a mixed sensation, and a and b are the intensities of the two partial psychophysical processes which call it forth, and if α and β are the sensations which these processes would call forth if acting by themselves, then for the *degree of resemblance* of the mixed sensation to α we have (as the simplest and most plausible expression) $\frac{a}{a+b}$, and for its degree of resemblance to β, $\frac{b}{a+b}$. But if α a β resemble each other to a degree represented by $R(\alpha\beta)$, then these two expressions must be modified, and they become respectively

$$\frac{a+R(\alpha\beta)\,b}{a+b} \text{ and } \frac{b+R(\alpha\beta)\,a}{a+b}.$$

This gives an expression for the quality of a mixture in terms of the intensity of its two (or more) partial processes, and it constitutes the *fifth axiom*. By differentiating these expressions we are able to prove (among other things) that the degree of whiteness of a mixture of white with a color increases, for a given increment of the white-excitation, *more* when the color is yellow than when it is blue (since yellow is, to begin with, more like white than blue is). This formula (which gives $\frac{w}{w+c}$, for instance, for the degree of whiteness of a given gray) resembles that given by Hering, but with him w stands for the intensity of the partial *sensation*, here it is the intensity of the partial psychophysical process. Hering has, as is well known, given up the idea that the brightness of a pure color is $\frac{1}{2}$, but, since introducing his idea of the specific brightness of colors, he has not said what it is. Müller here fills up this lacuna. [He uses brightness—*Helligkeit*—therefore, as synonymous with 'degree of resemblance to white']. It is proved, in passing, that the addition of blue to a given gray does not *always* darken it, as Hering affirms, but only when the gray had a certain (not great) brightness at first, and, also, that yellow does not brighten a given gray, unless the gray had a certain (not small) brightness at first. From all this it does not follow that the Purkinje phenomenon can be wholly explained by the specific brightness of the colors.

According to Hering, the series of grays is a quality series, and not an intensity series. This is true, as matter of fact; we do not see a different grey in different intensities (or hardly different). But this is merely due to the accidental fact that, starting from a given grey, that of the self-light of the retina, for instance, any external cause which increases the white constituent does not also increase the black constituent, but, on the contrary, diminishes it. We are not in the position of ever being able to vary the black and the white constituents in the same direction at the same time. It is this, and not any theoretical difficulty, which prevents us from seeing a given quality of grey in different intensities.

When a sensation changes in quality we are able, to a certain extent, to form a judgment as to whether the change is in a constant direction or not (and even though there should be a change of intensity at the same time). A series of sensations in which the quality changes *continuously* and in a *constant direction*, we shall call a psychic quality series. Such a series is the series of grays. A psychic quality series may be *intrinsically limited* or not. The black-white series (unlike the series of tones) is intrinsically limited, because it is a series which consists in becoming more like white or more like black, and we cannot conceive of it as passing beyond a pure white or a pure black.

Corresponding to a given psychic quality-series there must be a psychophysical series, which must also be continuous and in a constant direction. But a series of processes of this description can be of either one of two kinds: (1) it may consist in a series of changes of a qualitative nature (for instance, of a vibration period); or (2) it may consist in a change of the relative intensity of the two constituents of a mixture. It is shown, by a very complicated argument, stretching over sixteen pages, that, in the case of the black-white series, we may assume that the underlying psychophysical process is of the second kind. This argument depends upon the axioms already stated, the assumption that the retinal process is of a chemical nature (and therefore not capable of a large number of different qualities), and that to every excitation in the visual nerve, which varies continuously and in a constant direction, there corresponds an excitation in the retina of a similar description, and finally that we are able to recognize a psychic quality-series (that is, can tell whether a series is varying in a constant direction or not). All this forces upon us the assumption of the six retinal processes of Hering. The further development of these considerations is reserved for another occasion.

It has been known for some time that Prof. Müller was engaged upon a profound modification of Hering's theory, and his conclusion of this subject will be awaited with great interest.

CHR. LADD FRANKLIN.

Die Lehre von den Spezifischen Sinnesenergien. DR. RUDOLF WEINMANN. Verlag v. L. Voss, Hamburg und Leipzig. 1895. Pp. 96.

The first part of this book is mainly historical. In the first section of this part the author gives Müller's well-known theory of 'specific energies.' He then goes on in several sections to give the forerunners of the theory, naming among others Newton, Eichel, Elliot and Autenrieth. He then gives an account of the so-called new form of the theory in its applications, citing the application of the theory by Natonson to the different qualities in the various 'modes' of sensation. He cites the work of Helmholtz in the sense of hearing, and in the sense of sight the Young-Helmholtz theory of color and also Hering's color theory. The author then continues the historical account through the various senses. Finally he concludes the first part with a critical summing-up in which he gives the criticism of the theory by Lotze, Weber and Dessoir, concluding first, that in the cases of admitted phenomena with a doubtful interpretation no sure decision between the theory and its afore-mentioned critics can be had, and that nothing prevents the assumption that, where the different senses give their own reaction in spite of apparently inadequate stimuli, nevertheless adequate stimuli may be present. Second, that there is another class of cases where the phenomena themselves are doubtful. Third, that still another class remain unaccounted for, where, for example, light waves are without effect for hearing, taste, smell; and sound waves are without effect for sight, etc. Finally, granted the newer form of the theory, it yet stands in opposition to the main point of the theory as given by Müller, which presupposes the indifference of stimulus, while the newer theory only shows that the nervous organization shows a greater complexity and 'division of labor' than was formerly supposed.

In the second part the author seeks to reach certain theoretic results. Müller's theory, he says, takes a group of facts and puts them together under a name. Suppose now we grant the facts, the theory intends to explain these and seeks to do so by ascribing a 'specific energy' to the sense nerves. This 'energy' is after all only a word, and we must seek the concrete explanation of the phenomena. In the first section of this part the author seeks to show that the question of the theory is purely a physiological matter. A sense reaction means two things, a physiological occurrence and a conscious reaction. Now it is admitted that with a nerve process x is always the determined sensation x'; not every similar stimulus, but every similar nerve process, must have the corresponding sensation. From this it follows that the physiological reaction must be specific, and the theory must explain this. Lotze, says the author, was the first to realize this. Müller failed to do so, and hence confusion resulted. What, then, asks the author in the next two sections, is specific energy? He agrees with Lotze's view, which he cites as the correct one. The phrase has a meaning, when applied to sensation, viz.: "the sensation of tone is the peculiar reaction of the auditory nerves." But to lay such a principle at the basis of nerve processes means to violate all scientific method in the admission of wantonly assumed forces. It means to give up explanation and rest in a word. We must, he claims, seek the explanation with Lotze in the universal mechanical laws which hold when one object impinges on another. After an analysis of Lotze's position Dr. Weinmann concludes that, instead of the doctrine of specific energies, we should speak of the doctrine of the 'diverse nature of the physiological bearer of the sensation.' In the next section he considers the question as to the seat of the 'specific energy' in this sense, and in the following section he takes up the question as to whether it is innate or acquired. Both of these sections we must pass by for want of space. In the third and last part of the book the author considers the epistemological aspect of the theory in its new form. He concludes that it cannot be used in support of Müller's subjective sense physiology, nor as an empirical proof of the doctrine of the subjectivity of the secondary qualities. Though in general it favors subjective epistemology, its interest is mainly physiological.

PRINCETON. C. W. HODGE.

The Recognition-Theory of Perception. Recognition. ARTHUR ALLIN. *American Journal of Psychology*, Vol. VII. 237–273, January, 1896.

Dr. Allin's articles are primarily an attack upon a common theory of immediate recognition, the supposition that it occurs through the fusion of images of an object with the percept of the same. Höffding is rightly named as the chief upholder of the theory, but it is traced in the writings of Herbart and of Spencer, of Mill and of Bain, of Wundt and of Ward, associationists and apperceptionists as well. Incidentally, the author finds opportunity for a vigorous and successful criticism of many traditional definitions and formulations of psychology. His main positions are the following:

I. a. Perception is not a case of immediate recognition. This is proved on the testimony of ordinary introspection. "Perception is not * * * * an act of memory * * * * If I burn myself, I know it is hot without any reference to former experiences (p. 240)."

b. In particular, perception is not a fusion of percept with similar images. The virtual abandonment of the position by the admission of its supporters that the fusion is 'theoretical,' 'metaphorical' or 'ideal' (p. 241) is clearly indicated.

Above all, Dr. Allin combats 'the unconscious' as explanation of the association. "If unconscious," he says, "then obviously it does not exist as a conscious or mental fact (p. 242)." "Statements of an unconscious, conscious act," he concludes, "are too obviously impossible to demand refutation (p. 242)."

c. Finally the theory that perception involves immediate recognition leaves no room for the explanation of sense-illusions, since it requires recognition, as well as perception, of all objects (p. 247).

II. This fusion of percept with similar images, even granting its existence, can not possibly be all that it is claimed.

a. Such fusion (so-called 'association by similarity') is not a case of the revival of former impressions, for such a resurrection of the past simply does not occur.

b. Such fusion is not association at all, for if it were, "then the two presentations associated must be separately cognized in order to be associated (p. 257)."

c. Such fusion certainly is not recognition. "*Objectively* considered, it may be a second cognition * * * but subjectively, it would be for the percipient's consciousness simply (Object and Object) becoming eventually fused into (Object) (p. 257)."

III. Recognition, however, both of the obviously associative type and in the form 'immediate recognition' unquestionably does occur and is to be explained.

a. The term 'recognition' refers to the consciousness that an object is again presented, not to a second nor to a thousandth presentation of an object without the accompanying consciousness of the repetition. Therefore,

b. The mere fact of reproduction does not convert an image into a memory image. "The stages of * * * imagination are not recognition proper (p. 255)." And,

c. Neither the bare presence of associated elements, nor the occurrence of a feeling of ease is in itself immediate recognition. "The added or differing associates in themselves are no memory."

IV. Positively, therefore, recognition is 'classification as known again' on the ground of certain observed characteristics. These include the lack 'of vividness,' 'of spatial localization,' 'of persistency,' 'of muscle and joint sensation;' the presence of associated objects; the rapidity, the ease and the pleasure of the recognition-consciousness. No one of these characteristics is identical with recognition or even necessary to it; but one or more of them form the accompaniment or psychological explanation of the recognition, its 'characterization causes.'

a. Immediate recognition is marked by an absence of accompanying definite associations, and by the presence of the pleasure feeling, or of a consciousness of ease and rapidity in the perception. This 'surprising immediacy and celerity' appears to Dr. Allin to constitute the prominent feature of paramnesia; but he also explains it by actual association with dream experiences and with waking images, and by general bodily conditions.

b. The classification, however, in which recognition consists, is of objects not of percepts (p. 267). "Perceptions when they once pass out of consciousness are never known again, for they no longer exist;" but a distinction gradually arises between 'certain presentations, faint, dim,' etc., to which 'there are no corresponding external qualities,' and other perceptual presentation 'fresh, full, vivid, steady in their spatial localization;' the latter are called 'objects present,' while the former are objects known again. "This" says Dr. Allin, "as far as

I can see is a simple classification like that of certain sensations into color * * * and sound sensations." Finally

c. "There is in recognition no 'identification of the past * * with the present' * * (p. 269)."

Dr. Allin's quotations amply verify the need for his protest against the 'recognition theory of perception' and for his insistence upon the obscured distinction between image and memory-image. The writer of this notice subscribes cordially to the critical conclusions of the author, but questions the adequacy of his analysis of recognition. The express identification (p. 267) of 'external reality' with 'the present' obscures many features of the consciousness of external reality, and ignores the most significant of them—the assumption of the parallel consciousness of other selves. The treatment of the past as the known-again-with-its-associates betrays an equally unsatisfactory analysis of the time-consciousness, but this follows logically from the central error of the theory: the assertion that recognition does not imply identification or comparison. This is argued by a reference to the cases of immediate recognition, which are admitted to 'take place without a second presentation of the same object.' But immediate recognition does, nevertheless, include comparison with the past experience of the subject, only the comparison is wavering and restless, and the identification is incomplete. When a face 'seems familiar' I am eagerly comparing it with faces I have already seen, trying to identify the present with the past. If this sort of comparison were entirely absent, then the 'feeling of familiarity' would not have risen to the plane of recognition at all.

Dr. Allin's definition of the 'recognized' as the 'known again,' though it recalls Höffding's *Bekanntheitsqualität* which he rejects, is psychologically quite satisfactory, for psychology avowedly adopts the matter-of-fact standpoint, and properly declines to enter upon a metaphysical search for ultimates. But to deny the identity of the 'known again' and the 'compared,' and to suggest the parallel of the 'known again' with sensation (p. 267), is to confuse contents of consciousness which are metaphysically as well as psychologically irreducible, with contents which lead by philosophical necessity to the inference of a self underlying phenomena.

The entire discussion would have gained in force and in arrangement if it had been presented as one essay, rather than two. There is a tendency also to over-quotation, which sometimes obscures the author's meaning and overloads his page, especially when he pauses to comment on some irrelevant error, or repeats a quotation already made. The occurrence in the body of the text of citation references to title, volume and page is also a serious annoyance to the reader.

WELLESLEY COLLEGE. MARY WHITON CALKINS.

PATHOLOGICAL.

Die Physiologie des Trigeminus nach Untersuchungen am Menschen, bei denen das Ganglion Gasseri entfernt worden ist. PROF. DR. FEDOR KRAUSE. Münchener Medicinwochenschrift 1895, No. 27–27.

Intracranial resection of the different branches of the trigeminus not having proved a complete safeguard against relapse, the author determined to perform a more radical operation, that of removing the ganglion Gasseri together with the trigeminal root situated centrally from it. The author promises a special monograph on the histological changes in the ganglion Gasseri in cases of neuralgia. The phenomena of abrogation appearing in patients thus operated on are interesting. There must be a much greater possibility of accurately determining the functions of this nerve-root in this way than in experiments on animals. The observations contained in the above treatise refer to cases in which these phenomena were investigated in patients at intervals of from 18 days to two years after the performance of the operation. During the operation no usual investigation of the phenomena of abrogation was attempted, although the author, so far as it was possible and the condition of the patients permitted, commenced his examination before the extirpation of the ganglion. The circumstance must be noted that in all investigated cases resections of the peripheral trigeminal branches had been performed several years before. The general result of these interesting researches is the demonstration of complete anæsthesia in the entire region of the three branches of the trigeminus.

Über periodische Schwankungen der Hirnrindenfunctionen. RICHARD STERN, aus der med. Klinik in Breslau. Archiv für Psychiatrie Bd. 27, Heft 3, p. 850–917. 1895.

The author describes two remarkable, morbid phenomena, hitherto unnoticed, which appeared in two men as subsequent phenomena after serious injuries on the head. They consist principally in a complex of symptoms which the author attributes to a periodically recurring relaxation of the functions of the cerebral revolutions. This periodical relaxation, designated by the author as 'fluctuations,' appears at the same time in sensory, intellectual and motor regions, and influenced, in the first case, all sense-activities, in the second even the breathing. The reaction-times measured during this condition were about three times the normal length. Speech and writing were both injured by these peculiar fluctuations, the activity of the memory was greatly lessened and mental work (Kraepelin's method being applied) much more slowly performed. The work is of great interest and is worthy of further notice.

Beitrag zur Pathologie des Gedächtnisses. P. OTTO BARTHEL. Inaug. Diss. München. 1894. P. 1–48.

After a few introductory remarks, the author gives an account of the disease of two individuals, presenting the symptom of a peculiar loss of memory, in consequence of which the patients appeared to have remained stationary at a certain period of their lives. The first of these, a day laborer of 55 years of age, had been injured in his growth by a blow on the head, and although hereditary predisposition was not traceable, his mania, of a religious and sexual nature, gradually developed into a condition of secondary imbecility. This man, in answer to any questions addressed to him, replied that he was 23 years old, at which age his mania broke out. He was able to recall correctly all events fixed in his memory up till this time, whereas all later incidents were for him nonexistent.

The second account relates to a pupil of the gymnasium, who became ill in his twentieth year, being at present 47 years old. An attack of typhus prepared the way for the disease, as also mental over-exertion by which the patient had tried to supply intellectual deficiencies. He lived entirely in his 21st and 22d year.

The author then communicates a number of similar cases drawn from earlier literature and comes to the conclusion that however varied the psycho-pathological conditions may be in the individual, mental weakness forms the link which binds them together. With the appearance of this weakness the patient becomes unable to assimilate fresh material for the memory. "This symptom is the expression of true agennesia of the memory, and it forms the boundary-line between health and disease in primary psychopathy; in secondary conditions between primary and secondary alteration."

FRIEDRICH KIESOW.

LEIPZIG.

EXPERIMENTAL.

On the Apparent Size of Objects. W. H. R. RIVERS. Mind, N. S. V., 71–80. Jan., 1896.

When the ciliary muscle is paralyzed by atropin, there occurs a micropsia of the affected eye; objects appear to it smaller. Donders and others explained this phenomenon as due to the greater effort of accommodation, causing a judgment that the object was nearer, hence smaller. Rivers distinguishes two kinds of micropsia—affecting objects at the fixation point, and objects beyond it—and claims that they are due to entirely different causes.

Micropsia at the fixation point is a phenomenon or irradiation. It affects black objects on white ground, not white on black. A small artificial pupil before the affected eye corrects it. Hence it is due to dilatation of the pupil increasing irradiation, and not to an affection of accommodation.

Micropsia beyond the fixation point is observable by the normal eye, but more easily under atropin. Rivers shows that this form is not due to irradiation, and adopts an explanation which is a modification of that of Hering. What determines the apparent size and distance of an object not fixated, is its relation to the fixation point, and not to the eye. The retinal image has remained constant, but it is multiplied by a smaller factor with greater distance from the fixation point. So far then as localization relative to the fixation point is concerned, there is no evidence that the alteration of spatial relations is in any way dependent on accommodation. As to the localization of the fixation point itself, the atropin experiments show that this takes place in the absence of any peripheral accommodation, and with exclusion of peripheral influences from the unused eye by treating that with atropin also. Rivers therefore regards these experiments as going far towards proving that the localization of the fixation point depends on central factors.

Objects nearer than the fixation point appear larger to the normal eye, and especially so to an eye in which spasm of the ciliary muscle is produced by eserin. This macropsia can be interpreted in harmony with the explanation given for micropsia.

BROWN UNIVERSITY. E. B. DELABARRE.

Die Wirkung akustischer Sinnesreize auf Puls und Athmung. P. MENTZ. Philosophische Studien, Bd. XI. pp. 61–124, 371–393, 562–602.

The attempts to judge indirectly the quality and intensity of psychical states by means of the accompanying vasomotor and respiratory change have, unfortunately, produced meager results and the present investigation emphasizes the fact that such attempts meet many difficulties which with our present limited knowledge are insurmountable. The complicated nature of the purely physiological phenomena concerned is by no means fully understood and the results depend upon so many and varied conditions that exact measurement is out of the question. The author found that the changes which appeared to furnish the most reliable basis for judgment were the increase and decrease of in the rapidity of the pulse, in other words the shortening or lengthening of the abscissa of a single pulse curve. The respiration curves are less constant and for the most part neglected. This may be regarded as a deficiency in the investigation, since it is true beyond a doubt the circulation is very much influenced by the breathing and the question is at least open whether the changes in the action of the heart and arteries may not in reality be largely secondary phenomena depending on respiration.

The first series of experiments was made with single noises and tones of moderate intensity as stimuli, with the result that the pulse and often the breathing showed a decrease in rate. These well-known accompaniments of agreeable sensations are attributed in this case to the pleasure arising from the mere exercise of the function. When the stimulus was repeated the decrease was less marked. If the intensity of the sound was increased, a limit was reached where the indications of pleasure disappeared and after a period of indifference the pulse rate increased. The pleasure produced by musical notes was most intense at middle *e* and gradually diminished as the ends of the scale were approached until, after passing through a point of indifference, signs of unpleasant sensation became apparent. When the sensation was received passively, that is without any strain of attention, the pulse rate, as above noted, decreases; when, on the other hand, the subject voluntarily concentrates the pulse increases in rapidity. In regard to tempo it was found that a certain rate, which varied with the individual, gave pleasure, and from this rate in both directions—when the tempo was made faster or slower—the pleasure passed through an indifferent stage into its opposite. When series of sounds are used the rhythm of inspiration and expiration tends to coincide with that of the sounds. The same results appear when the subject represents to himself a certain rhythm without hearing it. Attempts to deal with the higher emotions, such as surprise, etc., produced nothing definite. When musical compositions were passively heard, the effects were those above pointed out as the result of involuntary attention to agreeable sensations; when, on the other hand, the subject made an effort to analyze, in other words voluntarily strained the attention, the result was a quickening of the pulse.

LEIPZIG. C. H. JUDD.

Untersuchungen über Temperaturempfindungen. FRIEDRICH KIESOW. Philosophische Studien, Vol. XI. 135–145. 1895.

The author used, for searching out temperature points on the arms of seven persons, the brass cylinder (9 cm. × 3 cm. with conical ends tapering 1 cm.) of Goldschieder. Cylinder was passed through a piece of cork or rubber tubing; warmed by a gas-flame, for qualitative experiments, and in warm water the temperature of which was read from a Celsius thermometer for quantitative determinations; cooled in a solution of salt and chlorcalcium. For marking the points three colors were used. A square was marked on the arm and the points within it searched out and marked. The doctrine of separate temperature-points was thoroughly confirmed; they remained constant for 1½ months. There is a marked difference in intensity of different points. A figure in the text shows the square of one subject. Intervals between points are at first indifferent, but after 3 seconds diffusely and superficially cold. The importance given to hair-cells by Goldscheider as points of temperature sensation is at least questionable.

For testing the 'specific energy' of temperature-points four kinds of stimuli were used—mechanical, electrical, needle-point stimulus, and the reversed or opposite stimulus. All the experiments demanded exercise in both the experimenter and subject, the mechanical succeeding first. For these a wooden suitably-pointed cylinder was used. Following Goldscheider, the skin was somewhat stretched. The cold points 'blaze' out when touched, while the warm rather glow; the latter are the more difficult to locate. As to cold points, the author is convinced of their existence. After two weeks, out of 46 possible cold points 21 proved to be positively cold. In another case, 9 out of 30 possible ones proved positive. The warm points took a longer time and were less clear. Finally 10 out of 30 on the author proved positively warm; on another subject, 5 in 15; another had only cold sensations; another, for two days, had 10 cold and 10 warm points.

For electrical stimulus Faradic current was used. Here the skin might react to the touch of the electrodes with its own sensation instead of to the current, to prevent which the electrodes were warmed. Cold points show a direct increase of intensity of sensation with the increase of current up to a certain point. In the case of warm points the sensation was due to current—shown by using a cold cylinder as a test. Where the current gave a weakly warm sensation the cold cylinder, on the same point, gave cold. All the subjects gave a large per cent. of both points.

With the needle warmed in a flame, warm points were always painful; by far the most sensations were cold; many points gave only pain. In the experiments with opposite stimuli, cold stimulus for warm points and warm for cold points, the temperatures were 15°–20° C. for cold stimulus and 38°–40° C. for warm. A cold sensation was never produced on a warm point with cold cylinder; but scarcely any cold point did not give warm sensation to stimulus beyond 47° or 50°. The article states that beyond this point painful temperature-sensations always came, but the author informs us personally that the statement should be modified. "The great majority of cold points on the skin are at the same time sensitive to warmth." The experiments are being continued, and the author hopes for clearer results with better apparatus. GUY TAWNEY.

LEIPZIG.

ETHICAL.

Ueber Werthaltung und Wert. A. MEINONG. Archiv. für syst. Philos. I., pp. 327–346.

In his recently published *Psychologisch-ethische Untersuchungen zur Werttheorie* the author sought to determine the relations between the value or worth of an object and the feeling attaching itself to the knowledge of the existence or non-existence of the object. The value of an object was defined as its capacity to evoke a feeling of pleasure (for this is what is meant ultimately by *Werthaltungsgefühl*). This feeling was distinguished from the value, but at the same time the greatness of the value was held to depend on the intensity of the feeling in a normally constituted individual. But the objection was near at hand that this ratio is not true to fact. A highly valued friendship may be attended by little feeling, while a trifle may call out an altogether disproportionate feeling. The present article seeks to supplement the theory by a conception taken from economics, where it is customary to make value depend upon the urgency of the want to be satisfied and upon the other available means for supplying it. This leads to the revised formula that the effective value of an object is determined not only by the value which its existence brings with it, but also by the discomfort or pain (*Unwert*) which would be occasioned by its absence. This latter is evidently the chief factor in estimating the value of such common objects as air and water, for here the lack of any particular portion can ordinarily be fully compensated for, although under certain conditions the value may become priceless. Further, in these cases, as in the case of long-standing friendship, the feelings obey the law of fatigue and the accustomed ceases to excite positive feeling.

This second element, however, involves the consideration of desire; for the degree of discomfort referred to above will depend on how much I desire the object. Hence value might also be defined as 'the capacity of an object to maintain itself in the struggle of motives, or, if the expression be preferred, in the struggle for existence,' or again, "the value of an object represents the force as motive which belongs to an object either intrinsically or by virtue of the nature of its environment or of that of the appreciating subject."

From the fact that the value of an object is related to feelings dependent respectively on the existence and non-existence of the object it follows that as these factors are mutually exclusive the value can never be 'felt' in its totality. We are forced to resort to an intellectual apprehension. We pronounce a 'judgment of value' (*Werturtheil*). This is not to be confused with another use of the term *Werturtheil*, in which it has been defined as signifying 'judgments which arise through simultaneous action of ideation and feeling'—a technical theological usage.

An adequate theory of value would require a more thorough study of the part played by choice, and volition in measuring value. The time and labor expended, or to be expended, form a very common standard, and one that we regard as more reliable than the attendant feeling. Further, the reaction of a choice which identifies a given object with the self is also a very important factor in our estimate of value. J. H. TUFTS.

UNIVERSITY OF CHICAGO.

Skizze einer Willenstheorie. G. SIMMEL. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. IX. 206–220. Oct., 1895.

The author's discussion is based on a theory of instinct as the first step in volition and as a series of purely physical changes beginning with stimulus and ending with movement, the Spencerian conception of instinct. It is a 'closed' physical 'unity which does not transcend itself so as to contain within itself a teleological moment.' Instinct is the conscious side of the innervation which we finally regard as an act. Fear, *e. g.*, is nothing other than the feeling of the beginning of the flight movement.

The theory reduces will to a mere psychic accompaniment (Mitklingen) of a closed physical series which issues ultimately in movement or actions. In consciousness, the act seems to follow the will, but in reality, the feeling of having willed follows and results from the act. An apparent contradiction arises in cases of volitions which do not result in immediate action, as in willing to be rich. The author answers that as a child cannot desire anything without immediately acting out the desire, therefore this apparent case of will without accompanying action is 'a secondary and complex psychological product' which represents no elementary function, but must be explained by a synthesis of simpler deeper lying processes. Complicated psychic states bring with them a large number of sympathetic innervation-sensations to which the volition-tones of such reflections as the will to be rich is probably due.

But the author does not seem to see that every act of accommodation contradicts this theory, the only principle of which is habit. Adaptations, if they ever occurred on such a basis would be purely accidental. The small beginnings of assimilative processes seen in the child's recognition of the meaning of objects resembling those with which it is more familiar—processes going on at the same time that the child must act out every desire—would be impossible on this theory except as happy accidents. The theory has the defects of a materialistic and mechanical conception of the will.

LEIPZIG. GUY TAWNEY.

NEW BOOKS.

The Primary Factors of Organic Evolution. E. D. COPE. Chicago and London, The Open Court Pub. Co., 1896. Pp. xvi+547.

Outlines of Logic and Metaphysics. JOHANN EDUARD ERDMANN. Translated by B. C. Burt. London, Swan, Sonnenschein & Co.; New York, Macmillan & Co. 1896. Pp. xviii+253.

The Philosophy of T. H. Green. W. H. FAIRBROTHER. London, Methuen & Co.; New York, Macmillan & Co. 1896. Pp. vi+187.

Fear. ANGELO MOSSO. Translated by E. Lough and F. Kiesow. London and New York, Longmans, Green & Co. 1896. Pp. 278.

The Theory of Knowledge. L. T. HOBHOUSE. London, Methuen & Co.; New York, Macmillan & Co. 1896. xx+622.

Grundriss der Psychologie. W. WUNDT. Leipzig, Engelmann. Pp. 392.

NOTES.

WE have received the first number of the first volume of a new psychological *Archiv*, to be entitled *Beiträge zur Psychologie und Philosophie*, and edited by Prof. Götz Martius, of the University of Bonn. Numbers will appear at irregular intervals, and can be obtained separately. In an introduction the editor explains the philosophical standpoint to be represented by this publication; it is, in brief, that the connection between mind and matter is neither that of complete independence in connection with a preëstablished harmony, nor that of simple dependence of either upon the other, but something far more complicated than this—something upon which there is always hope that light may be thrown by the results of experimental psychology. The four papers which compose this number are all on brightness (*Helligkeit*). We shall notice them in a future number of this REVIEW.

THE fourth International Congress of Criminal Anthropology will be held at Geneva, August 25–29, 1896. Applications for membership should be sent to M. Maurice Bedot, Musée d'histoire naturelle, Geneva, Switzerland. The time and place are convenient for those who attend the Psychological Congress at Munich, August 4–7, and the published program is of great interest to psychologists.

A MARBLE bust in memory of the philosopher Luigi Ferri was placed on March 16th, the first anniversary of his death, in the hall of the University of Rome, where Ferri taught for twenty-four years. For this memorial about $200 had been collected by subscription.

PROFESSOR W. WUNDT has been elected a foreign associate and M. J. Lachelier a member of the Paris Institut (Academy of Moral and Political Sciences).

THE provisional program of the International Congress of Pyschology, to be held at Munich from the 4th to the 7th of August, announces 102 papers, and others will be announced later.

A SECTION of the new New York Academy of Sciences has been formed devoted to psychology, anthropology and philology. The first meeting was held on April 27th, and meetings will be held on the fourth Monday in the month during the Academic year. An Anthropological Club for informal discussion was formed in New York on March 4th. At this meeting the recent works on children and child psychology by Sully, Baldwin and Chamberlain were discussed.

PROF. WUNDT'S *Grundriss der Psychologie* is being translated into English by Mr. C. H. Judd, and Prof. Baldwin's *Mental Development of the Child and the Race* is being translated into German by Dr. Kiesow and into French by Prof. E. Nourry.

FELIX ALCAN announces as in press *La psychologie des sentiments* by Prof. Ribot and *Les type intellectuels* by Prof. Paulhan.

THE number of the *Zeitschrift für Psychologie, etc.*, issued on Jan. 11, contains an index of psychological literature for 1894. The list contains 1,504 titles and is very complete, especially with regard to publications on the senses.

PROF. E. B. DELABARRE, of Brown University, has been appointed director of the psychological laboratory of Harvard University for next year during the absence of Prof. Münsterberg. In the same University James Edwin Lough, A. M., has been appointed instructor in experimental psychology and C. M. Bakewell, A. M., instructor in psychology.

DR. MARK WENLEY, recently examiner in philosophy in the University of Glasgow and lecturer at the Queen Margaret College, has been appointed professor of philosophy in the University of Michigan.

PROF. JAMES SETH, of Brown University, has been elected professor of ethics in Cornell University.

EDGAR A. SINGER, JR., has been appointed to a senior fellowship in the University of Pennsylvania, under the George L. Harrison Foundation.

MR. S. I. FRANZ AND MR. L. B. MCWHOOD have been appointed fellows in psychology in Columbia University.

A COURSE in experimental psychology will be given at Bryn Mawr College by Prof. Lightner Witmer, of the University of Pennsylvania.

IT is announced that Prof. Ladd, of Yale University, and Prof. Earl Barnes, of Stanford University, will lecture at the University of Chicago during the summer term.

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THE PSYCHOLOGICAL REVIEW.

THE REFLEX ARC CONCEPT IN PSYCHOLOGY.

BY PROFESSOR JOHN DEWEY,

University of Chicago.

That the greater demand for a unifying principle and controlling working hypothesis in psychology should come at just the time when all generalizations and classifications are most questioned and questionable is natural enough. It is the very cumulation of discrete facts creating the demand for unification that also breaks down previous lines of classification. The material is too great in mass and too varied in style to fit into existing pigeon-holes, and the cabinets of science break of their own dead weight. The idea of the reflex arc has upon the whole come nearer to meeting this demand for a general working hypothesis than any other single concept. It being admitted that the sensori-motor apparatus represents both the unit of nerve structure and the type of nerve function, the image of this relationship passed over into psychology, and became an organizing principle to hold together the multiplicity of fact.

In criticising this conception it is not intended to make a plea for the principles of explanation and classification which the reflex arc idea has replaced; but, on the contrary, to urge that they are not sufficiently displaced, and that in the idea of the sensori-motor circuit, conceptions of the nature of sensation and of action derived from the nominally displaced psychology are still in control.

The older dualism between sensation and idea is repeated in the current dualism of peripheral and central structures and functions; the older dualism of body and soul finds a distinct echo in the current dualism of stimulus and response. Instead of interpreting the character of sensation, idea and action from their place and function in the sensori-motor circuit, we still incline to interpret the latter from our preconceived and preformulated ideas of rigid distinctions between sensations, thoughts and acts. The sensory stimulus is one thing, the central activity, standing for the idea, is another thing, and the motor discharge, standing for the act proper, is a third. As a result, the reflex arc is not a comprehensive, or organic unity, but a patchwork of disjointed parts, a mechanical conjunction of unallied processes. What is needed is that the principle underlying the idea of the reflex arc as the fundamental psychical unity shall react into and determine the values of its constitutive factors. More specifically, what is wanted is that sensory stimulus, central connections and motor responses shall be viewed, not as separate and complete entities in themselves, but as divisions of labor, functioning factors, within the single concrete whole, now designated the reflex arc.

What is the reality so designated? What shall we term that which is not sensation-followed-by-idea-followed-by-movement, but which is primary; which is, as it were, the psychical organism of which sensation, idea and movement are the chief organs? Stated on the physiological side, this reality may most conveniently be termed coördination. This is the essence of the facts held together by and subsumed under the reflex arc concept. Let us take, for our example, the familiar child-candle instance. (James, Psychology, Vol. I, p. 25.) The ordinary interpretation would say the sensation of light is a stimulus to the grasping as a response, the burn resulting is a stimulus to withdrawing the hand as response and so on. There is, of course, no doubt that is a rough practical way of representing the process. But when we ask for its psychological adequacy, the case is quite different. Upon analysis, we find that we begin not with a sensory stimulus, but with a sensori-motor coördination, the optical-ocular, and that in a certain sense it is the movement which is primary, and the sensation which is secondary, the movement of body, head and eye muscles determining the quality of what is experienced. In other words, the real beginning is with the act of seeing; it is looking, and not a sensation of light. The sensory quale gives the value of the act, just as the movement furnishes its mechanism and control, but both sensation and movement lie inside, not outside the act.

Now if this act, the seeing, stimulates another act, the reaching, it is because both of these acts fall within a larger coördination; because seeing and grasping have been so often bound together to reinforce each other, to help each other out, that each may be considered practically a subordinate member of a bigger coördination. More specifically, the ability of the hand to do its work will depend, either directly or indirectly, upon its control, as well as its stimulation, by the act of vision. If the sight did not inhibit as well as excite the reaching, the latter would be purely indeterminate, it would be for anything or nothing, not for the particular object seen. The reaching, in turn, must both stimulate and control the seeing. The eye must be kept upon the candle if the arm is to do its work; let it wander and the arm takes up another task. In other words, we now have an enlarged and transformed coördination; the act is seeing no less than before, but it is now seeing-for-reaching purposes. There is still a sensori-motor circuit, one with more content or value, not a substitution of a motor response for a sensory stimulus.[1]

Now take the affairs at its next stage, that in which the child gets burned. It is hardly necessary to point out again that this is also a sensori-motor coördination and not a mere sensation. It is worth while, however, to note especially the fact that it is simply the completion, or fulfillment, of the previous eye-arm-hand coördination and not an entirely new occurrence. Only because the heat-pain quale enters into the same circuit of experience with the optical-ocular and muscular quales, does the child learn from the experience and get the ability to avoid the experience in the future.

More technically stated, the so-called response is not merely *to* the stimulus; it is *into* it. The burn is the original seeing,

[1] See THE PSYCHOLOGICAL REVIEW for May, 1896, p. 253, for an excellent statement and illustration, by Messrs. Angell and Moore, of this mutuality of stimulation.

the original optical-ocular experience enlarged and transformed in its value. It is no longer mere seeing; it is seeing-of-a light-that-means-pain-when-contact-occurs. The ordinary reflex arc theory proceeds upon the more or less tacit assumption that the outcome of the response is a totally new experience; that it is, say, the substitution of a burn sensation for a light sensation through the intervention of motion. The fact is that the sole meaning of the intervening movement is to maintain, reinforce or transform (as the case may be) the original quale; that we do not have the replacing of one sort of experience by another, but the development (or as it seems convenient to term it) the mediation of an experience. The seeing, in a word, remains to control the reaching, and is, in turn, interpreted by the burning.[1]

The discusssion up to this point may be summarized by saying that the reflex arc idea, as commonly employed, is defective in that it assumes sensory stimulus and motor response as distinct psychical existences, while in reality they are always inside a coördination and have their significance purely from the part played in maintaining or reconstituting the coördination; and (secondly) in assuming that the quale of experience which precedes the 'motor' phase and that which succeeds it are two different states, instead of the last being always the first reconstituted, the motor phase coming in only for the sake of such mediation. The result is that the reflex arc idea leaves us with a disjointed psychology, whether viewed from the standpoint of development in the individual or in the race, or from that of the analysis of the mature consciousness. As to the former, in its failure to see that the arc of which it talks is virtually a circuit, a continual reconstitution, it breaks continuity and leaves us nothing but a series of jerks, the origin of each jerk to be sought outside the process of experience itself, in either an external pressure of 'environment,' or else in an unaccountable spontaneous variation from within the 'soul' or the 'organism.'[2] As to the latter, failing to see the unity of activity,

[1] See, for a further statement of mediation, my *Syllabus of Ethics*, p. 15.

[2] It is not too much to say that the whole controversy in biology regarding the source of variation, represented by Weismann and Spencer respectively,

no matter how much it may prate of unity, it still leaves us with sensation or peripheral stimulus; idea, or central process (the equivalent of attention); and motor response, or act, as three disconnected existences, having to be somehow adjusted to each other, whether through the intervention of an extra-experimental soul, or by mechanical push and pull.

Before proceeding to a consideration of the general meaning for psychology of the summary, it may be well to give another descriptive analysis, as the value of the statement depends entirely upon the universality of its range of application. For such an instance we may conveniently take Baldwin's analysis of the reactive consciousness. In this there are, he says (Feeling and Will, p. 60), "three elements corresponding to the three elements of the nervous arc. First, the receiving consciousness, the stimulus—say a loud, unexpected sound; second, the attention involuntarily drawn, the registering element; and, third, the muscular reaction following upon the sound—say flight from fancied danger." Now, in the first place, such an analysis is incomplete; it ignores the status prior to hearing the sound. Of course, if this status is irrelevant to what happens afterwards, such ignoring is quite legitimate. But is it irrelevant either to the quantity or the quality of the stimulus?

If one is reading a book, if one is hunting, if one is watching in a dark place on a lonely night, if one is performing a chemical experiment, in each case, the noise has a very different psychical value; it is a different experience. In any case, what proceeds the 'stimulus' is a whole act, a sensori-motor coördination. What is more to the point, the 'stimulus' emerges out of this coördination; it is born from it as its matrix; it represents as it were an escape from it. I might here fall back upon authority, and refer to the widely accepted sensation continuum theory, according to which the sound cannot be absolutely *ex abrupto* from the outside, but is simply a shifting

arises from beginning with stimulus or response instead of with the coördination with reference to which stimulus and response are functional divisions of labor. The same may be said, on the psychological side, of the controversy between the Wundtian 'apperceptionists' and their opponents. Each has a *disjectum membrum* of the same organic whole, whichever is selected being an arbitrary matter of personal taste.

of focus of emphasis, a redistribution of tensions within the former act; and declare that unless the sound activity had been present to some extent in the prior coördination, it would be impossible for it now to come to prominence in consciousness. And such a reference would be only an amplification of what has already been said concerning the way in which the prior activity influences the value of the sound sensation. Or, we might point to cases of hypnotism, mono-ideaism and absent-mindedness, like that of Archimedes, as evidences that if the previous coördination is such as rigidly to lock the door, the auditory disturbance will knock in vain for admission to consciousness. Or, to speak more truly in the metaphor, the auditory activity must already have one foot over the threshold, if it is ever to gain admittance.

But it will be more satisfactory, probably, to refer to the biological side of the case, and point out that as the ear activity has been evolved on account of the advantage gained by the whole organism, it must stand in the strictest histological and physiological connection with the eye, or hand, or leg, or whatever other organ has been the overt center of action. It is absolutely impossible to think of the eye center as monopolizing consciousness and the ear apparatus as wholly quiescent. What happens is a certain relative prominence and subsidence as between the various organs which maintain the organic equilibrium.

Furthermore, the sound is not a mere stimulus, or mere sensation; it again is an act, that of hearing. The muscular response is involved in this as well as sensory stimulus; that is, there is a certain definite set of the motor apparatus involved in hearing just as much as there is in subsequent running away. The movement and posture of the head, the tension of the ear muscles, are required for the 'reception' of the sound. It is just as true to say that the sensation of sound arises from a motor response as that the running away is a response to the sound. This may be brought out by reference to the fact that Professor Baldwin, in the passage quoted, has inverted the real order as between his first and second elements. We do not have first a sound and then activity of attention, unless sound is taken as mere nervous shock or physical event, not as conscious value. The conscious sensation of sound depends upon the motor response having already taken place; or, in terms of the previous statement (if stimulus is used as a conscious fact, and not as a mere physical event) it is the motor response or attention which constitutes that, which finally becomes the stimulus to another act. Once more, the final 'element,' the running away, is not merely motor, but is sensori-motor, having its sensory value and its muscular mechanism. It is also a coördination. And, finally, this sensori-motor coördination is not a new act, supervening upon what preceded. Just as the 'response' is necessary to constitute the stimulus, to determine it as sound and as this kind of sound, of wild beast or robber, so the sound experience must persist as a value in the running, to keep it up, to control it. The motor reaction involved in the running is, once more, into, not merely to, the sound. It occurs to change the sound, to get rid of it. The resulting quale, whatever it may be, has its meaning wholly determined by reference to the hearing of the sound. It is that experience mediated.[1] What we have is a circuit, not an arc or broken segment of a circle. This circuit is more truly termed organic than reflex, because the motor response determines the stimulus, just as truly as sensory stimulus determines movement. Indeed, the movement is only for the sake of determining the stimulus, of fixing what kind of a stimulus it is, of interpreting it.

I hope it will not appear that I am introducing needless refinements and distinctions into what, it may be urged, is after all an undoubted fact, that movement as response follows sensation as stimulus. It is not a question of making the account of the process more complicated, though it is always wise to be-

[1] In other words, every reaction is of the same type as that which Professor Baldwin ascribes to imitation alone, viz., circular. Imitation is simply that particular form of the circuit in which the 'response' lends itself to comparatively unchanged maintainance of the prior experience. I say comparatively unchanged, for as far as this maintainance means additional control over the experience, it is being psychically changed, becoming more distinct. It is safe to suppose, moreover, that the 'repetition' is kept up only so long as this growth or mediation goes on. There is the new-in-the-old, if it is only the new sense of power.

ware of that false simplicity which is reached by leaving out of account a large part of the problem. It is a question of finding out what stimulus or sensation, what movement and response mean; a question of seeing that they mean distinctions of flexible function only, not of fixed existence; that one and the same occurrence plays either or both parts, according to the shift of interest; and that because of this functional distinction and relationship, the supposed problem of the adjustment of one to the other, whether by superior force in the stimulus or an agency *ad hoc* in the center or the soul, is a purely self-created problem.

We may see the disjointed character of the present theory, by calling to mind that it is impossible to apply the phrase 'sensori-motor' to the occurrence as a simple phrase of description; it has validity only as a term of interpretation, only, that is, as defining various functions exercised. In terms of description, the whole process may be sensory or it may be motor, but it cannot be sensori-motor. The 'stimulus,' the excitation of the nerve ending and of the sensory nerve, the central change, are just as much, or just as little, motion as the events taking place in the motor nerve and the muscles. It is one uninterrupted, continuous redistribution of mass in motion. And there is nothing in the process, from the standpoint of description, which entitles us to call this reflex. It is redistribution pure and simple; as much so as the burning of a log, or the falling of a house or the movement of the wind. In the physical process, as physical, there is nothing which can be set off as stimulus, nothing which reacts, nothing which is response. There is just a change in the system of tensions.

The same sort of thing is true when we describe the process purely from the psychical side. It is now all sensation, all sensory quale; the motion, as psychically described, is just as much sensation as is sound or light or burn. Take the withdrawing of the hand from the candle flame as example. What we have is a certain visual-heat-pain-muscular-quale, transformed into another visual-touch-muscular-quale—the flame now being visible only at a distance, or not at all, the touch sensation being altered, etc. If we symbolize the original visual quale by v, the temperature by h, the accompanying muscular sensation by m, the whole experience may be stated as vhm-vh*m-vhm'; m* being the quale of withdrawing, *m'* the sense of the status after the withdrawal. The motion is not a certain kind of existence; it is a sort of sensory experience interpreted, just as is candle flame, or burn from candle flame. All are on a par.

But, in spite of all this, it will be urged, there is a distinction between stimulus and response, between sensation and motion. Precisely; but we ought now to be in a condition to ask of what nature is the distinction, instead of taking it for granted as a distinction somehow lying in the existence of the facts themselves. We ought to be able to see that the ordinary conception of the reflex arc theory, instead of being a case of plain science, is a survival of the metaphysical dualism, first formulated by Plato, according to which the sensation is an ambiguous dweller on the border land of soul and body, the idea (or central process) is purely psychical, and the act (or movement) purely physical. Thus the reflex arc formulation is neither physical (or physiological) nor psychological; it is a mixed materialistic-spiritualistic assumption.

If the previous descriptive analysis has made obvious the need of a reconsideration of the reflex arc idea, of the nest of difficulties and assumptions in the apparently simple statement, it is now time to undertake an explanatory analysis. The fact is that stimulus and reponse are not distinctions of existence, but teleological distinctions, that is, distinctions of function, or part played, with reference to reaching or maintaining an end. With respect to this teleological process, two stages should be discriminated, as their confusion is one cause of the confusion attending the whole matter. In one case, the relation represents an organization of means with reference to a comprehensive end. It represents an accomplished adaptation. Such is the case in all well developed instincts, as when we say that the contact of eggs is a stimulus to the hen to set; or the sight of corn a stimulus to pick; such also is the case with all thoroughly formed habits, as when the contact with the floor stimulates walking. In these instances there is no question of consciousness of stimulus *as* stimulus, of response *as* response.

There is simply a continuously ordered sequence of acts, all adapted in themselves and in the order of their sequence, to reach a certain objective end, the reproduction of the species, the preservation of life, locomotion to a certain place. The end has got thoroughly organized into the means. In calling one stimulus, another response we mean nothing more than that such an orderly sequence of acts is taking place. The same sort of statement might be made equally well with reference to the succession of changes in a plant, so far as these are considered with reference to their adaptation to, say, producing seed. It is equally applicable to the series of events in the circulation of the blood, or the sequence of acts occurring in a self-binding reaper.[1]

Regarding such cases of organization viewed as already attained, we may say, positively, that it is only the assumed common reference to an inclusive end which marks each member off as stimulus and response, that apart from such reference we have only antecedent and consequent;[2] in other words, the distinction is one of interpretation. Negatively, it must be pointed out that it is not legitimate to carry over, without change, exactly the same order of considerations to cases where it is a question of *conscious* stimulation and response. We may, in the above case, regard, if we please, stimulus and response each as an entire act, having an individuality of its own, subject even here to the qualification that individuality means not an entirely independent whole, but a division of labor as regards maintaining or reaching an end. But in any case, it is an act, a sensori-motor coördination, which stimulates the response, itself in turn sensori-motor, not a sensation which stimulates a movement. Hence the illegitimacy of identifying, as is so often done, such cases of organized instincts or habits with the so-called reflex arc, or of transferring, without modification, considerations

[1] To avoid misapprehension, I would say that I am not raising the question as to how far this teleology is real in any one of these cases; real or unreal, my point holds equally well. It is only when we regard the sequence of acts *as if* they were adapted to reach some end that it occurs to us to speak of one as stimulus and the other as response. Otherwise, we look at them as a *mere* series.

[2] Whether, even in such a determination, there is still not a reference of a more latent kind to an end is, of course, left open.

valid of this serial coördination of acts to the sensation-movement case.

The fallacy that arises when this is done is virtually the psychological or historical fallacy. A set of considerations which hold good only because of a completed process, is read into the content of the process which conditions this completed result. A state of things characterizing an outcome is regarded as a true description of the events which led up to this outcome; when, as a matter of fact, if this outcome had already been in existence, there would have been no necessity for the process. Or, to make the application to the case in hand, considerations valid of an attained organization or coördination, the orderly sequence of minor acts in a comprehensive coördination, are used to describe a process, viz., the distinction of mere sensation as stimulus and of mere movement as response, which takes place only because such an attained organization is no longer at hand, but is in process of constitution. Neither mere sensation, nor mere movement, can ever be either stimulus or response; only an act can be that; the *sensation* as stimulus means the lack of and search for such an objective stimulus, or orderly placing of an act; just as mere movement as response means the lack of and search for the right act to complete a given coördination.

A recurrence to our example will make these formulæ clearer. As long as the seeing is an unbroken act, which is as experienced no more mere sensation than it is mere motion (though the onlooker or psychological observer can interpret it into sensation and movement), it is in no sense the sensation which stimulates the reaching; we have, as already sufficiently indicated, only the serial steps in a coördination of *acts*. But now take a child who, upon reaching for bright light (that is, exercising the seeing-reaching coördination) has sometimes had a delightful exercise, sometimes found something good to eat and sometimes burned himself. *Now the response is not only uncertain, but the stimulus is equally uncertain; one is uncertain only in so far as the other is.* The real problem may be equally well stated as either to discover the right stimulus, to constitute the stimulus, or to discover, to constitute, the response. The question of whether to reach or to abstain from reaching is the question what sort of a bright light have we here? Is it the one which means playing with one's hands, eating milk, or burning one's fingers? The stimulus must be constituted for the response to occur. Now it is at precisely this juncture and because of it that the distinction of sensation as stimulus and motion as response arises.

The sensation or conscious stimulus is not a thing or existence by itself; it is that phase of a coördination requiring attention because, by reason of the conflict within the coördination, it is uncertain how to complete it. It is to doubt as to the next act, whether to reach or no, which gives the motive to examining the act. The end to follow is, in this sense, the stimulus. It furnishes the motivation to attend to what has just taken place; to define it more carefully. From this point of view the discovery of the stimulus is the 'response' to possible movement as 'stimulus.' We must have an anticipatory sensation, an image, of the movements that may occur, together with their respective values, before attention will go to the seeing to break it up as a sensation of light, and of light of this particular kind. It is the initiated activities of reaching, which, inhibited by the conflict in the coördination, turn round, as it were, upon the seeing, and hold it from passing over into further act until its quality is determined. Just here the act as objective stimulus becomes transformed into sensation as possible, as conscious, stimulus. Just here also, motion as conscious response emerges.

In other words, sensation as stimulus does not mean any particular psychical *existence*. It means simply a function, and will have its value shift according to the special work requiring to be done. At one moment the various activities of reaching and withdrawing will be the sensation, because they are that phase of activity which sets the problem, or creates the demand for, the next act. At the next moment the previous act of seeing will furnish the sensation, being, in turn, that phase of activity which sets the pace upon which depends further action. Generalized, sensation as stimulus, is always that phase of activity requiring to be defined in order that a coördination may be completed. What the sensation will be in particular at a given time, therefore, will depend entirely upon the way in which an activity is being used. It has no fixed quality of its own. The search for the stimulus is the search for exact conditions of action; that is, for the state of things which decides how a beginning coördination should be completed.

Similarly, motion, as response, has only a functional value. It is whatever will serve to complete the disintegrating coördination. Just as the discovery of the sensation marks the establishing of the problem, so the constitution of the response marks the solution of this problem. At one time, fixing attention, holding the eye fixed, upon the seeing and thus bringing out a certain quale of light is the response, because that is the particular act called for just then; at another time, the movement of the arm away from the light is the response. There is nothing in itself which may be labelled response. That one certain set of sensory quales should be marked off by themselves as 'motion' and put in antithesis to such sensory quales as those of color, sound and contact, as legitimate claimants to the title of sensation, is wholly inexplicable unless we keep the difference of function in view. It is the eye and ear sensations which fix for us the problem; which report to us the conditions which have to be met if the coördination is to be successfully completed; and just the moment we need to know about our movements to get an adequate report, just that moment, motion miraculously (from the ordinary standpoint) ceases to be motion and become 'muscular sensation.' On the other hand, take the change in values of experience, the transformation of sensory quales. Whether this change will or will not be interpreted as movement, whether or not any consciousness of movement will arise, will depend upon whether this change is satisfactory, whether or not it is regarded as a harmonious development of a coördination, or whether the change is regarded as simply a means in solving a problem, an instrument in reaching a more satisfactory coördination. So long as our experience runs smoothly we are no more conscious of motion as motion than we are of this or that color or sound by itself.

To sum up: the distinction of sensation and movement as stimulus and response respectively is not a distinction which can be regarded as descriptive of anything which holds of psychical events or existences as such. The only events to which the terms stimulus and response can be descriptively applied are to minor acts serving by their respective positions to the maintenance of some organized coördination. The conscious stimulus or sensation, and the conscious response or motion, have a special genesis or motivation, and a special end or function. The reflex arc theory, by neglecting, by abstracting from, this genesis and this function gives us one disjointed part of a process as if it were the whole. It gives us literally an arc, instead of the circuit; and not giving us the circuit of which it is an arc, does not enable us to place, to center, the arc. This arc, again, falls apart into two separate existences having to be either mechanically or externally adjusted to each other.

The circle is a coördination, some of whose members have come into conflict with each other. It is the temporary disintegration and need of reconstitution which occasions, which affords the genesis of, the conscious distinction into sensory stimulus on one side and motor response on the other. The stimulus is that phase of the forming coördination which represents the conditions which have to be met in bringing it to a successful issue; the response is that phase of one and the same forming coördination which gives the key to meeting these conditions, which serves as instrument in effecting the successful coördination. They are therefore strictly correlative and contemporaneous. The stimulus is something to be discovered; to be made out; if the activity affords its own adequate stimulation, there is no stimulus save in the objective sense already referred to. As soon as it is adequately determined, then and then only is the response also complete. To attain either, means that the coördination has completed itself. Moreover, it is the motor response which assists in discovering and constituting the stimulus. It is the holding of the movement at a certain stage which creates the sensation, which throws it into relief.

It is the coördination which unifies that which the reflex arc concept gives us only in disjointed fragments. It is the circuit within which fall distinctions of stimulus and response as functional phases of its own mediation or completion. The point of this story is in its application; but the application of it to the question of the nature of psychical evolution, to the distinction between sensational and rational consciousness, and the nature of judgment must be deferred to a more favorable opportunity.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

III. The Organic Effects of Agreeable and Disagreeable Stimuli.

BY JAMES ROWLAND ANGELL AND SIMON F. McLENNAN.

Amid all the recent discussion upon the significance of the activities of the physical organism under conditions of emotional excitement, and in other affective states, there has been an apparent consensus of opinion, that states of consciousness belonging to the two general classes agreeable and disagreeable, are accompanied on the one hand, by conditions of expanded vascularity and heightened muscle tone of the voluntary system, and on the other hand, by vascular constrictions and depressed muscle tone of the voluntary system. The greater emphasis is ordinarily laid upon the vascular alterations and accompanying disturbances in the involuntary system. The confidence in this doctrine rests upon a fairly wide basis of experiment, and yet certain restricting corollaries need to be pointed out. The present piece of work, forming part of a broader inquiry into the nature of affective states, can only hope to add a mite to the general store of information upon the topic in hand.[1] Nor does it pretend to deal with the more fundamental physiological aspects of the problem still left unsolved. For example, we have no theory to offer, and no conclusive evidence to show, whether the fluctuating vascular conditions to be commented upon are due entirely to alterations in the action of the heart, or in part to the vascular system; nor yet whether the dilators or constrictors of the latter system or both are concerned in producing the changes.

[1] Many of the points we shall touch upon have already been more or less fully reported, but seldom with any proper emphasis on their connection with one another, which is what we shall dwell on.

Incidentally we may say that the frequency with which alterations of breathing, pulse beat and blood supply occur in conjunction, would point to the probability that the effects are due to no one set of organic processes, but rather to diffused disturbances in several of the higher centers. This diffusion might, of course, be a secondary phenomenon of the nature of a reflex, discharged from a center primarily affected, but the results give no evidence specially suggestive of such a state of things.

This general class of considerations, however, appears to us to possess less immediate importance than those which we propose to urge. We desire to emphasize, on the basis of a large number of experiments (we retain as trustworthy over 1,100 of all we have made), conducted under conditions of great care, certain fundamental difficulties connected with this method of investigating affective states. While offering very little that is distinctly new, we purpose to bring into strong relief the discrepancies of the method, and to call in question again the exact significance and worth of results attained through its use.

Before proceeding to a detailed discussion of our subject, a few words are in order concerning our apparatus, method of work, etc.

A modification of Mosso's plethysmograph, hung in a swing, served to give us both the vasomotor disturbances and the pulse beat. The plethysmograph was connected with Marey tambours writing on the drum of a Stoelting kymograph—a machine which is practically noiseless and exceedingly constant in its running. The hand and arm up to the elbow were immersed in the water of the plethysmograph. The changes in the breathing were registered by means of tambours arranged as a pneumograph. A wooden spur attached to the breast and pressing against a tambour permitted us to get the slightest fluctuations. We abandoned the use of a cardiograph and sphygmograph, upon finding that our other arrangements were going to give us the essential points in which we were interested, at a great saving of labor. The senses of smell, taste, hearing and sight were experimented upon, such stimuli being used as would, supposedly, produce affective states readily distinguishable as agreeable, or disagreeable. For sight stimulations rotating disks were employed; for smell, cologne, bayrum, assafœtida, iodoform and turpentine were used; for taste, sugar, salt, capsicum and quassia; for sound, (1) noises of various kinds, *e. g.*, rasping, snapping, grinding, (2) tones from mounted tuning forks. The external conditions, atmospheric and otherwise, were kept as constant and favorable as possible. The subjects, with whom most of the work was done, were selected from a considerable number of students, as being those who gave the most unequivocal results.

Stated again and a little more narrowly, we were concerned with the interpretation of certain organic disturbances due merely to processes, *initiated* in the centers, as compared with those due to *peripherally* excited affective conditions. We noticed very early in the experimentation that the alterations in organic conditions under examination depended, as has been recognized,[1] in very large measure upon the thorough processes at that time in progress. It is, of course, impossible to control these entirely, and the difficulty is increased when, as in the present instance, it becomes highly desirable to have the mind as nearly quiescent as possible in order to obtain unmistakable evidence of the alterations due to the various stimuli employed to produce the agreeable and disagreeable states. That is to say, before drowsiness comes on, as it generally does, if the mind is kept quiet for a little, it is often necessary to make considerable effort of attention to keep the thought processes from running off into all sorts of vagaries of revery, any portion of which may call up affective disturbances. The mind, if kept 'empty,' as we say, is frequently kept so only with strain, and this strain then diverts attention from the incoming stimuli and so complicates the affective conditions, which they are intended to set up. Thus, one gets the not affective state brought on by stimulus, but the affective state modified by and blended with the prevailing mental state, which may itself be already affective, or in any event unfavorable to the unambiguous effectiveness of the stimulus. In this connection we found that by artificially

[1] In this general connection may be mentioned Mosso's observations upon attention and cortical circulation.

altering the thought processes, regardless of the external stimuli, organic disturbances could be produced essentially similar, in kind if not degree, to those which occurred under the affective conditions induced by peripheral stimulation. Indeed, were there not experimental evidence for it, one might fairly anticipate, from the general interconnection of mental and bodily states, that the change in mental processes would, regardless of its affective tones, manifest itself in some change of bodily condition.

To be still more specific on this head, we find, for instance, that disagreeable stimulations of taste and smell produce incoordinated and spasmodic breathing, depressions and irregularities of pulse and decrease of blood supply to the periphery—meaning by the periphery not simply the skin, but the total member concerned, in this case the hand and forearm. These conditions become increasingly violent and spasmodic, as the intensity of the stimulation and the lack of expectation in the subject increases. The degree of uniformity in this increase we have not attempted to measure accurately. The general fact, has, however, been shown clearly. Moreover, the after effect as revealed in these ways continues for a very considerable time. A reverse condition in the organic processes manifests itself when agreeable stimuli of moderate intensity are employed, with a somewhat important exception to be mentioned later. But now we find these identical motor disturbances repeated in the same form, though generally in less degree, when the subject is left to his own meditations, or when he is required to indulge in mental gymnastics, such, for example, as performing mathematical calculations and this too, quite regardless of any peripheral stimulation. Similar results show themselves when the subject is allowed to read. Often marked changes in the breathing and blood supply—less marked in the pulse—occur when the thought process reveals little or nothing adequate, subjectively considered, to produce the disturbance. These fluctuations could, of course be accounted for on purely physiological grounds, as due to changes in the chemical conditions in the blood, brought on by any one of a dozen physical causes directly affecting the centers. But when the mind is kept perfectly passive, and especially when the first stages of sleep are coming on, we find, almost without exception, that all these organic processes are quiet and undisturbed. This fact tends to render it probable that the changes are often, if not always, caused by cortical conditions regarded independently of the mere physical environment. We have already stated that the atmospheric conditions, etc., were kept as constant as possible. It is possible that some of these results are due to what one may call the general mood, which prevails at the time, but this does not militate against our contention.

From the standpoint of method then, we must maintain that any attempt to use these particular organic activities as avenues of approach, in the study of delicate affective conditions, appears essentially impossible, at least with any appliances now at hand. And this not because affective conditions are not represented here, but because so many other factors, in no fair sense to be recognized as affective, enter in. In the case of coarser affective states the results of previous investigations are substantially corroborated by our own.[1] But it is not *invariably* true that the deliverances of consciousness and the performance of the organism coincide, *e. g.*, it does not *always* occur that a stimulus pronounced agreeable is followed by observable increase of blood supply to the periphery. Indeed, there is considerable difficulty in obtaining stimulations of short durations, whose effects, therefore, are at

[1] Without furnishing a complete tabulation of our results, which could not, unless accompanied by cuts of the curves, be made very intelligible, we may say, that with stimulations felt as clearly disagreeable, about 90% of all the cases show a fall in the various curves. The percentage of cases of rise in the curves, corresponding to agreeable stimuli, has been considerably smaller. But this is in large measure, no doubt, to be accounted for by the relative weakness of the pleasure tone arising from the stimuli. The difficulty encountered in obtaining stimuli to produce agreeable affective states is mentioned elsewhere in the paper. In cases where the subject reports the stimulus as indifferent, we get both kinds of result, with consequent ambiguity in the significance.

It is interesting to note, that in cases where the attention was strongly focused on intellectual activities, such as reading or mental arithmetic, about 25% of the results show alliance with the agreeable affective states by a slight but continued rise in the curves, while 75% show a more or less sudden and marked decrease. The disturbances in these processes, due to mere shifting of the attention, have already been somewhat studied abroad. It remains for someone to work up the significance of these facts for a psycho-physical theory of æsthetics.

all readily comparable, which possess any considerable strength of pleasure tone. Nor is there any obvious and exact correspondence in the degree of the subjectively expressed feeling tone with the *amount* of the disturbance in the organism. Stimuli to the various senses naturally show the widest differences in the degree which they affect particular ones of these processes, and the subjective effect keeps pace only in a general and often remote way. For example, agreeable and disagreeable odors influence the breathing process in a very pronounced manner. A very faint whiff of ammonia will in a merely reflex way produce considerable disturbance of this character, and yet, it may not be judged so disagreeable as a flickering light, which brings about much less change in these organic processes. So we feel justified in reiterating that the very complex conditions, under which affective states may be and are induced, renders it essentially impossible to employ this means of investigation, when delicate results are sought. In any event the general statement that agreeable states and disagreeable states are accompanied respectively by increase and decrease in the functional activities of the organic processes here considered, requires to be offset with the statement that other mental conditions, besides those subjectively recognized as affective, produce similar results, and that the amount of the bodily manifestation does not seem to run exactly parallel with the subjective estimation of the agreeableness or disagreeableness of the conscious state.

We mentioned above a minor point of divergence from most observations upon which we should comment. It is this, not infrequently it happens that a stimulus felt to be pleasurable, produces for a few seconds a decrease in the blood supply to the periphery and then a subsequent increase. So far as we could determine, this was in no sense due to the intensity of the stimulation, for then we should with relatively intense stimuli obtain subjective conditions, in which the agreeableness was questionable, but rather to a condition psychologically equivalent to shock or surprise, and springing in the case of our experiments from even the slightest maladjustment of expectation. This was by no means of sufficient intensity always to excite notice on the part of the subject, but it tends to lend new and striking testimony to the intimate connection of attraction with affective conditions. For, stated again and more concisely, we have here a case of an agreeable stimulus producing the characteristic external manifestations of a disagreeable stimulus, not because the existing mood, or affective state, is unpropitious, for both these may be neutral, but simply because the adjustment of the attention is not perfect. This peculiarity was especially marked in the case of the tuning-fork stimulations. We had supposed these would in most cases be felt as agreeable. But this was far from being always the case when at all loud, or long continued, or unexpected, they became distinctly disagreeable.

In conclusion, we may be permitted, perhaps, to insist that out of the purely negative considerations, which we have been urging, certain equally positive inferences are to be drawn. We believe that the results traversed in what we have said lend striking confirmation to the essential solidarity of consciousness, and to the utter futility of attempting to attack the problem of the peculiarities of any one aspect, without due regard to all the others involved. Affective states as such do manifest certain fairly constant and experimentally demonstrable motor expressions, but the same motor expressions are also characteristic of other conscious states, not recognizable as predominantly affective; nor do the bodily manifestations of these affective states run absolutely parallel with the latter. Observable changes in the one do not always betoken observable changes in the other. The impossibility of asserting in any particular case the relative significance of the bodily modifications for the affective state on the one hand, and the merely intellective, or cognitive, state on the other, renders it exceedingly problematic how one is to interpret results gained from this method. It is greatly to be hoped, however, that we may have a really careful test made on the intensive side of the exact relation obtaining between the amount of the bodily manifestation, and the subjective estimate of the degree of the agreeableness or disagreeableness felt.

IV. Simultaneous Sense Stimulations.

Practice Study.[1]

BY AMY TANNER AND KATE ANDERSON.

The published work of Urbantschitsch,[2] who has apparently made the most extended examination of the phenomena under consideration, contains no account of apparatus and but little of his method of procedure. The present study has been carried on with special reference to the peculiar effects produced upon attention and the interpretation of the same. In general, we find confirmation of Urbantschitsch's reported observations, but the wide divergences shown by his different subjects, and by the same subjects under different conditions, together with his apparent disregard of the effects of attention, render it probable that much of his report is untrustworthy and that more careful experimentation would give less equivocal results. We have confined ourselves to the partial interaction of auditory, visual and electrically stimulated tactual-muscular sensations, whereas Urbantschitsch examined the effects of the stimulation of each sense upon all the others.

Stated in terms of attention, the problem is this: When attention is focused upon a barely perceptible sensation, does the addition of another sensation render the first more or less perceptible? Or, in another form—is the threshold for any sensation raised or lowered by the presence of another sensation?

The same question can, of course, be put from the purely physiological side. Is the functional activity of any sense organ conditioned by the activity of any other? Is the inertia of the central nervous tracts connected with any sense organ affected by excitation in other sensory regions? The question is, however, essentially psycho-physical, and no solution which neglects this truth can really do justice to all the facts.

[1] In accordance with the usage of the laboratory in which their work has been done, the authors, in connection with an introductory course, began this study in a field *already* worked, in part for merely disciplinary purposes, in part to determine how adequately and carefully the previous investigations had been conducted.

[2] *Pflügers Archiv.*, 1888.

We find that in a large majority of cases, visual sensations just below or just at the threshold are, upon the introduction of a second sensation from either the same or another sense organ, brought clearly above the threshold. We find that in some considerable number of cases the mere enlargement of the field of attention results in a brightening of the center of the field. Upon this point Ladd[1] states: "Distraction of attention, if the aggregate of psychic energy be not increased, necessarily follows upon the introduction of any such new factor or object." If this statement be true, we must, on the basis of such results as are here offered, assert that the sum total of psychic energy is increased, ordinarily, if not invariably, when a new stimulus is given. Such a conception is certainly not current in our text-book treatments of sensation.

If we make sharply the distinction between the content of attention and the attentive activity itself, and also the distinction between attending to a clear sensation, and clearly or intensely attending to a sensation, then our results compel us to revise the old statement that the more things we attend to simultaneously the less clearly do we perceive any one, and to say that, when attention is directed to a content presented to any one sense, the simultaneous stimulation of other senses may enlarge and render more clear the field of the first sense, though we cannot speak with entire confidence as to whether or not the *activity* of the attention in this direction is or is not increased.

In merely neural terms the results seem to mean that the nervous system represents at any moment a certain amount of inertia; that this is attacked by every sense stimulation, and that the inertia of any region, such as that represented by the visual tracts, may be in part so overcome by disturbance from other regions, that nervous impulses otherwise ineffective may successfully penetrate to their appropriate cortical centers and there set up the processes which parallel consciousness. This result probably extends in some cases to the sense organs themselves. A portion of our experiments comes under this latter head. Such are the answers to our original inquiries when expressed in both psychical and physiological terms.

[1] *Psychology, Descriptive and Explanatory*, p. 72.

The tests here reported were made during the spring and fall of 1895. Three months were occupied in perfecting apparatus and making preliminary experiments. The present report refers to six weeks of work done under the improved conditions.

Our apparatus consisted of three tubes — light-tight, blackened inside, and sliding into each other. At one end was a perforated pasteboard slide arranged so that the light from colored glasses could be seen through the perforations. The light was daylight, reflected from a white pasteboard in front of the slide, and the glasses used were red, yellow, green and blue.

The auditory stimuli were given by tuning-forks of 256 v. and 2048 v., and also by the whirring noise of the vibrator on a DuBois Reymond coil. The intensity of the tones of the forks was controlled by a ball pendulum which was employed to strike them. Electrical stimuli from the coil were used to stimulate the skin of the palms. The added visual stimuli were colors of the same size as those first exposed. This case, of course, introduces the question of merely retinal peculiarities as distinguished from central effects. In the cases here reported the intensities of the stimuli were kept constant. In passing we may say that from other tests we incline to think, in opposition to Urbantschitsch, that the observable changes due to alterations of intensity are, except near the upper and lower limits, of relatively small significance.

The experiments were carried on in a quiet room in which experimenter and subject were alone. The subject sat at the open end of the tube with his face supported by a head-rest, and his head and shoulders covered by a camera cloth so that he was in total darkness and could see none of the operator's movements. We used only the right eye in our experiments, and in order to cover the left, and yet not strain it, the subject wore spectacles in which the right glass was removed and the left covered with black felt.

In order to obtain good conditions each subject was used only twenty minutes a day, and in case of unusual fatigue, cold, etc., was not used at all. Every precaution was observed to prevent the subjects from knowing either the purpose or results of the tests. In working for such purposes, with sensations near the thresholds this is indispensable to accuracy.

The method of procedure was as follows: At the outset of the experimentation the color thresholds for the various subjects were determined by means of the sliding tubes. After a brief pause, some one of the colors was shown, either at or just below the threshold. Then the second stimulus was given and the effect on the color reported. The length of time consumed by the various parts of the test was kept fairly constant, save that occasionally a little longer time than usual was allowed for the giving of the second stimulus, in order to be sure the changes were not due to pulses of attention, which so certainly figure in the report of Urbantschitsch. The greater constancy of our own results also argue in favor of this conclusion.

TABLE I.*

	Added Stimulus.—2048 v. fork.					Added Stimulus. 256 v. fork.				
	Continued Stimuli.				Totals.	Continued Stimuli.				Totals.
	Yellow.	Blue.	Green.	Red.		Yellow.	Blue.	Green.	Red.	
Faintly perceived	14	32	28	38	112	14	30	18	45	107
Unperceived.	5	19	14	17	55	1	11	8	13	33
Wrongly perceived.	41	9	18	5	73	45	19	34	2	100
Color brought out or intensified	49	56	56	57	218	52	53	51	56	212
Unchanged.	11	4	4	4	22	8	7	9	4	28

* In all these tables the number of subjects is three; and twenty tests were made with each of the subjects with each of the continued stimuli.

Table II.

	Added Stimulus—Red.					Added Stimulus—Yellow.				
	Continued Stimuli.				Totals.	Continued Stimuli.				Totals.
	Yellow.	Blue.	Green.			Blue.	Green.	Red.		
Faintly perceived.	41	29	45		115	15	39	28		82
Unperceived.	18	30	12		60	45	21	22		88
Wrongly perceived.	1	1	3		5	0	0	10		10
Color brought out or intensified	54	51	54		150	41	59	49		150
Unchanged.	6	9	6		21	18	1	11		30

	Added Stimulus—Blue.					Added Stimulus—Green.				
	Continued Stimuli.				Totals.	Continued Stimuli.				Totals.
	Red.	Green.	Yellow.			Red.	Blue.	Yellow.		
Faintly perceived.	13	15	41		69	17	9	47		73
Unperceived.	33	44	18		95	42	51	9		102
Wrongly perceived.	14	1	1		16	1	0	4		5
Color brought out or intensified	42	47	36		125	48	48	47		143
Unchanged.	18	13	24		55	12	12	13		37

Table III.

	Added Stimulus.—Electricity.					Added Stimulus.—Noise.				
	Continued Stimuli.				Totals.	Continued Stimuli.				Totals.
	Yellow.	Blue.	Green.	Red.		Yellow.	Blue.	Green.	Red.	
Faintly perceived.	27	9	2	18	56	58	21	17	30	108
Unperceived.	8	20	34	30	92	1	6	24	14	45
Wrongly perceived.	25	31	24	12	92	20	32	19	16	87
Color brought out or intensified	27	23	34	26	110	25	21	27	22	95
Unchanged.	33	37	26	34	130	35	39	33	38	145

TABLE IV.—RESULTS.

Number of tests.	Faintly perceived.	Unperceived.	Wrongly perceived.	Color brought out or intensified.	Color unaffected.
1680	722	570	388	1212	468

From the above experiments we see that 72+per cent. of the whole number gave positive results; with the 2048 fork, 90 per cent.; with the 256, 88 per cent.; with red, 88 per cent.; with yellow, 83 per cent.; with green, 80 per cent.; with blue, 70 per cent.; with electricity, 46 per cent.; with noise, 40 per cent.

The fact that only 40 per cent. of the experiments with noise are positive, contradicts the assumption that the results vary with the sense. Otherwise we might say that the associations of eye with ear are closer than those of one part of the retina with other parts, or than those of hand with eye. Further experiments may show that pleasure and pain have some constant relation to the effects; but here, so far as inquiries were made, the added stimuli were indifferent, except the electricity, which was disagreeable to A. A. F.

With colors, the red and yellow, which gave the largest per cent. of positive results, are visible at greater distances than the green and blue. But where red and yellow are the continued stimuli the results are no more positive than where green and blue are. That red and yellow should have this peculiarity apparently furnishes another presumption in favor of Heiny's theory.

Although the conditions were not all that could be desired, we failed, in connection with a considerable number of tests conducted by Miss Faith Clark, to find any *constant* positive effect upon the accommodating apparatus of the eye, either from the stimuli here mentioned, or from gustatory or olfactory stimulations. The elimination of the disturbances due to fluctuating attention on this form of the experiment are much more difficult and require much more delicate appliances.

SOME REMARKS UPON APPERCEPTION.

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The great significance which the conception of apperception has obtained in modern psychology necessitates a thorough understanding of the content of this conception. An historical investigation of the meanings that have been ascribed to the conception of apperception gives no positive and satisfactory result; not only has apperception been differently conceived by different philosophers, but many philosophers have used one of these conceptions for another, and even in the construction of a 'theory of apperception' have blended together a number of different psychical phenomena. Nevertheless, in order to analyze this notion, it will be necessary to arrange the separate types of these different conceptions into their different classes.[1]

In all we are able to gather from the history of psychology three types of the notion of apperception.

1. Apperception as an event which imparts clearness to representations.

2. Apperception as reflective knowledge.

3. Apperception as an act of knowledge produced by the impact of two groups of representations.

(1) *Apperception as an event which imparts clearness to representations.* The historical development of this conception begins with Descartes in his definition of a clear perception, his 'clear perception' being namely, one which manifests itself immediately and explicitly in an attentive intellect (Principes de la phil., trad. par Aimé Martin, p. 29f), just as when an object sufficiently affects the eye and the latter is disposed to see it. This conception was also used by Leibnitz in the 'Nouv. Ess.'[2]

[1] In regard to the proof for this affirmation see my 'Zur Analyse des Apperceptionsbegriffs.' Calvary & Companie, Berlin.

[2] *Nouv. Ess.*, p. 23, *Opera philosophica.*

According to the definition found there, apperception is to be distinguished only quantitatively from perception; from the summation or the strengthening of perceptions arises apperception. Lately the word apperception has been much used by Wundt in the sense of the clear perception of Descartes. This is particularly marked in his use of the Cartesian illustration in which perception is compared with a visible object. Perception, according to Wundt, corresponds to vision in the field of vision, apperception to vision in the fovea centralis.[1]

(2) *Apperception as reflective knowledge*, '*connaissance reflexive*.' This reflective knowledge consists in the act of thought, concerning the relation of the object of recognition and the thinking *ego*.

The first to ascribe this meaning to apperception was Leibnitz,[2] but this conception was left by him relatively undeveloped. He defines reflective knowledge as an act of thought about something that is taking place in our *ego*. This theory was taken up and extended by Wolf. According to Wolf, when we become conscious of the perceived object, we perceive a certain act of the soul, namely, apperception. We distinguish ourselves at once as perceiving subject from the perceived object. We recognize that the subject is different from this object.[3]

The fullest development of this theory is attained in the conception of Kant in his concept of transcendental apperception, which is namely the representation of the *ego* in relation to all other representations.[4] 'This original and necessary consciousness of the identity of self,' being at the same time the consciousness of an equally necessary synthesis of all phenomena through representations.

In many respects Wundt's theory is similar. Indeed, according to Wundt, apperception is at bottom the same function as will. But the essence of will is the feeling of individual doing and suffering.[5] This feeling, considered by Wundt as the single permanent state of the soul is what Kant designated

[1] *Phys. Psych. cf.* p. 236 f II, 4 Auflage.
[2] *Monad.* p. 15 f.
[3] *Psych.* Ration. p. 19.
[4] *Krit d. r. Vern.* p. 121, 2 Aufl. ausg. v. Kehrbach.
[5] *System* p. 384.

the transcendental *ego*. Thus apperception is the first manifestation of the will, which forms at the same time a basis for the continuity of consciousness and the identity of the individual.

(3) *Apperception as the production of knowledge through the impact of two groups of representations.* This conception of apperception was originated and developed by Herbart and his school. According to this conception, apperception is the process by which the incoming representation sets in motion already existing representations and at last, according to the laws of psychical mechanics, produces an end result, which must be considered as dependent on all active forces. This theory is based upon the assumption of an eternal existence of the representations and an artificial psychical mechanics, whose laws, carried out speculatively, are assumed to be analogous to physical laws.[1]

These are the three types of the definition of apperception, disclosed by an historical survey. Has one of these definitions a stronger claim to existence than another? Are all three definitions a delineation of three different phases of the same event, or are all three definitions a delineation of three separate and distinct events, which are classed as one and the same? And is apperception in all or any of these theories conceived as an especial and important function of the soul: as an especial form of activity? We can hope to answer these questions only by an analysis of the psychical acts, which have given rise to these psychological theories. Such an analysis necessitates the rejection of all speculative elements, and the setting forth of the psychical facts of experience, which are the kernel of these theories. This is the method, which will be attempted in the following pages.

Apperception as an Event which Imparts Consciousness or Clearness to Representations.

It is known that Kant used the conceptions apperception and consciousness interchangeably and this interchange is often ob-

[1] It may be noticed here, that the meaning which Steinthal, a disciple of Herbart, attributes to attention, is nearly the same as the meaning of apperception in the sense of 'Connaissance reflexive.' See *Einleitung in d. Psych.* p. 231. 2 Aufl.

served in all philosophers, who have occupied themselves with the theory of apperception, most of them having used as synonyms clearness and consciousness. But when it is a question of defining 'Clearness' more particularly the two definitions of apperception and clearness usually differ. According to Kant, 'empirical apperception' is the consciousness of self, as determined by our condition (Krit d. r. Vern. p. 673, 2 Aufl. Kehrbach), and 'clearness' the state, where the consciousness of a conception rises to a consciousness of its difference from other conceptions (p. 692). Thus notwithstanding that apperception, as an event, which imparts clearness to conception, may be easily identified with consciousness, this identification has been avoided, whenever the question of exactness arose.

The reason for this lies probably in the fact that most psychologists (with the exception of Steinthal) who have elaborated the apperception theory have used not the conception of *consciousness as a state*, but the conception of *consciousness as a substance*. On the other hand, the conception of *apperception as a function* of consciousness could not pertain to a substance. Consequently the relation of apperception and consciousness was as follows: Consciousness was believed to have the power of giving greater clearness to representations, this power being nothing else than apperception. In this way when we possess a representation, its clearness is not an inherent element, but something added to it by some extra power of consciousness. Apperception does not create clearness or consciousness, but serves to divide and to heighten it.

In addition to consciousness, considered as a substance, a ground for the assumption of an especial power of the soul for the regulation of clearness lies in the fact that the most intensive representations are not always the clearest. This problem of the separation of intensity and clearness is familiar to all who follow the history of modern psychology.

In reference to this problem it has happened as with many other problems, that it was not solved, but postponed, *i. e.*, since no fixed relation could be determined between intensity and clearness, the regulation of intensity was attributed to an especial power of the soul. This power of the soul, *i. e.*, apperception, was enabled to perform its function *spontaneously*. In this way it was believed possible to overcome the difficulty of distinguishing the phenomena of clearness and intensity in representations.

Recently this theory has been modified by Wundt. The regulation of consciousness is ascribed not to the metaphysical spontaneity of the soul, but to the empirical faculty of the will. The state of affairs is in consequence not greatly changed, since the metaphysical spontaneity of the soul is hereby simply relegated to the will, to which is given the creative power in evoking representations. In order to enter clearly into the foregoing questions, it is necessary to consider more closely the relationship between intensity and consciousness, and the theory of the spontaneity of the soul.

The Separation of Intensity and Consciousness in Psychical Phenomena.

The natural presumption is, that the most intensive in the psychical life is at the same time the clearest. This presumption is perhaps based upon an unauthorized generalization from a fact often observed. But wider experience shows us that often representations so weak as to be almost unable to awaken any considerable feeling suddenly rise into complete clearness. To explain this phenomenon Herbart enunciated his law of 'Hülfen,' according to which representations rise into consciousness, not met by reason of their own force, but in consequence of a favorable combination with other representations they are enabled to awaken consciousness and to thrust into the background other representations. In recent times two opposed theories are held in relation to this problem: the theory of association and the theory of will, but neither of these theories is sufficient to explain the problem and the adverse criticism of the adherents of both these theories is justified. In fact, if we ascribe to the will the bringing into consciousness of representations, it is necessary to consider will in a certain measure anthropomorphic. Since we know that we *choose* among conceptions, we now attribute this faculty to the will. It is no longer the man who executes the choice, but his will acts in this manner. The matter is thereby rendered not in the least clearer. What we have hitherto ascribed to our entire human nature is merely relegated without warrant to a province of our nature. This theory is furthermore unwarranted since it excludes the possibility of a consistent carrying out of the principle of the dependence of the psychical phenomena upon the physiological condition of the organism. The physiological phenomena are determined by the law of causality and subject to the law of the conservation of energy. A free activity of the will could in nowise be attached to such a strictly determined condition.[1]

Although the association theory does not contain this fundamental mistake, it is nevertheless insufficient, since it has not succeeded in giving the characteristics of the psychical phenomena which are conditions of a higher grade of consciousness. So it remains unknown, why in the whole mass of knowledge of the individual only especial representations and conceptions are able to undergo such associations that they reach the highest grade of consciousness.

The fault of these theories lies in the fact that they proceed to explanations before all the facts are discovered and their relations to one another described. In addition, in spite of the wish of many psychologists, that the objective, *i. e.*, the physiological condition of consciousness should be determined, most authors make no effort to do so. And yet this is the only way to obtain a general solution of the problem. Only on the basis of the relationship between psychical phenomena and central nervous processes, can we pretend to reach a satisfactory explanation.

Such an attempt we find in the theory of R. Avenarius (*Kritik d. reinen Erfahrung*, p. 51, B. I and p. 18, B. II), which considers intensity and consciousness as dependent on the centro-nervous processes from which they derive their especial character. In this way the difference between intensity and consciousness (in the sense of state of consciousness) is kept distinct. We know from experience, that the intensity among physical phenomena is dependent upon the size of the

[1] *Zur Analyse des Apperceptionsbegriffs*, p. 135.

waves. Hence we approach the supposition, that the *intensity of our representations depends immediately upon the size of the centro-nervous vibrations of a thinking individual.*

Concerning consciousness (consciousness considered as a state), we see that through the variations in the environment of an individual the consciousness of the individual may be transferred from one object to another. This fact leads us to the affirmation that *consciousness depends upon some deviation from the customary arrangement of the vibrations in the centro-nervous system* (Krit. d. r. Erf. See *Abhebung*, p. 57, B. II). Thus, since it often happens, that the increase in intensity of one part of the general impression is sufficient to change the customary arrangement of the centro-nervous vibrations it comes about that intensity and consciousness coincide.

The scientific value of this theory is apparent, since it is possible through variations in the environment of the individual or in his customary mass of knowledge to bring about a conscious state and to conduct the consciousness from one representation to another. We can proceed experimentally to augment or to decrease the intensity, we are able also to augment or to decrease consciousness in proportion to the time, which passes during the variation of some customary mass of knowledge.

Spontaneity in the Production of Representations.

The theory of spontaneity was formed by Leibnitz upon the ground of 'inner experience.' He declared in a letter to de Bayle: "L'experience interne refute la doctrine Epicurienne." According to him the spontaneity of the soul is necessary in order to explain that which we have observed in the 'inner experience.' Thus we see from an historical investigation that the ground which we mentioned above for the assumption of the spontaneity of the soul, namely, the separation of the phenomena of consciousness and intensity, is not the only one. An element presumably still more important lies in the fact that *we are able, without any known external cause to originate independent chains of thought.*

A third ground for this affirmation lies in the fact, that we are able to experience directly the feeling of the spontaneity of our acts. With the first of these three reasons, we are at present not occupied, but when we investigate the two latter more closely, we see that the explanation of these facts by means of the help of the theory of spontaneity arises from the old tendency to consider psychical phenomena of a central character as independent of physiological processes. But if we enter the ground of the dependence of psychical phenomena upon physiological states of the nervous system, there disappears the inconceivability of both the independent production of chains of thought and the sensation of spontaneity.

Indeed, it is well known to the physiologist, that the activity of the nerves arises not only in consequence of peripheral stimulation, but also as a result of the nutrition of the centers. The functions of the organism, which originate by means of the latter method, are the so-called 'automatic functions.' This automaticity of the central nervous system originates nervous processes, which have as correlates chains of thought, the causes of which can not be traced to other psychical phenomena. This fact has long been known and used in psychiatry, in the treatment of disturbances arising from an under or an over supply of blood to the brain.

We see, therefore, that the spontaneity of the soul is requisite to explain facts of this kind, but under the condition that we derive the psychical states unreservedly one from another as we do in dealing with physiological states. But so long as we retain the conception that psychical states are the corollaries of physiological states, which follow among themselves the law of causation, the theory of spontaneity remains useless.

The third ground for the assumption of spontaneity, the direct sensation of impulse in thought, has been so much discussed that this feeling no longer plays a rôle as something of mysterious importance. Although psychological analysis has not yet definitely determined, whether this feeling rests upon sensations coming from the organs and from muscular contractions, or whether it is a sensation of motion, produced in the central nervous system, it is nevertheless classed among the feelings under the name of a *feeling of innervation*. Consequently it is subject to the same laws as other feelings. Thus we see, that all three reasons for the acceptation of the spontaneity of the soul prove worthless. Consequently, apperception, as an event, which imparts clearness to representations is a useless conception, which really means no more than clearness, and has grown out of a conception of consciousness as a substance and rests upon a false idea of the spontaneity of the soul.

Apperception as Reflective Knowledge.

Apperception, in the sense of reflective knowledge was first taken into consideration as a fact of observation, but afterwards this fact received explanations, which came to be substituted for the fact itself. Naturally, through this circumstance, the whole theory, instead of being elucidated, was considerably obscured. We shall now endeavor to determine the results of observation.

When we recognize with full consciousness something that belongs to the world of thoughts or of affairs, this act of recognition is bound up with a *feeling of appropriation*. We know and feel at the same time, that *we* appropriate it, *i. e.*, *the ego appropriates the thought or the object which confronts it*. Along with this act we experience *feelings from the organs*, corresponding to the way, in which the knowledge originates. Thus we feel that an object is *perceived* or a thought is *conceived*.

Knowledge through reflection is, therefore, nothing else than a becoming conscious of this especial content of knowledge. In fact, according to the authors of the theory of reflective knowledge, the latter is merely the bringing into relation of the *ego* to an object thought of or perceived. That is to say, it is *the act of becoming conscious of the fact, that a certain determined value is thought of or perceived by the ego*.

Reflective knowledge, as this especial content of knowledge deserves indeed the important place that has been assigned to it in psychology, as will be seen from the following statements: Only a part of this content of knowledge is varied during the course of the individual life, namely, things or thoughts which form objects of recognition, for the *ego* with the organic feelings, which accompany perception or ideation, remains ever the same. It is, so to say, a *constant* in the psychical life.

Kant expressed this fact in the well-known proposition that the conception of self accompanies or may accompany every representation. In modern philosophy it has been especially pointed out by W. Schuppe[1] and R. Avenarius[2] that the *ego* and its *experiences* form a *single* system. Both *the self and its experiences belong always and inseparably together.*

But since notwithstanding that the representations of the *ego* may accompany every state of consciousness, this accompaniment does not always take place, we must determine more accurately what are the facts. Although the self and its experiences form a system in which the self represents a relatively unchanging member, nevertheless, the self is also an object of experience like all other experiences. Thus it is possible that the self with its experiences forms a content of knowledge, but on the other hand, it is possible that this content is formed only by the experiences of the self, *i. e.*, by things or by thoughts. In this case not the whole system of the self with its affairs and its thoughts is known, but only a part of the same. Not the whole relation is recognized but only one of its members is *absolutely* apprehended.[1] So for example, when a man recognizes a table and reflects only upon this object, he perceives the table *absolutely.* If, however, he recognizes the table as an *object perceived by him*, it is a case of *relative* knowledge. So it may be seen, that every absolute act of knowing may pass over into a relative act of knowing.

Is Reflective Knowledge an Especial Function of Mind?

We have stated above that reflective knowledge is a *content* of knowledge, which is deserving of especial attention. The question now arises whether reflective knowledge is not an especial function of mind? Apperception in the sense of reflective knowledge has generally been considered an especial function of the soul, even the highest function. We have seen

[1] *Erkentnisstheoretische Logik.*
[2] *Weltbegriff.*
[1] I use the expressions 'absolute' and 'relative' knowledge according to the terminology of Avenarius in *Weltbegriff*, p. 15.

already that knowledge through reflection is knowledge that is directed to the relation between the *ego* and its affairs and thoughts. But among our thoughts there are countless cases of knowledge directed to relations. Therefore, we can believe that reflective knowledge is a special function of mind only when the relation between the self and its thoughts and affairs includes elements of an especial nature.

The relation between the *ego* and its experiences was indeed conceived as something especial and different from all else. The reason for this lay in the conception of the *ego*, as a territory sharply bounded off from its whole environment. In this enclosed province the 'inner experience' was believed to reign. On the contrary the events of which the *ego* took cognizance were conceived as lying in the province of the 'outer experience.' As the two sorts of experiences were believed to be fundamentally different, apperception was assigned the important rôle of bringing the 'outer experiences' into the 'inner experiences' of the *ego*. The outside world was to be brought by apperception into the *ego*. The 'outer experiences' must come into contact with the 'inner experiences.'

So long as we remain in the province of the philosophy, which assumes the 'outer' and the 'inner' experiences as fundamentally different, we must accept 'apperception' as a special function of mind. But in case we do not accept the two kinds of experiences, as things fundamentally different, we cannot accept the act of bringing these experiences into relation as an event, which is peculiar among examples of relative thinking. Given the content of reflective knowledge, it is not possible by psychological analysis to prove the bringing of the 'outer experiences' into the 'inner experiences.' We have been able to prove only the feeling of appropriation, a state which depends upon certain modifications in our sensation of motion. Therefore, so long as we remain upon empirical ground we cannot receive as an element of apperception the act of bringing the 'outer' into the 'inner' experience. This is merely a theory based upon the assumption of a fundamental difference between the inner and the outer world—only a method of elucidation of the way in which it comes about that the *ego* sharply bounded off from the outside world appropriates something of the latter to itself. But since we demand not a speculative but an empirical psychology, we may not treat as facts the results of speculation. We must become clear upon the fact, that we cannot prove empirically the bringing of the 'outer' into the 'inner' world. If any one for the sake of some metaphysical theory wishes to assume that such a transformation may take place, he is naturally free to do so, but so far as we are concerned, we shall not make use of this assumption. The more so, as in the light of recent criticism the philosophical justification for such a course seems more than doubtful.[1]

Reflective knowledge must, therefore, be considered as a *content of knowledge*, which is produced through the relation between the *ego* and its affairs or thoughts, and which is accompanied by the specific organic feeling of thought or perception, together with the feeling of appropriation, which is a modification of the feeling of motion.

Apperception as the Production of Knowledge Through the Impact of Two Groups of Representations.

Apperception, as understood by the Herbartian school, may, in contrast to knowledge, through reflection, be defined as follows: Reflective knowledge is an event, where any conscious content of knowledge comes into contact with the consciousness of self, whereas apperception in its third signification is only the first part of this act. Only the act of the production of knowledge was investigated by Herbart. Every knowledge may, in the moment of its formation be divided into two parts: a new content of knowledge, and the determination of the same through previously acquired knowledge.[2]

This fact Herbart interprets from his speculative standpoint as the motion and impact of two different groups of representations of which the newer group awakes and is modified by the older group. To-day, when the belief in the eternal existence

[1] Reference may here be made to Schuppe's *Erkentnisstheoretische Logik*, R. Avenarius, *Weltbegriff*. E. Mach's *Zur Analyse der Empfindungen*.

[2] Wundt expressed nearly the same idea, when he declared that every cognition is at the same time a recognition.

of representations is no longer seriously maintained, such an hypothesis is no longer permissible. But the fact, which forms the basis of Herbart's theory remains. It is possible to prove both experimentally and by the aid of comparative philology, that with every act of knowledge there takes place either a combination of new with already existing values (namely a recognition in a certain limited sense of this word), or a grouping of new values within a class whose value has been previously known. Generally speaking, it may be said that an entirely new value cannot possibly form an object of knowledge until by means of frequent repetition of this value a new habit has been formed. Every act of consciousness be it single or composed of a chain of thought is always a process of reducing the unknown to the known. If, therefore, recognition does not take place immediately, the chain of thought will not be finished[1] until there is found a value which is equal or similar to or contained in the new value. If now this possibility is absent two others are at hand.[2]

In the normal development of a chain of thought the individual, after sufficient time, will form a new habit of thought. The chain of thought is in this case closed through a repetition of the originally given value but with a totally different quality of feeling. The 'unknown,' with its negative characteristics, disappears in presence of the feeling of the 'known.' A new habit of thought has been formed.

The other possibility is where the development of the chain of thought does not occur in a regular way. The interest of the thinking individual becomes transported from the original value to some other value and the former is characterized as 'without importance.'

The determination of every act of knowledge through knowledge previously acquired is effected not only for the general character of the representations but for every element and quality of feeling of such representations. Every new experience must be considered *only as a variation* of previously ac-

[1] It may of course happen that the chain of thought will be uninterrupted, but this will take place usually only in cases that have no great importance for the thinking individual.

[2] I follow here Avenarius: *Kritik der reinen Erfahrung* p. 292, II.

quired knowledge. It need not be mentioned that these relations in the psychical world have their basis in the formation of the central nervous system of the thinking individual. The nervous system, constructed in a certain definite way, permits the existence of only certain definite physiological functions and these have as their correlates only definite psychological values.

Summary.

If now we sum up the results of our analysis of apperception in the three different significations of this conception, we find that these different significations are not false conceptions of the notion, but a use of the same nomenclature for three different phenomena. In addition, apperception in the first meaning of this word and also in the meaning of the Herbartian school are partial phenomena which can be excluded from no act of knowledge. Apperception as reflective knowledge may arise, but does not of necessity arise with every act of knowledge.

If now the question arises to which of these three different phenomena the name apperception most properly belongs, it seems to us that the phenomena of reflective knowledge may most rightfully lay claim to this title. Clearness is covered, or at least should be covered by the word consciousness. To apperception, considered as an act of knowledge, produced by the impact of two groups of representations must be given a meaning entirely different from the meaning used by the Herbartian school, since the movement of representation is entirely non-existent, there being only a determination of every new act of knowledge through the mass of previously acquired knowledge. Thus reflective knowledge remains a special content of knowledge, which is of particular importance for the formation of the psychical personality. The historical sanction for this use of the word apperception was given by Kant, who often describes it as 'the representation of the self.' But if we are to meet the demands of modern psychology, we must avoid all excursions into the domain of transcendentalism, and endeavor to deal with the notion of apperception as an empirical one, which must be treated according to empirical methods.

TYPES OF IMAGINATION.

BY RAY H. STETSON.

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In order to make any accurate determination of one's imaginative thought material, it is essential to define what is meant by 'forms of imagination,' 'mind-stuff,' 'symbols in consciousness,' etc. That the memory actually possesses a vast store of images corresponding to every sense, and representations of all the varied movements possible to the organism, there can be no doubt; otherwise it would be impossible to recognize any impression, or execute any movement. In terms of physiological psychology, this simply means that certain permanent modifications of the central nervous organs have taken place, and does not involve the implication that all these images be present to consciousness, or become genuine *mental* factors. Images of every sort, in this general sense, are possessed by every individual, but each mind chooses out of this varied stock of presentations those of one or more senses, which serve it exclusively as symbols for the embodiment of a large part of its thought. It is in this last sense that a person is called a 'visualist,' or a 'tactualist.' The images with which we are concerned are a part of the conscious life of the mind, and can be called up voluntarily. Memory images here treated of as forms of imagination may be defined: 'the appearance in consciousness, under voluntary control, of images without any sensory stimulus.' For example, A may be an exclusive visualist. A certain idea may come to him through hearing; as the sounds strike his ear they are recognized by the memory, and their meaning attaches to them, but they are at once translated in consciousness into visual images, the exclusive form of his representative thought. Although auditory images were awakened, the idea is not stored in the accessible memory as auditory. Now the idea present to A in visual imagery may act as a cue to call up motor images of the vocal organs; not necessarily however—as Baldwin has pointed out—through kinæsthetic images present to consciousness. Thus, while this idea has called up successively with A, auditory, visual and motor images, and while the auditory images, on direct sense stimulation, and the motor images, on a visual cue, may have been momentarily present to consciousness, it is not necessary that they be so present, and their momentary presence so conditioned does not affect the fact that A's imagination is exclusively visual.

No doubt but few images of the normal individual are strictly of any one type. Though the predominant factor in any one image called into consciousness can usually be determined, the image is nearly always tinged or enlarged by other elements. Sometimes the subordinate image factor, for example motor—visual predominating—is called up by the predominant image factor as a cue, sometimes the subordinate element is an essential of the whole image and appears as a part of it.

In order to determine the important point of the imaginative type of the individual, Professor Jastrow suggests in the *Popular Science Monthly* of September, 1888, an objective method for determining the 'internal language' of the person. His rules follow:

(1) Determine the limit of the capacity of both hearing and sight for receiving impressions.

(2) Determine the amount of error, and the nature of error of each sense.

(3) Determine which of the two processes of perception carried on simultaneously makes the greater impression.

From these united tests he would determine the type of imagination as to whether it be visual or auditory. For motor and tactile he has no complete method, and seems to think that they are nearly always combined with visual or auditory images, since motions are nearly always under the guidance of the eye or ear. But because the motion is always under a guidance it does not follow that the image need be inseparable from the visual images; indeed many of our movements always conducted with the aid of the eye are possible without it, *e. g.*, piano playing and writing. And it may be just as important to treat motor images apart, though they are usually associated with other sorts, as it is to treat auditory images apart, which with the most of us always occur with visual.

While such a method of determination would no doubt be valuable to the individual, as determining his best method of perceiving, *i. e.*, *receiving* things, it does not at all follow that this may be a basis for a psychological determination of his type of imagination. There is no doubt some general correspondence between the large part which some of our senses play in experience and the predominance of their images in imagination, but it is also certain that the connection is not invariable—witness the anomalous 'tactualists,' 'motiles,' and even 'olfactaires.' It must be true in any case, whatever the predominant type of imagination, that the individual gains most of his ideas through sight and hearing, and it is certain that he will have facility in receiving impressions through those senses, but the relative ease with which he uses hearing or sight may be as well the result of an original idiosyncrasy as of his type of imagination.

In determining a person's type of imagination, it is rather the sense images which he uses in *expressing* his ideas, and the sense-images to which he *appeals* in another person, which are to be looked to as an objective clue to his mind stuff than the images used in receiving ideas. Professor Jastrow himself mentions the possibility of image translation, and Wilbrand has determined in a case of aphasia that the brain area concerned in visual reception is not identical with the area concerned in memories of visual images. E. A. Kirkpatrick, in an article in The Psychological Review, notes how prone school children, upon whom he was making memory experiments, were to translate presentation forms into other forms. With children the process often shows to the observer by their murmuring words to themselves which are presented written, counting out numbers given orally on their fingers, etc. In adults, it is not often that the images substituted in the mind are allowed to picture themselves forth in movements.

In the studies from the Harvard Psych. Lab. given in The Psychological Review, a report is given of an investigation on memory, in which a similar method was used, but for a somewhat different purpose. The object seems to have been to arrive at general conclusions concerning the memory, and not to investigate the type of the individual, but some of the conclusions reached bear upon the subject in hand. The method consisted in the presentation of simple visible and audible contents under varying conditions. The things chosen were numbers and colors. Extended experiments were made with five subjects. Two of the conclusions drawn are:

(1) When two senses act together in recollection they hinder each other.

(2) When isolated, visual memory surpasses by far aural; when combined, the aural excels the visual, with one exception.

If these five men were average types, these results are of some importance as determining the sort of presentation best used, but in each case the statement of the conclusion reached is false. The statements should read:

(1) When two senses act together *in presentation*, recollection is hindered.

(2) The memory of *isolated visual presentations* surpasses by far the memory of *isolated aural presentations*. When visual and aural presentations are combined, the aural are best remembered. (One exception.)

There may be a great difference between 'visual memory' and memory of visual presentations. Moreover, the presence of one exception in the second conclusion quoted is not very reassuring. An exception of 20 per cent. is rather large, especially when only five cases, however accurately determined, are under consideration. The material used in presentation is peculiarly liable to mental translation. Many of us remember a series of numbers presented through the ear by images of the vocal organs (motor). About 45 out of a 100 people examined in a given case reported a visual scheme for numbers. Judging from that, two of the five subjects may have remembered their numbers by the aid of such schemes.

If it were not open to objection, such an experimental method of investigating an individual's type of imagination would be the best. It gives definite results and has the charm of being a laboratory method, but it seems obvious that no presentation method can avail for the determination of the predominant memory images. The only objective method capable of giving exact results would seem to be the examination and classification of cases of aphasia. These are comparatively rare, and the evidence is so fragmentary in each single case, that only the most general conclusions can be drawn.

Introspection has as many disadvantages in this field, as elsewhere, but coupled with such objective determinations as seem to give legitimate clues, perhaps its results may be worth examination.

A list of test questions was submitted to a class of 100 college juniors who had enough knowledge of the subject to understand what was wanted but who were not biased by any preconceptions.

(1) Observe dreams for some time and report as to the relative prevalence of the sorts of images. (While many dreams, perhaps even all, are suggested by sense-stimuli always present to sight, hearing, and touch, in sleep, it is still true that the paths of association awakened by such sensations will probably be those most frequently used, whether the occasioning disturbance be extra-cranial or not).

(2) Give general method of recall in recitation, conversation, etc., as well as possible. A special report was secured as to method of recalling the forgotten name of a person. (This gives some clue as to visual images of person or name, or auditory images of person's voice or sound of name, or vocal-motor images of pronunciation of name.)

(3) *a* As to memory of breakfast table; were objects colored? (referred to Galton's experiment mentioned in James' *Briefer Course.*)

b As to schemes, visual or otherwise, for numbers, colors, days of week, months, or any other series schematized in imagination. With any clues as to the origin of these schemes.

(4) *a* As to method of recalling a piece of music (auditory, visual or motor).

b As to which is best remembered in music first heard; rhythm (motor) or melody (auditory with perhaps vocal motor).

c As to whether a melody remembered is recalled with a tone color, or merely as abstract melody.

d As to whether images are called up in listening to music, and of what sort.

(5) *a* As to the possibility of conceiving bubble and toddle with open lips and passive tongue.

b As to presence of suppressed articulation in reading.

c As to how concept *riding-a-wheel* is conceived. (At least three-fourths of class had ridden wheels.) These will give some clue as to the presence of motor images.

(6) As to presence of images for concepts: Relation, cause-and-effect, classification. (This may show tendency to image abstracts and also the presence of visual and motor images.)

(7) As to any change in the type of imagination which may have occurred.

(8) State conclusion as to which type the individual thinks predominates. What sorts were second and third, and if all the sorts of imagery were present.

Where unusual types were reported, (auditory, motor, tactile,) as predominating this list was supplemented by a second, bearing on that especial sort.

Of 100 cases reported, 82 were from their own conclusions and other data judged to be predominantly visual, 6 auditory, 4 motor, 1 tactual; 5 from their own conclusions, corroborated by their reports were equally visualists and 'audiles'; 2 were equally visualists and 'motiles.'

The auditory constituted a large element in 20, the motor in 10, the tactual in 4.

None lacked visual or auditory images, though one considered the auditory doubtful. One lacked motor images, 3 lacked tactual images, some 4 of the class, age 19–24, were without much imagery.

Serial schemes for numbers, etc., were usually confined to the visualists, though with some exceptions. 41 reported schemes for numbers, letters of alphabet, days of week, etc. As a rule, where one common series was schematized, others were. In 12 cases the origin of the scheme was reported. Appearance of a page where first learned is usually the orgin of alphabet schemes, also often of number schemes. Many number schemes, whose form in detail cannot be accounted for, have breaks or sharp turns at six, eight, twelve and twenty, probably marking the successive stages at which numbers up to that point exclusively were dealt with in their early school work. Days-of-week schemes often show traces of calendars. The very common circular arrangement of the months is probably due to vague associations with the zodiac and the earth's revolution; several reported this definitely. Of course, the circle is a very convenient symbol for a series returning upon itself, but there is no reason why the days of the week be not so arranged as well as the months. The majority of number schemes reported rise from right to left; there is nearly always a break at twenty, and the scheme usually does not extend beyond one hundred. Usually days of the week rise from Sunday to some point in the middle of the week and then slope down to a second Sunday.

One of the most interesting schemes reported shows a remarkable power of visualizing. Numbers, days of week, years in century, and even days of month are represented by regular rows of five squares placed beneath each other. If the number does not fill out a row, a corresponding blank is left. Thus nine is represented by:

□ □ □ □ □
□ □ □ □

Days of the week and month run on in same order, Sunday being a dark square:

■ □ □ □ □
□ □ ■ □ □

This scheme is thought to have been derived from a calendar:

Auditory images are usually of words or music where the images play a large part in thinking. In either case, as James points out, it is easy to mistake a motor image of the vocal organs for a recall of the *sound*. Often unnoticed vocal-motor images serve as the necessary cue to the recall of a strain of music or of the sound of a passage read. The motor element is so easily overlooked that probably not a few who think that the auditory plays a large element in theii imagination would find the real element motor. Even in memory of music where it would be expected that auditory images would greatly predominate, the reports seem to bear this out. Of 83 reporting as to how they recalled a memorized piece of music, 25 reported, by auditory, 23 by tactile and motor, and 35 by visual images.

The true 'audile' in reading often hears the words as though in the head or even in the eyes, but not in the throat.

Motor images seem from the reports to be a much more important element in imagination than is usually assumed. Of the abstract concepts, cause-and-effect and classification, which at least one-half of the class image, the first was nearly always reported as containing a motor element, usually coupled with a visual, and the second, 'classification,' was also motor in coloring in a majority of the images reported. It seems but natural that the transitive states should be pictured in memory by motor images, as the very name 'transitive' indicates. The difficulty of distinguishing auditory and motor images has already been noted. In an equal number of cases, rhythm was better remembered than melody. It would seem as though any recall of rhythm must necessarily be motor. The number of cases in which a piece of music is recalled by auditory images, 25, is but little larger than the number of cases in which the recall was by motor and tactile, 23. The deaf Beethoven, writing grand compositions, has been cited as a remarkable instance of auditory imagination; but the same deaf Beethoven, working out those compositions at the piano he could not hear, and extemporizing on stringed instruments he could not tune, shows the importance even to him, master of tones, of motor and tactual experiences in the expression of ideas.

Not a few of our concepts of motions are symbolized in motor images. In a large number of cases, 'riding-a-wheel' was represented by a distinct feeling of motion in the legs, or of the whole body in mounting. About 60 of 100 reported suppressed articulation in all reading. No doubt many of our gestures are the results of motor forms of imagination. To be sure, many gestures, especially expressions of emotion, are spontaneous and preceded in consciousness by no motor image which they realize; but it is not difficult to distinguish such gestures from those which do delineate motor images. The latter are conscious, under control of the will, and are often felt as giving expression to motor impulses. Witness the man who shapes his thought in his hands as he speaks, who has a definite feeling, located in his hands, that in a logical train of thought one thought *draws* another after it. Probably motor and tactile images are of great importance in imagining emotional states. In some cases the thought of the cause of the emotion will call up a faint repetition of the bodily state, but often this does not or cannot happen, and if the emotion is recalled at all it must be by the aid of motor and tactile images of those states.

To a person whose imagination is largely motor, many conceptions cannot be grasped without this motor element. It seems the very life of the notion. Power, sublimity, life, are such concepts. None of the persons reporting as 'motiles' could conceive of a personification without involving a motor element. To many a person in his ordinary thought, it is this motor element which distinguishes the beautiful from the sublime; it is his way of representing power and might. One person in the class, who cannot conceive what a motor image would be like, never appreciates any personification, or has the slightest tendency to personify; although interested in oratory, he has no tendency to gesture.

A motor element is involved in many of the schemes reported for months and days of week. Climbing up hill during winter months or early part of week, and going down during summer months, or last of week. In the case of the days of the week, the scheme with the hill in the middle, rarely toward end, was formed during the college course, when the press of work is greatest at mid-week.

Aesthetic enjoyment is largely conditioned by motor and tactile imagery. One of the chief demands in modern painting and sculpture is action, *i. e.*, the capability of immobile things to rouse motor imagery. Fromentin's Arab Falconer would be sorry sight if it only roused visual associations; but the distorted drawing and the grand swing of the uplifted arm suggest motor images of mad, dashing energy and fierce, free muscular life. We participate in the mad galop of the steed and sympathize with the rider because 'we picture to ourselves how it feels,' *i. e.*, we call up motor and tactual images. It is delightful to poise in imagination with a sculptured Mercury, or to share for a few moments the brawny firmness of some bronze hero. Even in poetry and music we *feel* the wild galop suggested by the rhythm of Browning's 'How They Brought the Good News from Ghent,' or of Schubert's 'Erlkönig.' Lotze makes our enjoyment in watching many forms of animal life, and in viewing many pictures, to consist in an ability to imagine how we should feel in their places; we get a joy out of the swift flight of the swallow or the petty industry of the ant, because we live over again to ourselves what we imagine their life to be. No doubt our representations are far enough from the reality, but they must consist in more or less vivid motor and tactile images with the possible accompaniment of other sorts. Architecture, at first glance, is thought of as appealing only to the eye; but if this were the case, we should not have a sense of unreality and disappointment in finding what seemed an imposing marble temple at a little distance to be but framework and stucco. A part of the interest in architecture comes in imagining the vast load borne up by the aspiring under-structure. If architecture be 'frozen music,' this inter-play and combination of opposing forces respresents the rhythm. To most of us it is essentially a matter of motor representation. We get a vivid example of this when at the foot of a perpendicular cliff or towering edifice, we have the sense of its pressing down upon us, overhanging and about to crush us.

Tactual images are agreed by all to play the slightest rôle in imagination. They are oftenest entirely lacking. A single case of tactual images predominating was reported among the hundred examined. In this case, general concepts are not imaged in any way. Dreams are almost exclusively tactual. It would be hard to imagine a tactual scheme for numbers, yet this man's imagination solves the problem easily enough. His number scheme consists of the representation of the series of sensations produced by tapping the tips of the fingers of the right hand successively upon a surface. Thus sensation at tip of little finger corresponds to '5' and its tens, sensation at tip of thumb—second round—for '6' and its tens, etc. Scheme was formed while learning the multiplication table by counting on his fingers.

Little was reported as to any remembered change in types of imagination. Two people of unusual types, tactile and motile, report that the visual is increasing. Four report a recent increase of auditory images; the increase was referred to attention to music, if any reason was given. In no case was there a tendency to increase of imagery. Five report a decrease. The members of class, of course, were not old enough to furnish any examples of the decided decrease toward middle age of the image-tendency noted by James and others.

It is hard to obtain data as to the imagination of children and illiterate persons, so that the effect of education on the imagination is largely a matter of speculation. It seems quite probable that in the early childhood of the individual and the race all sorts of impressions are stored in the memory and recalled. Voluntary attention does not then determine the trend of the imagination, and to the child, auditory, motor, tactile and even gustatory images would seem to have as good a right to be remembered as the visual. Inherited tendencies in the neural tracts may have something to do with the result, but aside from that, it is very probable that early life is characterized by a much larger variety of images and a more exclusive use of them than later life. There is little doubt that motor images have a full share in the imagination of the child and the savage. Vocabularies of infant speech, as usually given with a large percentage of substantives and a small percentage of verbs, would not seem to bear this out, but Professor Dewey, in an article in THE PSYCHOLOGICAL REVIEW (v. I., p. 63), calls attention to a common error in compiling such vocabularies. He shows that the young child uses words with little or no feeling for a fixed part-of-speech significance, and that the same word may be noun, verb and adjective. He emphasizes the large percentage of verbs actually to be found in a child's speech. Among savages, the universal tendency to descriptive gesture and sign language attests the motor element in the primitive imagination. From a utilitarian standpoint, motor images are quite as important as visual to either the savage or the child. Movement constitutes a large part of the life of either. Baldwin states that in the growth of language-memory, the auditory and visual memories must get the start of the motor memory, though later the adult becomes vocal-motor. No doubt, a large part of the imagery of the adult—especially if 'audile' or 'motile'—consists of images of words, but there is no reason why the mind-stuff of a child should not be largely motor before the development of motor speech. With the adult, much of image-thought is not by word-symbols of any sort; words at best can usually mark only the distinct, substantive parts. The transitive and the vague upon which James lays so much stress can seldom be expressed in words, and may very well, in both child and adult, be symboled in consciousness by motor-tinged imagery. Even distinct concepts are not always represented by words; we are constantly making new concepts, unnamed, which we represent in thought by some convenient sign and use for the time being. The child who knows but a few words, visual, auditory, or motor, but who thinks a multitude of actions and things, must be dependent in much of his thought on wordless representative imagery.

Perhaps in early child life, all sense presentations are remembered equally well, but later the superior expressiveness of sight, and the fact that it is so largely used in all our processes of education, leads the mind to devote its chief attention to that form of memory. Gradually voluntary control of the auditory, motor, tactual, gustatory, and olfactory memories becomes less and less—degree varying with the individual—or is entirely lost. Some original structural peculiarity of the central organs may account for unusual types. There is no doubt that attention to a single sense tends to develop memory images of that sort. Among the few changes in type of imagination noted in the reports mentioned above, several had increased their auditory memory by the study of music, and a tactualist reported himself as constantly becoming more visual.

It almost goes without saying that the mental life of lower orders is limited by the definiteness and variety of images possible to them. The senses limit their thought. The world to the earth-worm must be a matter of tactual and motor images.

The image-tendency seems to reach its culmination in the early life of the adult, along with the æsthetic and emotional impulses. In middle life, the use of symbols, even visual, grows less and less, and often fades out altogether. That Galton's older, scientific men visualized less than younger, more insignificant men need not be due to any incompatability between scientific generalization and imagery; it is probably due to their being *older* men. Descriptive science is greatly aided by visualization. The reason that old men dwell so much on their earlier life, and disregard their middle age may be that those early memories are still connected with image associations. The work of middle-life has no such corresponding series of pictures. They live over their early childhood and youth because they can *live* it over again in imagination.

Perhaps the difference between men and women, which Lotze characterizes as a tendency of men to regard the mechanical and of women to regard the ideal, may be due to education. The life and education of the girl emphasizes the visual and auditory in their more delicate forms. The boy, on the other hand, has a far greater opportunity for the acquisition of motor images and impulses. He has a contempt for a girl who may know how a thing *looks* or *sounds* but who doesn't know how it *works*.

Imaging has many disadvantages. It is often a positive hindrance, *e. g.*, motor images in reading, visual number-schemes in calculation. It often leads to a narrow view, if the type is exclusive, and the symbol is taken for the reality. Some one has pointed out the vice of visualization as the basis of the philosophies of Locke, Berkeley and Hume. Much of the feeling and expression of other men must be lost to the person with a pure type of imagination. Most men possess enough of the common types, though subordinated to the ever-present visual, to enable them to call up on suggestion the image present to another.

But there are also advantages. Facility in image-thinking must enhance æsthetic appreciation, if it is not essential to it. For the development of details in dynamic science, gestures in oratory, figures in writing, and for the appreciation of all forms of art, an imagination richly stocked is of great value. True enough, as James remarks, bare concepts do just as well as colored images in running the ordinary affairs of life. But bare concepts are meager material with which to enjoy in memory a favorite picture, bring back a bit of music or a dear face, live over again a deep emotion.

ON INDIVIDUAL SENSIBILITY TO PAIN.[1]

BY DR. HAROLD GRIFFING.

The relative sensibility of individuals to pain is a problem of practical as well as theoretical interest. A more definite knowledge of the subject might be utilized not only in medicine but also in education. The data obtained are, however, quite limited; and most of the observations published have been made by anthropologists with but little consideration of the sources of error involved. The experiments here reported were undertaken with a view to throwing light on these sources of error rather than for statistical purposes.

The tests were made with Professor Cattell's pressure algometer and with the induction coil. The algometer registered the pressure exerted up to 15 kilo. It was applied by me on the palm of the hand and the forehead in most of the experiments; but in some, the latter ones, the observer applied the pressure himself, and the fleshy part of the thumb was used instead of the palm of the hand. The pressure was increased slowly at a rate as constant as practicable until the sensation became uncomfortable.

In the electrical tests the two forefingers of each hand were placed in separate cups of water and the alternating current was sent through the body from hand to hand. Four gravity cells were used with an induction coil. The distance of the primary coil from the secondary served as a rough indicator of the electromotive force of the current, which may be taken to represent the intensity of the stimulus.

The purpose of the first group of experiments was principally to find whether the sensibility of a sense organ might not be determined, in whole or in part, by the degree of protection given by other tissues. If decreased sensibility to pain may be due to the thickness of the skin, the determination of the dermal threshold for pain tells us nothing of a person's general sensibility to

[1] From the Psychological Laboratory of Columbia University.

pain. In order to investigate this source of error, before making the test with the algometer, I recorded my judgment of the probable result from the appearance of the hand. This was done on fifty-three students, four tests being made on each person, two on each hand. The men were marked *a*, *b*, *c* or *d*, according to their estimated sensibility. I give below the resulting average values of the pain threshold in kilograms, with the maxima and minima. The results are given in two groups, I. and II., since in group II., the observer applied the stimulus to the thumb, whereas in I., I applied it myself to the palm. Some of the tests in II. were kindly made for me by advanced students in the Columbia Psychological Laboratory in connection with other independent tests.

TABLE I.

NUMBER.		EST. SENS'Y.	AVGE. T.		MAX. T.		MIN. T.	
I.	II.	o	I.	II.	I.	II.	I.	II.
I.	6	A		5.1		8.2		2.1
19	10	B	10.0	6.9	13.8	15+	4.0	4.0
11	3	C	12.4	13.7	15+	15+	7.1	9.4
4		D	14.4		15+		13.7	

First vertical column gives number of observers; the second gives the estimated sensibility, the others the average and maximum and minimum values of the pain threshold (T).

From the results above given it is evident that, as might be expected, the thickness of the skin and subcutaneous tissues is an important element in determining the threshold for dermal pain. It is not, however, the only element involved. Some observers were much more sensitive and others less sensitive than one might expect from the appearance of the hand.

If the protection given by the tissues varies with individuals, then the relative sensibility of different parts of the body will presumably also be subject to individual variations. This I found to be true. Those having a high pain threshold for the hand were not always correspondingly sensitive to pressure applied to the forehead and top of the head.

Nevertheless, as will be seen by the following table, those who were sensitive on the hand were on the average more sensitive on the head. I give now the average values, with maximum and minima, of the pain threshold for the head, for the different sets rated according to the value of their hand threshold. The first half of the table is for the men classed group I. in the previous table, and the second is for those classed in group II. The top of the head was used with group II., the forehead for group I.

TABLE II.

NUMBER.	T. FOR HAND.	T. FOR HEAD.	MAX.	MIN.
12	Over 13	6.5	13.4	3.2
15	10 to 13	5.0	5.7	3.4
12	Under 10	3.5	4.6	1.5
3	Over 10	7.0	11.5	4.8
6	6 to 10	2.3	3.5	1.1
14	Under 6	1.9	5.1	1.0

The first vertical column gives number of observers.

The second vertical column gives threshhold values for three groups in order of sensibility.

The third, fourth, and fifth columns give average threshhold and maximum and minimum values for observers mentioned in second column.

It has generally been assumed that the sensibility to one form of painful stimulation may serve as an index to pain sensibility in general. But there is no evidence for such an assumption. In order to obtain data on the subject, I tested 27 observers with the induction coil as well as with the pressure algometer. In this way I found that the sensibility to electrical stimulation to be quite independent of pressure sensibility. Three observers, among the most sensitive to pressure on the hand and head declare, that they felt no discomfort at all when the maximum strength of current was given. One of these did not 'feel' the current when six cells were used instead of four, although the muscles of his fingers contracted from the stimulation of the current. On the other hand one student to whom a pressure of 15 + kilo. on the hand and 11 kilo. on the head, gave no discomfort, considered the electrical effect unpleasant when the current was of the average strength.

The above experiments do not, however, prove that the pain sensibility of the nervous system to different forms of stimulation is not the same. We can only conclude that the pain sensibility to sensory stimulation varies with the conditions of stimulation. Not knowing the path of the current in the body, we have no right to assume any special physiological or psychological basis for the data obtained from introspection.

The probable fact that in the pressure experiments, the variations cannot all be explained by the thickness of the skin and similar conditions, goes to show that there is such a thing as general sensibility to pain; and the general correspondence of the results for different places of stimulation may be interpreted in the same way. But even these results may be due to peripheral causes, and not to any property of the central nervous system, or of the consciousness by which it is accompanied.

THE THIRD YEAR AT THE YALE LABORATORY.

BY E. W. SCRIPTURE,

Yale University.

The report is taken up from the point at which it was left in THE PSYCHOLOGICAL REVIEW, 1895, II., 379.

The investigation of hallucination and suggestion by C. E. Seashore was continued throughout the third year and was brought to a close in May, 1895. The first section of this investigation concerned the suggestive influence of size on judgments of weight. Two sets of blocks were used. Set A varied in size, but had a uniform weight, while set B varied in weight, but had a uniform size. The problem was to pick out that block of set B which appeared of the same weight as a given block from set A. The difference thus made between the estimated weight of an A block and its true weight, gave the effect of the suggestive influence due to the difference in size. The experiments, carried out with the greatest care, gave a definite suggestive influence for each difference in size. The law governing the results is shown in Fig. 1. The investigation was extended to the various forms under which size shows its influ-

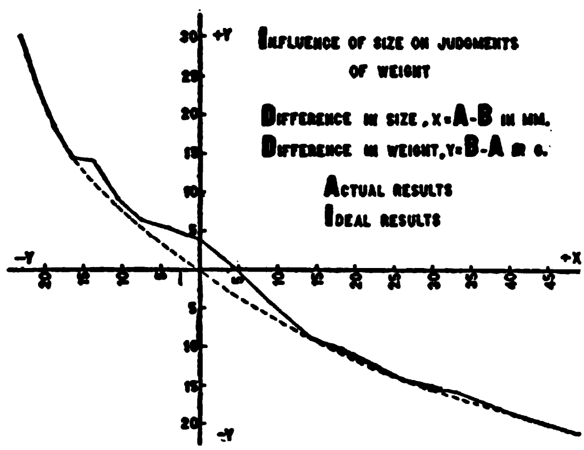

FIG. 1.

ence, *e. g.*, with the blocks indirectly seen, or seen and then hidden, or estimated by muscle-sense, or by touch, etc. The results are shown in figures 2 and 3.

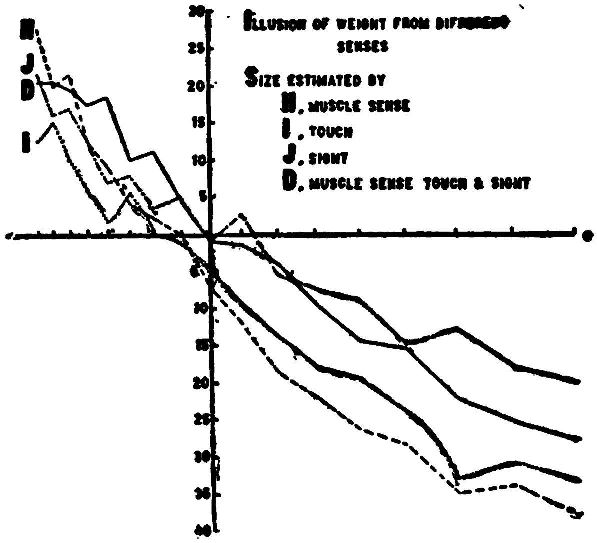

FIG. 2.

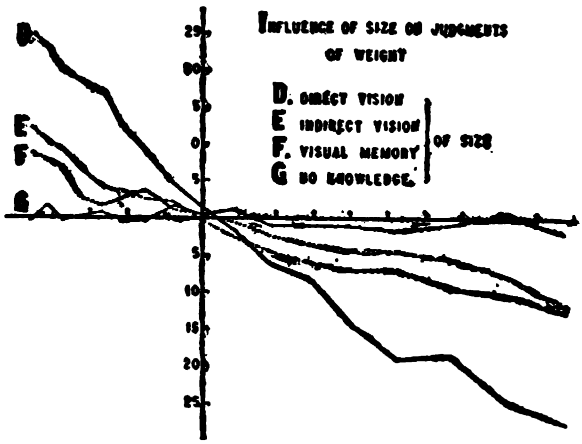

FIG. 3.

The next section investigated hallucinations of warmth. It was found that with an appropriate suggestion, a pure hallucination of heat could be regularly produced.

A third section investigated the effect of expectation and suggestion on the least perceptible differences in lights. It was found that the repetition of a number of experiments was sufficient to regularly create a perceptible difference when absolutely no difference existed.

A fourth section showed that expectation was sufficient to create a perceptible continuous change in the intensity of an illuminated disc.

In a fifth section hallucinations of a non-existing object were produced; in a sixth, of a non-existing tone; and, in a seventh, of non-existing sensations of touch, taste, smell and electrical stimulation.

In every case the experiments were quantitative; the scale was more or less an arbitrary one, but always admitted reduction to some standard unit of energy. Thus, on the principle that the intensity of an hallucination is the same as that of a sensation from which it cannot be distinguished, the intensity of the hallucination was measured in terms of the physical stimulus.

It is to be added that all these experiments were made on normal, healthy, unsuspecting individuals.

The second extended investigation was by John M. Moore on the subject of fatigue. In the first place the two eyes were fatigued by being required to estimate depths. The subject looking through a slit at G (fig. 4), had to adjust the bead B

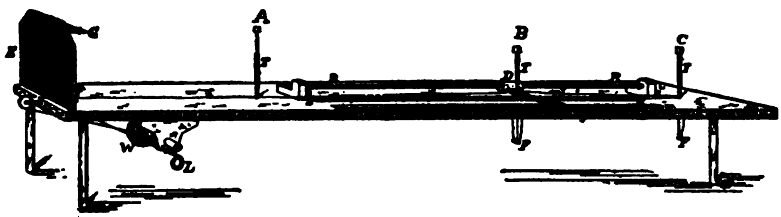

FIG. 4.

midway between the beads A and C, while seeing only one at a time. This middle bead B was placed always beyond the true middle, the error increasing with fatigue.

The experiment was repeated forty times in succession. The average amount by which the successive experiments increased the error of the first one is shown for one observer in Fig. 5.

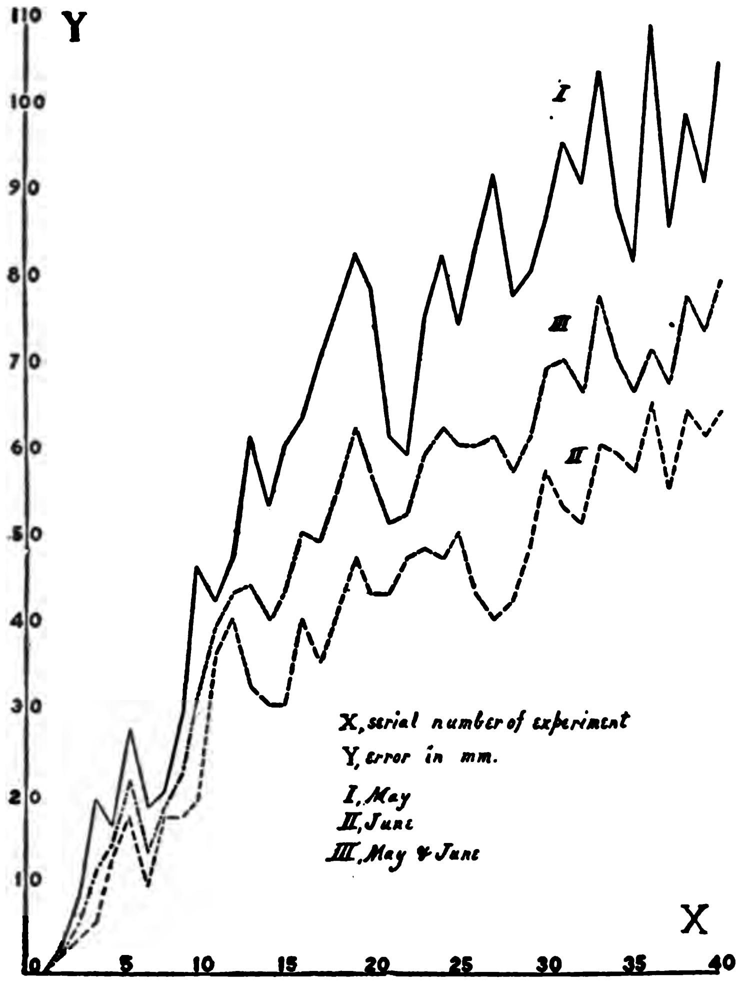

FIG. 5.

The investigation then proceeded to test one eye instead of two. Similar but less steep curves of fatigue were found. Thereafter the curve of fatigue was determined for steadily repeated accommodations of the eye with results as shown in Fig. 6. Finally the effect of fatigue on rapidly repeated taps

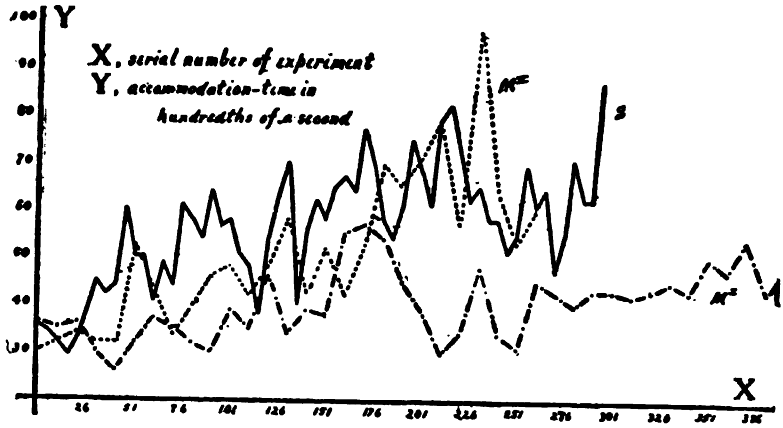

FIG. 6.

was determined. The general course of fatigue in this case is shown in Fig. 7.

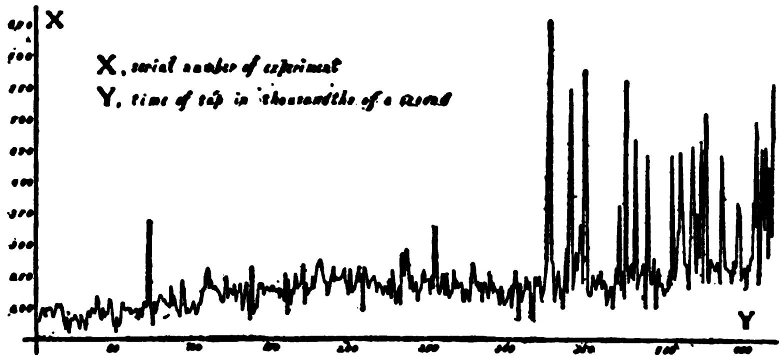

FIG. 7.

These two investigations were presented and accepted for the degree of Ph. D., making four such theses in three years.

Edward M. Weyer succeeded in measuring the reaction time of a dog finding it to be an average of 89σ. A method of measuring the dog's time of thought was devised with great labor; owing, however, to difficulties with the spark records, no definite conclusion was reached on this point.

The activity of the workshop was greater than ever, thirty or forty new instruments having been invented and constructed. Among them are to be found the pendulum chronoscope, the standard drum for very accurate measurements of time, the electric color-wheel with speed indicator, etc.

The results of the completed investigations were published in the *Studies from the Yale Psychological Laboratory*, 1895.

The officers of the laboratory were E. W. Scripture, instructor and director, and J. Allen Gilbert, assistant. Dr. Gilbert left at the end of the year to take the place of assistant professor in the University of Iowa.

DISCUSSION AND REPORTS.

A PSYCHOLOGICAL INTERPRETATION OF CERTAIN DOCTRINES IN FORMAL LOGIC.[1]

The tendency of the later modern logic has certainly been to free the account of the thought process from its limitations to grammatical forms and analogies. The extent to which logic has suffered from the bondage of grammar can hardly be overstated. Mere differences in the mother-tongues have been at the bottom of many of the disagreements between French and German or between English and French logicians. The rules of the syllogism, although international in their cogency, have done much to keep thought as an object of logical study within its mediæval bounds of word-gymnastics long after the thinkers themselves have left the cloister and its formal abstractions and turned their thinking, their scientific activity, into all phases of experience. Those time-honored rules, useful perhaps in the good old times, have become not altogether appropriate to the sort of thinking that goes on in a modern laboratory, nor are they quite adequate to a game of ball, which is as complete an expression of thought in these times as were the exciting tourneys of long ago with *ante rem*, *post rem* and *in re* for weapons.

The first attempts to free logic from language and grammar were in the different notations devised. The circular notation, for example, accomplished a good deal; and the algebra of logic has reached a high degree of abstraction from grammatical conditions. But logic in recent years has been getting most of its inspiration from psychology. Thus the simple suggestion of such a possibility as an 'unconscious conclusion,' whatever be the objections to the form that the doctrine first took, cannot but have had its lasting effect upon subsequent ideas of conscious conclusions. Logic, it must be admitted, has had rather a thick skull, but for all that the new ideas have made their way in. The 'unconscious conclusion' was one of the ideas that did much to give new life to comparative psychology, and comparative or biological psychology has revolutionized logic at least in one respect. It has materially changed the relation of language to thought. Lan-

[1] Read by title at the meeting of the American Psychological Association, Philadelphia, 1895.

guage, as spoken and written, is to-day regarded as but one part of the medium in which rational experience is expressed or made objective. Similarly the head alone is no longer looked upon as the seat of mind, but only of the brain, which is but a part, albeit, if distinctions must be drawn, an important part of the organism, in which, as a whole, mind resides. Environment in its entirety is the present-day logician's medium of thought.

Some simple evidences of the freedom of logic from grammatical limitations are (1) in the recognized universality and affirmative character of all judgments or rather of all judgment, since to-day we must talk rather of judgment than of judgments; thus you may have negative or particular sentences but neither negative nor particular judgments, and (2) in the notion that judgment is always *both* analytic and synthetic or *both* inductive and deductive. Judgment is, to commit a long compound, an analytico-synthetic tension. Sentences may be explicative and ampliative, analytical and synthetical; but judgment is like impulse; it is one; it is not to be cut up into classes; at the very most it has only manifold aspects or stages; with due caution it can be defined as the particular becoming universal, or the negative becoming affirmative, or the concrete becoming abstract, or quantity becoming quality. Negation, as an incident of judgment, means separation or differentiation, and so is a stage or aspect of organization.

The freed logic of to-day must refuse to treat lightly the mortality of Socrates, or even of Gaius, or the elementary character of that useful metal iron. It has no choice but to say that the statements, 'Socrates is a mortal' and 'Iron is a metal,' are either only sentences and then merely material parts of a total experience or cases of judgment only in so far as they can be said to represent a living present experience, in other words, as their predicates are taken to refer to wholly subjective mental states. The mortality of Socrates was a true judgment, I would suggest, to the Athenians of Socrates' time, to the grieved Chrito or the exultant Anytus, and the elementary character of iron has been a true judgment to some scientists. In a word, judgment is not a statement or a proposition; it is not an expressed relation between two terms or concepts; it is most safely described as the construction of reality or as self-conscious experience of reality, or in so far as present to consciousness as the tension of adjustment, or again as organization. The true predicate of judgment is no word; it is the judging self as distinguished from its object, the not-self or environment or grammatical subject. *A is B* is a judgment only in so far as *B* can be said to be somebody's self-conscious attitude towards *A* or a real subjective experience of *A* or as *B* is the unity of the self at the particular moment of the A experience. To express the same idea in still another way, keeping, however, the grammatical analogy, the subject of the real judgment is the 'universe of discourse'; the predicate is the speaker or more generally the agent. Again the grammatical subject is the psychological object, the world of experience; the predicate is the self, the psychological subject. Some may say that I am turning things around completely, that the world is the real predicate, and so on, but we may waive that possibility. *In any case a sentence is never the judgment.*

The doctrines in formal logic, the different rules of definition and of the syllogism and the like, appear in quite a new light when environment in its entirety as the real medium of thought and judgment as construction of reality or as organization are taken into account. The doctrines of formal logic make a sort of linguistic or grammatical *projection*, if I may take a metaphor from mathematics, of fluent activity, that is, of fluency in the use of the medium of thought, or again of what in biology is known as habit or accomplished adjustment. Of course projection is always a great disguise. For example, a mirror of complex surface may hide one even from himself. Formal logic projects fluent action or habit upon the plane of language and makes it all but unrecognizable. Moreover no expression of fluent activity is more distorted and disguised by the change than the natural use of language itself.

For evidence of the foregoing let us consider (1) the purely formal copula that logic has insisted upon, (2) the concrete and quantified predicate, (3) the abstraction of time, (4) the definition that must be virtually an equation, and (5) the different rules of distribution. Many other sources of evidence might be cited, but these are the simpler ones. We shall take them up in the order given, although before the review is finished all five will have collapsed into one—which is just what ought to happen.

1. In the real judgment the copula is not a word at all. It is the tension of consciousness, the attention. The copula as a word is a mere name, naming in the sentence in which it is found the fact of a former attention. Moreover, logic's formal copula is never used in living, that is, in natural discourse. As to the real copula, attention, if the tension of consciousness cease, what ensues? It is clear that an act expressing fulfilled adjustment ensues. Logic's formal eviscerated copula, accordingly, with not even the idea of existence left to it, to say nothing of the absence of all time ideas, indicates the passing of tension or—the same thing—the dying of consciousness in action.

2. The predicate as concrete and quantified is equally strange to the real judgment in consciousness. Reduce a judgment to an equation of quantities, and you do in so far make it, or you make so much of your environment as it contains a mechanism—even a sentence is a mechanism—which you are free to use or to act in. Do but recall that we have said that the true predicate in judgment is the self. The quantified predicate, then, is the adjusted self, or, biologically, the adjusted organism. In other words, quantification of the predicate is logic's way of viewing the identification of the self with its environment. Complete quantification makes subject and predicate one, not two. The man holding a sledge-hammer in the air, measuring positions and distances and arranging his own inner states at the same time, shows the conscious judgment, the tension of adjustment, passing into the fulfilled equation. The actual blow is the equation or definition of the experience. In real life we never quantify our predicates; we bring our hammers down instead. Quantification of the predicate, like evisceration of the copula, means action.

3. To the abstraction of time by formal logic some reference was made in what was said about the copula. It may here be added, however, that the timeless or tenseless copula and the quantified predicate go together. As just pointed out, the quantification of the predicate makes equation. An equation of course is timeless. An equation is also always a universal proposition. Even in such a case as *Some A is some B* through the quantification the qualitative difference between A and B is so far overlooked or transcended that the true judgment is rather *All AB is C*. *Some A is some B*, a wholly unnatural form, is but a linguistic projection of a timeless continuous experience AB. The lack of tense only testifies to this continuity. The tenseless copula and quantification in logic answer to the continuously filled time-interval that in psychology is no time to consciousness. In general, quantification, as its contagion spreads to the predicate, and by reaction more precisely measures the subject, shows experience becoming single, or continuous, and so from the standpoint of projection in language robs the copula of tense. The tenseless copula, finally, is evidently part and parcel of the logical distortion of the freed act.

4. To the definition as an equation reference has already been made. I only repeat that the fulfilled timeless equation of quantities is always in the act that proves adjustment.

5. Finally in the syllogistic rules of distribution we have but one more view of the way in which formal logic projects the facts of fluent action upon the medium of language. The rules of distribution

are demands for universality. In the case of the middle term the demand is absolute. But, since universality and tenselessness and freed activity belong together, the familiar rule that the middle term must be distributed only shows that reasoning depends unconditionally upon activity. I would suggest, in order rather to give my general meaning than to say anything at all final, that the fallacy of the undistributed middle represents in projection an unmediated impulse, while the illicit processes of the major and the minor terms represent respectively action under coercion from environment and rash or random action.

But I must conclude. I have said here if I have not shown (1) that environment in its entirety is the real medium of thought, (2) that judgment is, among other descriptions, the tension of adjustment, and (3) that formal logic as a body of doctrine is activity *projected* upon language. I recognize clearly enough that my ideal in this short paper has been better than the execution. If, however, only what I have wanted to do is now evident the labor has not been in vain. Nothing in philosophy is so much needed at the present time as the adjustment of the science of abstract thought to the science of organic action, and every little hint as to how that adjustment can be brought about cannot but be at least a little help. The evolution of consciousness must be almost meaningless until the simplest case of accommodation as seen by the biologist is identified with the most perfect case of abstract thought that the logician knows.

ALFRED H. LLOYD.

UNIVERSITY OF MICHIGAN.

COMMUNITY OF IDEAS OF MEN AND WOMEN.

Prof. Jastrow's criticism[1] upon the methods, and therefore the results, of a Wellesley College study of the mental community of men and women[2] has led me to repeat the experiment, following with extreme precision the lines which he has laid down. At the outset, I wish to acknowledge the justice of Dr. Jastrow's fundamental criticism upon the divergence of method. The earlier Wellesley experiment should indeed have conformed exactly with the methods of the experiment whose conclusions were questioned. For reasons which I shall later indicate, the divergencies were not considered 'essential,' and the results of the later experiment which I now report seem to me to confirm

[1] PSYCHOLOGICAL REVIEW, January, 1896. p. 68.

[2] *Ibid.* July, 1895, p. 363.

this view in some important features, though not in all. In this new test, lists of 100 words were written 'as rapidly as possible' by about fifty women students of Wellesley. Twenty-five of these lists were chosen entirely at random from among those written in the shortest time. The average time occupied was five minutes and one-sixth second, a somewhat shorter time than that reported by Dr. Jastrow (five minutes eight seconds); the lists are therefore free from any suspicion of being 'less natural and unreflective' than those which he studies. With the efficient aid of Miss Mary A. Dartt the words have been very carefully classified. I have guarded the entrance to every one of the twenty-five classes by a scrupulous consideration of each of the 2,500 words, and have even ventured to submit certain doubtful cases to Dr. Jastrow, to whom my thanks are due for his kind adjudication of their claims.

Before giving the results, I may remark, in reference to one of Dr. Jastrow's comments, that the reason for using only fifteen of the earlier lists, in the count of different words, is a mysterious loss of records. Making every allowance, however, for the increase in repetition with the growing number of words, it is overwhelmingly probable that the ten last lists together contained 300 new words, a number more than sufficient to bring them up to the total of the Wisconsin men's lists. Indeed, the ten cards last classified in the present experiment successively added more than thirty new words to the 'different words' already accumulated, and two of the very latest lists were among the most varied.

The first point at issue is the bearing of the experiment upon the relative tendency to repetition among men and women. Leaving the earlier comparison out of account, the number of different words is given in connection with the Wisconsin results:

Wisc. Univ. Men.	Wisc. Univ. Women.	Wellesley Women (1896.)
1375	1123	1306

The comparison of the percentage of different words (52 %) in the Wellesley lists, with that in the lists of the Wisconsin men (55%), seems to me an insufficient basis for the conclusion that 'there is less variety among women than among men,' especially as it is possible that the slightly greater rapidity with which the Wellesley lists were written may have reduced the number.

The comparison of 'unique words' or words appearing but once, also shows a greater originality on the part of the Wellesley women. Among our 2,500 words, there are 868 which occur but once, while the

Wisconsin men's lists include 746, and those of the Wisconsin women only 520. Dr. Jastrow (who has kindly read this paper in manuscript) regards this large number of unique words as 'suspicious,' adding that "it suggests that a very few students added an unusual number of different and unique words." Our records, however, do not confirm this hypothesis, for they show that the lists which contain an unusually small number of 'unique' words approximately balance the particularly full records. Dr. Jastrow adds that he should have rejected a record containing a long list of prepositions evidently following upon the chance occurrence of the first of them, "as the associations are purely verbal and artificial. What we want," he adds, "is one hundred different ideas." I am sorry that this suggestion came too late to be followed, yet I think that it proposes an unattainable standard, since it seems to me impossible to distinguish, in such lists, between 'verbal' and 'idea' associations.

The view that some other influence than that of sex may account for the difference in 'repetition' between the Wisconsin men and the women, seems to me to be further strengthened by the results of certain experiments in controlled association, first performed by Dr. Jastrow[1] and recently repeated at Wellesley. Ten concrete monosyllabic nouns were successively shown and the subjects were directed, after each, to write 'the first word suggested.' Dr. Jastrow finds that in this case "the tendency to repeat is not stronger in women than in men," and our Wellesley results from 42 records (a number equalling that of the Wisconsin men and greater than that of the Wisconsin women) shows an even lower tendency to repetition; 50% as compared with 65%. Yet if there is really among women a greater tendency to repetition, it should show itself in every form of unreflective and immediate thought.

The Wellesley results distinctly, therefore, oppose the generalization concerning the tendency of women to repeat each other. On the other hand, they seem to confirm several conclusions concerning prominent objects of imagination. The full classification is the following and includes the earlier Wellesley records, for purposes of comparison:

[1] *Educational Review*, II. p. 448.

TABLE OF DIFFERENT WORDS.

	Men of Wis. Univ.	Women of Wis. Univ.	1896. Women of Wellesley.	1894. Women of Wellesley.
1. *Animal Kingdom*,	254	178	146	223
2. *Wearing Apparel and Fabrics*,	129	224	97	96
3. Proper Names,	194	153	81	141
4. Verbs,	197	134	279	114
5. Implements and Utensils,	169	121	139	132
6. *Interior Furnishings*,	89	190	212	84
7. Adjectives,	177	102	300	234
8. *Foods*,	53	179	88	56
9. Vegetable Kingdom,	121	110	101	91
10. *Abstract Terms*,	131	97	101	280
11. Buildings and Building Materials,	105	117	86	106
12. Parts of Body,	101	105	66	34
13. Miscellaneous,	91	97	123	162
14. Geographical and Landscape Features,	97	80	70	142
15. Mineral Kingdom,	74	96	30	54
16. Meteorological and Astronomical,	85	76	109	26
17. Stationery,	60	86	69	26
18. Occupations and Callings,	71	47	24	33
19. Conveyances,	62	52	19	79
20. *Educational*,	34	76	102	167
21. Other Parts of Speech,	96	5	164	41
22. *Arts*,	33	61	17	44
23. *Amusements*,	30	53	17	102
24. Mercantile Terms,	30	29	18	15
25. Kinship,	17	32	42	18
Total,	2,500	2,500	2,500	2,500

The figures of the two Wellesley experiments certainly differ at several points and thus bear out the view of Dr. Jastrow and of Mr. Havelock Ellis, that the lack of extreme rapidity in writing brought about the divergence of the earlier Wellesley results. This difference is very marked in the case of abstract terms which fall far below the figure of the first Wellesley results, though it is proper to add that in the fear of overcrowding the class and in the effort to follow exactly Dr. Jastrow's principle of division, many words which seemed to me genuine abstracts were omitted. The prominence of the class of interior furnishings is the case of most marked agreement with the Wisconsin results. Foods also appear two-fifths more often than in the men's lists, yet only half as often as in the Wisconsin women's lists. On the other hand 'wearing apparel and fabrics,' supposedly objects of ardent feminine interest are named one-fourth less often than in the men's lists; and 'arts' and 'amusements' fall below any previous level. The results thus confirm some, yet not all, the conclusions concerning differences in predominant objects of interest. They certainly need to be supplemented by other figures since, as Dr. Jastrow remarks, "in dealing with such small groups . . . large room must be allowed for accidental variation."

It still seems to me, however, that such investigation is likely to lead to the confusion of two distinct problems and that one of these is practically insoluble. A statistical study may truly, if sufficiently extended, establish characteristic differences in the interests of men and women, and all Dr. Jastrow's conclusions may in fact be interpreted in this way. Mr. Havelock Ellis, however, and Dr. Jastrow, perhaps, by the expression 'masculine and feminine mental traits,' attempt a distinction between masculine and feminine intellect *per se*, and this seems to me futile and impossible, because of our entire inability to eliminate the effect of environment. Now the differences in the training and tradition of men and women begin with the earliest months of infancy and continue through life. Most of the preferences which have been substantiated by both experimenters, for instance that of women for the surroundings of a home, are obviously cultivated interests. On the other hand, the only characteristics discussed on which the supposed fundamental distinction of masculine and feminine intellect could be based, are the prevalence of abstract terms and the tendency to repetition. On the former score, the figures certainly show more abstract terms on the men's lists, yet the whole number of words considered seems to me too small to warrant fixed conclusions. The number of 'repeated words' is however large enough to form a fair basis for preliminary conclusions, yet just at this point the Wellesley figures definitely oppose those of the Wisconsin experiment. The question of the essential difference between masculine and feminine mind seems to me, therefore, untouched by such an investigation.

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

Miss Calkins has submitted the above notes to me before publication; it may, therefore, be appropriate for me to record my conviction that the main points at issue, the relative variability of men and women and the differences in their interests, still seem to me to suggest the solution originally outlined in my paper. On re-reading that paper, I can find no suggestion of a claim for a wider application of the generalizations reached than that of the special results presented. The repetition of the Wellesley results have shown that similarity of method is necessary to comparable conclusions, and they show this so strikingly as to form, in my view, a valuable illustration of the applicability of the statistical method to such problems. On the other hand it is equally clear that the results still differ considerably; this means to me that the data are dissimilar and must be considerably added to by repetition of the experiments in other institutions, before any more definite conclusions can be reached. Inasmuch as the second Wellesley test has brought the results more nearly in accord with the Wisconsin results; and inasmuch as the Wisconsin men and Wisconsin women form fairly comparable groups; and inasmuch as there is other evidence of greater uniformity amongst women than amongst men; and inasmuch as the exceptions to this can in some measure be accounted for, I must still claim that as yet the indications, imperfect as they are, still tend toward the conclusions first suggested.

JOSEPH JASTROW.

UNIVERSITY OF WISCONSIN.

PSYCHOLOGICAL LITERATURE.

Studies of Childhood. JAMES SULLY. D. Appleton & Co., New York. 1896.

The name of this veteran psychologist assures a courteous reception among us, for all his work. Nevertheless, one may fear that this contribution to the psychology of childhood is likely to be undervalued. If the author had proposed a perfect interlocking system of anthropogenesis, or new and daring suggestions toward such a system, if he had covered his pages with comprehensive or with suggestive tables of statistics, or if finally he had written just the book that lies before us twenty years ago—in any of these cases, his work would have been received as an event of first-rate importance. In twenty years, however, a great deal has happened. One thing at any rate has happened and that is differentiation in points of view and in the methods which go along with them. If Mr. Sully belonged more distinctly to some altogether modern group, his book, strong as it undoubtedly is, would be met with the kind of applause and of attack which mean so much more than mere courtesy to a professional colleague. But Mr. Sully's book does none of the things indicated above. It has no closed philosophy of anthropogenesis. It has no startling new theory. It has no statistics. So far as the spirit and method of the book are concerned (much of the *material* is entirely modern) it *might* have been written twenty years ago. And, therefore, instead of applause or attack, the book is likely in many libraries to be placed respectfully upon the shelf with the books of its era.

The reviewer sincerely hopes that this melancholy prediction will prove false. Mr. Sully's book deserves no such fate. On the contrary, it deserves not only from the laity, for whom it was primarily written, but also from professional psychologists, attentive consideration. Mr. Sully has not written the sort of book upon child study which many of us would like to see, but perhaps many of us fail to recognize the independent and permanent value of the kind of book which he has written. The intimate personal, natural history study of children of which the work is composed, was indeed possible as long ago as there were children and thoughtful men to study them, but in all probability, such study of children will never cease to be necessary. The reviewer believes in the future of a more systematic child study, but the discriminating observations of one who sees with a trained mind, and indeed of a mind trained to be more faithful to fact than to any theory, are invaluable at every date.

Mr. Sully tells what he proposes to do in the following words: "The following studies are not a complete treatise on child psychology, but merely deal with certain aspects of children's minds which happen to have come under my notice and to have had a special interest for me. In preparing them I have tried to combine with the needed measure of exactness a manner of presentation which should attract other readers than students of psychology, more particularly parents and young teachers."

In the introduction, the author discusses critically though moderately, the various methods of child study now current, concluding with the opinion that 'what is wanted is careful studies of individual children as they may be approached in the nursery.'

The author has made a large collection, or perhaps he would prefer to say, selection of observations upon children. A primary rule of selection has been to take observations in which the child with its surrounding circumstances were well known to the observer. Many of the observations were made by the author himself. Others were contributed by his friends and correspondents. Still others were taken from scientific and general literature.

The author has grouped this material about certain main chapters in Psychology (Imagination, Reason, Language, Fear, Morals, Art, &c.). He has written under each head the conclusions or impressions arrived at, supporting these by quotations from the 'observations.'

As an example of the characters of the book I shall give a resumé of the section entitled, 'Germs of Altruism,' (pp. 242–251). The various forms of primitive egoism having been considered in the preceding section, it is now pointed out that children are instinctively attachable and sociable, craving human and animal companionship and miserable when left alone (one case). This primitive form of feeling is not sympathy in the higher sense but a kind of imitation. Thus a dog answers the howl of another dog and a child cries when its parents pretend to cry (case at nine months). Out of such imitation springs the germ of a higher sympathy (two cases in proof of this transition). Later comes a distinct sympathetic apprehension of the other's trouble (case at fourteen months). Early exhibitions of sympathy (case at three years). Consolation (case at two and a half years: case showing more thoughtful sympathy at five years). Helpfulness (case at twenty-five months). Attempts to give pleasure (case at forty months). Love for animals supplanting fear of them (two cases, one at fifteen months). Sympathy for inanimate objects, dolls, &c. Dread of artistic representations of cruelty (case under four years). Dislike of sad stories. "It appears to me incontestable that in this spontaneous outgoing of fellow feeling toward other creatures, human and animal, the child manifests something of true moral quality."

This brief example which is characteristic of the book will show why it is necessary to cut short this review. There is no way to summarize these refined commentaries shading each into the next from page to page. Just for this reason, however, the book will be valuable to intelligent amateurs who wish help in the observations of their own children.

WM. L. BRYAN.

UNIVERSITY OF INDIANA.

The Psychology of Number and its Applications to Methods of Teaching Arithmetic. By JAMES A. MCLELLAN and JOHN DEWEY. New York, Appleton. 1895. 12mo., 16 and 310 pp. (International education series, Vol. 33.)

No more useful work could be imagined than the application of the results of modern psychology to the improvement of the methods of teaching arithmetic. On the whole, this task is admirably accomplished by the authors of this work. Every intelligent teacher of arithmetic will read the book with profit. The first half is devoted to a careful psychological analysis of the origin of the idea of number as it appears in the fundamental operations of arithmetic. The latter half constitutes a kind of teacher's guide in which the successive stages in the ordinary grammar school course are separately discussed, and specific directions are given about the methods to be followed in teaching. The main fault of the book would seem to be diffuseness and somewhat wearisome repetition; the essential principles and their application might be set forth in a book of less than half the size. But perhaps the authors know their public better than does the reviewer.

The leading thought of the whole work is the demand that, in teaching elementary arithmetic, the idea of measurement should be introduced from the beginning and insisted upon throughout, that continuous quantity, in preference to discrete objects, should be used for illustration, that number should be regarded as a means of valuation, and counting as a particular kind of measuring.

It is doubtless true that, to the mathematician, such a view of number and arithmetic has something startling, to say the least. Ever since the dawn of scientific mathematical thought, from the times of Pythagoras to the very latest researches of Weierstrass, Kronecker, G. Cantor, and so many others, has a fundamental distinction been recognized between pure number and continuous extension, between counting discrete objects and measuring quantity; and arithmetic, or the science of number, being regarded as the natural starting point of the whole science of mathematics, the efforts to bridge over the apparently insuperable gulf that separates number from continuous quantity have taxed the keenest minds. Indeed, the tendency to 'arithmetize' the whole of mathematics, to base it exclusively on the idea of the whole number as the only sufficiently simple and clear notion of the human mind is a distinct characteristic of, at least, one phase of the most advanced development of modern mathematics. And now we are apparently told in this book that all this is wrong, that the psychologist does not recognize this radical distinction between pure number and continuous quantity, between counting and measuring, that the primary notion is not the absolute integer but continuous quantity, and that, therefore, the idea of pure number should be discarded as far as possible from the first teaching of elementary arithmetic.

How can such directly opposite views be reconciled? First of all, by the fact that elementary arithmetic as taught in the schools is not mathematical science; it is far more a practical art than a science. It is the λογιστική of the Greeks which must have existed long before the foundation was laid for a *science* of mathematics by a clear and definite recognition of the difference between pure number and continuous quantity.

Our authors ascribe the psychological origin of number to the desire, or rather to the necessity in which man finds himself, of evaluating and measuring as accurately as possible, 'to the progressively accurate adjustment of means to end.' Counting thus appears as a means of valuation, number as an expression of value. The reasoning used in proving this position is plausible, if not quite convincing; it is certainly far from accounting fully for the peculiar nature of number. There are numerous cases of counting into which the idea of valuation or measurement does not enter except through a strained interpretation. On the other hand, measuring is often performed, even with considerable accuracy, without any use of number. What is essential in measuring is the actual 'application' of a unit or scale to the quantity to be measured. Similarly, what is essential in counting is the establishment of a one-to-one correspondence between the things to be counted and the known series of natural numbers. Now while our authors insist very much, and very appropriately, on this analogy between counting and measuring, they do not insist sufficiently, it would seem, on the essential distinction between the two operations, on the distinction between number and continuous extension. It must be conceded, however, that this distinction can only be fully appreciated at a higher stage of mental development, that it belongs properly to ἀριθμητική and not to λογιστική, and that the teacher of elementary arithmetic is mainly interested, as Professor Dewey says in his letter to *Science* (Vol. III., No. 60, p. 288), in the "task of finding out what sort of a mental condition creates a demand for number and how it is that number operates to satisfy that demand."

We cannot refrain from quoting another passage from the same very interesting letter: "The trained mathematician as such is, of necessity, interested in the further use of certain finished psychical products. As a mathematician any reference to the preliminary development of these products can only disturb and divert him. But the problem for the pupil *is how to get the standpoint of the mathematician;* not how to use certain tools, but how to make them; not how to carry further the manipulation of certain data, but how to get meaning into the data." The justness of these remarks will be felt by those who have had experience in teaching mathematics; the beginner's main difficulty lies in 'getting meaning into the data.' And from this point of view the psychological method of our authors is of interest not only for that applied art, elementary arithmetic, but for mathematical teaching generally, even though one may not feel ready to subscribe to Professor Dewey's severe arraignment of 'our text-books of algebra, geometry and high analysis.'

It is exceedingly desirable that the attempt be made by mathematicians 'to rethink the psychical conditions and steps through which their present magnificent apparatus has grown out of primitive, non-mathematical or crudely mathematical forms up to its present high estate.' But there is some danger that, by insisting too much on this psychological analysis, the pupil, instead of being actually lifted up to the pure mathematical idea, may be left behind with the 'primitive, non-mathematical or crudely mathematical forms' in his mind. To come back to our starting point, it might happen, that, owing to excessive attention to the metrical function of number and to its application to measurement, the pupil might never attain to a clear notion of pure number. Even though the logical number concept and the symbolical aspect of arithmetical operations may be considered as lying beyond the limits of elementary arithmetic, the teacher of this subject should keep them clearly in his mind. And we should have wished to see more attention paid to this mathematical side of arithmetic in a work primarily addressed to the teacher.

ALEXANDER ZIWET.

UNIVERSITY OF MICHIGAN, May 3, 1896.

Darwin, and after Darwin. II. *Post-Darwinian Questions; Heredity and Utility.* By the late GEORGE JOHN ROMANES, M. A., LL. D., F. R. S. Chicago, The Open Court Publishing Co., 1895. Pp. X + 344.

The Primary Factors of Organic Evolution. By E. D. COPE, Ph. D. Chicago, The Open Court Publishing Co., 1896. Pp. XVI+547.

We are often told that with the advance of knowledge specialization has become extreme. Yet between the zoology and the psychology of fifty years ago, there was but little connection, whereas to-day the more important works in zoology, such as these by Romanes and Professor Cope, could only have been written by serious students of psychology, and in turn every psychologist must read these books. It is perhaps one of the prerogatives of psychology to demonstrate that there are not only sciences, but that there is also science.

Both of the books before us offer special pleading rather than judicial examination. Cope acknowledges and justifies this. He writes: "the factors of evolution which were first clearly formulated by Lamarck are really such * * * and the book is a plea on their behalf." Romanes, on the other hand writes: "I have endeavored to be, before all things impartial." Cope writes with unusual *Derbheit*, with directness and condensation based on intimate knowledge of facts at first hand, whereas Romanes is more diffuse and gives the impression of being an able amateur.

Romanes' *Darwin, and after Darwin* is, it is true, a posthumous work, and did not receive its author's final revision. It has, however, been edited with much care and skill by Prof. C. Lloyd Morgan. The present part is concerned with 'questions of heredity and utility.' In the introduction of 36 pp., the views of Darwin and of the post-Darwinian schools are reviewed. It is so well known that Darwin admitted the hereditary effects of use and disuse, and with increasing emphasis as time passed, that it seems scarcely likely that confusion has been caused, as Romanes claims, by applying the term 'Darwinism' to the factors in evolution made leading by Darwin's works. Wallace doubtless called his book 'Darwinism' as a tribute to the greater man, not in order to identify Darwin's views with his own. Neither does it seem necessary to argue at length against the claim of Wallace that man has not descended by natural changes from other species. It has been said that as each has a blind spot in his eye, so each has an idiotic spot in his brain, but such spots may be properly left to atrophy. In enumerating the American Neo-Lamarckians Romanes confuses the definite views of Cope, Hyatt and Ryder with the somewhat agnostic attitude of Osborn and of Brooks.

Five chapters of Romanes' book are devoted to 'characters as hereditary and acquired.' The phenomena of reflex action are brought forward as probably the most cogent in favor of the Lamarckian factors, the argument being similar to that from co-adaptation urged by Spencer and others. Romanes argues that reflex actions cannot take place unless all parts of the machinery concerned are already present and already coördinated in the same organism. As the stages of its development cannot have presented any degree of utility, they cannot have been preserved by natural selection. The arguments from co-adaptation (including reflexes) seem to the present writer valid but not conclusive. Romanes states that he perceives that Spencer's arguments based on co-adaptation are equivocal; his own from reflex actions are equally so. If congenital variations can be organized by use into useful reflex actions the variations are already useful, and when further congenital variations occur which tend to relieve consciousness from the burden of interference, they are also useful and will be preserved by natural selection. Here as everywhere the survival of useful variations is accounted for by natural selection, but not their origin. The Lamarckian factors, when they refer to environment, do attempt to account for variations, but when they refer to the guidance of consciousness they invoke a *deus ex machina* and argue *ad ignorantiam*.

Probably the most valuable part of Romanes' book is the account of his repetition of Brown-Sequard's experiments on the hereditary effects of local injuries. The occurrence of epilepsy in guinea pigs born of parents which had been made epileptic by injury to the spinal cord or section of the sciatic nerve was not tested by Romanes, but has been corroborated by others. Romanes states that he has not been able to furnish any approach to a full corroboration of Brown-Sequard's experiments, but he has found gangrene of the ears in the offspring of animals in which this condition had been brought about by injury to the restiform body. Brown-Sequard's results are among the most curious in the history of science. It is the essence of a valid experiment that it can be verified by any competent experimenter, yet many of Brown-Sequard's experiments remain, in spite of their importance, isolated observations. Brown-Sequard's positive statements in regard to the 'elixir of life,' have, perhaps, made some men of science sceptical in regard to these experiments.

The second section of Romanes' book is on 'Utility' and the four chapters are all entitled 'Characters as Co-adaptive and Specific.' There is an extended discussion claiming that the student of evolution should regard adaptations rather than species, and pointing out the difficulties in the way of defining species. Much of the argument seems superfluous; when Darwin named his work *The Origin of Species*, he did not mean to exclude varieties, genera and families, and the briefest statement of the doctrine of evolution makes it clear that species represent mere degrees of gradation. Romanes holds that many characters are useless and have developed independently of natural selection. He gives as the causes of these, climate, food, sexual selection, isolation and laws of growth. That variations are conditioned upon climate and food is sufficiently evident, but it does not follow in the cases given by Romanes that the persistent adaptations are not useful under the changed conditions. Sexual selection (the taste of the female) and isolation might preserve variations when no longer useful, but do not seem to be efficient causes of their origin. 'Laws of growth' is a phrase apparently used to cover ignorance. There is no one who claims that every character is useful *per se.* The single organism and its relations to the physical and organic environment are endlessly complex, and while it is impossible to prove that every trait is useful, it is equally impossible to prove that any given trait is not or has never been correlated with some useful trait. Further it should be remembered that an organism cannot be permanently adapted to a changing environment, and that 'natural selection' can only build with the materials offered it. The most zealous advocate of natural selection can only claim that it *tends* to establish useful traits and obliterate such as are useless and harmful.

Romanes' book closes with two appendices and two notes. One appendix deals with panmixia and the other with adaptive characters, discussing further the views of Darwin, Wallace and Huxley.

As Romanes' book is itself polemical throughout, the reviewer is apt to follow unconsciously similar methods. But no one can read this book, with its wide and deep interest in fundamental problems, with its sincere and eager search for truth, without a keen appreciation of the irreparable loss science has suffered in the death of Romanes.

Professor Cope's *Primary Factors of Organic Evolution* is less polemical than Romanes' book and undertakes to offer fewer arguments and more facts. But Cope is even more dogmatic than Romanes and writes as though problems were settled that have as yet scarcely been adequately stated. Still the work is one of great value and importance. The strong impulse that leads men to adopt a definite theory and search far and near for arguments and facts in its support is wholesome for science, for thus stepping stones are laid on which we pass to wider knowledge. If those who make no hypothesis make but few mistakes, they also make but little progress.

The book before us opens with an introduction giving Lamarck's statement of the causes of evolution and tracing the subsequent history of the theory of evolution; and the final chapter of the book reviews the opinions of American Neo-Lamarckians. The three parts of the book are entitled, respectively, 'The Nature of Variation,' 'The Causes of Variation' and 'The Inheritance of Variation.' A large part of the details is outside the province of this REVIEW and the competence of the present reviewer. As far as the paleontological evidence for a given phylogeny is concerned we can but learn from the author, who probably has no rival in intimate acquaintance with extinct species.

In the first part Cope brings forward cases of variation in coloration and structure, quoting at length from others as he does throughout the book.[1] He concludes that variations are not promiscuous but take place in certain definite directions. Just 100 pages are then given to tracing certain phylogenies or genealogies based largely on the author's paleontological research. The third chapter is on the parallelism between phylogenetic and embryonic development, Cope regarding the parallelism as closer and more important than do most recent writers. The fourth chapter entitled 'Catagenesis,' is on regression or degeneracy. 'Sports' are held to be of no importance in evolution. The whole argument of this part is directed to showing that evolution has been due to determinate variation, giving progressive advance along certain main lines.

Under the second part, entitled 'The Causes of Variation,' we

[1] The long quotations from the author's previous publications and from other writers are in many cases superfluous, but in others they are useful, so long as writers will contribute original work to journals such as 'Agricultural Science,' 'The Radical Review' and 'New Occasions.'

find a chapter on 'Natural Selection,' which the author correctly sees to be no cause of variations, whereas the 'Energy of Evolution' and the 'Function of Consciousness,' which he holds to be efficient causes of variation are not placed in this part but under the part on 'The Inheritance of Variation.' The subjects here discussed are environment and the movements of the organism as causes of variations, these factors being called 'physiogenesis' and 'kinetogenesis.'

The chapter on physiogenesis is short and inadequate. Every little boy knows that the organism is affected by the environment and adapts itself to it, even though he may not know what these words mean. The first time he goes swimming in the spring the sun burns his skin, after that it becomes brown and is no longer burned. But what the little boy does not know, and what Cope does not attempt to explain, is how there comes to be an organism that reacts in this way on the environment. Yet this is surely the central problem of Lamarckism. Is the environment the efficient cause or merely the occasion of development and evolution? Later in the book Cope argues that the energy of evolution is not that which characterizes inorganic matter, and thus seems to me to give up the more important aspect of Lamarckism altogether, for I think that the movements of animals and consciousness cannot be regarded as efficient causes of evolution, unless their origin and hereditary transmission can be accounted for without returning in a circle to the nature of the structure and functions of the organism.

Kinetogenesis is discussed at length, 139 pages being given to the subject. The details, largely drawn from Cope's own researches on the vertebrate skeleton, are interesting and show how the structure of an animal is fitted for the movements that it makes. Changes in structure in the individual follow on the movements that it habitually makes, but then why does the creature make these movements? Because they are useful under the circumstances perhaps, but then why does the animal do what is useful? What after all is the efficient cause of an organism that can make these movements and then become still better adapted to making them? If we are referred to 'laws of growth' and 'anagenetic energies,' we have only words no more adequate as a scientific explanation than the *logos* in the first chapter of St. John.

Part III. is on the inheritance of variation. It is amply clear that variations are inherited or there could be no organic evolution. Whether the variations that have resulted in evolution are congenital or acquired by the individual in its life-time, is, as we all know, a vexed question. Cope, however, is very sure that all characters now congenital have been at some period or another acquired by the individual. The evidence offered in support of this point of view is not extensive, consisting chiefly of Hyatt's observations on the impressed zone of the nautiloids, and cases from breeding collected by Brewer.

The paleontological evidence seems to be ambiguous. If we admit that adaptations in individuals due to mechanical causes have preceded the establishment of these adaptations as hereditary characters, this in itself does not prove that the effects of use are inherited. As Osborn has recently pointed out (*Science*, N. S., Vol. III, p. 530) congenital variations that facilitate a useful action would be preserved by natural selection, and it would appear as though the variations were caused by the action. The cases quoted from Brewer are direct evidence, and if admitted would prove conclusive, but miscellaneous observations that cannot be repeated or confirmed by experiment, have never been important factors in the advancement of science. What we need is an extended series of quantitative experiments on variation and heredity.[1] If these were properly conducted we could learn whether or not a given change in environment or habit would, in a given number of generations, produce any congenital alterations in a species. So long as such experiments are not made it would seem that we are talking too much and working too little.

The chapters on 'The Energy of Evolution' and on 'The Function of Consciousness,' would perhaps be regarded by the author as the most important in the book. In the former he argues that the forms of energy of the inorganic world are also exhibited by organisms, but that to account for assimilation, reproduction and growth, 'anagenetic' energies, 'antichemism' and 'bathism' must be assumed. It may be necessary to go back to vitalism, but if one can do no more than say that life is an exhibition of 'bathism,' the preceding arguments of the book have indeed ended in bathos. It is, I fear, true that Cope is more successful in showing that we cannot account for life by physical and chemical energies than in proving that organic evolution has resulted from 'physiogenesis' and 'kinetogenesis.'

Cope holds that progressive organic evolution is due to the movements of the organism and that the movements are due to conscious-

[1]The nearest approach to these is in observations made incidentally in practical horticulture. Cope does not discuss this evidence, to my mind much the strongest hitherto adduced in favor of the inheritance of characters produced in the individual by the action of the environment.

ness—an effort to attain "some position which is favorable for the pro curement of relief from some unpleasant sensation or the acquisition of some agreeable one." Consciousness is thus the *vera causa* of organic evolution. The earth is supported by the elephants and the elephants stand on the tortoises, but then what do the tortoises stand on? Presumably on the earth, for Cope probably holds, though I believe he does not explicitly state, that consciousness is a function of the nervous system. Consciousness and the nervous system take turns in lifting each other to higher places and so we rise in defiance of the laws of gravitation and logic. I do not forget that we are supposed to have the help of antichemism and bathism, so we thus have in addition two words to push us along. When Cope takes the part of metaphysics *versus* common sense, and writes "it is more probable that death is a consequence of life, rather [*sic*] than that the living is a product of the non-living" and "conscious states have preceded organisms in time and evolution," we can but admire the courage of one who writes these things in a paleontological book published in the days of the triumph of material science. Cope promises a special volume on the evolution of mind and its relation to the organic world, and it is but just to wait for this rather than to enter into an extended criticism of a single chapter of the present book.

In conclusion it may be acknowledged that we owe chiefly to Cope and the other American Neo-Lamarckians the clear formulation and partial proof of the proposition that variations are not promiscuous nor multifarious, but are of certain definite kinds and in certain definite directions. This represents an important advance beyond Darwin's position. But we must wait for a second Darwin and a greater Darwin to teach us the efficient causes of variations and of heredity.

J. McKEEN. CATTELL.

The Whence and the Whither of Man. By JOHN M. TYLER, Professor of Biology, Amherst College. New York, Charles Scribner's Sons, 1896.

The above title is given to a series of ten lectures delivered at the Union Theological Seminary, being the Morse sections of 1895. The fund was left by Professor Samuel Morse, for lectures on the relation of the Bible to the various sciences. To the question of the whence and the whither of man the Bible gives a clear and definite answer. The object of these lectures is to show that science gives an answer in the main in accord with that of the Bible. The first few lectures were devoted to tracing 'the great line of development through a few of its characteristic stages from the simplest living beings up to man.' The different stages are marked by predominant sets of functions which succeed one another in an orderly sequence. The lowest forms are characterized almost exclusively by nutrition and reproduction. To supply the needs of digestion muscles are developed. Development of the muscular system brings about the nervous system and finally, as connection between stimulus and reaction becomes less and less direct, the growth of the brain. The lower functions, the digestive and muscular systems, have already completed their development; the higher functions, the intellectual and spiritual, are capable of further and apparently infinite development. Professor Tyler looks upon the lower functions as the means for the development of the higher. The end of evolution is the development of mind. If comfort and security, plenty of food and favorable conditions for reproduction, were the goal of development, the clam should be considered the highest product of evolution. The development of mind is parallel to that of body. Already in the hydra we see signs of sentience. In the higher animals we see undoubted signs not only of reasoning, æsthetic emotions and voluntary action, but of moral sentiments, of unselfish love. Evolution is the conformity to environment. The lower animals come into vital relation with but a small part of it. Environment includes all the forces in existence, material and spiritual. Conformity to environment produces therefore in the first place digestion and reproduction, then muscular power, then shrewdness, but finally unselfishness and righteousness. Environment therefore is ultimately God—a personality making for righteousness.

I pass over the chapters showing that this answer of science to the question of the 'whither' of man is substantially that of the Bible—also the very interesting chapter on the present aspects of the theory of evolution, which is a kind of appendix to the rest of the book. The lectures are for the most part fresh and interesting and the argument is clear. But they fail, as most such attempts fail, to give a perfectly definite answer to the main question. We are told that man's future is spiritual; but we already suspect as much. Does 'spiritual' mean the biblical doctrine of a future life for the individual or does it refer to the future existence of the race as such? And, if the latter, of what will that existence consist? These are questions which one inquiring about the 'whither' of man would certainly ask and I cannot see that Professor Tyler has answered them.

WARNER FITE.

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Fear. ANGELO MOSSO. Translated from the fifth edition of the Italian, by E. Lough and F. Kiesow. Longmans, Green & Co. London, New York, and Bombay. 1896. Pp. 278.

This book comes from one of the best known living physiologists. To Prof. Mosso the world owes some of the choicest methods and apparatus ever invented; his peculiar domain being the study of blood circulation, respiration and fatigue, with special reference to mental activity.

In attacking Fear, Prof. Mosso again shows his keen scent for crucial problems. Yet we must confess that the results, this time, revive our impression of how wonderful the inventor was within his old sphere, rather than excite us with valuable contributions for his new subject.

The first eight chapters deal with 'How the Brain Acts,' 'Circulation of the Blood in the Brain during Emotion,' 'Pallor and Blushing,' 'Respiration,' 'Trembling,' and kindred topics. In them the author has collated the principal facts now known regarding these matters, and has done so in language as simple as a child's fairy tale—and often as extravagant. The trouble, however, with this part of the book, from a scientific stand-point is, that late experiments of highest repute[1] explain the mysteries of blood distribution on simple principles which rob the Mosso school of investigation of their chief charm; namely their seeming promise to lead to a solution of the problems of emotion. These eight chapters, therefore, are now behind the times, and misleading if significance be given to them in the last mentioned sense.

Next follow chapters on 'Expression,' 'Phenomena Characteristic of Fear,' 'Fright and Terror,' 'Maladies Produced by Fear,' 'Hereditary Transmission,' and 'Education.' These are disappointing; they contain little that was new even at the date of appearance of the first edition, and by getting no further than did Darwin, Spencer, and Mantegazza, they emphasize how inadequate the conjectures of these great men were in this peculiar field. It is true that to-day very little is definitely known about fear; and this author has perhaps made as good a collection of the fragmentary suggestions currently supposed to have bearing on the subject as is to be found anywhere. But we had a right to expect more from a man of Professor Mosso's originality and rank.

The truth is, the book is full of careless statements and cheap hand-

[1] Shields, John Hopkins. First number of *American Journal of Experimental Medicine*. 1896.

ling of traditional themes. An example of this may be found in the author's so-called 'confirmation' of Mr. Spencer's theory of the origin of emotional expression; which theory is that, in emotional excitement, general waves spread through all the motor nerves, and effect the muscles proportionally to their bulk, and the inertia of the parts they move. In support of this Professor Mosso offers the fact that he stimulated the facial nerve of a dog electrically, and a weak current caused an attentive pricking of the ears; a stronger one gave a movement of the nose and eyes; then the lips and mouth opened; and finally, with a powerful current, the dog assumed the fierce expression of one about to attack—the conclusion being reached that, 'the mechanical part of expression is therefore much simpler than one thinks.' But can any careful man seriously suggest that our various emotional expressions may be arranged in a serial order dependent on the intensity of general nervous discharge! If so, at what point in a child do those for violent laughter pass over into the contortions of crying, or the reverse? And why not explain the movements of Paderewski's fingers by the same 'simple' plan, since they must be the most easily moved members of his body?

As another example of this sort of looseness, Professor Mosso attributes 'frowning' to sympathetic coördination with the eye muscles for purposes of scrutiny and attention. But why then, at the theatre, do persons in rapt attention and scrutiny of the comedian's antics raise the brows in the most open and expansive manner? And do we not scrutinize the marvellous as closely as the disgusting, yet with the brows set quite oppositely? We are not likely to arrive at any profound insight into emotion, until scientists are willing to guess at its problems a bit more searchingly than they would at a newspaper riddle.

Again, in the chapter on Heredity the doctrine of Acquired Characteristics is asserted as unquestioningly as if the great Weismann controversy never existed. Yet regarding its scientific aspect it remains to be said that the fundamental error of this book is the author's entire neglect of the psychologic side of his subject. Never once does he even try to approach it; and one should know, from the first, that a treatise on fear, with the psychology of fear left out, must be as unsatisfactory as an attempt at mint julip, which gets no further than the glassware.

In summary: The translators tells us that this is a 'splendid little work.' Rather it is a splendid little Vaudeville; a potpourri of all sorts of things, from Professor Mosso's Physiological Scrap Book, thrown together for the popular stage. The book is valuable, as any work from this distinguished scientist must be; but we feel that he stepped down to write it. It is good to bring science to the people, but in doing so one should never descend to tawdry, and much of the rhetoric of the present book comes near this. Scarcely does a cock-sparrow perform more preposterous antics at courting-time than does this author, in places, to drive his subject home upon the attention of 'popular readers.' (Pp. 36, 74, 200, for example.)

The work of the translators, Mr. and Mrs. Kiesow (formerly Miss Lough), is extremely commendable, and the type excellent.

HERBERT NICHOLS.

JOHNS HOPKINS UNIVERSITY.

Evolution in Art: as illustrated by the Life-history of Designs, By ALFRED C. HADDON, Professor of Zoölogy, Royal College of Science, Dublin. London. Walter Scott, 1895. Imported by Charles Scribner's Sons. Pp. XVIII, 364. $1.25.

There is a great deal in the title given to a book; and psychologists, interested as they are in all that relates to evolutionary doctrine, will I fear suffer some disappointment when they find that Professor Haddon's excellent treatise deals with little more than the indications that some art forms are developed by slow processes determined by the inheritance and the character of men as affected by their environment. But this disappointment is likely to be displaced by a sense of satisfaction, that they have been induced to read a work that might have been passed over had the title been more accurately descriptive of the contents.

Professor Haddon undertakes to study certain designs used in art, treating them as products of biological evolution; and he succeeds in showing, rather by accumulation of indirect evidence than by formal argument, that the processes discoverable in the psychic life of man are adequate to account for the original use of the principal decorative designs, found amongst the savage tribes to which he turns his attention; and that the persistence of certain of these forms, modified to a greater or less degree, is on the whole exactly what we should expect to find in consideration of our knowledge of the psychic life of man as, influenced by imitation, he passes through the normal processes of mental evolution.

Of the higher forms of decorative art the author, perhaps not unnaturally, has little to say; for to him, as to all biological evolutionists, the genesis of man's capacities seems most clearly exemplified in the lives of uncultured barbarians.

Where Professor Haddon touches upon matters of distinctly psychological significance (*e. g.* p. 308) he shows himself to be a somewhat crude materialist; but as he is not often led away from purely biological discussion, this crudity does not take from the worth of the book which will surely be of value to all who interest themselves in artistic development.

In these days when we are beginning to realize that art must be treated scientifically, that it is no mystic gift from the gods which we must worship but which we may not defile by ordinary investigations, all books are welcome which, like the one before us, tend to lead the man who devotes his life to artistic production to take a common sense view of the nature of his endowments.

The book is fully and satisfactorily illustrated. The classification, thrown in somewhat at haphazard on p. 8, appears to the writer of this review to require full explanation; as it stands it does not seem logical, and it is clearly not necessary to the argument of the book.

H. R. MARSHALL.

Inductive Logic. By JOHN GRIER HIBBEN, PH.D. New York, Charles Scribners' Sons. 1896. 8vo. Pp 345.

The preface of this work states a very good reason for its existence, and this reason is the impression often obtained that deductive logic constitutes the whole body of logical doctrine, while as a matter of fact the largest amount of our actual reasoning is inductive and should receive corresponding emphasis and consideration in methodology. This is quite true, but the value of deduction method is liable to depreciation by contrast, unless we give equal respect and attention to the natural demand for a certitude which induction does not give, and which governs many attempts to apply deduction for that purpose. Besides this, the training in deductive methods with all its laboriousness and elaboration, is the best corrective of dogmatism in the inductive field, by exposing the difference between methods which give assurance, and those which keep within the limits of probabilities until verification has done its work. It is assurance in conviction that most inquirers seek, and if they are taught by indirection that it can be obtained by inductive reasoning alone, there will be little to discriminate between conjecture and certitude, and much to encourage an unhealthy dogmatism. Not that I am charging this tendency to the present work, but only that there is equal danger in discussing induction without deduction.

The method of treatment is somewhat open to criticism. The first chapter properly treats of the nature of the process, but unfortunately implies by both its title and the discussion that 'induction' stands always for an inference or process of ratiocination, though the fact is that it often is synonymous with scientific method, which may be more than reasoning, and in one of its historical meanings is only a process of generalization by observation, the *inductio per enumerationem simplicem*. In the fourth chapter this simple enumeration is directly classified as one form of the inductive inference in the face of the fact that logicians generally repudiate it as a process of inference. Even Bacon excluded it from the 'induction' which he was discussing, and which he intended to treat as going far beyond mere observation. The second type of inference seems equally faulty in that it identifies analogy and comparison. A man may define analogy to suit this purpose, but many logicians consider analogy as treating only of a resemblance in relations and not a resemblance of essential qualities. This view ought at least to be mentioned and discriminated from the conception here maintained. In the chapter on Analogy there is no trace of the method employed by Bishop Butler and similar writers and discussed by Ueberweg and Jevons. Much confusion must follow such a loose identification between analogy and comparison.

Less objection can be presented to the several chapters following the fourth and including the topics Causation, Causal Analysis, Inductive Methods and Verification and Prediction. But it is quite singular that the subject of Hypothesis should be postponed to the thirteenth chapter ; for if anything is of the nature of an inductive inference hypothesis is such. But it is here treated as if it were something else altogether and yet is not defined as more than a preliminary to experiment. This would make scientific method begin with hypothesis to be followed and ended by verification, and exclude the necessity of inductive reasoning altogether, unless we at last decided to identify hypothesis and inductive inference, which is not consciously done in this instance. According to the author's definition of inductive reasoning, as taking us beyond the premises, he ought to make hypothesis the very essence of inductive inference, as the very step which takes us beyond the premises, and such a course ought to place the discussion of it before that of verification, and at least in the chapter pretending to define induction. But the author evidently intends to treat it as wholly distinct from the ratiocinative process known as inductive, and yet he would not regard it as deductive, nor as a form of observation. He must then regard it as a third kind of reasoning new to logicians, or has not discovered its identity with induction as defined by himself. The oversight probably comes from the tendencies to use the term 'induction' as a name for scientific method and forgetting its distinct meaning as a ratiocinative process.

Only one other criticism requires to be made here and it is that in the present critic's opinion many of the illustrations in the body of the work might better be used for practical examples and exercises, at the end where there is a very good collection of them. Illustrations are very important, but only a few require to be carefully analyzed in order to explain the matter of method. For this reason more attention might have been given to an abstract-explanation of the method, and then left the teacher to require its application in the same way to a large number of promiscuous examples.

Taking the book as a whole and considering its merits, it is certainly very clearly written and free from technicalities of style or undue philosophic speculation. It will serve very well the purpose for which it was written, and is hardly inferior on the whole to Fowler's work on the same subject.

JAMES H. HYSLOP.

COLUMBIA UNIVERSITY.

VISION AND GALVANOTROPISM.

Spectrobolometrische Untersuchungen über die Durchlässigkeit der Augenmedien für rote und ultrarote Strahlen. E. ASCHKINASS. Ztsch. f. Psych. u Phys. d. Sinnesorgane. XI., 44–53.

The fact that the eye communicates to the brain a sensation of light over only a small portion of the spectrum, may be due to either of two facts—the nervous apparatus, or, if a chemical process intervenes, the chemical apparatus may react only to waves of a limited range of lengths, or the invisible rays may be so absorbed by the media of the eye as not to penetrate to the retina. It has been conclusively shown that for the ultra-violet rays the latter is not the case, and that the ground of their invisibility is in the insensitiveness of the retina. With regard to the ultra red rays the evidence has been conflicting. Aschkinass has therefore applied the spectro-bolometric method, which has now been brought to great perfection, to the determination of the question, with the result of showing that the media of the eye have an absorption-spectrum very nearly the same as that of water, and that our blindness to ultra red is due to the same cause as our blindness to ultra violet, namely, the insensitiveness of the retina.

Color Saturation and its Quantitative Relations. By A. KIRSCHMANN. Am. Jour. of Psych., VII, 386–404. 1896.

The principle contribution of this paper is the description of a color disc constructed so as to exhibit from center to periphery a constantly changing saturation with an invariable color-tone and intensity. This is accomplished in the following manner: A number of concentric rings are drawn on a circle, and from the center are drawn, at equal distances apart, twice as many radii as there are concentric rings, if there are fifteen rings, two adjacent radii would be twelve degrees apart. If the points where the radii meet the corresponding circles (the *n*th radius the *n*th circle on each half of the diagram) are connected by a curved line, a symmetrical heart-shaped figure or leaf will result, such that the fractions of the successive rings inside the leaf decrease in an arithmetical ratio from the center to the periphery. If now a leaf of this shape is cut out of colored paper and pasted on to a gray disk, a color will be obtained, upon rotation, of constantly diminishing saturation from the center to the periphery. But in order that the intensity may at the same time be invariable, it is necessary that the gray chosen should be of exactly the same brightness as the colored paper of which the leaf is made. To avoid the vain search for such a gray for all papers, it can be made for each ring by another application of the method already used. The brightness of the colored paper is first determined by the method of Rood, or of the author, to be equal to that of a gray composed on the rotating disc of a given proportion of white to black, and the portions of the rings outside the leaf must then be made up of black and white in this same proportion. When the number of rings is very large, the boundary of the leaf becomes an Archimedic spiral, for which the author gives the equation, and also for the boundary of the black and white surfaces. The discs so constructed actually exhibited a constantly changing saturation with an invariable color, tone and brightness. They have been used for testing the validity of Weber's law for degrees of saturation; the results of this investigation are not yet published.

If these discs are made in black and white, since the change of brightness proceeds in an arithmetical ratio, the disc does not seem to grow brighter or darker towards the edge by regular degrees; that would be effected by a separation of the black and white by a curve giving equal multiplicative increments instead of equal additive increments. Such a curve would be, in polar coördinates, a transcendental curve analogous to the logarithmic curve in rectangular coördinates. The equation of this curve is given. The corresponding discs have been made, the construction being for each third of a circle, to obviate the necessity for very rapid rotation, and the increase of intensity being either from center to periphery or from periphery to center. In both cases the discs, when in rotation, present to the eye a surface with apparently uniform transition from black to white, that is, they make the impression of an arithmetical increase of intensity, and hence form a beautiful means of demonstration of the psycho-physical law.

There is also a modification of the double cone representing the entire gamut, or rather volume, of light sensation, by which expression is given to the fact that yellow is the brightest color of the spectrum, and violet blue the faintest, and that the whole spectrum grows more yellowish as it grows intense, and more bluish as it grows faint.

Zur Theorie des Galvanotropismus. JACQUES LOEB und S. S. MAXWELL. Pflüger's Archiv. LXIII., 121–144.

Every advance made in the investigation of those phenomena of nature which are of a positive and negative character, whether it be the effect of the two opposite directions of the electric current, the results of katabolic and anabolic changes in nutrition, or the action of opposite groups of muscles, is of wider than immediate interest; one never knows when general principles may be made out which will enable us to disburden the world of anachronistic theories of light-sensation which ought long since to have received their final quietus. There is therefore a special interest attaching to this paper on galvanotropism, by writers one of whom has for some time made the subject peculiarly his own.

The phenomenon of animal galvanotropism was discovered by Hermann. He found that the larvæ of frogs and other animals have the remarkable property, when in a long trough through which an electric current is passed, of placing themselves with the head towards the anode (the antidrome position), and that those which remained in the opposite (homodrome) position exhibited a constant restlessness. His explanation of the phenomenon was that the current acted upon the central nervous system, and that the latter was permanently excited by the ascending current, and more or less paralyzed by the descending current, and that the larvæ sought instinctively the position of least excitation. The experiments here described, which were performed on crabs, have convinced the authors that the assumption of a quieting effect by the descending current, and of an exciting and painful effect by the ascending current, is incorrect. They find that the immediate effect of the constant current consists, in crabs, in a change of tension of like sense in associated muscle groups, and of such a nature that on the anode side of the animal the tension of the flexors is greater, on the cathode side that of the extensors. This difference of tension of antagonistic muscles causes these animals, on account of the peculiar mechanism of the organs of locomotion, to swim towards the anode; if the current is strong, they become completely stiff when in the antidrome position; in the homodrome position they are not completely stiff, and can still swim backwards toward the anode. The same assumption of changes of tension of associated groups of muscles the authors believe to be sufficient to account for all the corresponding phenomena exhibited by vertebrates.

Interesting photographs are reproduced of the crabs with their legs stretched out and bent in accordance with this rule. The real state of things was not discovered before, because animals without legs were largely experimented upon. Galvanotropism will, without doubt, prove an efficient means for the determination of the position of motor centers.

The effect of the electric current upon the retina is to cause complimentary color sensations according as it is ascending or descending. It is odd that Hering has not dwelt upon this circumstance more than he has done as support for his conception, that complementary colors are connected respectively with assimilation and dissimilation. But it is perhaps fortunate for his theory, for it now appears that the polar quality of the current translates itself in the animal organism, in this well-marked instance, into a *quality-change* of some sort in the muscle-regulating nerves and not in a simple variation in assimilatory or dissimilatory activity. For it will hardly be assumed that an increased tension of a flexor muscle is brought about by increased nutrition and an increased tension of an extensor muscle by increased degeneration in the muscle, or in the nerve which regulates it.

C. Ladd Franklin.

Baltimore, Md.

The Colour-Sense in Literature. Havelock Ellis. Contemporary Review, May, 1896.

Mr. Havelock Ellis gives the results of his investigations as to the color words most used by representative writers from Homer down to the present day, including Olive Schreiner and D'Annunzio. His only Latin poet is Catullus, thus omitting Vergil and Ovid both of whom had a very keen sense of color. From a partial survey of twenty-five authors, Hebrew, Greek, Latin, French, Italian, English, etc., he sees that the color analysis of a writer's style will furnish a very delicate means of telling "at a glance something about his views of the world which pages of description could only tell us with uncertainty." What this something is the writer of this article does not tell us. But he concludes that the use of color in the writers of to-day, as it much more nearly resembles that of the time of Chaucer and of Shakespeare, than does the usage of subsequent writers, shows that in our color sense at least we are not degenerate.

We are told that in literature red symbolizes man; blue and green, nature; white, yellow and black, imagination; and the results of Mr. Ellis' investigation as shown in his tables seem to corroborate this generalization, as red is the most predominant color, black, white and yellow come next, and blue and green occur least often. But when he says that a poet, in using black, white and yellow, 'the color of golden impossibilities' is marked thereby as a poet of the imagination, his statement is an example of what may, if not supported by extensive statistics, turn out to be false analogy. WILFRID LAY.

PATHOLOGICAL.

L' Etat Mental des Mourants. P. SOLLIER, A. MOULIN, ALEX. KELLER. Revue Philosophique, XLI., 303–313. March, 1896.

Continuing the discussion begun by M. Egger (see above p. 236), MM. Moulin and Keller record in detail youthful experiences of their own in drowning, the latter also a more recent experience of syncope, while Dr. Sollier reports observations, notably of several grave cases of morphinomania, in which the sudden suppression of the habit seemed to the patient to threaten fatal consequences. The most constant phenomenon in all these cases is the feeling of blissful repose preceding the loss of consciousness. In only two of the ten instances here cited is there any vivid revival of the past life. M. Keller says that among twenty cases known to him, not one presented this phenomenon with any precision. The most distinct idea in the minds of both the drowning youths was that they would never again see their relatives. Both also experienced hallucinations. As to the feeling of fear, Moulin felt none till after the rescue and then it seems to have been due to chill; Keller, however, speaks of the self as full of fear till the recognition of the impotency of the struggle brought repose—an evident, and pos sibly false, interpretation. Sollier is careful to distinguish the cases; the question raised by M. Egger, he says, relates to the reaction of the self, not to death, but to the idea of death, and this varies within a wide range of possible combinations of circumstances. Confining himself to death from sudden accident, on the one hand, and to death from rapid exhaustion of the organism on the other, he proposes the following theory to account for the feeling of beatitude, the analgesia and anæsthesia noted by M. Egger, and the vivid resurgence of memory-images. The more negative than positive feeling of blissful repose he regards as the direct result of the bodily insensibility. The latter, in the case of accident, is due to distraction of the attention, its concentration on the object. The feeling of self is here, in the struggle for self-preservation, at its maximum, and hence, possibly, the spontaneous, panoramic vision of the total past self. In the pathological cases, the bodily insensibility is due to physical exhaustion. Here, too, the self is thrown back upon its past organization. Thus in the first case, the revival of memory-images is connected with exaltation of the former, in the second, with the suppression of the actual self. This is probably a little schematic and Dr. Sollier has himself the good sense to say that it is only a theory and to suggest further inquiry along the lines indicated.

H. N. Gardiner.

Smith College.

Ueber die Delirien der Alkoholiken und über künstlich bei ihnen hervorgerufene Visionen. H. Liepmann. Aus den psychiatrischen und Nervenklinik der Königl. Charité (Prof. Jolly), Berlin. Archiv für Psychiatrie, Heft I. 171–232. 1895.

In the above treatise the author communicates observations made in the summer of 1894 on 125 alcoholists at the Charité in Berlin. His first method of procedure was to make himself acquainted with the previous history of the patient's illness. On account of unavoidable incompleteness in this method, however, the author next endeavored to produce artificial visions in the delirious. The above work is therefore divided into two parts: I. Spontaneous delirium, II. Sense deceptions artificially produced: pressure visions.

I. The author considers the affective side of spontaneous delirium and the fancies of the delirious, dwelling more particularly on the anomalies of their sense-activity, their illusions and hallucinations. Taken as a whole the conclusion arrived at is, that the primary effect dominating the inner life in delirium tremens is fear, which then leads to actions for self-preservation. Elementary sense-anomalies appeared in many cases even before the outbreak of the actual disease.

II. In common with Näcke the author propounds: "If all or some of the hallucinations of the delirious drunkards arise from stimuli, then conversely, artificial production of such must call forth hallucinations." The simplest and least harmful irritant appears to be a continued pressure on the eyeball. The author suggests that Purkinje's figures produced in healthy subjects by pressure on the eye, which belongs to the domain of normal sensations, is transformed in the delirious into complicated visions, in which they see and even fluently read printed and written characters. The author then seeks to prove that visions so produced are caused by the pressure as such and are not a continuation of spontaneous visions. He further shows that this method is not only applicable in the case of delirious alcoholists, but could also be extended to the investigation of hallucinations in general, although he only wishes his own plan to be considered as a beginning in the investigation of sense deceptions.

It may be added that the author holds delirium tremens to be an acute exacerbation of chronic alcoholism and that in the 'Abortivformen' described by Näcke, he only recognizes a lower degree of the same disease.

FRIEDRICH KIESOW.

LEIPZIG.

EXPERIMENTAL.

Experimentelle Studien über Associationen. By GUSTAV ASCHAFFENBURG. Psychol. Arbeiten, 1895, I, 209–299.

The present investigation deals with normal cases entirely, and is preliminary to a study of the effects of fatigue and stimulants upon association. For the kind of stimulus used in these experiments —a word spoken by the operator—the author adopts the following classification: I. Immediate Association: a. Internal, (embracing)· 1. Co-ordination and subordination: 2. Relation of predication: 3. Causal relation. b. External: 1. Space and time; 2. Identities (synonyms, etc.); 3. Revival of former verbal succession. c. Stimulus acting merely as sound: 1. Completion of word; 2. Association through sound or rhyme. d. Stimulus producing merely reaction· 1. Repetition of stimulus-word; 2. Repetition of former reaction without meaning; 3. Association with former stimulus; 4. Reaction with no traceable connection. II. Mediate Association.

Three distinct methods of research were employed. In the first only the initial stimulus was given by the operator; the subject was required to write down in order his successive associations until a list of 100 had been made; these were then examined and classed according to the above scheme. In the second series the subject responded to the given word by a single association, and a number of such associations (usually 100) were included in the series. Finally, experiments similar to these latter were made, in which, however, the reaction-time was also measured, by means of a pair of lip keys. Seventeen different persons in all acted as reagents, and the entire investigation consisted of 44 series of about 100 experiments each. The comparatively small number of series recorded may be attributed to the amount of time necessarily spent in preliminary practice. Certain series are open to admitted objections, on account of the variability in physical and mental conditions which it was impossible always to avoid.

The external associations were in general more numerous than the internal, and show on the average a shorter duration. The reaction-time varied greatly among the reagents, the average lying between 1100σ and 1400σ; one reagent gave a much lower average, and one a considerably higher (2000σ); hence a long association time cannot be considered *prima facie* evidence of pathological conditions. Lack of complete attention is indicated by a tendency to associations of the types *c* and *d*, as well as by a break in the series under the first method; while repetition of the same associations in the same series may indicate the presence of unfavorable physiological conditions. An interesting fact brought out is the frequency with which the same stimulus led to the same word-association in different individuals. The same stimulus-series was given to five different persons; all five had 2 responses in common, four had 4, three 16, and two 39, out of 100. Another series given to four different persons shows similar results. These percentages may be regarded as the measure of their common 'intellectual atmosphere.'

Apart from such general conclusions as these the results are rather negative. They serve to emphasize especially the fact of ***individual differences under substantially the same conditions***—the fact that important differences exist among normal individuals in respect to both the kind of association and the length of reaction-time.

PRINCETON. H. C. WARREN.

Die Aufmerksamkeit und die Funktion der Sinnesorgane. By W. HEINRICH. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Vol. IX., Nos. 5 and 6, pp. 343–388.

The present theories of attention, says Dr. Heinrich, are unscientific since they are arrived at by explaining the known by the unknown (Wundt, Külpe); or else by reducing attention to association (Ziehen) or to muscle sensations (Münsterberg) which do not explain all the phenomena of attention. Experimentation must be introduced to control conditions, and do away with introspection. It is generally belived that attention is independent of the sense organs. This is thought to be supported by the experiment of Helmholtz in which he found he could change the direction of visual attention without the aid of visual objects. Helmholtz, therefore, concluded that the attention is wholly independent of the accommodation of the eye.

Dr. Heinrich considers this verdict unscientific, and devised a method to test it by physical measurements. A perimeter was used with an ophthalmometer having cross threads on its front end for a fixation point. The latter was placed close behind the perimeter so that the axes of the two instruments coincided. With the left eye fixated on the threads and the right eye covered, the subject was required to direct his attention to white squares, 2½ cm. to 4 cm. on a side, placed at an angle of 50°, 60° or 70° to the left along a meridian, and to tell what letter was printed on the square. In one form of the experiment the light came from a gas lamp placed near the perimeter on the nasal side of the left eye. In another form the light came from one northeast window, the other windows being darkened. In both cases the image of the light was reflected in the middle of the pupil. The head was steadied by a prop held in the teeth.

Following are the diameters of the pupil in millimeters taken from the ophthalmometer when the white squares were at different positions, and also when the subject performed a difficult problem in multiplication.

	With gas lamp.	With daylight.
Square fixed centrally	3.1437	3.0091
" " laterally 50°	3.6899	4.9094
" " " 60°	4.1245	
" " " 70°	3.3247	3.9514
With reckoning	4.3943	6.0565

Dr. Heinrich concludes that:

1st. The pupil enlarges when the attention is turned to an object seen laterally. This is dependent upon the angle of the laterally seen object.

2nd. When the subject turns his attention wholly from the object as in reckoning, the pupil is enlarged most of all. Since the light was constant these changes were not due to changes in light, but to changes in attention. Later modifications of the experiment in which black squares were used, and when the light came from behind, show that the adaptation is inconstant when the attention is not on a visual impression in the axis of vision. The curvature of the lens was found to undergo similar changes. J. P. HYLAN.

CLARK UNIVERSITY.

EPISTEMOLOGY.

Zur Kritik des Seelenbegriffs. A Vannerus. Archiv f. System. Philos. Bd. I. Heft. 3, 1895.

The author investigates Prof. Wundt's conception of the soul, as neither an *ensemble* of associatively bound together elements, nor a material or spiritual substance which underlies the empirical flow of changing states, but as on the contrary a real activity which is actual in that it is immediately known in psychic experience—an activity which is a known *Ding an sich.* The author admits that from a psychological standpoint the Wundtian conception is the true one; but contends that from an objective point of view the conception exaggerates the fact of activity. Activity presupposes an actor. Yes, replies Wundt, in the objective world of material things; but "the unity of volition (Willenseinheiten) to which the ontological regression leads is not an acting substance but rather a substance-producing activity." To the author, who substitutes a unified whole and its contained elements for the changing appearance and underlying reality which Wundt names the 'substance-concept,' 'an absolute change is a logical and psychological impossibility.' Activity, event and change are all causal conceptions; and thought must embrace their ground just as much as the effect. The comparing, relating functions (*e. g.*,) demand a permanent subject. We may conceive this common factor in all mental states as an activity, indeed as a *pure* activity if we hold fast the thought that it includes a constant factor which consists in this that it is always one and the same activity, viz.: the relational function. If the author means an activity which *in form* is always one and the same, his conception as here expressed is probably identical with that of Prof. Wundt; but if he means one and the same *actor*, the discussion becomes a defence of the 'substance-concept' of which, as the author writes, Prof. Wundt is 'the sworn enemy.' But the relation of the author's own position to that of Wundt is not perfectly clear in some parts of the discussion. The author's many references to Wundt's works make the discussion very helpful, as well as suggestive, to those who wish to study the Wundtian conception in the original.

The author seems finally to make a separation of the changing content of consciousness from its underlying ground, conceived as permanent; but this is of course just the effort to reduce the soul to mechanical conceptions which Prof. Wundt regards as both unnecessary and seductive. It may still be true that the soul cannot be logically, *i. e.*, mediately conceived, and must be immediately realized in the unity of its own activities; and this possibility the author does not seem to discuss.

Grundlinien einer Theorie der Willensbildung. PAUL NATORP: Archiv f. system. Philos. Bd. I. Hefte 2 and 3. 1895.

The author's problem is pedagogical, 'the content of that which should be developed out of man,' the theory of knowledge and æsthetics being as important as ethics in determining the answer. 'Will is direction of consciousness' determined by the unconditional demand for 'unity in the manifold,' to which consciousness stands related not only as a legislator to his law but also as subject. The author's Unconditioned is this object of demand not merely as something existing but as something which ought to exist. Will is thus something fundamentally different from the determination of action by the position in which one finds himself or by an estimate of positions in which he is able to place himself. From a formal point of view it is the necessary reference, imposed by consciousness upon itself, to the unconditioned as law. Its material content is determined by experience, the bond between them being expressed as direction, striving or tendency (Trieb).

What, *in concrete*, is the object of will? Three considerations determine the author's answer. Tendency in some direction is presupposed—this is Bain's spontaneity and Baldwin's 'Law of Excess.' Will, formally considered, gives to tendency its form— the unity of direction which consciousness unconditionally demands, at the same time rendering tendency objective in its reference. Tendency is *a priori* and must be centrally grounded and not peripherally. The objective reference of Will is ignored by all who (as Hume) see in it merely a sum of given simultaneously operating forces. The firm belief in a thing, that it means unity, explains both fanaticism and heroism. Rational will, the third consideration, adds to unity of tendency the insight that this is the Unconditioned. Whence then, rational will? The author's answer is social Technique, the influence of society with her opinions, institutions and customs on the individual—'moral consciousness is social consciousness.' The individual depends on society for self-consciousness, for perception or his view of his not-me, for his opinions and ideals. The pupil must not see with the teacher's eyes, but imitate the teacher in the use of his own. The primary influence of society is on the will; one learns to will by putting himself in the point of view of others. The law for the individual is; to give to the will the direction which society is disposed to demand, *i. e.*, the law of humanity. The good is a problem for the individual only in so far as he participates in the life of the whole. What morality is for each depends upon what it is for persons in general.

The author develops a system of cardinal virtues embracing Truth, Moral Strength, Purity, the moral ordering of the emotional life; and Justice, 'the love of the wise man.' This part of the discussion is very interesting and throughout suggestive.

On a Kantian basis, the author goes to the bottom of the question of will. In the account of the social nature of the objective consciousness, the author's view resembles that of Prof. Royce. The principle of imitation as the law of individual appropriation of that which society offers its members, the principle which Prof. Baldwin in his last work has shown to be of tremendous importance in this connection, is also hinted at by the author. Rather than the theory of self-dependence and self-legislation of the transcendental idealists, the discussion leans toward the opposite extreme of making the individual entirely dependent on society, just as Prof. Royce seems to in comparing the relation of the individual to society to that of a hypnotic subject to an operator. That instinctive sense of unity which consciousness contributes to Will, especially in individuals possessed of a high degree of what the author names Tendency, *i. e.*, Geniuses, often asserts itself against society in favor of an ideal so superior to society, it may be, that the latter cannot appreciate it. Moreover, the author emphasizes the legal, formal side of the Unconditioned as aim. This has two difficulties—moral development becomes the problem of following a rule, life is 'sicklied o'er with the pale cast of thought;' and then there are no ideal formal laws capable of obvious application to conduct. This conception really makes unnecessary the principle of imitation at which the author hints. Finally it remains unclear to us what the author's Unconditioned, as a matter of concrete content, is, although he has given it a clear formal determination.

Neither Spinoza's absence of determination, nor Kant's sum of all conditions, nor Spencer's sum of all force would answer. All that the author says points to self-consciousness in the form of self-activity as that which alone transcends the empirical manifold and becomes the aim of Will. But if we ask for a closer definition of self-activity, we simply come back to the question to answer which the interesting discussion was written. The individual depends on society, but society is a sum of individuals, and streams do not rise higher than their source. According to this it seems as though our moral emotion ought to be what it is, not merely an aspiration after something better than we are; whereas it has in fact a positive demand that we be that which we ought. It seems as though this difficulty is not solved by the discussion. GUY TAWNEY.

LEIPZIG.

Ueber Glaube und Gewissheit. JULIUS BERGMANN. Ztsch. f. Philos., 1896, CVII., 176–202.

Following the tendency established by earlier theological writers, many philosophers down to the present time have inclined to regard the belief based upon our ordinary avenues of knowledge as implying some degree of uncertainty. This is the reason for the many attempts that have been made to discover other avenues of knowledge capable of yielding more certain results. Herr Bergmann, on the other hand, claims that certainty is the essence of belief of every sort.

"The belief in the content of a judgment . . . is never something added to the judgment, but the judgment itself *is* this belief." Every judgment carries with it a belief not only in its own truth, but also in its own certainty. In order to this certainty, one must have an *assurance* of truth. Such assurance is found, either (1) in the *identical* character of the proposition; or (2) in its agreement with *experience;* or (3) in the fact that the judgment follows as a *consequence*, from recognized truths. The only real assurance of the truth of an opinion, then, is the perception of its truth; *i. e.*, the perception (which may be but dimly present in consciousness) that one of these criteria holds with respect to it. Belief and the feeling of certainty thus become functions of the *understanding;* and the understanding is the sole judge as to whether a thing which has been considered true and certain is so in reality. Truth is not an attribute of the notion as such, but merely of the notion as predicated, and hence belief is always belief in objective certainty, or truth.

The present paper is a defense of the writer's views as elaborated in his work: 'Die Grundprobleme der Logik,' and to this he refers in several places for amplification—*e. g.*, in his discussion of Kant's synthetic judgments *a priori;* Herr Bergmann holds these to be merely special forms of the analytic judgment, as of course his scheme would require of all necessary judgments. H. C. WARREN.

PRINCETON.

L'Hégémonie de la Science et de la Philosophie. A. FOUILLÉE. Revue Philosophique. Philadelphia. January, 1896.

The Hegemony of Science and Philosophy. INTERNATIONAL JOURNAL ETHICS. January, 1896.

The problem which the author would consider under this title is stated at the outset in the form of a question. Are there, as the Kantians hold, limits beyond which scientific methods do not apply, regions in which speculation must be controlled by entirely different principles? Or is there, as Aug. Comte would hold, a 'cerebral unity' of mankind capable of being constructed on the data of science alone? To which of the two must we grant the true intellectual hegemony (p. 137).

The author distinguishes between two senses in which the term science is used. In its broader sense it means 'a rationally established system of facts and ideas which, over a given range of objects, confers certainty, assurance, probability, or even a doubt that knows why it doubts' (p. 143). Thus understood science includes any belief founded on reason; universal philosophy as well as so-called special sciences. It excludes belief 'founded on the authority of others, not regulated, and incapable of demonstration, or on the imagination or feelings to which a supernatural bearing is given.' In its narrower, its 'true' sense, however, science 'hinges on the relations of objects to each other, independently of their relation to the sentient and thinking subject.' It is 'the perception of the constant relation between things such as these appear to us, independently of what they may be in themselves' (p. 144). Is it to science, in this latter sense, to philosophy, or to religion that the hegemony belongs?

In favor of science it may be said that it is the strongest bond of agreement in society. Scientific ideas are the only ones identical among individuals. Science must, then, 'take an ever-increasing part in the utilitarian and even moral direction of humanity' (p. 144). 'Science is nothing else than that social knowledge which is one of the essential elements of social consciousness' (p. 145).

But, though the idea of an 'organization by science' can merit only universal assent, the question remains whether the individual sciences are sufficient to found the true 'cerebral unity' of the human race. Three hypotheses are possible. Either (1) religion and philosophy will be absorbed in the particular sciences, or (2) they will continue to coexist with science, but within more and more circumscribed spheres, or (3) they will grow with the growth of science itself.

Taking up arms for philosophy, Fouillée holds that, while it is manifest that the metaphysics which would explain the 'facts of experience by means of entities and of causes which cannot be verified by experience or established in a definite relation with it' ought to disappear (p. 147), yet the history of true metaphysics, from Plato to Hegel, shows no tendency to grow poorer. Not only so; but science is, and must be, theoretically and practically incomplete. Theoretically it abstracts (1) from a sentient and thinking subject, (2) from the whole of existence. Practically it abstracts from the moral aspect of the universe. Philosophy rests assured of a 'perennial function' in correcting the abstraction that has thus been made of the thinking subject, and in reëstablishing the unity of nature and of thought (p. 148). And, further, to it 'the intellectual hegemony in the practical order of things belongs,' 'because the rational basis of morality depends neither on the positive sciences nor on religious faith, but on philosophy itself' (p. 150). Science treats the world of organisms as machines; philosophy regards them as conscious, as animate. Science treats inanimate objects as phenomena, philosophy in animating them treats them as real (p. 151). Only for philosophy is a moral attitude possible (p. 152).

To the science, then, 'that is at once objective and subjective, with philosophy as its indispensable crown,' not to purely objective science, belongs the moral hegemony of humanity. There remains, however, a certain validity to sentiment, especially to religious sentiment. Not, indeed, to sentiment supposed to be a faith that increases our assurance without increasing our knowledge, but to sentiment that is the resultant of tendencies, for the most part inherited, unanalyzed and complex; but not for that reason unanalyzable (pp. 153–155). A 'good sentiment' is a collective reason instead of being reason in detail; but it is none the less reasonable for not having been reasoned out' (p. 155). Religion, while it may lose its mythology as metaphysics must lose its entities, may pass over into philosophy; but cannot be merged into the pure sciences with their objective methods. "Religion is a philosophy of sentiment and of imagination which is chiefly social, although it addresses itself to the individual; it is a poetry of consciousness, seeking after the loftiest universal ideal." (p. 160).

But while we grant a moral and intellectual hegemony to philosophy and science, it is not to these as contemplating dead facts and their relations, but to them as actions and productions. The question being *put* to nature is essential to its being *answered*. The result is a 'philosophy of action, in which thought is no longer merely a reflection and a copy of the model subjectively presented; but a creation of new effects in harmony with those already existing' (p. 104). Fouillée concludes, therefore, that 'the true hegemony belongs to the intelligent volition of universal ends, a volition which exists as obscure consciousness in religion, but reaches in philosophy and in science the clear consciousness of its goal and of its means' (p. 164).

Enough of the substance of Fouillée's article has been here given to convince the reader of its healthy tone and comprehensive view. Nothing could be freer from that paltry spirit of reconciliation that cannot rest until it has left the ghosts of philosophy, science and religion locked in an empty embrace in a vacuum that was once filled by the fruitful struggles of their substantial selves. And on the whole, despite some vaguenesses that naturally springs from the difficulty of putting a system of philosophy in a few words, one feels the justice of the author's conclusion. It is, for example, true that 'pure' science is content to rest upon certain abstractions, that religion misleads insofar as it separates certain emotions, as different in kind, from the rest of experience. But one may be inclined to question whether Fouillée has not mistaken an illegitimate abstraction of the scientist for the necessary, or at least convenient, abstraction of science. Must science abstract from the sentient and thinking subject? Must it abstract from the rest of the universe? Must it omit moral aspects? Are, in short, its 'phenomena' to be opposed to the 'realities' of philosophy? Science may, indeed, speak of an azoic age, may define sound as air vibrations, may employ the concept of 'l'homme machine.' But science may also ask what is the relation of the azoic age to the rest of experience, may also define sound as sensation, may also regard certain, or all, actions as meaningful. The azoic age, the sensation, the meaning, however, must be such as can be 'verified by experience or established in a definite relation with it.' To abstract from them, thus understood,is to set a limitation from considerations of economy, of division of labor. To include them is to bring in no new principles. That from which science does seek to abstract, is not the 'thinking' subject;' but the individual point of view—in short, illusion. Since to perform this abstraction is to consider only that which can find confirmation, the object of science is, in a true sense, the object of a 'social consciousness.' The more accurate, the more complete the confirmation; the more perfect, the more 'objective' the science.

It is true that the scientist may seek to establish for his object a false independence from the rest of experience. He may rob it of all characteristics that make it an experience at all. But in so doing he sins against the intelligibility of his science itself. The philosopher, however, is in danger of committing the same sin if he would make the 'reality' of his object depend upon a 'meaning' which is not itself a phenomenon to be verified by and related to experience. Such an eject, based upon a false interpretation of analogy and of experience falls most naturally under the head of those very 'entities' and 'causes' for employing which Fouillée so justly condemns false metaphysics. The 'meaning' of actions, however, is a conception that has proved so difficult in the past, that one must rest in doubt as to whether one should accuse Fouillée of a fallacy, or oneself of a misunderstanding. Properly understanding them, however, science need not abstract from 'meanings.' If it does not, its separation from philosophy is merely a practical result of the economy of thought. Science rests, in its historical position, a less reflective philosophy, philosophy a more reflective science. Is philosophy destined to disappear in the growing reflectiveness of science? Perhaps, when there are no more reflections to be made. But then we shall be neither scientists nor philosophers; but in the happier sense of the word—sophists.

EDGAR A. SINGER, JR.
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NEW BOOKS.

La psychologie des sentiments. TH. RIBOT. Paris, Alcan. 1896. Pp. xi+443.

L'année psychologique. Publiée par. H. BEAUNIS and A. BINET. Paris, Alcan. 1896. Pp. 1010.

The School of Plato. F. W. BUSSELL. London, Methuen & Co. New York, Macmillan & Co. 1896. Pp. xvi+346. $2.75.

Outlines of Logic and Metaphysics. JOHANN EDWARD ERDMANN. Translated by B. C. Burt. London, Swan Sonnenschein & Co. New York, Macmillan & Co. 1896. Pp. xviii+253. $1.60.

Primer of Philosophy. DR. PAUL CARUS. Revised edition. Chicago, Open Court Publishing Co. 1896. Pp. xiv+232.

Studien zu Methodenlehre und Erkenntnisskritik. FRIEDRICH DREYER. Leipzig, Engelmann. 1895. Pp. xiii+223. M 4.

Hegel as Educator. FREDERIC LUDLOW LUQUEER. Thesis for the degree of Doctor of Philosophy in Columbia University. New York. 1896. Pp. 185.

Agnosticism and Religion. J. G. SCHURMAN. New York, Charles Scribner's Sons. 1896. Pp. 181.

NOTES.

A DICTIONARY OF PHILOSOPHY AND PSYCHOLOGY.

Macmillan & Co. have made arrangements for the issue in New York and London of a 'Dictionary of Philosophy and Psychology' under the editorial supervision of Professor Baldwin of Princeton University. The work is to have the following general features:

1. It will contain concise definitions of all the terms in use in the whole range of philosophical study (philosophy, metaphysics, psychology, ethics, logic, &c).

2. It will contain such historical matter under each term as may be necessary to justify the definition given and to show that the usage suggested is the outcome of the progress of philosophy, together with special historical articles.

3. It will have very full bibliographies both of philosophy generally and of the special topics which are connected with it.

With these features to give it character, and with the contributions of the leading men in this department of thought, chosen from England, America, and for the German and French usage, also from Germany and France, to give it authority, it is hoped that it may come to be a standard work, and serve two main purposes as follows:

First, It should, if successfully carried out, render to philosophy, in a measure, the service of 'setting' the terminology, in the different philosophical disciplines; and thus remove what is by common consent the greatest hindrance to their advance, *i. e.*, the varying and conflicting usages of terms which now prevail.

Such a book should serve both the teacher and the student in a most essential way. Teachers would have a consistent and, as far as the influence of the book might extend, uniform system of meanings with which to introduce these topics in the class room; and students would have the corresponding advantage of learning once for all an accepted terminology.

Second, It should serve as a general introduction to all the philosophical disciplines for all those who take interest in them.

Further, it is expected that men who are most competent in the several departments will contribute, and that in the result their work may present a fairly adequate statement of the present state of these studies in the world. All the matter in the Dictionary will be original and signed.

The following assignments of topics with the names of the authorities who will contribute original matter may be already announced:

General Philosophy and Metaphysics, Prof. Andrew Seth, Edinburgh University; Prof. John Dewey, Chicago University; *History of Philosophy*, Prof. Josiah Royce, Harvard University; *Logic*, Prof. R. Adamson, Glasgow University; *Ethics*, Prof. W. R. Sorley, Aberdeen University; *Psychology*, Prof. J. McK. Cattell, Columbia University; G. F. Stout, W. E. Johnson, Cambridge University; Prof. E. B. Titchener, Cornell University; The Editor, Princeton University; *Mental Pathology and Anthropology*, Prof. Joseph Jastrow, Wisconsin University; *Biology*, Prof. Lloyd Morgan, University College, Bristol; *Bibliography*, Dr. Benjamin Rand, Harvard University.

THE first number of *Kant Studien*, the new philosophical journal, edited by Dr. Hans Vaihinger, of the University of Halle, was published by Leopold Voss on April 25th. The number contains, in addition to an introduction by the editor, articles by Professors E. Adickes, K. Forländer, A. Sadtler and A. Pinloche, the last in French. Forty-three pages are devoted to reviews and 'Kantiana.'

THE American Association for the Advancement of Science meets this year at Buffalo, from August 23d to 29th. The anthropological section offers an opportunity for the reading of psychological papers, and the meeting is a favorable occasion to meet men of science working in allied departments.

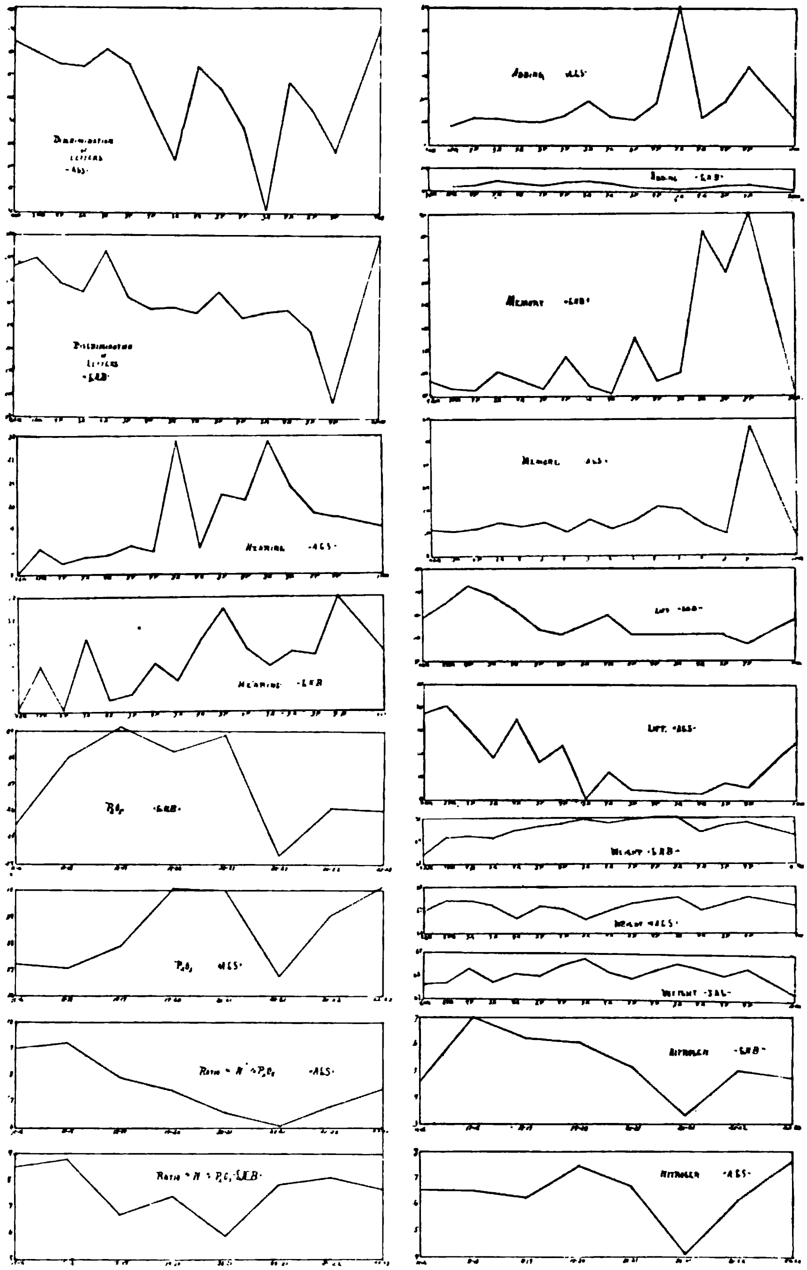
DR. FRANZ BOAS has been appointed lecturer on physical anthropology in Columbia University.

DR. ARTHUR ALLIN, honorary fellow in psychology in Clark University, has been appointed to the professorship of psychology and pedagogy in the Ohio University at Athens.

DR. CHARLES H. JUDD has been appointed instructor in psychology at Wesleyan University.

Illustrations to article by

PROFESSOR G. T. W. PATRICK AND DR. J. ALLEN GILBERT.



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THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF IOWA.

ON THE EFFECTS OF LOSS OF SLEEP.[1]

BY PROFESSOR G. T. W. PATRICK AND DR. J. ALLEN GILBERT.

The object of the following experiments was to determine some of the physiological and mental effects of enforced abstinence from sleep. In an address before the International Medical Congress at Rome in 1894, M. de Manacéine reported some experiments upon young dogs on the effects of absolute insomnia. The animals were kept from sleeping, and died at the end of the fourth or fifth day. (*Arch. Ital. Biol.* XXI, 2. PSYCHOLOGICAL REVIEW II, 1, p. 81.) So far as is known to the present writers, no experiments upon human subjects have hitherto been made on enforced insomnia for psychological purposes. The plan of our experiments was as follows: It was proposed to keep the subjects awake continuously for about 90 hours, to make a series of physiological and psychological tests upon them at intervals of 6 hours in respect to reaction-time, discrimination-time, motor ability, memory, attention, etc.; to observe secondly, the general effects of insomnia, and finally to observe the depth, character and amount of sleep following the period of waking. This plan was successfully carried out with three subjects, the depth of sleep being ascertained, however, in the case of only one. The subjects were in each case constantly attended by either one or two watchers.

[1] One of the three experiments described in this article was reported in a paper by Professor Patrick at the December meeting of the American Psychological Association at Philadelphia.

They took their regular meals at 7 a. m., 12.30 p. m., and 6 p. m., the food being normal in character and amount. In addition they ate a very light lunch at 12.30 a. m. The days were spent in occupations conforming as nearly as possible to the usual daily work of the subject. The nights were spent at first in reading or playing light games, and toward the end of the experiments in any way best adapted to keep the subjects awake, such as walking, working upon apparatus, or playing active games. Each set of experiments, however, took nearly two hours, so that this occupation consumed almost one-third of the time both day and night.

We give first a general account of the subjects and experiments. The first subject, J. A. G., is a young man of 28 years, assistant professor in the University. He is unmarried, of perfect health, of nervous temperament, of very great vitality and activity. He is accustomed to about 8 hours of sound sleep from 10 p. m. to 6 a. m. He awoke at his usual time Wednesday morning, November 27, and remained awake until 12 o'clock Saturday night. The second night he did not feel well and suffered severely from sleepiness. The third night he suffered less. The fourth day and the evening following he felt well and was able to pass his time in his usual occupations. During the last 50 hours, however, he had to be watched closely, and could not be allowed to sit down unoccupied, as he showed a tendency to fall asleep immediately, his own will to keep awake being of no avail. The daily rhythm was well marked. During the afternoon and evening the subject was less troubled with sleepiness. The sleepy period was from midnight until noon, of which the worst part was about dawn.

The most marked effect of the abstinence from sleep with this subject was the presence of hallucinations of sight. These were persistent after the second night. The subject complained that the floor was covered with a greasy-looking, molecular layer of rapidly moving or oscillating particles. Often this layer was a foot above the floor and parallel with it and caused the subject trouble in walking, as he would try to step up on it. Later the air was full of these dancing particles which developed into swarms of little bodies like gnats, but colored red, purple, or black. The subject would climb upon a chair to brush them from about the gas jet or stealthily try to touch an imaginary fly on the table with his finger. These phenomena did not move with movements of the eye and appeared to be true hallucinations, centrally caused, but due no doubt to the long and unusual strain put upon the eyes. Meanwhile the subject's sharpness of vision was not impaired. At no other time has he had hallucinations of sight and they entirely disappeared after sleep.

The period of 90 hours being completed at 12 o'clock Saturday night, the subject was allowed to go to sleep, which he did immediately. He was awakened at intervals of one hour to ascertain the depth of sleep, but fell asleep again at once after each awakening, and slept until half past ten Sunday morning. He awoke then spontaneously, wholly refreshed, felt quite as well as ever, and did not feel sleepy the following evening. He slept, however, two hours later than usual Monday morning.

The special tests made upon this subject, 14 in number, are shown with the results in Table I. They were all repeated every 6 hours throughout the whole period, and repeated again finally after the subject had slept. The results of the latter tests are shown in the last column. In reaction-time and discrimination-time, the effects of practice were eliminated as far as possible by preparatory training preliminary to the experiment. A few words of explanation of methods and apparatus are necessary. The pulse was taken at the beginning of each set of tests and then again at the end immediately after the subject was fatigued by tapping with the forefinger as rapidly as possible for 60 seconds. The subject was weighed the same time after each meal and in the same clothing. Grip was taken with an ordinary hand dynamometer. Pull was taken with the same instrument, the subject using the second finger of each hand.

For reaction-time the stimulus was a telephone click, with signal, the reaction being the release of a key, the subject being in the dark room, away from the recording drum. Each reaction-time given represents the mean value of from 10 to 15 reactions. For discrimination a modification of the same apparatus was used, the subject reacting only to the loud stimulus.

J. A. G.

Table I.

	November 27.			November 28.				November 29.				November 30.				Dec. 1. After Sleep.
	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	12 m.
1. Pulse	88	89	68	62	81	72	74	74	68	65	63	63	61	72	61	77
2. Temperature (Centigrade)	36.72	36.39	36.17	35.78	36.56	36.67	36.56	35.67	36.44	36.56	36.11	36.28	36.00	36.50	36.39	36.17
3. Weight (Kilograms)	67.70	67.75	68.30	67.78	68.19	68.04	68.52	68.83	68.27	67.99	68.35	68.60	68.41	68.13	68.47	67.39
4. Grip (Kilograms)	48.08	46.95	51.94	47.86	47.17	44.45	40.83		44.91	48.08	47.17	45.36	43.99	49.67	43.77	50.35
5. Pull (Kilograms)	27.22	27.67	28.12	26.31	26.76	25.86	22.68	22.68	26.31	25.86	24.95	23.59	22.68	26.99	23.13	27.67
6. Reaction-time. Mean (Sec.)	.122	.132	.129	.149	.133	.129	.139	.143	.146	.130	.144	.146	.139	.165	.148	.128
Mean Variation.	9	26	28	20	10	16	24	25	21	10	50	21	31	26	20	13
7. Discrimination-time. Mean (Sec.)	.258	.240	.242	.253	.225	.215	.216	.271	.207	.210	.213	.213	.206	.201	.158	.205
Mean Variation	50	56	51	48	38	32	43	67	63	63	40	65	62	36	43	52
8. Sensibility to pain. Lower threshold		3250	3000	3100	2750	3100	2650	2800	3150	2800	2750	2850	3300	3150	3250	3200
Upper threshold		4450	4350	4550	4050	4650	4450	4300	4600	4250	4200	4300	4550	4850	4400	4800
9. Acuteness of Vision (cm.)	137.2	132.1	139.7	134.6	142.2	156.2	150.5	120.6	137.2	143.5	137.2	152.4	148.6	156.8	171.4	125.7
10. Memory (Sec.)		540	260	159	290	330	200	105	240	70	262	290	123	190	545	125
11. Addition of Figures		228	254	248	238	249	223	215	205	216	196	210	200	250	224	277
12. Voluntary Motor Ability	42.2	42.2	40.1	39.0	40.0	41.2	38.6	35.5	39.5	39.0	35.0	38.9	41.0	39.0	39.7	41.3
13. Fatigue. Per cent. of Loss	24.1	24.6	22.6	20.5	18.0	24.0	13.7	12.1	17.0	13.9	11.4	20.6	17.6	17.9	13.6	17.7
14. Pulse after Fatigue	89	81	92	82	75	76	58	59	62	62	54	58	63	59	52	84

Sensibility to pain was tested by a specially prepared algometer, arranged to bring any desired pressure upon the middle of the fingernail of the first finger, the finger being inserted between two horizontal bars, the one pressing upon the fingernail being a very dull wooden knife edge. The figures record the pressure in grams, the lower threshold representing the first feeling of pain, the upper threshold the point at which the pain could no longer be endured. Acuteness of vision was tested in the dark room by finding the greatest distance at which the subject could read a section of a page from Wundt's *Studien* by the light of one standard candle at a distance of 25 cm. The memory test consisted in committing to memory 10 of the Ebbinghaus nonsense syllables. These were used in the ordinary way, but we consider this test of very slight value, for it is impossible not to learn these lists by association, and impossible to get different lists which offer equal ease or difficulty in association. The effects of loss of sleep upon attention and association we attempted also to ascertain by determining the greatest number of figures in prepared columns that could be added in three minutes. Voluntary motor ability was tested by having the subject tap with the forefinger as rapidly as possible upon a key for 5 seconds, using the recording drum and graphic chronometer. He then continued tapping for 60 seconds to fatigue the muscles. The number of taps during the last 5 seconds was then recorded. In the table is given first the number of taps in the first 5 seconds, then the percentage of loss in the last 5 seconds due to fatigue. The results of the special tests may best be studied from the table. Attention is called, however, especially to the following. The steady increase in the subject's weight during the experiment and the sudden decrease in weight after sleep are noteworthy, and apparently not to be accounted for by accidental circumstances. His average weight during the last 24 hours was 18 ounces greater than the average during the first 24 hours, and at 9 o'clock Saturday night the subject weighed 27 ounces more than at 9 o'clock Wednesday morning. During the 10½ hours' sleep, however, which followed the experiment, the subject lost 38 ounces, being 11 ounces more than he had gained during the experiment. In the tests with the dynamometer the subject lost slightly and gradually in strength of both grip and pull, regaining all after sleep. On Saturday afternoon, however, the subject made what appeared to be a spurt, in view, perhaps, of the approaching end, and gripped and pulled nearly as much as at the beginning. The reaction-time beginning with 122σ increased somewhat regularly, reaching its maximum, 165σ Saturday afternoon, after 81 hours without sleep, and dropped back to the normal immediately after sleep. The discrimination-time appears to decrease, but as it does not increase after sleep the result cannot in this case be attributed to loss of sleep. The acuteness of vision uniformly *increased* throughout the experiment, falling below the normal after sleep. The slight retardation in the increase in the second night corresponds with the period of slight sickness at that time. There is a significant decrease in voluntary motor ability. The decrease in this subject's pulse-beat after fatigue by tapping is abnormal and apparently a result of loss of sleep.

The above experiment upon J. A. G. was regarded as somewhat preliminary. It was, therefore, decided to repeat the experiment upon two other subjects, making such modifications in the special tests and apparatus as seemed to be desirable. The second subject, A. G. S., was a young man of 27 years, instructor in the University, unmarried, quiet and of excellent health. The third subject, G. N. B., was a young man of 24 years, instructor in the University, unmarried, of German parentage, stout and perfectly healthy. At the time of the experiment, A. G. S. was accustomed to 9 hours of sound and regular sleep; G. N. B. to 8 hours. These two subjects entered upon their sleep fast at 7 o'clock, Tuesday morning, March 17, 1896. 90 hours was again the period determined upon. On Friday night, March 20, at 11.15, the last set of experiments being completed, they were allowed to retire, so that their waking period was actually 88¼ hours. In the case of these two subjects there was no illness, no hallucinations of sight, and no serious suffering or discomfort. A. G. S. became very sleepy during the last 24 hours and had to be watched constantly. On Friday, at 9 p. m., after a brisk walk in the cool air, his temperature sank to 35.3° Cent. (95.6° F.), but in 15 minutes rose to 36.3° Cent. (97.3° F.). Of the three subjects he was the only one who apparently could not have prolonged the experiment beyond the period of 90 hours without danger. G. N. B. had less trouble in keeping awake and showed outwardly but slight effects of the abstinence from sleep. Both subjects slept immediately upon retiring at 11.15 p. m., Friday. They both slept uninterruptedly until 10.30 a. m., Saturday. They both awoke then for a few moments and slept again, A. G. S. until 11.15 a. m., G. N. B. until 2.40 p. m. They both felt wholly refreshed upon awaking, required no further extra sleep, and felt no ill effects from the experiment.

The special tests made upon these two subjects are shown with the results in Table II. and Table III., and exhibited, in part, in graphic form in the subjoined curves. They were as before, repeated every 6 hours. To eliminate, as far as possible, the effects of practice, the tests were begun two or three days before the beginning of the sleep fast. The first three sets of results in the tables, being taken the first day before any loss of sleep, should represent the normal reaction of the subject. These, taken together with the results of the tests made after awaking shown in the last column of the tables, make a fairly adequate standard for comparison with the results obtained during the sleep fast. The tests in respect to pulse, temperature, weight, grip, reaction-time, discrimination-time, sharpness of vision, voluntary motor ability, and fatigue, were the same as described above for the first subject. The strength of pull was taken with an ordinary lift dynamometer, the subject, standing upon a small platform with bent knees and straightened back, lifting his utmost by means of two handles connected by ropes with a large spring balance. In the memory test, the nonsense syllables were discarded and 18 figures substituted. 18 small squares of cardboard were provided upon which were printed the 9 figures, each figure thus appearing twice. For each experiment a random order of these figures was made, and then modified, if necessary, to prevent adjacence of same figure and suggestive combinations. The subject, timed with a stop watch, committed to memory the list, the watch being stopped when the subject announced his readiness to recite the list. Each experiment consisted in committing to memory three such lists. The tables show in seconds the average of these three trials in each case. No. 11 was a test in adding numbers. The sheets of figures used by Miss Holmes in studying fatigue in school children and described in the Pedagogical Seminary, Vol. III., No. 2, were used. The subject was required to add each set of 40 figures by twos, setting down the results. He then added the results and then added the original figures in a different order. Any variation recorded in the two results indicated errors. The tables give the time required for the whole process. Test No. 12 was designed to determine the subject's facility in seeing and naming letters. A page from The Psychological Review was used; the subject reading the lines backward merely named the letters as fast as possible. The tables record the number of letters, average of two trials, named in one minute. Test No. 9 was designed to show the acuteness of hearing by discrimination of the intensity of two sounds. The sounds were vibrations of a tuning fork heard in a telephone in the silent room, the intensity being varied by a resistance board, only one telephone being used. The results in the tables have only relative value, indicating the number of divisions upon the resistance board by which the resistance had to be increased to enable the subject to detect the difference in the intensity of the sounds.

We may call special attention to a few of the results. In both subjects we again observe an increase in weight throughout the experiment with decrease after sleep. But with these subjects the decrease is less than the increase. In strength of lift both subjects lose quite regularly and seriously, but regain nearly all after sleep. In the memory tests, the results are very marked, especially with G. N. B. His average time in normal condition for committing the 18 figures was 134 seconds. No remarkable increase in this time was observed until the expiration of 72 hours. At 9 a. m. Friday the subject required 960 seconds to commit the first set of figures and failed entirely to commit the third set, working at it for *20 minutes*. At 9

A. G. S.

Table II.

	March 17.			March 18.				March 19.				March 20.				Mar. 21. After Sleep.
	9 a.m.	3 p.m.	9 p.m.	3 a.m	9 a.m.	3. p.m	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	1 p.m.
1. Pulse	74	68	75	61	73	73	72	71	79	62	67	61	74	68	63	76
2. Temperature (Centigrade)	37.11	36.39	36.78	37.11	37.00	37.22	36.89		36.89	36.44	36.56	36.33	37.06	36.67	35.33	37.22
3. Weight (Kilograms)	67.02	67.47	67.47	67.24	66.68	67.24	67.13	66.68	67.02	67.36	67.47	67.59	67.02	67.36	67.59	67.24
4. Grip (Kilograms)	33.56	39.92	30.39	33.11	33.56	29.03	24.04	24.04	28.12	29.48	26.31	26.76	29.03	30.39	27.22	33.56
5. Pull (Kilograms)	155.58	163.30	140.62	117.94	150.60	113.40	127.00	81.65	107.05	89.36	88.45	49.44	49.44	95.26	92.99	131.54
6. Reaction-time. Mean	.121	.134	.138	.134	.141	.138	.143	.154	.147	.150	.141	.146	.143	.148	.193	.160
Mean Variation	0.6	1.5	0.8	0.9	2.2	1.2	7.1	1.5	1.7	1.9	2.4	1.5	1.9	2.5	4.0	2.9
7. Reaction-time with discrimination and choice. Mean	.158	.200	.310	.175	.202	.201	.182	.162	.188	.280	.189	.170	.222	.176	.311	.231
Mean Var.	5.9	4.2	7.4	4.1	3.5	4.5	2.9	3.6	4.1	8.3	4.7	3.9	5.3	4.3	8.0	6.0
8. Acuteness of Vision (C.M.)		103.8	103.8	113.6	122.3	112.8	96.1	105.1	115.4	116.6	119.2	109.0	119.7	118.0	123.3	119.7
9. Discrimination of Sound	8.0	12.5	10.0	11.2	11.6	13.0	12.5	31.0	12.5	22.0	21.0	31.0	23.0	18.7	18.0	16.5
10. Memory (Sec.)	115	110	112	143	129	145	102	159	120	152	217	202	139	100	570	88
11. Addition of Figures		85	119	118	105	103	130	192	111	108	185	610	113	190	345	109
12. Naming of Letters	165	160	155	154	162	155	134	113	154	144	127	91	147	135	117	171
13. Voluntary Motor Ability	38	36	33	37	41	36	30	34	36	36	37	28	39	38	34	42
14. Fatigue. Per cent. of Loss	29.0	13.9	15.1	13.5	29.3	16.6	13.3	26.5	19.4	25.0	21.7	0.00	20.5	23.7	26.5	21.4
15. Pulse after Fatigue	80	69	69	66	79	71	77	65	72	75	64	62	70	64	61	83

G. N. B.

TABLE III.

	March 17.			March 18.				March 19.				March 20.				Mar. 21. After Sleep.
	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	4 p.m.
1. Pulse	63	64	63	68	68	67	67	69	70	62	64	68	74	65	73	84
2. Temperature (Centigrade)	36.22	36.44	36.33	37.17	36.78	37.22	36.56	36.67	36.33	36.61	36.56	36.89	37.06	35.78	36.56	37.22
3. Weight (Kilograms)	68.49	69.29	69.29	69.17	69.51	69.74	69.85	69.99	69.85	69.99	70.08	70.08	69.40	69.74	69.85	69.29
4. Grip (Kilograms)	42.64	34.47	38.10	33.11	39.36	43.09	37.65	34.01	37.19	37.65	42.64	43.09	44.45	47.63	44.00	41.73
5. Pull (Kilograms)	118.84	129.28	146.15	138.35	125.19	117.94	106.60	111.13	120.20	113.40	113.40	113.40	113.40	111.13	95.26	117.94
6. Reaction-time. Mean	.145	.148	.157	.130	.142	.143	.134	.187	.136	.137	.141	.123	.139	.141	.142	.124
Mean Variation	1.8	1.3	1.4	0.8	1.1	1.7	1.8	2.9	3.7	1.0	1.7	1.8	1.1	2.9	3.2	1.6
7. Reaction-time with discrimination and choice. Mean	.167	.170	.200	.140	.185	.177	.214	.170	.178	.147	.158	.133	.153	.143	.175	.166
Mean Var.	3.6	7.2	5.6	1.4	3.7	1.6	5.5	4.6	6.8	1.8	2.7	3.7	2.3	1.2	2.3	4.0
8. Acuteness of Vision		110.3	115.4	141.0	132.1	134.6	127.4	129.5	134.6	119.2	137.2	126.9	130.9	128.7	135.9	134.6
9. Discrimination of Sound	12.8	20.0	12.5	24.8	14.0	15.0	20.5	17.5	24.5	30.0	23.0	20.0	22.5	21.0	32.0	21.5
10. Memory	170	133	128	206	170	135	273	143	112	353	169	201	820+	645	900+	106
11. Addition of Figures		120	125	141	135	122	140	141	135	120	118	115	118	123	130	109
12. Naming of Letters	177	180	169	165	183	163	158	158	156	165	154	156	157	148	117	188
13. Voluntary Motor Ability	41	37	38	39	41	42	34	39	39	39	40	44	42	42	40	40
14. Fatigue. Per cent. of Loss	19.5	16.2	26.3	28.2	29.3	28.6	14.7	28.2	25.6	33.3	25.0	34.1	26.2	26.2	35.0	22.5
15. Pulse after Fatigue	70	69	60	69	79	63	64	69	79	64	55	64	77	69	70	96

p. m. he could not commit the figures, and having made no progress after 15 minutes he desisted. The attention could not be held upon the work. A kind of mental lapse would constantly undo the work done. With both subjects an energetic 'waking up' by means of brisk walking and fresh air was often necessary during the latter time in order to address themselves to these mental tasks. After sleep, A. G. S. easily committed the figures in 88 seconds, and G. N. B. in 106 seconds, this being in both cases the shortest time in which the work was done. In respect to the number of letters named in one minute, there is with both subjects a steady decrease with the progress of the insomnia, with immediate return to the normal after sleep. In adding numbers similar results appear in a marked form in the case of A. G. S., but with G. N. B. adding time was affected but slightly. Reaction-time increases with A. G. S., as with J. A. G., but the reaction-time of G. N. B. is not lengthened. In respect to reaction with discrimination and choice the results are irregular and unsatisfactory. There is an irregular increase with A. G. S., but an actual shortening of time with the other two subjects.

Attention should be called to the length of sleep following the sleep fast and its relation to the whole amount of sleep lost. A. G. S. found it necessary to make up but 16 % of the lost sleep, as measured by time; J. A. G. 25 %; G. N. B. 35.3 %; As restoration was in each case apparently complete, explanation must be sought in one of two hypotheses or in both. The first is that, owing to the greater 'depth' of sleep after the sleep fast, the anabolism accompanying restoration was more rapid. The second is that the partial restoration which normally accompanies the waking period was, in the case of this long waking, greater than usual; that the subjects, in other words, although apparently awake and, indeed, as wide awake as they could be kept, were nevertheless at times partially asleep. There are reasons to believe that the results depend upon both of these causes. Our subjects well illustrated the fact that sleep is a matter of degree. All that could be done both by objective diligence and subjective effort to keep the subjects wide awake was done. If the subject, contrary to his own intention, closed his eyes, although he immediately opened them in response to his watcher's command, still there was time for a short and, perhaps, refreshing 'nap.' Again, one of our subjects, who was kept jogging about the streets during a sleepy period at 5 a. m., afterwards could remember little about the walk. Another subject, standing with eyes open, reflectively gazing at a piece of apparatus upon which there were some pieces of rope, suddenly reported that he had had a dream about a man being hung. With our first subject we undertook to test the delicacy of the muscle sense by means of lifting weights. These weights were small tin pails loaded with graded weights and lifted by a detachable handle. Lifting these pails was found to be very monotonous and sleepy work. The subject was not permitted to let his attention wander, and yet he reported at least four dreams. For instance, he lifted two pails, carefully judged their relative weight, and as he set the second one down, instead of saying that No. 1 or No. 2 was the heavier, he said 'trimmings,' evidently having fallen asleep as he was lifting or setting down the pails and dreamed that they contained trimmings. It must be understood that these dreams were instantaneous and the subject as wide awake as he could be kept, but these facts reveal a cerebral condition related to sleep. This hypothesis alone, however, would not seem to account fully for the small proportion of sleep made up. And, indeed, a study of our special tests shows that restoration took place chiefly during the profound sleep following the sleep fast, and took place rapidly. That this sleep was actually more profound and that the profound part of it was longer than usual was shown by our experiments in depth of sleep in the case of J. A. G. reported below.

The depth of normal sleep for the consecutive hours of the night has been studied by Michelsen and by Kohlschütter, and the results presented in the so-called sleep curves. The depth of sleep was determined by these observers by the intensity of sound necessary to awaken the sleeper. Their results show the greatest depth of sleep at the end of the first hour. After the first hour the curve drops abruptly and rapidly. Already at the end of the second hour sleep is light and continues slowly to become lighter until morning. In the case of our first subject, J. A. G., we attempted to ascertain the relative depth of sleep for the consecutive hours of the profound sleep following the sleep fast, for the sake of comparing our results with the normal sleep curve. As a sound stimulus would not be practicable, for the reason that, the experiments all being made in the same period of sleep the sleeper would soon become accustomed to it, we substituted a pain stimulus. An electric garter, to which the subject had become accustomed by wearing it for some nights preceding the sleep fast, was attached to the sleeper's ankle and connected with an induction coil in an adjoining room, and so arranged that the current could be closed for a constant time, viz., .334 sec., by means of a pendulum, and that the strength of the current could be varied by means of a resistance tube. It was agreed that the sleeper should announce his awaking by means of an electric button at his bedside. The current was turned on at intervals of one hour. Unfortunately the least resistance that could be arranged with the resistance tube failed to awaken the sleeper at the first three periods, so that it was necessary to cut out the tube and the pendulum and apply the direct current and measure it roughly by the time the circuit had to be closed. Our results, therefore, lack the exactness necessary for the construction of a curve or table, but still show plainly the relative depth of sleep for the consecutive hours. The deepest sleep was found at the end of the second hour, when the subject could not be aroused sufficiently to ring the bell, but responded by a cry of pain. The next deepest sleep was found at the end of the first hour and the next at the third hour. The current used at these three times was one which it was altogether out of the question for the subject to endure when awake. At the end of the second hour, just after the experiment, we entered the sleeper's room and attempted to awaken him by speaking to him in a loud voice without avail. At the fourth hour the sleep was less deep, and continued to become lighter regularly until awaking, but the decrease in depth was very much less rapid than in the normal sleep curves reported above. At 10 a. m. a very slight current awakened the sleeper, and at 10:30 he awoke spontaneously as stated.

The tendency of our subjects to have short semi-waking dreams suggested to us that in enforced insomnia there would be offered a good opportunity for a study of dreams. This, of course, was incompatible with our purpose, but in the cases of A. G. S. and G. N. B., at the end of the sleep fast and before allowing the subjects to retire, we undertook a few experiments in dreams. We allowed the subjects to sit with head supported behind, and to sleep for periods of 30 seconds, one

TABLE IV.

J. A. G.	2d day before experiment.	1st day before experiment.	1st day of experiment.	2d day of experiment.	3d day of experiment.	4th day of experiment.	4th day of experiment. (Sleep.)	1st day after experiment.	2d day after experiment.
Hours			24	24	24	14	11¾	24	
Total amount urine (ccm.)			1475	1370	1270	805	400	950	
Grams N. per hour . . .			0.901	0.929	0.667	0.723	0.490	0.723	
Grams $P_2 O_5$ per hour . .			0.1327	0.1438	0.1105	0.1304	0.0564	0.0888	
Relation $P_2 O_5$ to N. . . .			1: 6.8	1: 6.5	1: 6.0	1: 5.5	1: 8.7	1: 8.1	
A. G. S.									
Hours	38		24	24	24	13½	12¾	24	24
Total amount urine (ccm.)	1308		1510	1700	1420	750	525	1000	1240
Grams N. per hour . . .	0.655		0.661	0.628	0.745	0.661	0.414	0.6175	0.761
Grams $P_2 O_5$ per hour . .	0.0765		0.0708	0.0791	0.1011	0.1000	0.0674	0.0907	0.1023
Relation $P_2 O_5$ to N. . . .	1: 8.6		1: 9.3	1: 7.9	1: 7.4	1: 6.6	1: 6.1	1: 6.8	1: 7.5
G. N. B.									
Hours	24½		24	24	23	13½	16½	24½	24
Total amount urine (ccm.)	920		1240	1205	1730	650	365	705	705
Grams N. per hour . . .	0.4853		0.7094	0.6270	0.6123	0.5195	0.3390	0.5020	0.4765
Grams $P_2 O_5$ per hour . .	0.0574		0.0802	0.0931	0.0826	0.0815	0.0435	0.0616	0.0613
Relation $P_2 O_5$ to N. . . .	1: 8.5		1: 8.8	1: 6.7	1: 7.4	1: 6.4	1: 7.8	1: 8.1	1: 7.8

minute, three minutes, etc., then awakening them and asking for their dreams. No dreams were obtained in any case. If the period was less than one minute the subject sometimes had a hazy memory of something like a dream which could not be put into words. If the sleep was longer it was apparently profound and dreamless. These rough experiments confirm, of course, the generally accepted opinion that dreams are the product of light sleep, representing indeed the reinstatement of consciousness after the early and profound sleep.

Through the kindness of Dr. E. W. Rockwood, of the University, a chemical analysis of the urine was made throughout the experiments in the case of each of the subjects. The object of the analysis was to determine the influence of continued waking upon the relative amounts of nitrogen and phosphoric acid respectively excreted. The results are fully exhibited in Table IV. as compiled by Dr. Rockwood. Considered in relation to the fact that each subject increased in weight during the insomnia, the results are significant. They show not merely that there was an increase in the excretion of both nitrogen and phosphoric acid during the period of insomnia, but that relatively more phosphoric acid was excreted than nitrogen. A certain amount of support is thus given to the theory of a special connection between mental activity and the katabolism of the phosphorized bodies of the nervous system.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF HARVARD UNIVERSITY.

I. The Relations of Intensity to Duration of Stimulation in our Sensations of Light.

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These experiments were made for the purpose of ascertaining the exact relation existing between the duration of a stimulus and the intensity of the resulting sensation. They were suggested by the phenomena of color-mixing by means of Maxwell's discs. This method of color-mixing shows that the influence of a given color upon the final mixture varies with the size of the sector of that color. In these rotations the color does not vary in intensity, but the time during which it stimulates a given portion of the retina changes. The relative time, however, is the only effective element, for after the colors once fuse, any increase in the speed of a rotation produces no change in the intensity of the colors.

My first experiments repeated the conditions given by Maxwell's discs, but with apparatus so arranged that the experimenter could determine the exact amount of variation in the intensity of the sensation, resulting from a given difference in the duration of the stimulating light.

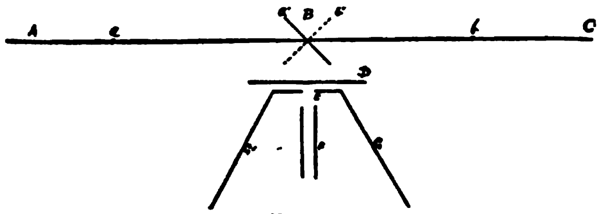

Fig. 1.

The apparatus used may be understood from the ground plan shown in Fig. 1.

A B and B C are two wooden arms along which slide lamps *a b* of one standard candle power each. The lamp *a* stands a little higher than *b*; and the dead white reflector *a′*, standing at its level, reflects its light through the upper half of the slit E. The reflector *b′* reflects the light from *b* through the lower half of the same slit. G is a large black screen to protect the subject's eyes from lateral lights. In it is the slit E 1 cm. wide by 4 cm. high. F is a black tube to fix the eye and still farther cut off side light. D is a dead black disc rotated by a color wheel. A window, *d′ d″*, is cut out of this disc, as is shown in Fig. 2.

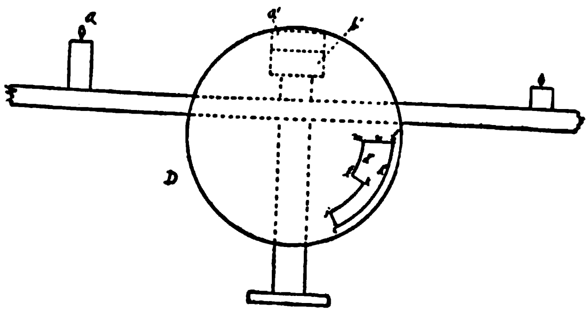

FIG. 2.

The lines *m h*, *l k* and *j i* are radial and *m l*, *k j* and *h i* arcs of concentric circles. D is placed so that when the window covers the reflectors *a′* and *b′* the line *j k* is level with the horizontal line dividing them. Consequently *a′* stimulates the eye while *d′* is passing between it and the slit, and *b′* while *d″* is passing. The absolute duration of this stimulation will depend upon the rotation-rate of the disc, but we have seen that it is only the relative time we need to consider in comparing the effects of stimuli of different duration. The relative times of exposure to the eye of the lamps *a* and *b* will be proportional to *i n* and *k n* severally.

The room was darkened and the lamps placed 20 cm. from the reflectors (correction being made in this and every other experiment reported for any over-estimation of intensity due to position, etc.). The disk, with *j n* and *k n* in the ratio of 2 : 1, was rotated 100 times per second, under which conditions the after-images from a' and b' fused completely, so that each reflector gave a continuous impression. But the lower one now appeared much darker than the upper one. In order to determine how much darker, the lamp b was moved toward b', thus increasing its objective intensity until the two reflectors again appeared equally illuminated. In other words, until the intensity of sensation lost through the shorter time of stimulation was compensated by the greater intensity of the stimulus. The relative intensity of the reflected lights may now be calculated from the distances of the lamps, and the ratio between the original intensity of b and the final one will express the loss of intensity due to its shorter time of stimulation.

The results of this experiment are given in Table I., each ratio given being the average of ten determinations. The subjects were Dr. Singer and the writer.

TABLE I.

Ratio of $d' : d''$	Ratio of Intensity.	Subject.
1.35 : 1	1 : 1.35	S.
1.35 : 1	1 : 1.39	L.
2.05 : 1	1 : 2.05	L.
2.93 : 1	1 : 2.97	S.
3.00 : 1	1 : 3.02	L.

It is clear from this table that when the difference of time between d' and d'' is not greater than that here employed, *the intensity of the resultant sensation is proportional to the time of stimulation.*

A second series of experiments followed these, differing from them only in this, that the light from a' was not interrupted at all, and hence always produced its maximum effect. The light from b' was interrupted by sectors of the disc D. If S represents the width, in degrees, of the sector, then 360 : 360–S will represent the relative duration of the stimuli from a' and b'. The rapid rotation of the disc caused b' immediately to appear much darker, a', of course, remaining unchanged. The intensity of b' was then made to equal a' by moving the lamp b nearer its reflector. From these data it is possible to determine the loss of intensity due to the shortened time. The experiment was made with light of different intensities. That reported in Table II. was produced by one candle-power at 100 cm. The experiments using other intensities gave the same results. Each number in the table represents the average of a large number of determinations. In this and all the following experiments the writer was the only subject, other persons being used only to confirm the results given.

TABLE II.

Ratio of duration.	Ratio of intensity.	Ratio of duration.	Ratio of intensity.
1 : .0055	1 : .006	1 : .222	1 : .239
1 : .0083	1 : .0078	1 : .25	1 : .272
1 : .011	1 : .011	1 : .306	1 : .385
1 : .014	1 : .012	1 : .361	1 : .463
1 : .0166	1 : .0164	1 : .5	1 : .538
1 : .0194	1 : .0177	1 : .611	1 : .645
1 : .0222	1 : .0225	1 : .666	1 : .662
1 : .025	1 : .0249	1 : .702	1 : .7
1 : .0277	1 : .0273	1 : .705	1 : .702
1 : .0555	1 : .059	1 : .803	1 : .813
1 : .083	1 : .091	1 : .888	1 : .909
1 : .1111	1 : .121	1 : .902	1 : .943
1 : .139	1 : .156	1 : .904	1 : .97
1 : .166	1 : .176	1 : .97	1 : .98
1 : .194	1 : .209		

This table shows that throughout the entire series a decrease in the time of stimulation results in a proportional decrease in the intensity of the resultant sensation. All of these experiments were then repeated with colored lights, produced by interposing gelatine sheets; red, green, blue and yellow were used. These gave the same results as those given in Table II.

It would appear from these experiments that the chemical processes in the retina take place only after a certain inertia has been overcome, and that this requires a certain duration of stimulation. When under ordinary conditions a stimulus of a given intensity excites the retina it produces a chemical disintegration, which is a growing process up to a fixed limit, which depends upon the intensity of the stimulus. When this limit is reached, the light produces its maximum effect. Beyond the point of maximum effect, time produces no difference of intensity; looking at a lamp for two minutes does not make it seem brighter than when it is seen for only one minute. But below this point of maximum effect, the duration of the stimulus is one of the factors determining the amount of disintegration in the retina and so the intensity of the resulting sensation. It was the object of a third series of experiments to find the point of maximum effect. The apparatus was the same as that already described, except that a large dead black screen swinging upon a pendulum apparatus took the place of the disc D. The screen contained a window similar to that in the disc.

Let us call the upper and narrower half of this opening s', and this lower and wider half s'', and the reflectors back of the screen a' and b' as before. The opening was so arranged that at the lowest point of the swing, both reflectors came simultaneously into view, a' being seen through s', and b' through s''. The relative time during which a' and b′ will stimulate the eye will depend upon the relative width of s' and s'', while the absolute time of both stimulations will depend upon the arc through which the pendulum swings. The absolute time of exposure was determined for each degree of swing by the ordinary means. The pendulum apparatus was made especially for this laboratory by Elbs Freiburg, after Münsterberg. The length of pendulum is 2 meters, but adjustable weights and counterweights give every rate of swing desired. The screen and opening were made in Cambridge, Mass.

In order to obtain the time for maximum effect, the lamps were placed so that a' and b' gave sensations of equal intensity and s' and s'' were then adjusted to the relation of 1 : 2. When now the pendulum was allowed to swing through a small arc the two reflectors seemed equal, but as the amplitude of the swing increased, a point was soon reached where a' appeared just perceptibly darker than b'. This marks the point where a' fails to produce a maximum effect. With the openings and lamps adjusted as before, the pendulum was now given a much larger swing. This caused *a′* to appear much less intense, while *b′* retained its former intensity. The amplitude of the swing was gradually decreased; with this *a′* becomes gradually more intense until it finally becomes equal to *b′*; after this no farther change will take place. This also makes the point of maximum effect for *a′*. These points were ascertained by a large number of experiments and their mean taken as the real time necessary to produce a maximum effect. This point of maximum effect was found for light of several intensities, as given in Table III. The light of a single candle-power lamp at 320 cm.—the limit of the apparatus—was taken as the unit.

TABLE III.

Intensity of Light.	Time of Maximum Effect.
1.	148 σ
4	110 σ
16	100 σ
64	85 σ
256	90 σ

It will be seen from this table that while the time becomes a little longer for the weaker stimili it remains very nearly the same for all but the very lowest. Other subjects gave similar results, but the absolute times varied somewhat.

It should be remarked here that the point of maximum effect —where duration influences intensity—is in no way connected with the threshold for time judgments. The judgment of a difference of duration does not go over into a judgment of difference of intensity.

A third series of experiments were made to determine the amount of intensity lost in a single stimulation by any given reduction of time below that required for the maximum effect. Two methods were employed to reduce the time, giving rise to two sets of experiments. One method used a difference of swing, while the window remained constant; the other varied the size of *s′*, while the swing was the same throughout. The first series employed the method of right and wrong cases to determine the position of the lamps; the second used the method of just perceptible differences.

The first of these experiments was as follows: The pendulum apparatus above described was used, s' and s'' were adjusted in the relation 1 : 2, and the lamp b placed 20 cm. from the reflectors. The pendulum was allowed to swing through a given arc, and a moved until a' appeared similar to b'. Table IV. gives the results of this experiment. The two series were separated by several months, both are given here to show the constancy of the results. Each number is determined by the method of right and wrong cases.

TABLE IV.

	Duration of stimuli.		Ratio of time.	Ratio of intensity.	
Arc.	$S'\sigma$	$S''\sigma$		1st series.	2d series.
60°	30	60	1 : 2	1 : 1.99	
50	40	80	1 : 2	1 : 1.94	
40	60	120	1 : 2	1 : 2.03	1 : 2.03
30	85	170	1 : 2		1 : 1.85
24	110	220	1 : 2	1 : 1.69	
20	150	300	1 : 2	1 : 1.23	1 : 1.23
18	180	360	1 : 2	1 : 1.08	1 : 1.06
16	250	500	1 : 2	1 : 1.10	1 : 1.00
15	310	620	1 : 2	1 : 1.10	1 : 1.02

We see from this table that until 40° is reached the lights keep the same ratio as the openings. Both reflectors did, however, become lighter as the duration of the stimulation became longer. Below 40° b' remains constant and a' approaches it; in other words, b' produces its maximum effect, at between 120σ and 170σ. After 18° the two reflectors are of equal intensity; a' is also producing its maximum effect. This point is somewhere between 150σ and 180σ. These numbers differ from those given in Table III. But the determinations for Table III. were made with a more perfect reflector, giving a much more intense light. Figure 3 gives the curve of intensities as obtained from Table IV.

Between the two points of maximum effect the intensity of the sensation is seen to be exactly proportional to the duration of the stimulus.

The other set of experiments under this head gave a wider

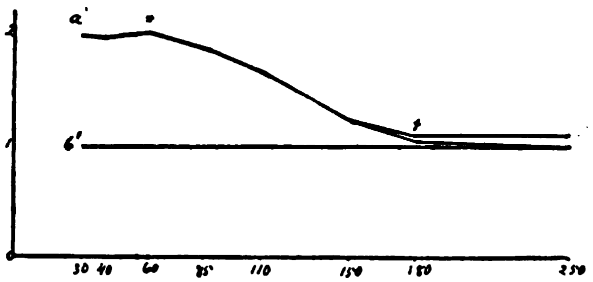

FIG. 3.

range of time for investigation. By the first method the time was limited by the maximum point for *b'*. With the second method the pendulum swung through a constant arc, and *s''* also remained constant, always producing a maximum effect, while the duration of *a'* was regulated by the size of *s'*. The intensity of light chosen was one candle-power at 80 cm. The time of maximum effect was first found for *s'* when *s'* and *s''* were in the rated 1 : 2 in the manner already described. It was found to be 100 σ, and this was taken as the unit of time.

The time of *a'* was now made one-half of the standard, 100σ, by reducing *s'*, and the loss of intensity resulting determined as before. This was repeated with other lengths of stimulation as given in Table V.

The first column gives the duration of *a'* in fractions of the maximum time, the second column giving the corresponding reduction of intensity.

TABLE V.

	Time.	Intensity.
1	1.	1.
2	.5	.55
3	.25	.33
4	.125	.115
5	.0625	.065
6	.0312	.029

The exact relation between the duration of short stimuli and the intensity of the sensation must be accepted. We see that it holds both for rapidly succeeding stimuli and for single stimulations.

The inertia of the retina against chemical disintegration may be accepted as a fact. The amount of this disintegration determines the intensity of the sensation. A strong stimulus acting for half the time necessary to produce its maximum effect gives rise to a sensation of exactly the same intensity as that produced by half as strong a stimulus producing its maximum effect. The stronger sensation does contain the weaker, temporally, for between the first moment of stimulation and the moment of maximum effect the disintegrating process will pass through the series *o* to this maximum. Each step in this series is the basis of a sensation of corresponding intensity. While these growing sensations as such do not enter consciousness, they may be the elements of our feeling of 'more' or 'less,' as concerning the intensity of sensations.

In this way we may conceive of a physiological basis of intensity which does not give a qualitative difference to the sensation.

II. Normal Motor Automatism.

BY LEON M. SOLOMONS AND GERTRUDE STEIN.

It is well known that many hysterical subjects exhibit a remarkable development of the subconscious life, amounting, in many cases, to that most interesting phenomenon known as double personality. It has often been argued that the performances of these 'second personalities' are essentially different from the merely automatic movements of ordinary people—so different, in fact, as to compel us to accept the name 'second personality' as a literal expression of the real state of things. Against this view it is urged that we underestimate the automatic powers of the normal subject. We are told that many of the acts which we usually do quite consciously might really be done without consciousness. In support of this assertion such facts are pointed out, as men completely undressing without knowing it, when their attention is distracted by other matters. If this latter explanation is to hold, however, something more than assertion must be forthcoming. The limit of automatism is something that is essentially capable of demonstration by experimental methods, and its investigation forms the subject of this paper.

It must not be understood that any attempt is made to answer the vexed question of a so-called 'subliminal consciousness.' This question cannot be settled experimentally, unless it be admitted beforehand that the automatic acts of normal subjects, between which and the 'second personality' an analogy is asserted, are themselves unaccompanied by consciousness. But this is by no means universally admitted. The question of consciousness, in all cases where it is not directly experienced, is essentially a philosophical one, and the facts of psychology have little, comparatively, to do with it. But the question of whether the performances of the 'second personality' are to be allied to the automatic acts of ordinary people, or whether they are to be allied to those acts which never go on save in the full glare of consciousness—by the aid of reflection, judgment and will; this question is perfectly definite, capable of satisfactory solution by observation and experiment, and of great importance to scientific psychology.

The object of our experiments, then, was primarily to determine the limits of normal automatism, and, if possible, show them to be really equal to the explanation of the second personality; and incidentally to study as carefully as possible the process by which a reaction becomes automatic. Above all, we wished to avoid anything like a real production of a second personality. For the experiments to really settle the point at issue it was essential that no suspicion should rest upon the complete 'normality' of the subject throughout the experiments. Our idea was to reproduce rather the essential *elements* of the 'second personality,' if possible, in so far as they consist of definite motor reactions unaccompanied by consciousness—or shall we say, out of deference to the subliminal consciousness theory, unaccompanied by 'conscious consciousness.' These elements appeared to us to be conveniently considered under four groups, as follows:

1. General tendency to movement without conscious motor impulse.

2. Tendency of an idea in the mind to go over into a movement involuntarily and unconsciously.

3. Tendency of a sensory current to pass over into a motor reaction subconsciously.

4. Unconscious exercise of memory and invention.

In the complete second personality all these elements exist at once. We proposed to prove their existence in normal subjects separately.

1. General tendency to movement. For these experiments a planchette was used. Both of us had previously tried in vain to 'write planchette.' Neither of us has any aptitude for will ing games, etc. We may both as far as we know stand as representatives of the perfectly normal—or perfectly ordinary—being, so far as hysteria is concerned.

The planchette used was a glass plate mounted on metal balls, with a metal arm holding a pencil. The subject placed one hand firmly on this and then proceeded to get himself as deeply interested in a novel as possible. In this way it is easy to show that although the arm does not really move spontaneously, yet any movement once started up tends to continue of itself. Further, very slight stimuli are capable of starting the movement. For example, as soon as the position of the arm grows uncomfortable, or would be uncomfortable if the subject attended to it, it is likely to begin movement. By slightly moving the planchette it is easy to start the arm to moving, after which it will continue of itself if not deliberately checked by the will of the subject. If the story that the subject is reading be sufficiently interesting, all this goes on without his knowledge. Where he is conscious of the movements of his arm, however, they appear to him to be *extra personal*. It is not he but his arm that is doing it. He cannot say whether his arm is moving spontaneously or whether it is being moved by the operator. Later, if allowed practice, he may learn to make this distinction, but the movements do not at all lose their extra personal character. He readily perceives that they are of two kinds de pending on whether the operator moves the planchette or his arm moves, but both these movements seem equally disconnected with himself. He gains his knowledge of the movement purely through sensations from the arm. He has no feeling of intention or desire; no fore-knowledge of what the movement is to be. As we shall see, this feeling of extra personality appears in all our experiments whenever knowledge of movement is gained purely from sensations—whenever there is no preceding feeling of intention. Where the attention of the subject is completely distracted by the reading, all knowledge of the experiment disappears and the movements go on entirely without his knowledge and quite as well. The only interference comes if the story gets too exciting, when emotional reflexes are likely to interfere either by causing violent movement or by stopping all movement.

Sometimes it is possible to 'teach' the arm some special movement, which it will then go on making of its own accord. For example, the operator may start the planchette to making *m* strokes, and as soon as the hand has caught the movement—shown by the absence of resistance—stop. The arm goes on making the strokes. Gradually, however, it gets them more and more out of shape until it has got into an elliptical movement which is more natural to it, apparently. When this habit—that of making wide elliptical movements, has become well developed, the arm loses its 'suggestiblity' and can no longer be taught special movements. The moment the planchette is released it starts back to its own movement. In connection with this natural movement it should be noticed that it is much more difficult for the subject to distinguish between spontaneous movements and movements impressed by the operator, when the impressed movement is the natural one, than when it is widely different from this. Apparently the arm quickly falls in to the suggested movement when it is its own natural movement; while in other cases this falling in is delayed, resulting in a tension in the muscles of the arm representing its 'hanging back' behind the movement impressed on the hand by the motion of the planchette. It is by learning to recognize this tension that the subject is enabled to distinguish between spontaneous and impressed movements. Introspectively this seemed to be about the method, that is, and it agrees well with the fact just noted.

From these experiments we concluded that in normal subjects there is a general tendency to movement from purely sensory stimuli, independent of any conscious motor impulse or volition. This tendency is ordinarily inhibited by the will, but comes out as soon as the attention of the subject is removed. This tendency to stop automatic movements and bring them under the control of the will is very strong. Nothing is more difficult than to allow a movement of which we are conscious to go on of itself. The desire to take charge of it is almost irresistible. But as we shall see later it is a habit that can be overcome, and a trained subject can watch his automatic movements without interfering with their complete non-voluntariness.

From now on, having demonstrated the tendency to spontaneous movement, we did not hesitate to make the mere movement element voluntary.

2. Tendency of ideas to go over into movement. For these experiments the subject was given a pencil which he kept moving over a paper as though writing—a sort of continuous movement—he meanwhile being engaged in reading a story. The writing movements quickly become automatic, and nothing prevents the subject from giving his full attention to his reading. Under these circumstances there is a very decided tendency to write down words read, especially simple words such as the, in, it, etc.

Sometimes the writing of the word was completely unconscious, but more often the subject knew what was going on. His knowledge, however, was obtained by sensations *from the arm*. He was conscious that he just *had* written a word, not that he was about to do so. While mere scribbling went on the subject would scarcely be conscious that he was doing anything; but the writing of a word—either because of the different character of the movements, or their greater energy—seemed to attract his attention. Small words would usually be completely written before the subject knew about it, but large words would only get started. But even where there was no interference from the attraction of the voluntary attention large words were seldom attempted, and, still more rarely, more than just begun. This fact may, however, very easily be referred to the fact that reading is so much faster than writing that subsequent words, with different motor reactions, interfere with the writing of a long word. But a word that can be written with one impulse is not affected by this. Succeeding words may be read before it is written, but their motor impulses do not reach the arm in time to interfere.

As experiments of this kind were of necessity also carried on during the next series, they were not prolonged.

3. Unconscious passage of sensation into motor reaction.

The first form of this which we tried was writing at dictation. As in the other experiments, the subject's attention was occupied as fully as possible in reading. He kept his pencil moving constantly, scribbling when no dictation was going on. These experiments were by far the most difficult we attempted, and required the most training.

At the first attempt the subject is entirely unable to follow what he is reading. He reads, but does not get the meaning. He is painfully conscious of the experiment and everything connected with it. He has an irresistible tendency to stop whenever a word is given to him and attend to that until it is written, and then go on with his reading. In a word, the conditions demanded by the experiment are opposed to all his habits of attention, and the successful carrying out of the experiment demanded that these habits be overcome. And yet, in spite of this, there were momentary lapses of consciousness right from the start. Very uncertain in character and very rare, but enough to encourage us to persevere.

One very quickly gets sufficiently accustomed to the experiment to follow the story. But the habit of turning the attention to the writing whenever a word is given is difficult to overcome. The facility one acquires in rapidly shifting the attention from reading to writing and back, without confusion or effort, is really quite remarkable. Where at first the effort produces nothing but confusion of the worst kind, in a few hours' practice one is able to read his story with perfect ease and comfort, undisturbed by the constant interruptions for writing, even when these are quite frequent—say every 15 or 20 seconds. But when the story grows interesting the attention is held too powerfully for this, and cases of pure automatism begin to appear frequently. The word is written or half written before the subject knows anything about it, or perhaps he never knows about it. For overcoming this habit of attention we found constant repetition of one word of great value. By such methods as these we gradually began to get control of our attention, and produce the necessary conditions for the experiment. There are four elements to be distinguished in the writing of a word at dictation. 1, The heard sound; 2, the formation of a motor impulse; 3, a feeling of effort; 4, sensation from the arm telling of the written word. 2 and 3 are frequently indistinguishable in consciousness, but they are distinct, for they come and go under different circumstances. 2 consists of a melange of visual and kinæsthetic material—whatever ordinarily innervates our writing—as well as other elements not easily described, and perhaps really a direct consciousness of a motor current. On this point more later.

The first thing to disappear is the feeling of effort. We hear the word, have an idea of how it should be written, and then it is written. The writing seems perfectly voluntary, but there is no sense of difficulty, of 'something accomplished.' The strong self-consciousness that accompanies a concentration of the will at any point is entirely lacking, but nevertheless the writing feels thoroughly voluntary. This feeling of effort reappears after a while, and then it is time to stop the experiment, for the arm is tired. It comes back also if the voice of the operator falls too low.

The next step is the disappearance of the motor impulse. The writing becomes non-voluntary. We hear the word, and we know what we have written; that is all. This is the general condition of things throughout the experiment, after the preliminary training is over. The writing is conscious, but non-voluntary and largely *extra personal.* The feeling that the writing is *our* writing seems to disappear with the motor impulse. This fact is doubly significant here, for in this case we have a fore knowledge of what the written word will be, since we hear this dictated word. The reaction of the arm is not really unexpected, yet it is still not felt to belong to the willing subject. It sometimes seemed that the visual element of the motor impulse might remain, and the reaction still feel extra personal. But opportunities for observing this were few, and we advance the proposition with hesitation. If true it would lead to the conclusion that the motor impulse contains a direct consciousness of a motor current, which is the essential element in an act of will; for the kinæsthetic element of the impulse is, with us, extremely slight, if, indeed, it exists at all in ordinary unstudied movements. This view, that the motor impulses, descending from the higher centers to the lower, are accompanied by consciousness, is one that all our experiments have tended to impress powerfully upon us. Yet the tangible, 'statable' evidence for it is extremely slight. It seems to be an unconsciously produced conviction proceeding from a multitude of elusive trifles.

Real automatism, that is, dropping out of consciousness of the other two elements, heard sound, and return sensations from the arm, comes only at intervals and for short periods at a time. But it comes *whenever the attention is sufficiently distracted.* In no case does withdrawal of the attention interfere in the least with the reaction. The writing goes on just the same, but below consciousness. The only exception to this comes on the emotional side. If the story gets very exciting the muscular tension, which is one of the expressions of intense suspense, stops the arm movements entirely, and, of course, with that the possibility of writing words. Also, in very exciting parts, the tendency to write words from one's reading is also increased, but this does not interfere much.

A very distinct stage in the process of becoming unconscious is where we find the word started before we are conscious of having heard it, or we learn the word first from our writing, and then perhaps recall its sound by the memory after-image; or we are uncertain what word was dictated, and while we are wondering the word is written. Every once in a while the story grows interesting, and we return to ourselves with a start to find that we have been going on writing just the same. In this connection it is important to notice that the return to consciousness is always from the motor side. We suddenly become aware that our hand is writing something. It is never the sound that recalls us. This, of course, may be an individual peculiarity to a certain extent, and possibly would not be true of everyone. Yet, Miss Stein has a strong auditory consciousness, and sounds usually determine the direction of her attention.

For a long time during these experiments nothing was more marked than the complete failure of automatism as soon as the voice fell below a certain degree of loudness. The moment that happened the writing would not continue without the formation of a motor impulse, usually accompanied by a feeling of effort. This minimal loudness was so near the point of difficult hearing that we could not say whether the feeling of effort really belonged to the identification of the sound, or the formation of the motor impulse.

After long practice this phenomenon disappeared quite suddenly. The minimum loudness took a big drop to a point rather below easy hearing. It now became very much easier not to attend to the dictation, and the intervals of complete unconsciousness lasted much longer, and occurred much more frequently. Our results were now entirely satisfactory and we stopped the experiment.

As to the extent of the unconscious intervals, they frequently extended for five or six words with complete unconsciousness, while the successive occurrence of several such intervals, separated only by momentary flashes of consciousness, was not uncommon.

As to the test for unconsciousness, of course, in the nature of things, the only test can be that of memory. One cannot directly observe unconsciousness. Here it will, of course, be said that there is no proof that it is not merely memory that is at fault. We may be momentarily conscious of these reactions, but forget them. Of course, the same objection can be made to any alleged case of automatism, and the fundamental object of these experiments, to establish an analogy between the acts of the second personality and what is ordinarily called automatism, is not affected by this objection. There is no proof, save that of memory, for the performance of the so-called 'split-off consciousness' being other than a performance of the primary consciousness, nor for any of the simple reflexes ordinarily called unconscious being really not cases of rapid alternation. Our problem, being purely one of similarity between two well marked systems of phenomena, is independent of the ultimate interpretation of either group. We simply wish to show that what holds for one holds also for the other.

Nevertheless, this question of alternation without memory, versus real unconsciousness, is an important one, and as we made observations bearing on this subject it will be well to record them here.

In brief, what we observed was a phenomenon different from true unconsciousness, but corresponding almost exactly to the conception of alternation without memory. The subject was absolutely unable to recall a single word written, but nevertheless felt quite certain that he had been writing, and that he had been conscious of every word as he wrote it. This, in fact, was the general condition of things through the greater part of the experiments, after training was well under way. The same sentence might be dictated to the subject over and over again, and at the end of the series he would not know what it was. Yet not a single instance of what we have called unconsciousness occurred during the interval. Of course, this is not conclusive, for obviously there is memory of some kind even in this case, though not a memory of what was written. But the important point is that real unconsciousness appeared, not as a last stage of this, but as an altogether different phenomenon coming quite suddenly, and under different conditions. The consciousness without memory seems to *approach as its limit*, simply a condition in which the subject has not the faintest inkling of what he has written, but feels quite sure that he has been writing. It shows no tendency to pass beyond this into real unconsciousness. It seems to depend on the lack of associations between the different words—one word going out of consciousness before another has come in to be associated with it. It is facilitated by slow dictation. And conversely real unconsciousness appears not as a final stage of a gradually decreasing memory, but quite suddenly. It may break into a period of consciousness without memory, and be followed by such again, but it is equally likely to break into a period of complete memory. In either case it comes entirely unheralded by any transition form, and departs as suddenly and silently. It does not seem to depend upon association elements at all—is entirely independent of the speed of dictation up to the limit of writing speed.

This identification of a phenomenon so strikingly in accord with the 'alternation-without-memory' theory, yet so strikingly different from the well known phenomenon of unconsciousness, seems to us to leave little room for reasonable doubt as to the correctness of the common sense view of the unconscious—the view, that is, that it really is unconscious.

This phenomenon of failure of memory, in spite of the presence of consciousness, will at once be recognized as corresponding quite closely to some well known hysterical phenomena. We shall come across more instructive instances of it later on in automatic reading.

It will perhaps be objected to these experiments that the long training required to bring them out destroys their value, for the hysterique does all these things without special training. It will be said that to prove that the second personality uses nothing but habitual brain paths it is scarcely permissible to establish new paths.

But it must be remembered that our training was purely a training of the attention. Our trouble never came from a *failure of reaction*, but from a *functioning of the attention*. It was our inability to take our minds off of the experiment that interfered. From the start, whenever, by good luck, this did happen, the reaction went on automatically. (The exception noted from intense excitement is, of course, of no importance in this connection.) The hysterique has no trouble here, for he is *unable* to *attend* to the sensation, attention to which bothered us. It is his anæthesias which make automatism possible. What in his case is done for him by his disease we had to do by acquiring a control over our attention.

But if there was no real creation of new paths, it will be objected that yet the lowering of the minimal loudness of dictation, so essential to the success of the experiment, was at least an opening up and 'smoothing' of old paths. This is doubtless true, but it must be remembered that training of this kind the hysterique can get during the early stages of his disease. The formation of a second personality is a late development, and sub-conscious acts of an irregular character occur for a long time before the organized second personality appears. During this stage paths which are not yet well worn may be opened up. It will be remembered that in our experiments we found automatism easier when the arm was fresh. When tired it suddenly failed. Apparently, the energy reaching it along the automatic path is no longer sufficient. Produce this backwards now. Imagine an arm in the condition of 'chronic rest' of an hysterical paralysis. Is it not altogether likely that it often acquires great sensitiveness from this, so that stimuli reaching it along the automatic path, not strong enough to produce a reaction in a normally exercised arm, may yet produce a reaction in the hyperæsthetic arm? In this way old paths may gradually be widened, until the second personality emerges —possibly with a sub-conscious hyperæsthesia to trouble some psychical researcher.

Automatic Reading.—This is a very pretty experiment because it is quite easy and the results are very satisfactory. The subject reads in a low voice, and preferably something comparatively uninteresting, while the operator reads to him an interesting story. If he does not go insane during the first few trials he will quickly learn to concentrate his attention fully on what is being read to him, yet go on reading just the same. The reading becomes completely unconscious for periods of as much as a page. In this experiment when well under way, it is the moments of conciousness that are rare. One remembers having read something at the beginning of the paragraph and suddenly finds himself at its end. All between is a blank. One feels that he surely must simply have suddenly let his eyes drop from one end to the other. Often, though the reading is entirely unconscious he is conscious of a confused murmer heard all the time—the sound of his voice—but it bears about the same relation to his consciousness as the murmer of the stream, beside which one reads on a summer day—a general background of sound, not belonging to anything in particular.

The reading is not entirely lacking in expression, and the pauses are made quite properly. But the tone is usually more monotonous than the reader's normal. Absurd mistakes are occasionly made in the reading of words—substitutions similar in sound but utterly different in sense. The usual suggestibility of the unconscious is shown in a tendency to insert words from the reading which is attended to. (Here it will be noticed appears an automatic path from ear to mouth.) The words read must be familiar for the automatism to work well. Dialect stories do not go well at all.

The eye movements in this experiment are most interesting. The tendency to raise ones eyes from the book one is reading, and turn them on the person one is listening to, is very strong. A compromise is frequently the result. One's eyes are focused at a point a little above the book, and the reading goes on out of the corner of one's eye. Tendencies of this kind, however, are not so hard to overcome as one supposes the first time he tries. Eye movements here seem to be simply a result of attention, not in any sense the thing itself.

The feeling of extra personality appeared here too. Whenever it happened, that is, that the subject after a period of automatic reading suddenly began to *hear* what he was reading, his voice seemed as though that of another person. This effect did not disappear immediately when he began to see the printed words. Not until he had, as it were, 'taken in hand' the process by which printed words pass into speech, did extra-personality disappear from his reading.

When both persons read with equal loudness, each trying to pay attention to the other, the conditions are very different. In the simpler experiments the problem is simply to pay attention to sounds, and not to sight and speech. When both read equally loud, however, this is not enough. It is easy enough to get the reading automatic, but to listen to another person's voice and not to one's own is another matter. Here comes in the distinction pointed out in the automatic writing between the mere entering of consciousness and the establishment of associations giving memory and meaning. It is not possible to hear only the other person's voice. If the centers for the consciousness of sound are in a condition to respond to afferent currents at all they respond to all, or, at least, do not discriminate except at haphazard. But it is possible to *grasp the meaning* of one only, the other being in the condition of the words written, but not remembered, in the automatic writing. This affords a most interesting field of observation, but as it concerns a different problem from the one in hand I speak no further of it here. It will form part of another series of experiments having as their problem the general relation of attention and memory. These two elements of attention are very distinct. The one a mere attending to certain classes of sensations—a physiological distribution—the other inseparably bound up with the laws of association and the act of thinking things together, the holding before the mind of a general conception which is gradually modified by new information.

4. *Unconscious memory and invention.*—The first experiments in this line were on automatic speaking, and were carried out in connection with the automatic writing at dictation. For this purpose the person writing read aloud while the person dictating listened to the reading. In this way it not infrequently happened that, at interesting parts of the story, we would have the curious phenomenon of one person unconsciously dictating sentences which the other unconsciously wrote down; both persons meanwhile being absorbed in some thrilling story.

In this experiment, as in the automatic reading already described, whenever it happened that the speaker became aware of his dictation solely by hearing his own voice, his voice seemed strange and extra personal. The dictation was of the character that we already had used during the experiments, short, simple words strung along grammatically, but not representing usually any special thought.

Spontaneous automatic writing.—This became quite easy after a little practice. We had now gained so much control over our habits of attention that distraction by reading was almost unnecessary. Miss Stein found it sufficient distraction often to simply read what her arm wrote, but following three or four words behind her pencil. All the phenomena observed in the writing at dictation were confirmed here—the order of disappearance from consciousness, extra personality, difference between memory and consciousness, etc. Two very interesting phenomena were here observed for the first time.

A marked tendency to repetition.—A phrase would seem to get into the head and keep repeating itself at every opportunity, and hang over from day to day even. The stuff written was grammatical, and the words and phrases fitted together all right, but there was not much connected thought. The unconsciousness was broken into every six or seven words by flashes of consciousness, so that one cannot be sure but what the slight element of connected thought which occasionally appeared was due to these flashes of consciousness. But the ability to write stuff that sounds all right, without consciousness, was fairly well demonstrated by the experiments. Here are a few specimens:

"Hence there is no possible way of avoiding what I have spoken of, and if this is not believed by the people of whom you have spoken, then it is not possible to prevent the people of whom you have spoken so glibly . . . ."

Here is a bit more poetical than intelligible:

"When he could not be the longest and thus to be, and thus to be, the strongest."

And here one that is neither:

"This long time when he did this best time, and he could thus have been bound, and in this long time, when he could be this to first use of this long time . . . ."

In this automatic writing from invention appeared more strongly than anywhere else the fact that the motor impulse is necessary for the feeling of personality. For it was easy here for long periods to get the process in a condition where there was often an expectation of what word would be written, but no *intention* to write it. One watched his arm with an idle curiosity, wondering whether or no the expected word would be written. In these experiments more than in any others did we feel the need of supposing that conciousness accompanies motor currents. If we wrote without watching what we wrote the writing was rapid and very illegible. By watching the writing, however, or, more correctly, by keeping our eyes on it, for there was no attention to it, the writing was kept even, legible, and at moderate speed. The control of movements by return sensation of sight is thus demonstrated to be an automatic process.

Subconscious exercise of memory.—The subject while his attention was distracted by listening to reading wrote some bit of poetry well known to him. The object was to see whether the memory, though in purely sound and speech terms, would yet go over into writing reactions automatically. The things written were bits of poetry that the subject had often repeated to himself, but never written. The experiment was successful. Its significance is that it shows that an act, to go on automatically, need not have been done before, provided all its elements have been done before. Thus in this case we have a combination of the automatic going over of ideas, or words, into writing reactions, the tendency of words written by the hand to call up in the mind their corresponding sound, and this to call up the next word of the poem which had been memorized in sound terms. The experiment is thus a justification of our general method of splitting up the second personality into its elements, and reproducing them automatically, instead of striving to reproduce the entire phenomenon at once.

Some general characteristics of the experiments.—In all automatism the tendency toward increased speed is marked. Writing tends towards a pace that very quickly tires, reading towards a rapidity that prevents distinct articulation, dictating toward a speed that soon becomes hopelessly fast for the writer. The increase of speed is gradual, and occasional corrections during flashes of consciousness suffice usually to keep down the tendency. The monotony of the automatic reading has its parallel in automatic writing. In the writing at dictation for example it was usually possible for the operator to tell from the way a word was written whether or not it had been entirely non-voluntary. The dropping out of consciousness produced no change in the writing if it was already in the non-voluntary stage. But the presence or absence of the motor impulse made an enormous difference. The purely non-voluntary writing has a perfect ease and smoothness about it, and a perfect characterlessness. The change is not in the appearance of the writing, but in the hand movements. The pencil movements are more regular in speed, and unaccented, while in the voluntary movements the writing is more jerky.

For distracting attention, literature that is easily followed and emotional in character is by far the best. The advantage of the emotional element is, of course, simply its well-known hold upon the attention. But the need that it shall be something which does not demand a reaction from the intellect of the person is a subtler affair. The mechanism appears to be this, that when the idea cannot be grasped without a conscious effort to keep past facts in mind to compare with present, the attention is kept in a general condition of alertness, unfavorable to the complete neglect of any class of sensations. These general attitudes of attention are very hard to describe, but very interesting and very distinct. One of the most suggestive, for example, was this: We noticed on several occasions that if, for any reason, we had missed any portion of the story, and wanted to go back and read it over again, the doing this stopped the automatic writing. This curious effect we traced to a general feeling of 'keeping things in check' for a moment. The idea of stopping the reading and going back brought the feeling that things must be held in check until this back reading had been done; and this feeling of holding in check expressed itself in stopping the automatic writing, as the intense excitement and suspense did, save that there was no marked muscular tension here.

Anything which favored rapid changes of attention was unfavorable to keeping the attention off the experiment. Stories that moved along smoothly and quickly and called for no reaction but an emotional one were the most favorable. Any *stirring up* of the attention was likely to bring it back to the experiment.

General Summary.—How far now have we gone toward proving our general proposition? We may sum up the experiments by saying that a large number of acts ordinarily called intelligent, such as reading, writing, etc., can go on quite automatically in ordinary people. We have shown a general tendency, on the part of normal people, to *act*, without any express desire or conscious volition, in a manner in general accord with the *previous habits* of the person, and showing a full possession of the faculty of *memory*; and that these acts may go on just as well outside the field of consciousness; that for them, not only volition is unnecessary, but that consciousness as well is entirely superfluous and plays a purely cognitive part, when present. By consciousness we here mean, of course, 'empirical consciousness' or 'conscious consciousness,' as we have called it elsewhere. A possible split off consciousness is expressly excluded from consideration for the reasons given in the introduction.

That the second personality shows, in general, no abilities beyond this will, I think, be readily admitted. But it will be claimed that in exceptional cases the performances of the second personality involve something more—a real judgment and discrimination, or the keeping before the mind of an idea which is gradually elaborated.

Of course, it is not possible to enter into a complete discussion of the theory of these phenomena here. But a few words in defense of the main contention of our experiments will not be out of place. We must leave out at once all the alleged phenomena of spiritualism, as being still under dispute and being equally inexplicable on either of the two theories between which it is the purpose of these experiments to decide. Ruling these out there remains a small number of cases apparently not fully explained as automatic, if our experiments be taken as showing the limit of automatism. These cases may be divided into two groups. The first are those where the reactions seem to be rather too intelligent to involve nothing more than habit and memory. These need not offer much difficulty. Without a full knowledge of the *past history* of the patient, it is not possible to tell just where the limits of habit lie. There is opportunity for large individual difference here, and we must allow for it. What one person would have to think about, another may be so familiar with as to do quite without thought. It will usually be far more reasonable to suppose special habits for unusual cases than to fly in the face of all analogy and suppose a real second personality present. It must be remembered, too, that real unconsciousness is hard to prove.

Our observations on consciousness without memory show that in many cases the 'second personality' may be helped over a knotty point by flashes of primary personality, and exceptional cases would have to be examined from this standpoint before used to overthrow the automaton theory.

The other group embraces the cases that appear in connection with hystero-epilepsy and post-hypnotic suggestion. The peculiarity of these cases is that instead of one act forming the stimulus for the succeeding one—which would involve nothing but simple association—we have a dominant idea present which guides proceedings. This, of course, suggests the action of voluntary attention. It is like the man who is at work on a problem and voluntarily keeps the problem before his mind until the right associations have been called up by it. The difficulty presented by these cases disappears, however, as soon as we remember that here we have to do with an essentially new element—a *fixed idea*—either the subconscious fixed idea of hystero-epilepsy, or the apparently similar subconscious idea of post-hypnotic suggestion. The presence of these fully explains this apparently voluntary and actively attentive character of the acts without calling in any aid from the voluntary attention. The mechanism of these fixed ideas need not concern us. If it be held that they are kept before the mind by a split of will, this is a theory of fixed ideas, which would have to be considered on its own merits. Our problem is not involved in it essentially.

If, then, it be admitted that these experiments satisfactorily answer the question raised at the outset, if they really show a complete analogy between the performances of the second personality and the automatic acts of normal persons, what general view of hysteria do they suggest?

The answer is fairly obvious. It will be remembered that these phenomena occurred in us whenever the *attention* was removed from certain classes of sensations. Our problem was to get sufficient control of the attention to effect this removal of attention. In hysteria this removal of attention is effected by the anæsthesias of the subject. We *would* not, the histerique *can* not, attend to these sensations. Whatever else hysteria may be then, this, at least, seems most probable. It is a *disease* of the *attention*. An hysterical anæsthesia or paralysis is simply an inability to attend to sensations from this part. The second personality is simply the natural correlate of the anæsthesias, when these have become fixed. When they are variable, irregular subconscious acts form their correlate.

In closing it may be well to sum up a few of the more important generalizations from the work.

There are two kinds of attention, or two manifestations of it. One is *physiological* in its distribution, and determining what classes of sensations shall be brought into consciousness. Its failure means the dropping out of consciousness, for the time, of the particular group of sensations with regard to which it has failed. The other is distributed according to logical and associational elements. Its function is to establish associations among the different elements of consciousness, and to bring out the full meaning of sensations, etc. Its failure means loss of memory and failure of judgment, will, etc., but not loss of consciousness.

In all habitual acts, and acts involving nothing but simple memory, the function of the higher powers of the mind is inhibitive and controlling only, and not productive, for whenever, by failure of attention, the acts are removed from the influence of these controlling and inhibitory powers they go on just the same. Consciousness itself here appears to play a purely cognitive part.

The feeling of personality—that a given act is done by us—always disappears whenever our knowledge of the act is acquired purely by return sensations. Mere fore-knowledge alone is not enough to make the act seem personal; it must be the fore-knowledge or expectation represented by the group of feelings we have called, for convenience, the motor impulse. This motor impulse seems to introspection to be much more than a mere expectation in sensory terms. It seems to have a feeling background in it, entirely indescribable, in other terms, and perhaps representing a direct consciousness of a motor current from the higher centers to the lower.

The feeling of effort is not essential to self-consciousness. Its function seems to be to bring a center into a more responsive condition. It accompanies movements of voluntary attention apparently.

Hysteria is, at least, a disease of the attention. Its anæsthesias, etc., and their correlated subconscious acts represent the failure of the first kind of attention. The weakened memory and intellect, when it occurs, represents the failure of the second type.

ON THE CONDITIONS OF FATIGUE IN READING.[1]

BY HAROLD GRIFFING AND SHEPHERD IVORY FRANZ.

The increasing part played by reading in the life of civilized man is a striking characteristic of modern culture. In fact, the man of to-day might be defined as a reading animal. The result of this strain upon the eye has been the wide prevalence of myopia, astigmatism and kindred disorders. But the functions which the optic mechanism is called upon to perform are not abnormal; the work of the eye differs only in degree from that for which it is fitted. If the eye were never fatigued, myopia would be rare.

Yet great as is their importance, we have little exact knowledge of the conditions of minimum visual fatigue. Cohn[2], Javal[3] and Weber[4] have treated the subject with great fulness, but their work was largely theoretical. Cattell[5] and Sanford[6] have, however, investigated the subject experimentally, with special reference to the relative legibility of letters.

The conditions of visual fatigue are obviously highly complex. They may be divided into two classes. On the one hand, we have all those conditions which pertain to the individual reader; for example, the time of reading, the position of head and eyes, and personal peculiarities, anatomical and physiological. Opposed to these are certain purely physical conditions. Such are the size and quality of the type, the intensity and quality of the illumination, the color and quality of the paper,

[1] From the Psychological Laboratory of Columbia University. Read in condensed form before the International Congress of Psychology, Munich, August, 1896.

[2] Cohn, *The Hygiene of the Eye in Schools*, Eng. tr., London, 1886.

[3] Javal, *Annales d'Oculiste*, 79–82; *Revue Scientifique*, 1881.

[4] Weber, *Ueber die Augenuntersuchungen in den höheren Schulen zu Darmstadt*, *Referat erstattet d. grossherz. Ministerial, März*, 1881.

[5] Cattell, *Philosophische Studien*, III.

[6] Sanford, *American Journal of Psychology*, I.

the clearness of the printing, the length of the lines, and the spacing between the letters and lines. It is this latter group of conditions with which we are now concerned.

(1) The Size of the Type.

Weber investigated the relation of the size of type to legibility by finding the maximum rate of reading. He arrived at the paradoxical result that although the rate of reading decreased for very small type it also decreased when the height of letters was over 2 mm[1]. By determining the time of exposure required for perception Cattell[1] studied the legibility of small Latin letters of different sizes, .7, 1.1, 1.8, 2.5 and 5.8 mm.[2] The times found were 3, 1.4, 1.1, .7 and .6 for one observer, and 4, 1.7, 1.3, .9 and .7 for the other. The relation is approximately expressed by an hyperbolic curve.

The investigations of Cattell we have extended and supplemented by different methods. By the first method, which we will call the method of rapid reading, we found the rates at which an observer could read printed matter in large and small type. Two passages of the Bible, each containing 622 words, were used. One observer read one passage A, in large type, and another passage B, in small type, and the next observer read the same passages, reversing the order of the type, reading A in small type and B in large type. The order in which the experiments were made was also reversed for alternate observers. The time was taken by the observer with a stop watch, but recorded without his knowing the result by one of the writers. The observers were mostly students, five being familiar with experimental psychology. The type was Roman, *i. e.*, the ordinary type used in English books. The large type, of which we here give examples, was Pica, 1.8 mm. in height, the small, Pearl, .9 mm. in height. In addition to these experiments we made some in which the time of reading was constant, 1 minute, the number of words read being determined.

Below will be found the ratios of the times and of the number of words read.

[1] *Op. cit.*

[2] Not given by the writer, but calculated by us from other data given.

TABLE I.—RELATIVE TIMES FOR LARGE AND SMALL TYPE.

OBSERVER.	K	A	D_1	B	S	F_1	F_2	F_3	G	H_1	H_2	D_2	AV.
$\frac{T_L}{T_s}$	.77	1.04	.82	.61	.90	.88	.72	.—	1.08	.96	1.01	.92	.90
$\frac{W_s}{W_L}$	.88	1.00	—	—	.91	.42	.65	.94	.80	—	—	—	

$\frac{T_L}{T_s}$ = ratio of time required to read large type to that required to read small type.

$\frac{W_s}{W_L}$ = ratio of number of words read in one minute in small type to the number read in large type.

In a few additional experiments the observers read at their natural rates. The resulting ratios $\frac{T_L}{T_S}$ for 4 observers were .87, 1.00, .86 and .81, the average .89, being the same practically as that obtained by the other method.

Thus it takes on the average about $\frac{9}{10}$ as much time to read large type, 1.8 mm., as to read small type, .9 mm. The difference in legibility would probably be much greater were it not that when the small type is read more words can be seen simultaneously. In this way we may explain Weber's paradoxical result. As the size of the letters increases beyond a certain limit the rate of reading will necessarily decrease; but this does not involve an increase of fatigue, as Weber assumed.

By a second method we found the relative number of words seen when exposed for $\frac{1}{20}$ sec. by Cattell's gravity chronometer.[1] Phrases of three and four words were pasted on white strips of cardboard and were shown for the time desired by a falling screen. The greater part of the screen was hidden from the view of the observer by a black sheet of paper with an opening where the letters were to appear. The phrases were cut from the books mentioned, the letters being 1.8 and .9 mm. high. None of the words were of more that two syllables. The same phrases were used for large and small type. There were 54 phrases of 3 words and 54 of 4 words, half in large type and half in small. Thus there were 216 + 162 words in all.

[1] For description of the instrument see *op. cit.*

The experiment was conducted as follows: The observer took his seat in a comfortable chair opposite the instrument and placed his chin upon a rest suitably adjusted, so that his eyes were slightly above the level of the letters exposed, and 30 cm. distant from them. The experimenter (one of the writers) stood behind the instrument so as to adjust the cards with the phrases. When the card was placed the observer fixated a gray cross on the black background of the movable screen directly in front of the letters, and let the screen fall by breaking the current with a Morse key. He then wrote down what he thought he had seen. A dozen or more practice trials were made before beginning the experiments proper. The observer was, of course, ignorant of the phrases that were to be given. Care was taken not to have a phrase already given in one type repeated immediately in another. Of eleven observers six completed only half of the series. We give below the results for the different observers.

TABLE II.—PERCENTAGES OF WORDS SEEN; LARGE AND SMALL TYPE.

	Three-Word Phrases.			Four-Word Phrases.		
Observer.	S.	L.	$\frac{S}{L}=\lambda$.	S.	L.	$\frac{S}{L}=\lambda$.
H.	.22	.56	.39	.13	.44	.29
C.	.46	.75	.61	.59	.75	.79
T. G.	.29	.75	.39	.23	.60	.38
I. F.	.60	.95	.63	.80	.88	.88
H. G.	.46	.81	.56	.66	.96	.69
P.	.46	.91	.50	.45	.85	.53
L.	.10	.54	.18	.18	.32	.56
R. G.	.76	.79	.96	.48	.68	.70
S.	.12	.47	.25	.12	.39	.31
A.	.68	.78	.87	.55	.69	.79
S. F.	.43	.85	.51	.59	.81	.73
Average			.53			.60

Vertical columns S and L give percentages of words seen for small and large type (.9 and 1.8 mm. high).

Vertical column $\frac{S}{L}$ give ratios in per cent. or the relative legibility λ of small and large type.

With the observers whose initials are given in block type the full set of experiments (108) were made, only 39 being made on the others.

In taking the average the values of λ for these five might be weighted. This would change the averages somewhat.

From the above table we see that on the average but little more than one half as many words were seen in small type as in large type. Individual variations are great, but these variations are probably not due to an appreciable extent to individual differences in the relative legibility of large and small type. For good observers the same difference in legibility would give different values of λ.

This theoretical conclusion is verified by the experiments. By arranging the observers in two groups according to the percentages seen, the values of λ is for the better observers in all cases lower than that of any of the four poorest observers.

A few experiments were made with 21 two-word phrases printed in very large type (4+ mm). The percentages of words seen correctly by three observers, together with the averages of the same observers for 1.8 mm. type as found from the table above given are as follows:

	Large	Very large
P.	.88	.93
L.	.43	.64
S.	.43	.70

Thus the legibility as shown by this method appears to increase regularly with the size. But since the number of words brought within the field of distinct vision decrease with the size, the relation is quite complex.

A few phrases (15) of two words each were used with the others. The percentages for two, three and four-word combinations were found to vary but little with the number of words.

From the table it will be seen that the values of λ were about the same for phrases of three words as for those of four words, the averages for phrases of 2, 3 and 4 words in small type being .42, .41 and .43.

In the above experiments the paper was not exactly the same for large and small type, being slightly grayish for the small type and of a more yellowish tint for the large. To eliminate this source of error, phrases of four words in large and small type were printed on the same white paper. From 200 experiments (800 words), 100 on S. F. and 100 on H., we found the following percentages of words seen:

	S	L	$\frac{S}{L}=\lambda$
H.	.12	.32	.37
S. F.	.83	.90	.92

The values of λ correspond quite closely with those previously found for the same observers, .88 for F. and .29 for H.

A modification of the preceding method was used by determining the time words composed of letters of different sizes had to be exposed in order to be seen. This we will call the time of exposure method. The same apparatus was used as before, the time of exposure varying with the extent of opening of the screen. This time can be determined to about .15σ, σ being .001 sec. The words were of not less than 5 letters, nor over 2 syllables, on white paper. The type, as here shown, was six point and 'eleven point,' .8 and 1.6 mm. high. On account of the preliminary practice necessary there were but three observers, two being the writers. The experiments were conducted in the same general way as those just described. The experimenter tried first very small times, increasing the time until the stimulus was perceived approximately 50% of the time. Then other words were shown which the observer had not seen. As the percentage seen tends to increase very rapidly from 0 to 100 (theoretically 99+), it was generally easy to determine at one sitting the time required either directly or by estimation from the percentage seen. The times of exposure found thus are now given in thousandths of a second.

TABLE III.—TIMES OF EXPOSURE FOR DIFFERENT SIZES OF TYPE TO BE SEEN.

OBSERVER.	L.	S.	$\frac{L}{S}=\lambda$
G.	1.6	1.9	.84
F	1.1	1.5	.73
"	1.3	1.7	.76
H	2.0	2.8	.71
"	1.6	2.5	.64
AV.			.73

L and S denote the times of exposure necessary for large and small type respectively, .8 and 1.6 mm.

$\frac{L}{S}$ or λ is the relative legibilty measured by this method.

The two values of L and S for F and H are for different days. The time of exposure seems to vary in the same individual.

From the above results it appears that the large type, 1.6 mm., requires about ¾ as great a time of exposure as the small type of half the height, .8 mm.

In the last two sets of experiments a few observations were made, which though not bearing on the special problems under investigation are yet of psychological interest. Observers generally failed to see any of the letters making up a word when they failed to perceive the whole word. There were, however, individual differences, some persons often seeing one or two letters only. At times an observer saw combinations without sense, though he knew such combinations were not given. In the time-of-exposure experiments the observer was at times conscious of perceiving letters without knowing what they were. Occasionally the observer had an impression that a given word was present, when the letters had not appeared distinctly. More often some letters were distinct, and he guessed the word, or else the whole word was distinct. One of the writers had a marked tendency to see again what had been given before, even when he knew that the word was not repeated. One of the observers, H., seemed to be an exception to the rule that one sees all letters exposed or none at all except within very small range of time. Some days it was very difficult to find the time required for this reason. But perhaps the most important phenomenon observed was the illusory perception of a word, the letters appearing distinct when not present. This has been already noted by Cattell and also by Münsterberg. The theoretical importance of this lies in the support which it gives to the hallucination theory of perception. The representative processes in perception seem to attain to the sensory vividness of true hallucinations. This does not, however, appear to take place in every instance, for F. seemed at times to see some of the letters and to infer by ordinary processes of association that a certain word was present.

To obtain more extended results and confirm those obtained by Cattell, by the time-of-exposure method, we determined the intensity of illumination necessary for the reading of letters of different sizes. The letters were printed in the simplest kind of type, commonly called Block. Two cards were, however, covered with words in Roman type, .8 and 1.6 mm. in height.

The observer sat in front of a stand from a projecting piece of which was suspended a small pendulum making a vibration in ½ sec. The pendulum swung in front of a screen having an opening where the letters to be seen appeared. The letters were, of course, shown ½ sec. The letters were posted on cardboard strips and these were placed in slits. The paper was the same for the different sizes, pure white. The slits were arranged so that the length of the cardboard exposed was either 15 or 3 mm., according to the size of the letters. For the two largest sizes, and also for the cards on which the words in Roman type were shown, the large area was used. The object was to show only one or two letters at a time, except when the Roman type was used, when a larger number was seen. A black screen in front of the pendulum with the necessary opening served to prevent distraction of the observer by the movement of the pendulum.

The observer's eyes were kept at a constant distance (30 cm.) from the stimulus by means of a chin rest. The light was that of a hooded petroleum lamp found to be fairly constant, shining through a square of ground glass 5 x 5 mm. The light emitted was approximately .02 candle power. The lamp was in a movable box sliding on wheels in iron grooves. Precautions were taken to avoid errors from reflected or diffused light. The letters used were in combinations of one to four words in one horizontal line. They were taken from a printer's sample book. The median plane of the observer was approximately perpendicular to the plane of the cardboard to be seen, and the lamp could be moved only in a straight line, making an angle of 45° with the plane of the cardboard.

With this apparatus after the observer had remained in the dark room long enough to avoid errors from adaptation (20 to 30 min.), the experiment was made as follows: A card with letters to be exposed was placed in the slit by the experimenter (one of the writers). The observer pushed back the pendulum to a fixed support with his hand, fixated a pencil cross on the cardboard piece fastened to the pendulum directly in front of the letters to be seen, and then let the pendulum swing forward, observing the letters as they were shown. As the pendulum swung back it was caught by the observer with the left hand and fixed with a catch. He then moved the lamp nearer with the right hand. At first this was done by the experimenter, but with less convenience and economy of time. This was repeated until the observer was quite certain he could perceive the letters correctly when exposed but once. The distance of the light from the letters was then read off on a scale. The square of the reciprocal of this distance represents the relative intensity of the illumination. The readings were, of course, taken by the experimenter. For this purpose we used the light from a small candle inside a blackened box shining through a cylindrical tube. Two or three determinations were generally made at one sitting for each of the variables under investigation, including several in addition to the type. Variations in the results made it necessary to average the records of some days separately, as given in the second horizontal columns for F and H.

We give below the average values of T, the illumination threshold[1] for reading in terms of one candle-meter (C.M.), or the light of a standard candle at a perpendicular distance of one meter.

TABLE IV.—ILLUMINATION THRESHOLDS FOR DIFFERENT SIZES OF TYPE.

OBSERVER.	N	H = .9		H = 1.6		H = 3.1		H = 6.0		h = .8		h = 1.6	
		Av	MV	Av	MV	Av	MV	Av	MV	Av	MV	Av	MV
G	10	27	.02	.12	.01	.042	.003	.014	.001	.36	.04	.14	.01
F	6	.24	.02	.08	.01	.028	.007	.010	.001	.22	.02	.12	.02
"	3	.17	.03	.045	.004	.018	.002	.008	.001	.13	.01	.05	.00
H	5	.077	.014	.035	.007	.014	.001	.003	.000	.19	.01	.07	.00
"	3	.19	.02	.09	.003	.043	.003	.009	.001	.35	.03	.13	.02

H = height of Gothic letters in mm.
h = height of Roman letters in mm.
N = number of determinations upon which average is based.
Av = average.
MV = mean variation.

[1] Calculated by the formula $T=\frac{\lambda \cos\theta}{d^2}$ where λ is the candle power of the light, d the distance of the light from the object, and θ the angle made by the normal to the surface.

A graphical representation of the results is shown in the accompanying figure. The ordinates give the intensity of illumination in candle-meters, and the abscissas the height of the letters in tenths of millimeters.

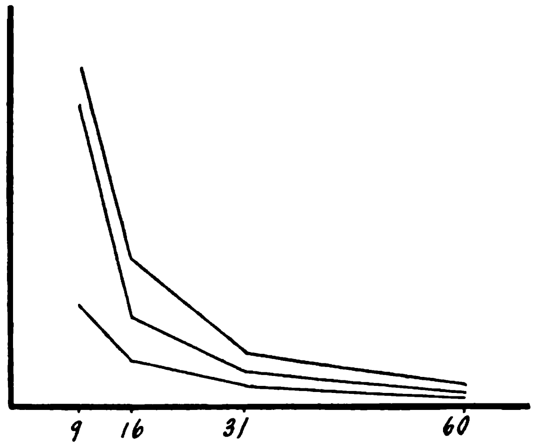

Fig. 1.

The curves resemble rectangular hyperbolas, the values of the variables corresponding roughly to the equation,

$$(s-k)\,i=k_1,$$

k and k_1, being constants depending upon the individual. Assuming such an equation we may infer that after the size of the type has reached a certain limit the increase of size is in direct proportion to the decrease of illumination. The fatigue coefficient increases slowly until the size of the type decreases to about 2–3 mm., after which its increase is more and more rapid. The lowest limit to the size of type in common use should be 1.5 mm. The same conclusion may be drawn from the experiments of Cattell already mentioned.

(2) The Quality of the Type.

On theoretical grounds it may be assumed that the legibility of letters decreases with increasing complexity of structure. From this point of view German type is open to serious criticism, and even our Roman type might evidently be much improved. Some of our letters have unnecessary features and they are as a rule much more complex in structure than those printed in the so-called Block type. Many letters, such as c and e, are easily confused, and there are decided differences ot legibility. These differences are, indeed, slight and difficult to determine. By finding the percentage of times each letter was seen when exposed for 1σ, more or less, Cattell[1] found the order of legibility of the small letters to be: d k m q h b p w u l j t v z r o f n a x y e i g c s. There seemed to be, however, individual differences. Sanford[1] by a different method found a somewhat different order of legibility.

In the writers' experiments, which were made only by the most delicate method, that of the illumination threshold, the following styles of type were used: Roman (small letters), that used universally in England, America and southern Europe for books and newspapers; German, or that used in Germany; Block, in which the letters are of uniform thickness and of the simplest shape, much like Roman capitals. Two styles of Block were used, as here shown; in one the letters being quite THICK, .5 mm., whereas in the OTHER they were .15 mm. Besides the ordinary Roman letters there were two other sets in semi-Roman type; one, **Roman II.**, having very thick and very thin lines, .05 to .5 mm.; the other, Roman III., being somewhat like the plainer Block and of uniform thickness, about .2 mm. The size of the letters was practically constant for the different groups, 1.5 mm. in height, there being, however, slight variations, to .1 mm. in the individual letters.

We give now the results in tabular form. The figures mean the same as in Table IV. The results for F. and H. are given in 2 columns on account of a variation in sensibility which made it necessary to average the results of the earlier experiments separately.

[1] *Op. cit.*

TABLE V.—ILLUMINATION THRESHOLD FOR DIFFERENT KINDS OF TYPE.

		Roman I.		Roman II.		Roman III.		German		Block thin.		Block thick.	
Observer.	N	AV	M V	AV	M V	AV	M V	AV	M V	AV	M V	AV	M V
G.	10	.22	.02	.12	.02	.18	.02	.21	.03	.20	.02	.09	.01
F.	6	.14	.01	.12	.02	.13	.01	.15	.02	.13	.01	.07	.01
"	3	.11	.01	.06	.001	.08	.00	.12	.01	.10	.00	.06	.00
H.	5	.10	.01	.06	.01	.12	.02	.12	.02	.10	.02	.04	.00
"	3	.22	.03	.15	.03	.22	.02	.24	.02	.22	.01	.10	.00

From the above table we may calculate the relative legibility λ of the different styles of type; λ of course being the reciprocal of the illumination threshhold given above.

The values of λ are now given.

TABLE VI.—RELATIVE LEGIBILITY OF TYPE, THAT OF ROMAN BEING I.

Observer.	Roman II.	Roman III.	German.	Block thin.	Block thick.
G.	1.8	1.2	1.0	1.1	2.4
F.	1.1	1.1	.9	1.1	2.0
"	1.8	1.4	.9	1.1	1.8
H.	1.7	.8	.8	1.0	2.5
"	1.5	1.0	.9	1.0	2.2
Av	1.6	1.1	.9	1.1	2.2

From the above we see that, contrary to our expectation, the difference in legibility between Roman and German type is relatively slight. Thin hair lines, if accompanied by thick lines, do not seem to diminish the legibility, Roman II. requiring nearly half as much light as Roman I. The complexity of the letters, within the limits here studied, does not seem to have decided effect on the legibility, for the value of λ for thin Block is about the same as for Roman. The greater legibility of Block type is due almost entirely to the thickness of the letters, as shown by these experiments. On the other hand, if a part of the letter is thick it is quite legible, even though thin hair lines are frequent. It is, however, probable that type such as Roman II. is more fatiguing than the results indicate. It may be possible for the mind to perceive certain objects with fatigue when other objects are either perceived without appreciable fatigue, or not perceived at all.

(3) The Distance Between the Letters and Lines.

The horizontal distance between the letters has been said by Javal to be of great importance. Certainly a word printed so that these distances is increased .8 to 1.3 mm. appears to be much more distinct. The effect of an increase in this s p a c i n g is here shown. But twelve experiments on three observers by the illumination threshold method gave negative results. We must conclude then that the spacing commonly used is quite sufficient. Greater spacing would, of course, be more expensive, and the decrease of fatigue not as great as might be brought about in other ways.

As regards the vertical space between the lines, technically called 'leading,' a slight effect on legibility was found when the distance with Pearl type, .8 mm. high, was increased from .8 to 1.3 mm. The illumination threshold method was used, and the experiments carried on simultaneously with the preceding. The following results were obtained:

Table VII.—Illumination Threshold for Type Leaded and not Leaded.

	.8 mm.		1.3 mm.		λ
	Av	MV	Av	MV	
G.	.40	.03	.36	.04	.90
F.	.25	.01	.22	.02	.88
"	.12	.01	.12	.00	1.00
H	.12	.01	.09	.01	.75
"	.35	.02	.27	.01	.77
Av					.86

Thus the average relative legibility of unleaded type to leaded type, as measured in this way, is about .9.

(4) The Intensity of Illumination.

Although the variation in the intensity of diffused daylight in a well lighted room is known to be very great, even when the other conditions such as time and place are constant, being roughly from 50 to 1500 candle-meters,[1] no results were obtained for variations in legibility due to this variation by the two gravity chronometer methods and the method of rapid reading. The problem is, however, difficult to investigate in this way by reason of the marked daily variations in individual sensibility. It was necessary, therefore, to use artificial light of low intensity. The relation of the intensity and the legibility under these conditions has already been studied by Cattell by the time of exposure method. Using the light of a petroleum lamp (about 10 candle power) 18 cm. distant at an angle of 55° as the unit of illumination, *i. e.*, about 260 c. m., the times of exposure for the intensities 1, $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{64}$, $\frac{1}{256}$, were 1.4σ, 1.7, 2.5, 6. and 20.

In order to supplement the work of Cattell we determined the maximum rate of reading for different intensities of illumination. It had already been found by Weber[2] that for low intensities the rate of reading varies with the illumination. Our experiments were made in the following manner: The book to be read, the Pearl type Bible already mentioned, was fastened on a wooden stand so as to be in front of the observer, and making an angle of 45° with the rays of light. The light used was a standard candle placed inside a blackened box. The conditions were such that the light came from behind the observer and to his left. The observer read one column as fast as possible, recording the time with the stop watch. With the lowest intensity, however, on account of fatigue, but half a column was read, the time being doubled for the whole column, The observer, it should be added, remained in a dark room long enough to avoid errors from adaptation. The experiments were made on one day by each observer. Below are given the results in seconds for the different distances in meters and the relative intensities in candle-meters.

[1] Cohn, *op. cit.*
[2] Weber, *op. cit.*

TABLE VIII.—TIME OF READING AT DIFFERENT INTENSITIES.

OBSERVER.	DAY-LIGHT.	¼ M. 11.2 C. M.	½ M. 2.8 C. M.	1 M. .7 C. M.	1½ M. .35 C. M.	2 M. .17 C. M.
H G	35	36	36	46	63	110
K	45	44	39	53	83	120
F	47	51	52	69	100	170
G	47	49	59	72	130	—
S	29	29	35	48	—	—

In these experiments the rate of reading does not appear to be appreciably affected by a decrease of illumination within a very wide range, the intensity of good daylight being about 500 times as bright as the lowest intensity here used, with which the rate of reading was not appreciably increased. We conclude then that within wide limits such as those of ordinary daylight variation in the intensity of illumination is not attended by great fatigue. But when the illumination decreases to a certain point, not far from 3 C. M., the fatigue becomes excessive. This is shown by the fact that very slight differences in the rate of reading are caused by conditions of great fatigue, an increase of about $\frac{1}{8}$ in the time of reading corresponding to decrease in the illumination threshold of 70 per cent.

The above experiments correspond quite well with those of Cattell by the time-of-exposure method. His results show that the fatigue coefficient increases very rapidly as the illumination decreases below approximately 4 C. M. His experiments also show that the fatigue coefficient is appreciably greater for the lamp light, about 250 C. M., than for daylight, and that it increases as the illumination is further decreased.

(5) THE QUALITY OF THE ILLUMINATION.

The use of artificial light has long been recognized as an important cause of visual fatigue. This fatigue may be partly ascribed to the conditions of intensity, the light of a good petroleum lamp at convenient reading distance being less than that of good daylight. We, therefore, tested the effect of artificial light of high intensity by using the light from an incandescent Welsbach gas burner giving clear, white light, 35 candle power at 25 C. M. and 45°, about 400 C. M. The times of exposure required for perception by the writers were found to be as given below.

	Small Type.		Large Type.	
	Welsbach.	Daylight.	Welsbach.	Daylight.
G	1.8	1.9	—	—
F	1.1	1.5 1.7	.8	1.1 1.3

These values are thus smaller, rather than larger, than those already found for daylight. We must suppose the decrease in time to be due to daily variations. The above measurements were made on one day, and the perceptive and retinal processes of F were more than usually delicate. The smallest time found .8σ, is about as small as any found by Cattell in all his experiments. It is evident, therefore, that with sufficient intensity of white artificial light the legibility of printed matter may be as great as in good daylight.

Gas light and lamplight have, in addition to their frequent unsteadiness, the disadvantage of a yellow color. Since, as will be seen later, yellow paper is unfavorable for reading, yellow light causing the paper to appear yellow must also be a source of fatigue.

(6) The Quality of the Paper.

If the paper used reflects very little light and is of such a quality that letters can be well printed, the exact hue is probably of little importance, provided a large quantity of light be diffused. But if the absorption be so great that the paper appears grayish, letters printed on it will not be so legible by reason of the lessening of the contrast between the letters and the background.

In experiments made by the different methods already described we used non-reflecting clear white paper and gray paper, technically called news-paper, the same as that used by many newspapers, only slightly darker. By the color-wheel method it was found that the white paper used had to have 30 per cent. black mixed with it to give a gray corresponding to this. Its relative luminosity was therefore about .70. Specimens of red and yellow paper were also used, the red corresponding to the spectrum color just to the left of Frauenhofer's line C, and the yellow that to the right of line D ($\frac{1}{6}$ of the distance to line E).

Experiments by the method of the percentage of words seen on one observer with 11-point type gave negative results, the percentages of words seen out of 150 being 32 per cent. and and 31 per cent., the same for white paper as for the newspaper. Of small type words, 6-point, given at the same time, the same observer, H., saw but 12 per cent.

By the time-of-exposure method, however, different results were obtained. Below are the times found for two observers.

	White.	News.	Yellow.	Red.
G.	2.8	4.0	4.0	—
F.	1.2	1.7	2.5	4.0

Thus the time of exposure is considerably longer for gray tinted paper, as well as red and yellow paper, than for white. The explanation of the greater legibility of the letters on white paper over those on the red and yellow is the same as for the gray. Color quality is not independent of intensity, white being essentially brighter than yellow, which in turn is brighter than red.

The illumination method was also applied to the study of the fatigue effect of white paper and gray newspaper. The letters were not read independently in these experiments, but in words. Upon the paper exposed were 10 to 12 words in 3 lines.

The values of the illumination threshold were as follows:

TABLE IX.—ILLUMINATION THRESHOLDS FOR WHITE AND GRAY PAPER.

OBSERVER.	G		F		F_1		H		H_1	
	Av	MV	Av	MV	Av	MV	Av	MV	Av	MV
W = White	.10	.01	.10	.01	.06	.01	.04	.00	.10	.02
N = News	.20	.02	.16	.02	.08	.01	.07	.00	.23	.01
$\frac{W}{N} = \lambda$	.50		.62		.75		.57		.43	

According to these results the gray tinted newspaper required about twice as much illumination as the white. This is somewhat more than might be expected from the relative absorption powers of the papers, but the quality of the printing varies with the paper, not being quite so clear on the newspaper.

Summarizing briefly our results we conclude that the size of type is the all important condition of visual fatigue. No type less than 1.5 mm. in height, that in which this article is printed (eleven point), should ever be used, the fatigue increasing rapidly even before the size becomes as small as this. The intensity of illumination is apparently of little consequence within the limits of daylight in well lighted rooms. Very low intensities, less than from 3 to 10 candle-meters, are sources of even greater fatigue than small type, and 100 C. M. may be considered a safe limit. Yet the illumination in German school rooms has been found to be frequently less than 2 C. M. White light rather than yellow light should be used for artificial illumination. The form of the type is of less importance than the thickness of the letters. White paper should be used, though it is possible that the greater amount of light reflected from pure white paper may cause some fatigue. Additional 'leading' or spacing between the lines, is also desirable.

THE ACCURACY OF OBSERVATION AND OF RECOLLECTION IN SCHOOL CHILDREN.[1]

BY SHEPHERD IVORY FRANZ AND HENRY E. HOUSTON.

Whether accuracy of observation and of recollection differs at different periods of our lives is a problem suggested by Prof. Cattell's paper on this subject.[2] In order to study this subject, questions similar to those used by Prof. Cattell and by Mr. Bolton, with the changes necessary for time and place and for the age of the scholars, were asked the pupils of the Horace Mann School, New York City, and of the Paterson, N. J., High School.

The following were the questions used: (1) What was the weather a week ago to-day? (2) Two weeks ago? (3) Which way do the seeds in an apple point? (4) How many years ago did George Washington die? (5) How many feet is it from the schoolhouse door to the corner of the street? (6) How many seconds does it take you to walk this distance? (7) How many times have you entered the schoolhouse gate (or door) since vacation? (8) How many ounces does this book (showing a text-book used by the class) weigh? (9) Draw on a scale of one inch to twenty feet, a ground plan of the lower hall.

The accompanying Table[3] gives the percentages of correct answers or the average estimation together with the average residual for the two schools.

[1] From the Psychological Laboratory of Columbia University.

[2] The Accuracy of Recollection, J. McKeen Cattell, *Science*, N. S., II., 761–766, 1895. See also The Accuracy of Recollection and Observation, F. E. Bolton, PSYCHOL. REV., III., 286–295, 1896.

[3] Owing to the fewness of answers in some grades it was thought best to combine the several grades of the H. M. S. as follows: I., II., III., IV., V., VI., VII., VIII., High, thus making about forty or fifty answers in each group.

The figures in the Table marked with a cross (†) denote the actual magnitude as used for the Columbia and Wisconsin Students.

As the books used as standards of weight were of different weights, we

Table I.

	Actual M'gn't'de.	H.M.S. I.II.III.	H.M.S. IV.V.VI.	H.M.S. VII.VIII.	H.M.S. High.	H.M.S. Total.	P.H.S. I.II.III.	Columbia.	Wisconsin.
Age.		7–9.	10–12	13–14	14–17	7–17	14–17	—	—
No. of Answers.		56	63	48	34	201	325	56	92
Weather, 1 wk. previous.	H. M. S. clear. P. H. S. cloudy.	40%	81%	95%	85%	78%	4%	11% stormy clearing.	32%(?) stormy.
Weather. 2 wks. previous.	H. M. S. clear. P. H. S. stormy.	34%	49%	65%	65%	53%	29%	— —	— —
Direction of Apple Seeds.	H. M. S. P. H. S. =	51%	52%	26%	51%	45%	49%	41%	49%
Yrs. since W's death. Av. Est. Av. Res.	H. M. S. P. H. S. 96.	97 54	87 33	97 12	99 8	95 26	102 13	— —	— —
Distance in feet. Av. Est. Av. Res.	H. M. S. 400 P. H. S. 260	160 120	183 150	167 74	226 93	181 118	197 97	356 179 [310]	276 — [450]
Time in seconds. Av. Est. Av. Res.	H. M. S. 80 P. H. S. 55	65 45	82 52	97 61	97 49	84 54	70 45	66 36 [35]	182 — [160]
Frequency. Av. Est. Av. Res.	H. M. S. 100* P. H. S. 180	179 162	252 185	122 38	152 76	183 131	452 314	4022 2669 [?]	— —
Weight in Ounces. Av. Est. Av. Res.	H. M. S. 10 P. H. S. 14	7.8 4.5	7.6 4.1	6.5 2.4	6.0 2.4	7.1 3.5	12 5.5	17 5 [24]	20.5 — [24]
Proportion, Width, Length. Av. Est. Av. Res.	H. M. S. 10.2 P. H. S. 1.74	— —	8.7 3.9	7.8 2.5	8.5 3.6	8.3 3.	1.14 .30	— —	1.7 — [2.0]
Length in mm. Av. Est. Av. Res.	H. M. S. 211. P. H. S. 118	— —	116 39	145 35	158 29	141 37	105 —	— —	6. — [9.6 in]
Width in mm. Av. Est. Av. Res.	H. M. S. 13. P. H. S. 16	— —	15 6	21 8	23 8.7	19 7.8	87 —	— —	3.5 in. — [4.7 in]

Taking the figures more in detail, it will first be noted that the H. M. S. has a much larger percentage of correct answers to the two weather questions than any of the other schools. This is no doubt due to the fact that the weather on the two days about which the pupils were asked was 'clear,' and as we have more clear days than other kinds we should expect an increase according to the probability. Not knowing the probability of this and the other kinds of weather, we cannot compare the other schools, but considering the H. M. S, alone it seems likely that accuracy of recollection increased with age.

In the next question, however, this is not the case, for the younger scholars in the H. M. S. had the same percentage correct as the older, and a trifle greater percentage than the College students. Some chance variation caused a decrease to 5 per cent. in the seventh grade, whence the total for that group (VII., VIII.) was reduced to 26 per cent.

In the quantitative estimations it will be noticed that, like the College students, the younger children underestimate weight and size (proportion) and overestimate time. They also overestimate frequency and with the Wisconsin students underestimate distance and size (length of building). The H. M. S. and the P. H. S. overestimated the breadth of the hall or building, while the Wisconsin students underestimated the corresponding magnitude. In these estimations, however, there seems to be no regular increase or decrease in accuracy, except in the cases of 'weight,' 'length,' 'width,' and 'time.' Taken as a whole, however, the older scholars are more accurate than the younger. This is shown, also by the average residuals, which for the

have here reduced the estimations, taking ten ounces as a standard. The validity of this procedure is somewhat doubtful, but it was necessary in order to make any comparison of the grades. We, however, give here the actual magnitudes, the average estimations, and the residuals for the several grades.

TABLE IA.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	High.	
Magnitude.	—	12.5	10.5	10.5	13.5	10.5	14.	19.5	18.	
									I.	II.
Av. Est.	—	12.5	6.	8.7	8.7	7.8	10.	11.	10.4	12.
Av. Res.	—	5.	4.5	5.4	5.	3.7	4.	3.4	3.7	3.6

older scholars are considerably smaller than for the younger. The questions are so complex in themselves, all including observation, with errors of judgment, and memory with its errors, that no general conclusion can be drawn.

Accuracy according to Sex. From the following Table showing the percentage of right answers and the average esti-

TABLE II.

	H. M. S.		P. H. S.		WISCONSIN.	
	BOYS.	GIRLS.	BOYS.	GIRLS.	BOYS.	GIRLS.
Weather, 1st wk	74%	81%			19%	54%
Weather, 2d wk	49%	57%			—	—
Apple seed	48	43			50% [only part]	46%
Yrs. since W.'s death	95 (96)	91	89. (96)	102.	—	—
Distance	231 (400)	151	189 (260)	196	296 (450)	261
Time	72 (80)	90	46. (55)	67.	177 (160)	187
Frequency	191 (100*)	178	505 (180)	468	—	—
Weight	7.8 (10)	6.7	11. (14)	12.	22.8 (24)	19.8
Proportion	9.7 (10.2)	7.0	1.26 (1.74)	1.08	—	—

N. B.—The actual magnitudes are shown in parentheses.

mations for the H. M. S., the P. H. S. and the Wisconsin students. One sees that the girls remember the weather better than the boys, but that the estimations of the boys for distance, time and proportion are nearer the standard. The boys in the H. M. S. came nearer to the date of Washington's death, while the boys and girls of the P. H. S. were about equally correct. With weight the H. M. S. boys again came nearer, while the girls of the P. H. S. were more exact. With frequency the girls in both cases were more correct. The general conclusion to be drawn is that in quantitative measurements the boys are more exact. This is also what Mr. Bolton found with the Wisconsin students.

Relation of Confidence to Accuracy. When the students were asked the questions they were told to denote by the letters A, B, C or D, respectively, whether they were sure their answers were correct, fairly confident, doubtful, or if their answers were only a guess. The following table gives the average estimation when the students were confident (A and B), and when they were doubtful (C and D).

TABLE III.

		Yrs. since W's death.	Weight in Oz.	Distance in Ft.	Occurrence.	Time in Seconds.
H. M. S.	A. and B.	88.5		152	205	91
	C. and D.	138.		285	213	101
		(96)	(10)	(400)	(100)	(75)
P. H. S.	A. and B.	100	12	203	386	
	C. and D.	104	11.5	214	475	
		(96)	(14)	(160)	(180)	(55)

Here, too, the evidence is conflicting and no general conclusion can be drawn. In the estimation for years since W's death, and for number of occurrences the more confident answers are nearer the truth. When we look at the estimation for distance, however, we see that the two schools disagree. The small difference, too, between the estimates in some cases (*e. g.*, years P. H. S., distance P. H. S., occurrence H. M. S.) together with a large variation (in most cases one-third of the average estimation) makes it unwise to hazard any conclusion.

It was found that scholarships did not at all influence the results. Those classed as the best students estimated as wildly as those considered the worst; those considered as of medium ability were a little more accurate than the two extremes.

DISCUSSION AND REPORTS.

REMARKS ON PROFESSOR LLOYD MORGAN'S METHOD IN ANIMAL PSYCHOLOGY.

The method of animal psychology has generally been so indefinite and uncritical, and the results so unsatisfactory, that many psychologists must often have felt that it is quite premature to enter the field at all till we have some clearer basis in a knowledge of what lies nearer us—human psychology. However this may be, there are certainly now a number of able investigators in this province, and not the least of these is Professor Lloyd Morgan. In these remarks I wish to take up certain points made by him in the suggestive book entitled 'An Introdution to Comparative Psychology.'

In the first place it is an obvious remark that the proper method of experimenting on animal intelligence should not be an exciting one, as disturbing to cool deliberate action. It is curious that Professor Morgan acknowledges this (p. 260) and yet brings up the instance there cited, the bone-swinging experiment, as evidence against perception of relations. We may criticise in the same way those experiments upon which he lays great stress as evidence against the perception of relations, namely, the fox terrier pup carrying a stick through railings, that here the activity is of too exciting a nature to be favorable to intelligent adaptation. Yet it may be urged that even herein that the dog is constantly changing his grip, there is evidence that he perceives the necessity of another way than the present. An unchanging stubborn bull dog hold would be less intelligent. The method of trial and error is a real method and is learned to be such, and with a consciousness that his present hold is a bad one he shifts his grip. There is for him a *how* but it is *any how*. We would suggest that young children be tested with the stick-railing experiment. But for the testing perception of space relation we think the spectacle of a cat on a wet day, looking down from a high point before it jumps and makes its way to the house is more suggestive. Here is an opportunity for cool deliberate inspection and comparison, and the cat appears to do this. It seems to pause and judge distance with reference to its ability at jumping, to estimate the shortest path to its destination, and the relative wetness of different ways. It passes its eye from point to point, and picks its way; and it gives the signs of perception of space relations so plainly that if we saw similar action in a man we should unhesitatingly ascribe such consciousness to him.

With respect to the perception of relations, Professor Morgan adduces on the negative side an observation which illustrates to a certain degree an exciting method, but also the most common fault, an incomplete study of facts. I allude to the story (p. 301) of a dog which after repeatedly chasing a rabbit in vain, the rabbit escaping in a drain, at length made straight for the drain and headed off the rabbit. Upon this brief account two theories of action at once suggest themselves, first (Wilson), the dog may have consciously taken the shortest cut to head off the rabbit, second (Morgan), the dog in following the rabbit, fast disappearing toward the well-known drain, has the association of rabbit and drain at length so predominant that he follows this line of vision—straight line—at once to the drain. In this last case the idea of rabbit entering drain becomes stronger motor impulse than the sight of rabbit running. One objection to this second interpretation is that this mere ready made association of rabbit and drain could only send the dog along the *usual* path to the drain, and this usual path is the rabbit's. However, and we wish to lay special emphasis on this, both the above interpretations are speculations which are, perhaps, worth making, but only as helping to scientific study of the facts. Such a study would mean this: that the master of the dog is a competent dog psychologist, thoroughly acquainted with dogs in general, and this dog in particular, in all his ways and expressions, and yet not biased for the dog—masters like parents are liable to be prejudiced in favor of their charges—and that he sees the dog clearly when the making for the drain was *first* accomplished; then, judging from the method of expression of the dog at that instant whether there was evidence of hesitation or deliberation, attain a competent judgment of the case, which would, to a certain extent be verifiable for other psychologists if a photograph of the dog had been taken in the act. From a similar study of a large number of such cases by trained observers, we would have the only scientific evidence obtainable as to whether dogs in general show that they can on occasion compare distances to a destination, and consciously choose what is thought to be the shorter. That is, the main evidence must always be from a complete record of expression, and that interpreted most cautiously.

Right here we wish to remark that Professor Morgan does not make clear to us how a perception of relations is confined in its serviceability only to human 'descriptive intercommunication' (pp. 239, 243, 293). This assumption is quite too readily made, and used quite too much in an *a priori* fashion. In fact we may ask if perception of relations does not arise at first, not for communication, but by its immediate serviceability to the individual. Thus in the case of the dog and rabbit a definite understanding of space relations accomplishes more quickly and surely the catching of the rabbit; the dog profits by it quite as obviously as the human hunter who plans a short cut to head off a rabbit.

Again in explaining the apparent perceptions, *e. g.*, of distance, by animals, Professor Morgan insists that the relations, if perceived at all, are not focally but only marginally perceived, to use his optical terms. But this theory, which is fundamental with him, that consciousness in the development of its forms is first marginal merely, a side part in the total body of consciousness, and only gradually becomes focal or central, as in man perceiving a relation, seems quite contrary to the first assumption of evolutionary psychology, namely, that new modes originate in severest effort, and are thus in all their earlier developments preëminently focal. But when a body of consciousness, *i. e.*, a mind, is once formed and becomes hereditary, then much that has been focal in the long past becomes marginal. Thus vision in its origin was certainly not focal marginal, but a single focal point, and the highly developed vision that holds a considerable field of vision outside the single focus is really reflex of myriad ancestral focalizings. Hence, what is marginal to my vision is not the pin head on the cushion to my left, but the cushion itself, which is of such a size as to have been attentively perceived by numberless ancestral generations; but if for thousands of years my ancestors had exercised themselves in looking at single pin heads, I have no doubt that the pin head would be as plainly marginal as the cushion. So also we conceive that the present order of evolution indicates that the perception of relation was first realized, but faintly to be sure, in an intense focalizing effort. It is certainly a misuse of terms to make focal equal all clear and distinct consciousness; that which we are straining the eye to see is often far more faint than the marginal. We should say then that the development of perception was, like all other consciousness, from dim focal states to clear focal states, and then to marginal states.

The over use of hypothesis, and that often doubtful hypothesis, mars much of Professor Morgan's writing on animal psychology, but when he makes a survey of all the facts, his interpretations are in general just. However, since we are on negative criticism, let us note an instance of experiment where the interpretation is very obviously defective; I refer to the throwing red currants to his chicks (p. 298). Though he calls this a 'parable,' I understand it has basis in fact. The chicks, seeing the strange objects, utter the 'what sound,' or note of interrogation. But having gained experience of taste of currents, what kind of sound will they utter upon coming upon them again? Mr. Morgan says, if they could attain to make 'currant sound,' this would mean absolutely nothing to the chick who had never experienced currants. 'It is a sound indicative of certain experiences that it has never had,' and hence 'of no indicative value,' and hence as 'value' or serviceability is the *rationale* of existence of psychosis and its expression there being no *rationale* here, the existence of a real 'what sound' and responsive answer may be denied.

To this we must suggest that while there is no 'currant sound' (Mr. Morgan here really falls into the language fallacy he elsewhere so justly condemns), there may be a sound indicative of edible object. Suppose a group of chicks before some currants, piping the interrogative, and the mother hen comes along with wide experience of those things we class and call currants, will she not give at once the food note? Just as when I come upon a comrade eating some strange thing, and to my interrogation he grunts 'yum! yum!,' and I know it is an edible. I take it that the food signal, which is certainly widely developed and widely useful among animals, is really no more than a kind of 'yum! yum!,' or a wholly indefinite pleasure sign of the edible. Every chick has food experience and so can appreciate food signal, though currant signal, if possible, would be of no use. I may suggest further with reference to the nature of the psychosis, of chick which has experiences of black caterpillar and learns to let it alone (p. 301) or red currant and learns to appropriate it, that we are not confined to the alternate hypothesis of mechanical association and full formal reason. Suppose a chick has seen and swallowed several currants with satisfaction, and running a little farther sees another currant, what is then its real psychosis! I am inclined to think, if the chick is yet in the active investigating stage, *i. e.*, beyond where it pecks at everything, but is becoming actively discriminatory, we may interpret the psychosis as identification. It recognizes that red object before it as the very identical object it has just experienced with such lively satisfaction, and it eats it (again). There is for it simply the single identical thing constantly reappearing, and so no things or classes of things. It eats its cake, and has it too. To the chick there is one worm and one only, which to its great joy it is continually re-finding. (On this phase of psychosis I have made some fuller remarks in 'Evolutionary Psychology of Feeling,' p. 2, 85 ff.) And this does not deny that the identifying act is largely instinctive, *i. e.*, impelled by heredity force. Most of the apparently intelligent activities of young animals are doubtless fully three parts instinct to one part individual intelligence.

We have emphasized the need for an unexciting method in animal psychology, and for one which shall make it a point to secure in every case all the facts of expression. But while we can judge with some aptness what psychosis our fellow men are experiencing, we are so distantly related to most animals that their mentalities must be quite diverse in tone, degree, and quantity from our own, the only basis for our judgment. Hence animal psychology should begin with those animals most akin to us, as the simians, and the psychologist should seek the most constant and intimate acquaintance with his charges, should practically live with monkeys till he becomes thoroughly conversant with their modes of expression. Further, in observing and experimenting for intelligence, mature animals should be chosen, those who have run through all the stages of hereditary mind. But they should not be old, the most favorable age being for a year or two following full development, when there is plasticity, and yet recapitulation is fully done, all the forms of mere instinctive adaptability being fulfilled. Professor Morgan's experiments were mainly if not entirely with young animals, as chicks and pups.

Further, the motive to the creative activity of real intelligence must be an adequate and favorable one. The hunger method is perhaps the most efficient in nature, and it is proverbial that hunger sharpens wit. In nature most of the progressive intelligence has been achieved by animals confronted by new circumstances in their search for food. Hence the test which would most likely to give definite positive results for initiative intelligence of animals would be to confine a just matured chimpanzee in a cage, and, having well starved him, put food near by under conditions which neither his nor his ancestors could have experienced, but conditions which might be overcome by some simple perception of relations and application thereof. This gives opportunity for cool deliberative action; and the monkey should be carefully studied and photographed and phonographed throughout the whole test. Of course such tests, if pursued too far, might well call for the interference of the Humane Society. In fact to reproduce the conditions which under natural selection stimulate creative intelligence may always mean cruelty. The successful mind has generally made its achievement at the critical life and death point. Yet it may be that with monkeys seeking food or liberty, there can be provided sufficient incitement to obtain positive results without cruelty.

We must add that wild animals, as being distinctly more alert than domesticated, are the most desirable subjects in seeking positive results as to the intellectual powers of animals. Adaptiveness not being forced by the conditions of existence on tame animals, they become little more than machines. Compare thus the wild sheep and the tame. The dog in being 'well trained' to be routinely obedient to his master is made a mechanical slave; he loses very largely that free initiative and strong intelligent individuality and independence which was his, when, in state of nature, he was his own master and had to provide for himself or starve. The dogs at our bench shows are mostly very stupid and helpless beasts. As domesticated animals are bred in the main not for psychical but for physical points, man has as a whole degraded and brutalized the brute. Certain passive and emotional states are indeed generally favored by man, as lack of temper, but it is only exceptionally, as in the collie dog, that the animal is bred distinctly for intellectual qualities. Even the collie fanciers look chiefly to the coat of the animal, form of the head, tail, etc. This tendency with breeders is, in truth, much to be regretted both from a sentimental and scientific point of view. If selection and breeding were definitely carried on with dogs wholly in psychical lines for a number of generations, we should have some far more interesting companions than the present prize beasts, and some far more suggestive material for the comparative psychologist.

While our remarks have been directed to some questionable points in Professor Morgan's animal psychology, we are thoroughly convinced that he has accomplished much that is suggestive, and that his basis of introspection is the only true basis. His caution is also admirable, but we do not think the law of parsimony is positive proof, as he seems to urge. Thus, as applied in the dog-and-drain case, the question is not what might be, but what are the actual psychic facts as interpreted from actual expression. The golden rule of science is that theory is good only as leading to facts and facts only as leading to theory, but animal psychology is yet far from attaining this full correlation.

HIRAM M. STANLEY.

LAKE FOREST, ILL.

RECOGNITION.

In Miss Mary W. Calkins' full, able and, if I may be permitted to say so, sympathetic review of my two articles on the 'Recognition Theory' of Perception of Höffding, Spencer, Ward and others, and on Recognition, there is an important misunderstanding. By the omission of two words, indicated of course by asterisks, I am understood and quoted as saying that there is in recognition no identification of the past with the present. It is, I believe, a misunderstanding which has very important bearings on a right understanding of recognition.

The remark I made was: "There is in recognition no 'identification of the past impression with the present one.'" The 'past impression' I hold to be gone forever, to be no longer existent and hence not a participant in any comparison or identification which may possibly take place in recognition. Former theories of recognition have, I believe, misrepresented the facts by asserting that in recognition and memory the 'former impression' is present and that it is known as past or known again. Then by some special *actus* of the 'mind' this 'past impression' is compared and identified with some 'present' object and we know that this object is known again. Again, there is a further *actus* supposed in the 'mind's' capacity of preserving, retaining and bringing to light again the former impression or object. The 'Retentive Faculty' is still abroad if not openly, still covertly.

Now I hold that in recognition it is not the old or former impression or 'way in which consciousness looks at a thing' (call it what name you will) which is present. It is gone and gone forever. It is the *object* (I speak simply of 'things' as they appear in consciousness and with no metaphysical theory in view) which is known as past or known again and not the former impression. Upon the basis of certain characteristics, as I explain later, I classify some objects as 'past,' some 'present' and others as 'future.' The former impression is not present, for it no longer exists.

Pastness or the known-again-ness of objects cannot therefore be explained, as is usually done, by a comparison and identification of the object of perception with the 'past impression.' Even if it did now exist the comparison would be between two objects, and whence then the pastness? How in the meanwhile has the object, as then perceived, become 'past'? If simply resurrected it ought to be the same as before. Furthermore, it is a definition in a circle to explain pastness by bringing in this 'past impression' as an explanatory term of pastness. That is the point to be more precisely elucidated by the definition. Why is an object regarded as past or known again? Is that 'past impression' which is used to explain pastness or known-againness to be explained in its turn again by a comparison or identification with other 'past impressions' and so on *ad infinitum?* We know of no such process surely in consciousness. Or again, if an object is known as past in itself, *i. e.*, inherently or ultimately, there appears to be no need of a process of comparison and identification and if such be the case, how is it that we regard the same object or event at one time as past, and at another as present, or future?

This leads to the second point. Miss Calkins believes 'the central error of the theory' to be 'the assertion that recognition does not imply identification or comparison.' It is further remarked that 'immediate recognition does, nevertheless, include comparison with the past experience of the subject, only the comparison is wavering and restless, and the identification is incomplete.' As above stated, I did not assert that identification and comparison in recognition were impossibilities or absent in the process. I merely said, "there is in recognition no 'identification of the *past impression* with the present one.'" (p. 269.) The process of comparison and identification may enter into some cases of memory and recognition, but is not an integral and necessary part of every case of recognition. After an idea-object (centrally excited) has arisen it may be classified upon certain characteristics as 'past;' then the perceptual object (peripherally excited) may be compared and possibly identified with the primary object. The perceptual object may then be classified as known again. In such a case comparison may be present, but it was not necessary for the classification of the idea-object as past. So it is with most cases of sudden recognition or of strange familiarity where we could not possibly have seen the object in question beforehand. Some characteristic, usually appertaining to objects we call past, associates itself unwontedly with the object perceived and the classification naturally ensues. No 'past impression' or even idea-object is apparently present or necessary for the recognition in question. In the classification of an idea-object as past, is it necessary that still another idea-object (past or former, or what you will) should be present, compared and identified and so on *ad libitum?*

Objects as they appear in consciousness are in themselves neither past nor present. So-called idea-presentations stand, in this respect, equally on a par with the sense-presentations. Comparison or identification of a sense-object A with the centrally excited or idea-object *a* will give us no recognition, simply object A or *a*. The pastness or known againness of either has still to be ascertained. Equally so, and this is a point little regarded, does the *presentness*, the *nowness* of certain objects require to be explained.

An object may be regarded at one time as past and at another as present. Why should it be thus classified differently at different times? Upon a consideration of these points I was led to note the characteristics of the objects (and their possible accompaniments) in each case. It then became evident that when objects were possessed of certain characteristics as *e. g.*, lack of freshness and vividness, absence of details, unsteady, easily changeable localization, lack of persistency, air of freedom, absence of certain muscle, joint and other sensations, the sudden introduction into consciousness of an object by association of ideas, which object does not in the case in question properly belong to the object perceived, the great rapidity and often surprising ease and quickness of the act of perceiving often accompanied by a second idea-presentation of the same object immediately following, or often a feeling of pleasure upon perception of an object, say a stranger in the street, when the cause of the pleasure is unknown, etc., then, I say, we have a consciousness of these characteristics and classify these objects as past or known-again. If, on the other hand, they possess vividness, full details, persistence or obstinacy of spatialization, persistency in abiding under certain conditions, etc., then we have a consciousness of these characteristics and put them in the other great class of objects which we name *present*. The former we call memories, the latter perceptions. Thus it happens that upon an object centrally excited, possessing great vividness, persistency, etc., arising, there may ensue the classification of it as belonging to 'objects present;' later it proves to be an hallucination. It may also be added that objects may be possessed of these characteristics, but they may not be noticed and there may be no ensuing classification. In such a case, there is simply what I may term 'object consciousness' passing on to another 'object consciousness.' Neither the characteristics nor the classification, taken alone, make up recognition, but both together. Moreover the characteristics may be variable, now one, now many, now this, now that; the classification into either present or past objects remains, however, the same. The characteristics are, however, obviously not the same for each great group or class.

It is thus clear that I do not, as Miss Calkins affirms, 'treat the past as the known-again-with-its-associates,' nor do I exclude comparison or identification from all cases of recognition. In my own experience in the majority of those cases of strange familiarity which are noted by so many, the process appears to me to be in most cases a surprising acceleration or ease in perception of the object which is undoubtedly hitherto unknown by me, and immediately following thereupon a second presentation of the same object. Now this acceleration and this consequent easy presentation of an object frequently given in experience, are characteristics upon which I base the immediately following classification as known again or past. There may be thus present comparison and classification, but it is evident from some of the other characteristics that they are not necessarily always present. It is said "when a face 'seems familiar,' I am eagerly comparing it with faces I have already seen, trying to identify the present with the past." It is quite obvious in such cases, however, that the comparison and identification comes *after* the strange feeling of familiarity which may be based on other characteristics than the presence of accompanying idea-presentations. Moreover as above stated, it does not seem correct to use the explanatory phrases 'past experience,' 'identifying the present with the past' etc., in explaining pastness.

ARTHUR ALLIN.

The points of disagreement between Dr. Allin and myself seem to me to be mainly metaphysical, and should perhaps have been untouched in my notice of his intentionally psychological articles. As I have there said, the "definition of the recognized as the 'known again' is psychologically quite satisfactory, for psychology avowedly adopts the matter-of-fact standpoint," that is, psychology deals with *facts* of consciousness, or relatively isolated, single realities, immediate and temporally located.[1] Now these facts of consciousness or impressions as Dr. Allin might call them, never recur and never rise from a buried past into a present. Under these circumstances the difficulty is to show why we do actually have an experience of what we call identity; why, in spite of the evanescence of the facts or events of consciousness, we do predicate sameness. The solution to this problem seems to me to be suggested by the following line of thought: besides the factual sort of consciousness, the series of conscious states which truly does form the proper object of psychological investigation, I believe myself to possess, actually and immediately, another sort of experience which is what I mean by the term 'self-consciousness;' and it is the characteristic of this sort of experience to be non-temporal and incapable of being split up into facts.

[1] *Cf.* F. H. Bradley, *Appearance and Reality*, p. 317, for a similar definition.

Now this self-consciousness evidently conflicts with the psychological or fact-way of regarding consciousness precisely where questions of time are involved, but the self-consciousness is the immediate experience, while the 'facts' are really artificial abstractions, necessarily hypothesized for the scientific study of consciousness, yet in no sense concrete realities. Moreover, in defining a 'fact' as temporarily related, it is easy to assume the fundamental validity of time distinctions, whereas from the strictly psychological point of view they are mere conscious elements. To say, therefore, 'the past impression is gone forever'—a statement to which I cordially subscribe—means: "Assuming, by use of the word 'impression,' the temporal way of regarding consciousness as a series of 'events,' then it follows that one 'event' is not temporally identical with another." This does not, however, affect the reality of the experience of 'identifying,' which is really a transcendance, not a comparison, of past and present.

Dr. Allin's close parallel of the 'known again' with the sensation[1] seems to me also to threaten the obliteration of an obvious distinction among 'facts of consciousness.' On the one hand, we have the admitted sensations, the 'red,' the 'shrill,' the 'hard'—what Dr. James calls 'substantive parts'[2] of consciousness. It is their characteristic to be independent, to stand alone as it were, or (adopting Dr. James's figure) to provide perchings and landing places to thought. Besides these, however, there are the 'transitive parts'[2] or 'fringes,'[3] the relations or links, themselves facts of consciousness, and facts only, from the psychological standpoint, yet lacking the independence and self-sufficiency of the substantive elements. Such 'transitive parts' are 'sameness,' 'agreeableness' and 'disagreeableness'—not to mention others which might lead us far afield; these seem to me to be distinguished from the substantive elements or sensations, just in this, that they inevitably suggest the immediate self-consciousness which, however, unlike the 'facts,' is untemporal; the puzzle of assumed identity and temporal diversity is thus an opposition of the two points of view.

Though I agree, therefore, with Dr. Allin, in the belief that, psychologically speaking, present and past are simple elements of consciousness, I nevertheless do not regard his analysis of these contents as psychologically sufficient. He seems to me himself to suggest the inadequacy by the statement, in the preceding 'discussion,' that recognition requires both the enumerated 'characteristics' (lack of vivid-

[1] *Amer. Journ. of Psy.*, *op. cit.*, p. 267.
[2] *Principles of Psychology*, I., 243, *et. alt.*
[3] *Ib.*, p. 258.

ness, rapidity and the rest), and an 'ensuing classification.' Since these characteristics 'may be variable, now one, now many, now this, now that,' it seems to me likely that they are mere accompaniments, not constituents, of pastness which apparently remains virtually 'what-is-classified-as-past.'

According to my view, which I can here barely suggest, the consciousness of time-distinctions is relatively late, and is one form of consciousness of multiplicity. It presupposes self-consciousness, for the past is primarily one's own past, and only later do mere objects of imagination unconnected with one's own experience, like the reforms of kleisthenes or the battle of Waterloo, become also 'past.' The essence of temporal multiplicity is, however, the consciousness of necessary connection. The 'past' is the 'irrevocable,' or 'irreversible;' the future is the 'supposedly reversible or unconnected;' the present is a later distinction and is negatively defined with reference to the other two. Further we surely can not go on any pretext of keeping within psychological bounds; it may indeed be questioned whether we have not already transgressed these in attempting any account of 'pastness.'

Two points in Dr. Allin's criticism of my review may be briefly mentioned. The word 'past' which occurs in the statement, quoted by Dr. Allin, about 'immediate recognition,' he regards as a case of 'definition in a circle.' The word was not used, however, as an explanation, but as a partial analysis—though a superficial one—of the 'known again,' which is not in my opinion equivalent with the 'past.' The sentence in which this word occurs is properly criticised, since it treats the subject rather popularly and inexactly, but I still hold, with reference to the main point at issue, that mediate and immediate recognition differ only in degree.

It remains to question two of Dr. Allin's explanations. He does not seem to me greatly to advance the discussion by insisting that recognition is of 'objects' not of 'impressions,' since he does not clearly define the former word. If he means frankly 'common-sense object,' then, indeed, he has an honest psychology, but it merely substitutes an every-day philosophical assumption for a more subtle one. I object also to the recourse (as by the expression, 'object centrally excited') to cortical conditions as explanation of psychological phenomena, for here again one has on one's hands a whole series of metaphysical assumptions—dualism, physical causality and so on—intermingling with one's psychology.

MARY WHITON CALKINS.

WELLESLEY COLLEGE, May, 1896.

THE COMMUNITY OF IDEAS OF MEN AND WOMEN.

In following the discussion between Dr. Jastrow and Miss Calkins on the Community of Ideas of Men and Women, I have been struck most forcibly by their not distinguishing the two problems which Miss Calkins finally states at the end of her article in the PSYCHOLOGICAL REVIEW for July. These problems, put in terms of the two principal points at issue between Dr. Jastrow and Miss Calkins, are:

1. Do women *naturally* tend more to repetition and to the use of concrete terms than men?

2. Do women tend more than men to repetition and the use of concrete terms, *on account of education and social traditions?*

The first problem deals with genuine mental differences of sex; the second with differences due to association, not therefore differences of sex, but differences which will change with changes in education and psychical environment.

Which problem are Dr. Jastrow and Miss Calkins discussing? I find it nowhere explicitly stated, but the fact that the two sexes as such are experimented upon to find their differences leads me to suppose that the first problem is the one under consideration. From that point of view it seems to me that some criticisms may fairly be made upon their method of collecting data.

If the problem is to determine inherent psychical sex differences the first essential to scientific experiment is to eliminate, as far as possible, or to allow for differences due to habit. This can be done to a large extent: (a) by selecting men and women who have had from childhood essentially the same physical and psychical training; (b) by a detailed account of the differences in training which do exist; (c) by a large number of cases chosen from different professions and different social strata; (d) by making a record, at the time when the lists are written, of the studies which the subjects are pursuing and of their occupations outside of their university work; (e) by having the subjects under the same conditions when making out the lists. I am even inclined to say that they should be given the same word to start with. After the lists are written it would also be an advantage to have the subjects write out the association between the words, in order to help in the classification. In this way, probably some apparently abstract terms would turn out to be concrete in meaning.

As nearly as I can judge from the articles, none of these conditions were observed by Dr. Jastrow and Miss Calkins. No measures were taken to eliminate or to allow for the influence of habitual associations.

To my mind, therefore, the experiments simply resolve themselves into a further illustration of the very well-known fact that habit determines the association of ideas—a fact which it is entirely unnecessary to prove and which is quite as strikingly illustrated by the different associations among men of different professions as by those between men and women.

It is probable that the most striking differences between the Wellesley lists of '94 and '96, and between the Wellesley and Wisconsin lists, might be entirely explained by a few inquiries into the studies taken by the different students at the time when the lists were made. For instance, the lists of abstract terms stand thus:

Wis. Men.	Wis. Women.	Wellesley, '96.	Wellesley, '94.
131.	97.	101.	280.

We are told that in the Wellesley list for '94 one paper alone contained fifty abstract terms out of a hundred. Until it is positively disproved, we could hardly escape the inference that this subject, for some unusual reason, had been much occupied in abstract thought. May it not be true that inquiries about the Wisconsin men and women would give the same kind of explanation? Take, for instance, the lists on the animal kingdom:

Wis. Men.	Wis. Women.	Wellesley, '96.	Wellesley, '94.
254.	178.	146.	223.

The discrepancies here might be doubly explained. Some of the subjects might have been taking zoölogy or biology, in the first place. In the second place, the care of animals always falls to the boys, very seldom to the girls.

So I might go through with the entire list, but these suffice to show my point.

We may grant and declare that women's associations differ from men's, because their habits of life are different. We may admit the certainty of there being some psychical differences between men and women on account of the physical differences of sex; but generalizations as to inherent psychical sex differences which are made on the basis of variations due to *individual habits* can have no validity.

It may be objected that the very fact that women do follow certain occupations to the almost entire exclusion of men, and *vice versa*, proves certain *particular* differences. This may be so, but it can not be demonstrated until men and women are not only nominally but actually free to enter any profession. At present some occupations are both nominally and actually, and many others are actually, closed to women, especially to married women. The same is true of men, although to a much smaller degree. In view of the recent enlargements of 'woman's sphere,' he would be a bold mathematician who would attempt to give its radius. The real tendencies of women can not be known until they are free to choose, any more than those of a tied-up dog can be.

Such generalizations as those of Dr. Jastrow and Miss Calkins, serve only to confuse the point at issue. Whether the experiments were to prove inborn psychical variations between men and women or differences in association due to differences in the modes of life, they fail equally because they do not consider the effect of habit; and in the latter case they have even less *raison d'être* than in the former, because the fact is already generally admitted.

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PSYCHOLOGICAL LITERATURE.

RECENT FRENCH WORKS.

Du fondement de l'induction, suivi de psychologie et métaphysique. T. LACHELIER. Paris, Alcan, 1896. Pp. 176.

This little book is a republication of a doctor's thesis which dates from 1871 and has been long out of print. Lachelier undertakes the investigation of the principle on which is based the operation by which we pass from knowledge of facts to knowledge of laws, or, in other words, by which we add to perceived facts the elements of universality and necessity, which characterize laws.

According to the author, "there are only three ways to account for principles, because there are only three ways to conceive of reality and the act by which the mind enters into relation with it. In the first place, one could admit, following Hume and Mill, that all knowledge is sensation and that principles are only the most general results of experience. Secondly, one might assume, following the school of Cousin, that these phenomena are but the manifestation of a world of reality inaccessible to our senses, and in this case the chief source of knowledge would be a kind of intellectual intuition, which discloses the nature of these realities and the action that they exert on the sensible world. Finally, according to a third hypothesis, that of Kant, our highest knowledge is neither a sensation nor an intellectual intuition, but an operation by which thought perceives immediately its own nature and its relation to phenomena."

Without stopping to discuss Cousin's theory, we may adopt on this point Lachelier's conclusion: "Substances and causes are only a desideratum of science, a name given to the unknown basis that maintains the order of the world, the statement of a problem transformed to a solution by a verbal artifice."

But attention should be called to the interpretation, quite incorrect in my opinion, which Lachelier gives to Mill's theory, for I think there is no radical difference between the points of view of Mill and of Kant. Lachelier has fallen into the error of believing that Mill takes sensation and experience, not as the point of departure of knowledge, but as themselves constituting knowledge; this is evidently what he regards as empiricism, and I do not say that Mill is not responsible for the mistake, as his language is often equivocal. But let us take first Mill's argument and see what Lachelier answers. It appears to him that Mill is in difficulties between the needs of science and the logic of empiricism. But I feel sure that Mill's argument does not beg the question when his meaning is thoroughly understood. This is summarized as follows: "The spontaneous induction that first suggested to men the regularity of the most common phenomena inspired them with only moderate confidence, but their confidence gradually increased as experience confirmed the results of their early inductions, and each fact that confirmed a special law spoke in favor of the law of causality, which thus collected for itself as much favorable testimony as all the others taken together. It is consequently not surprising that this law became finally invested with absolute certainty, while the others only attained a greater or less degree of probability," (p. 20). Lachelier does not accept this argument, but he does not indicate clearly his reasons. The only objection to be made to Mill, in my opinion, is that he has not been sufficiently explicit and has not said that the principle of induction is psychological, that it depends on our mental constitution and that all reasoning, even deduction, is based on the mechanism of our ideas and representations.

Let us now examine Lachelier's arguments, which are curious; the chief ones are as follows: What is spontaneous induction, and what place does it occupy in a system in which experience is regarded as the only source of knowledge? Is it the same thing to observe the occurrence of phenomena and to conclude that the same phenomena will recur under the same conditions? The author amplifies these curious statements in another place in the same book to refute the view of Royer-Collard, according to which the belief in the stability of natural laws depends on our own nature. "It is difficult to imagine a more complete confusion of ideas. Our nature is not able to teach us *a priori* regarding a fact of experience, but beyond experience and facts there is nothing but the truths of reason, which do not admit of contradiction. A judgment which is not empirical and yet is not necessary, is an absurdity which has no place in human intelligence." It would take too much space to answer point by point, but let us note some of the arguments. Lechelier admits a few lines further on that we are able to foresee certain events as probable. He admits that this foresight is accompanied by a strong and even irresistible tendency of the imagination, but then expressions of contempt follow, . . . "to seek the secret of the future in what is only the vague image of the past is to undertake to guess during a dream what will happen when we are awake." What a curious idea to compare with a dream the regular course of mental life! It is much to be wished that this question of the basis of induction, which is in the first instance psychological, should be taken up by psychologists and studied by means of exact observations.

Le mouvement idéalist et la réaction contre la science positive. A. FOUILLÉE. Paris, Alcan, 1896. Pp. 351.

This work, from the fluent pen of one of the best known of French philosophers, aims to bring to a focus the discussion on the fallibility of science so brilliantly opened by M. Brunetière. It will be remembered that M. Brunetière, in reviewing the questions which modern science is unable to answer, confined himself chiefly to the physical and natural sciences. It is in the name of the moral sciences that Fouillée speaks, and he thus changes somewhat the question at issue. He treats as the principal adversaries of science, or, to speak more exactly, as the teachings which limit the field of science, the agnosticism of Spencer, the idealism of Kant, and the philosophy of contingency, represented by Renouvier and Boutroux. These are the teachings he discusses and seeks to refute. It does not seem to the present writer that the intellectual unrest and the reaction against science which have arisen in recent years have anything in common with the discussion of these philosophical problems, and I think it would have been preferable to have written a natural history of the moral anarchy of our society, its causes and consequences, remaining as far as possible within the limits of observed facts.

The work has an appendix containing four short papers, (1) 'Adolophe Franck and the Philosophic Movement of the Past Fifteen Years,' (2) 'Descartes and Contemporary Teachings,' (3) 'Philosophic Instruction and the French Democracy,' (4) 'Philosophy in Examinations (*les concours d'agrégation*).' The author regrets the exaggerated place given to metaphysics and the history of philosophy.

Les principes du positivisme contemporain, exposé et critiqué. T. HALLEUX. Paris, Alcan, 1896. Pp. 351.

This little book written from the point of view of catholicism and inspired by M. Mercier, of Louvain, contains many ready-made formulas and purely verbal arguments, such as are usual in a catechism, The principal objection made to positivism contains a great deal of truth. It is that the positivists are mistaken in holding that all experimental knowledge can be reduced to the consciousness of a subjective being; in the most minute and exact observations the mind always controls the senses.

Les types Intellectuels: Esprits logiques et esprits faux. FR. PAULHAN. Paris, Alcan, 1896. Pp. 362.

In this book, with this curious and suggestive title, the author proposes a classification of intellectual types. The study is a continuation of his previous work, *Sur les charactères*, the two works being the development and application of the author's peculiar theories regarding systematic association. It may be briefly called to mind that systematic association consists in the property that all kinds of psychological elements possess of associating themselves together to form syntheses, not in obeying the laws of resemblance, of contrast and of contiguity, which are secondary laws, but in realizing a law of teleology. In intellectual phenomena systematic association takes the form of knowledge, whence a division of intellectual types into such as are logical and such as are illogical.

Logical minds are of various kinds: *well-balanced*, in which systematic association follows without effort from innate tendencies; *thinkers*, in whom equilibrium is sought after and attained with effort; *extremists*, in whom equilibrium is obtained by the subordination of the intelligence to certain elements; *specialists*, in whom systematization takes place only in a small field. Then there are the intellectual types dominated by phenomena of conflict, of inhibition and of contrast, the combatants, the critics, the dreamers, the sceptics, etc. The exaggeration of association by contiguity gives a limited and halting memory; the excess of association by resemblance, in the case of words, gives rise to rhyming poets; in the case of ideas, to the abuse of metaphor.

The author also subdivides illogical types. He distinguishes minds illogical through the excessive predominance of leading ideas; those illogical through conflicting adaptations; then, finally, those naturally incoherent. This last type is confined to children and hysterics.

All these distinctions are interesting, but we should not forget that the study of intellectual types should be made by observations of individuals, rather than by a treatise written in the library.

De l'aphasie sensorièlle. CH. MIRALLIÉ. Paris, Steinheil, 1896. Pp. 220.

This book is a thesis for the M. D. degree, in which the author chiefly presents the views of Dejerine, his teacher. It is well known that Dejerine, the eminent professor at the Saltpétrière, has made numerous researches on aphasia which have opened a new phase of the subject. Charcot's celebrated scheme of the four images and the four centers has received a serious attack; briefly, the chief points brought out are as follows:

(1) Charcot held, following Hartley, that we can use in inner speech four kinds of images, visual, auditory, articulatory motor and graphic motor, and that consequently language depends on the use of four distinct nervous centers, and, further, that each individual uses preferably a certain kind of images, some being visualizers, some being audiles, etc. Dejerine holds, on the other hand, that this distinction of mental types is not founded in fact, but results in a confusion between memory for words and memory for things. As far as memory for objects is concerned, it is quite true that there is a visual memory, an auditory memory and a motor memory, and that some kinds of memory may be more developed in the case of certain individuals and serve to characterize them; but in regard to words and thinking in words matters are quite different. In studying inner speech we must return to the point of view of Egger. We are all auditory, or, more exactly, auditory-motor, and those who 'read the words of their thought' or 'write them' are extremely rare exceptions. The whole discussion should be read in Mirallié's monograph, which, though presented in a somewhat schematic form, is highly instructive.

(2) The second part of Charcot's work, which has been refuted by Dejerine, is the explanation of agraphia. According to Charcot the act of writing depends on the calling up of graphic images, and agraphia is explained by the loss of graphic memory. Dejerine has brought forward many cases to show that the process of writing is entirely different from this; we use a visual copy, and it is the visual image which is lost in agraphia and prevents writing. This is proved by the fact that those suffering from agraphia can write when they copy a model placed before their eyes, whereas they are unable to make words from blocks containing the letters.

Mirallié's book includes further an exposition of the two forms of verbal blindness distinguished by Dejerine, and a plea in favor of sensorial aphasia which Wernicke defined, but which Kussmaul and Charcot have denied. There are in the book numerous anatomical drawings and specimens of handwriting, clinical observations given in detail, and a very complete bibliography of aphasia. It is impossible to recommend too highly the reading of this book to those who wish to understand the most recent studies of aphasia, a subject of the greatest possible interest to the psychologist.

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Der Kampf um einen geistigen Lebensinhalt: neue Grundlegung einer Weltanschauung. RUDOLF EUCKEN. Leipzig, Veit & Comp, 1896. Pp. viii+400.

The significance of this work is indicated by the chief words in the title. The present age is held to be one in which man is threatened with the loss of all sure foundation for the life of the spirit, or even actually dispossessed of a basis and a content for his spiritual existence. Thus our time is one of conflict. First of all, conflict between the opposing tendencies of thought, which distract us no more surely when considered in their manifold variety than they fail to satisfy the mind if taken in the form of the movements—*e. g.*, naturalism, idealism—that most have gained the suffrages of the modern world. Hence begins a deeper struggle, or rather the conflicting systems of the day include an element of which the leaders are often but dimly conscious, a contest for the realization of the life of spirit and for the satisfaction of the needs native to man in virtue of his relation to the universal spirit or reason of the world. This battle and this yearning unrest, moreover, are real, however much they may be denied or disguised by the complacent naturalism of the time, by our shallow culture and our lifeless art, by a social utilitarianism at bottom essentially selfish, or by a weakened church which fails to accomplish its high mission because of its insistence on the outworn traditions of the past. The age, therefore, must be summoned to continue its warfare; only with an adequate comprehension of the issues at stake and of the true objective point of the conflict. For the *Geisteswelt* and the *Geistesleben* are fundamental realities, not mere imaginings crystallized into words. Yet their reality is to be understood in a sense other than that which is commonly associated with the term; they exist not as fixed and finished products, but ever depend on the work of free creative activity. Especially for us, a constantly repeated deed, which implies freedom and is in its nature essentially ethical, is necessary, if we are to realize the spiritual life-process in ourselves. In this way only is the generation of a true spiritual actuality possible; and possible, the overcoming of the contradictions whose existence and whose power it is idle to attempt to ignore. *Das Ansichwahre und Ansichgute Platos, es wird zu einer lebendigen Wirklichkeit für uns nur in Verbindung mit jener Selbstthätigkeit Fichtes* (p. 33).

In tracing the conditions and the course of spiritual advance through conflict Professor Eucken divides his work into two main parts. The aim of the first or *Aufsteigender Teil* is the defense and elaboration of his chief thesis; that of the second or *Absteigender Teil*, the application of his results to the concrete conditions and institutions of to-day. Part I. subdivides again into three discussions of as many stages of the movement: A, *Der Kampf um die Selbständigkeit des Geisteslebens;* B, *Der Kampf um den Charakter des Geisteslebens;* C, *Der Kampf um die Weltmacht des Geisteslebens.* In these, besides the principles of the spiritual life already noted, two others may be mentioned as essential to the author's doctrine and constantly kept by him in view: the existence of a universal spirit or reason, to whom man is fundamentally related and whose ultimate victory is absolutely sure; and the development of the new world of spiritual activity with ever-increasing richness and complexity as one by one the various forms of opposition are overcome. In fact, though the contradictions and the conflict are painfully real, they contribute in their turn to the development of the spiritual life-process as it conquers the opposing forces by transforming and conserving them. Part II. brings the general view of the life of spirit thus gained into normative connection with the present status of affairs. Here, as throughout his treatise, Professor Eucken finds much to criticise in the organization and institutions of the age and sees hope only in the correction of present tendencies by resolute devotion to the spiritual ideals. This adherence, however, must not be partial, but inclusive. Even religion, with its clearest intimation of the world beyond, may exert a pernicious influence if it assume to be the whole of the spiritual process instead of finding its complements in morals, art and philosophy. It is only when all these several agencies, purified of their one-sided tendencies as well as of their direction to that which is empirical and lower, are combined into one collective movement that an age (or a man) can rise to the measure of its spiritual possibilities.

For the purposes of this Review detailed criticism is not in place. It may be remarked, however, that Professor Eucken furnishes one more interesting proof of the dissatisfaction of earnest thinkers with the outcome of recent speculation. Happily his criticism is more temperate than that of many other judges of the age. With all his directness of censure, his historical sense is too sure and his appreciation of modern culture too real for him to overwhelm us with an unrelieved jeremiad, even were his belief in the final triumph of spirit less absolute. Therefore his collateral discussions are often illuminating even for the adherents of principles which he rejects. And, as has been suggested by a critic of one of his earlier works in which among others, positions similar to those of the volume undei review have been foreshadowed, his general view of the world and of the age may prove acceptable to many who can not fully share in his positive philosophical doctrine.

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ETHNOLOGY AND ANTHROPOLOGY.

Ethnology. A. H. Keane, F. R. G. S. Cambridge Geographical Series. Cambridge University Press, 1896. Pp. xxx + 442.

One takes up with interest any professedly synthetic work on a subject as disordered in its material as anthropology or ethnology, and while any book with as ambitious a field as the title of the one before us would indicate must be almost immediately superseded it is worth while every now and then to pause and take our bearings. Of Mr. Keane's book, in the first place, it must be admitted that the title is misleading, although he partially guards himself by the definitions with which he very properly opens. Whatever may be said of other anthropological terms which are at present in such active dispute, 'ethnology' has come to be pretty generally regarded as including the comparative study of the varieties of man in their social aspects, and while the term 'ethnography' is, of course, a necessary one to denote the purely descriptive side of the subject it is always subsidiary to the larger, and an ethnology without an ethnography is an absurdity which apparently does not bother Mr. Keane in the least, since he hands over what we have all come to regard as among the main questions of ethnology to ethnography, and restricts his own work to a field which he divides into two parts, treating in the first place, under fundamental problems, such questions as the 'Physical Evolution of Man,' 'Mental Evolution of Man,' 'Antiquity of Man,' 'Specific Unity of Man,' and 'Varietal Diversity of Man,' and in the second part taking up the primary ethnical groups which he divides into four, 'homo Æthiopicus,' 'homo Mongolicus,' 'homo Americanus,' and 'homo Caucasicus.' If we accept this contracted field of ethnology, Mr. Keane's work is, on the whole, well done. One cannot expect too much from a general work of small compass, yet serious exception must be taken to such chapters as that on 'Mental Evolution of Man,' which is most inadequate from the point of view of comparative psychology, the chapter being a short one of nine pages treating chiefly of craniology, and similarly to the one on 'Mental Criteria of Race,' which confines itself almost wholly to a discussion of language, a comparative feature of prime importance, of course, but in the light of such researches as those of Tylor, Bastian, Lippert, Steinmetz and others no longer to be regarded as the only field of comparative value.

Yet with all its shortcomings the book satisfies a genuine need, especially of the general public. Mr. Keane's reading is wide, his presentation of arguments fairly complete and the arrangement of material logical, and his book is temporarily at least perhaps the best resumé at hand of our knowledge in the limited field of which it treats, which is unfortunately rather faint praise.

The Child and Childhood in Folk-thought. A. F. CHAMBERLAIN, M. A., Ph. D. New York, Macmillan & Co., 1896. Pp. x+464.

Dr. Chamberlain has produced a very useful, painstaking and disappointing work. It is useful in the number of items he has collected from all sorts of comparatively inaccessible sources; it is disappointing in the almost total lack of logical arrangement of the facts. He has strung his beads of quotation upon a thread of thirty-three rather sentimentally headed chapters from which even the excellent index does not suffice to bring order, and the result is rather a concordance to the literature of the primitive child than a systematic treatise on the subject. His method, if there be one, is undiscoverable, and the book seems to fail to fulfill either of its possible aims, for it is an impossible work for the layman to read consecutively and emerge with any tangible results, and it is exasperating to the anthropologist who seeks material on any one of the really innumerable subjects connected with the position and treatment of the child among primitive people. The latter will with some difficulty find scattered through the book excerpts to his purpose introduced possibly by a gem from the pen of Henry Ward Beecher or Joaquin Miller, but a discussion by the author rarely. Take, for example, the chapter on 'Child-life and Education in General,' one of the best in the book, by the way, and such questions as the first moral training, first punishable offences, methods of punishment, etc., are hardly touched upon, much less discussed, notwithstanding their immense significance. Facts as facts are always desirable however presented, but it seems a pity that one as fitted for the task as the author of this work should have failed so signally to utilize the extensive material he has recorded. Possibly this is reserved for further efforts. Let us hope that Dr. Chamberlain will see his way clear to bring future order out of present chaos. Of his book as it stands one can only say that as an example of industry it is remarkable; as science it is bad.

Die Anfänge der Kunst. ERNST GROSSE, Dr. Phil. Freiburg i. B. and Leipzig, J. C. B. Mohr, 1894. Pp. vii.+301.

This book is a little masterpiece. It is, so far as the writer is aware, the first attempt, certainly the first successful attempt, to establish a science of art, as distinguished from history and philosophy of art, upon a scientific basis by legitimate methods. The first task of any science is not practical utility but theoretical insight, and the first task of a science of art is not the application but the recognition of the laws which govern the life and development of art. This end is for the present an ideal, but an ideal in the struggle toward which the conformity of art phenomena to developmental law may at least be shown, even though the details of the laws themselves be not demonstrable, and it is as a pioneer in this field that Herr Grosse deserves the highest praise. He has grappled boldly with great obstacles, recognizes his failures, does not over-estimate his successes, and has finally 'blazed' a path which must be followed in the future and followed with most significant results. He recognizes two aspects to the task of describing and explaining the phenomena of art, an individual and a social, and turning his attention to the social confines his researches very wisely to the most fundamental and at the same time most neglected field, viz.: the primitive art of primitive peoples, and applies the method of comparative ethnology.

Without discussing the gaps and faults of that method as at present in use, we can turn directly to the author's treatment of his material. He follows the old division of the arts into arts of rest and arts of movement, quoting Fechner's description "dass die Künste der einen Art durch ruhende, die der anderen durch bewegte oder zeitlich ablaufende Formen zu gefallen streben; jene demgemäss ruhende Massen so umgestalten oder combiniren—diese solche körperliche Bewegungen oder zeitliche Aenderungen erzeugen das der Kunstzweck erfüllt wird." Taking up the arts of rest, commonly known as 'pictorial,' the author considers as the probable original form that of ornament, and as the original object to be ornamented, the human body, which is, therefore, discussed first, followed by the decorations of utensils and weapons; thirdly, he treats of drawings, paintings and sculptures which do not primarily serve decorative ends but have an independent meaning. The transition from the arts of rest to the arts of movement is represented by the dance, to which especial attention is given as being of extreme sociological importance, and which leads naturally to the consideration of poetry, since among primitive peoples the dance and song are always associated, and finally, primitive music is discussed.

One may choose for particular notice the chapters on ornaments of the body or 'die Kosmetik,' and on the dance as being perhaps the most valuable and suggestive. Regarding the former, after a consideration of the various forms of clothing and ornament among primitive people, Grosse goes on to discuss their practical meaning. One can divide all primitive ornaments of the body into two classes, those tending to attract and those tending to affright, not that any given decoration will fall under one of these heads to the exclusion of the other; on the contrary, it is usually both, for among primitive men as in our own level of civilization what makes a man terrible to his enemies or to other men makes him attractive to women. Undoubtedly the first and strongest incentive to ornamentation of the body is the desire to please, and in savage life it is one of the most powerful and indispensable factors in sexual selection. At that level the men are much more given to decoration than the women, contrary to the condition of affairs among civilized people. They resemble the higher animals in this respect. It is the primitive man who is the suitor, just as it is the male animal who woos the female. A primitive old maid is a thing unknown; the woman is always sure of marriage, while the man must often obtain a wife against great obstacles, and often remain in a state of forced and hated bachelordom for years. This sexual value explains the fact that decoration of the body is often first begun after the rites of puberty which mark the entrance of the boy to man's estate. But the man is warrior as well as potential husband, and has therefore a double object in bedecking his person, and as has been said, most ornament serves this double purpose. Red is not only the color for festivals, but the color for war; the headdress of feathers which increases the height is assumed in the dance as well as in battle, and the scars on the breast of the Australian, which excite the admiration of the women, arouse the fear of the enemy. It is hard to find an exclusively repellent ornamentation. Only certain patterns of painting the body seem to serve that end alone. As badges of authority, class and rank, ornament is little used among primitive men, for such distinctions do not exist there as they do among us, yet even there are seen the beginnings from which have developed the uniforms, gowns and accoutrements of our own military, academic and other degrees.

Space does not permit a discussion of the questions involved in the forms and development of the savage dance, though its rôle is all important in savage life. The pleasure in active and rhythmical movement, the pleasure in imitation and the relief in the expression of pressing emotion are a sufficient explanation of the passion with which primitive man cultivates this art. The significance of the primitive dance is striking. It fulfills not only a sexual end, but to a greater degree even a social one. The uniting of a body of men under the influence of a single emotion as seen in war dances, the union of heterogeneous tribal elements in certain dances of peace, suggest sociological bearings of the highest importance, and the field is one richly deserving the attention of the ethnologist.

It is one of the merits of Grosse's book that it does not attempt too many conclusions from rather scanty material. One point at least becomes evident; primitive art in most of its phases does not serve primarily an æsthetic end; it is first of all practical, and the purely æsthetic result is, so to speak, a by product. In music alone as a rule does the æsthetic appear as the single end in view. For the rest of the numberless questions suggested one can only refer to the book itself.

Herr Grosse is entitled to the greatest credit for what is in the opinion of the writer the most important contribution to this subject in many years.

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L'Année psychologique. 2e année, 1895. H. BEAUNIS and A. BINET. Paris, Alcan, 1896. Pp. 1010.

The second volume of the *Année* presents a decided advance over its predecessor. The plan adopted at the outset included three distinct parts: original articles, summaries of important books and articles appearing during the year, and an annual bibliography of all publications of interest to psychologists. The same general scheme is adhered to in the present volume, but we find several noticeable changes in details. A larger number of articles are summarized, and the summaries themselves are more in keeping, as regards length, with the value and interest of the works. Under the head of original articles are included a number of 'general reviews,' which add greatly to the value of the *Année*. In the same section, too, the work of the independent contributors is now separated from the more specialized studies of the Paris Laboratory. The fact that the volume has increased in a year from six hundred pages to over a thousand is in itself an indication of the proportions which the enterprise has assumed.

To the outsider this rapid expansion cannot appear as an unmixed blessing. A volume of the present size is not over easy to handle; if the number of original contributions should be further increased (they are still comparatively few), it might actually become unwieldy. We may ask whether, after all, the plan laid down is not too complex to be carried out as a single undertaking. Examination shows that the volume includes two distinct lines of work, which might readily be separated. The first is a general résumé and bibliography of the past year's work in psychology (Parts II. and III.); the second is the collection of original contributions (Part I.). Are these two departments equally well carried out? We think not. The 'Jahresbericht' is conceived and carried out on a magnificent scale. To compare favorably with it the original portion should consist of some of the very best work of the best French writers on psychology. Without wishing to cast a shade of disparagement upon the writers who have contributed to the volume, we are forced to say that the contents fall considerably below this standard. Aside from the general reviews—which are only 'original' in a limited sense—but two or three of the papers are complete or of permanent separate value. As a rule, they are rather studies, very good in their place, but scarcely in keeping with the broad purposes of the *Année*. If the writers would contribute their *best work* it might be well to retain this feature, but as matters stand at present it would seem wiser either to dispense with it or else to transform its character completely.

Another side of the same question appears when we come to the Studies of the Paris Laboratory. Does the *Année* aim to be the organ of that institution? If so, it ought, we think, to gather in a larger proportion of the Studies that are at present scattered about in various periodicals. If not, why fill its pages with material of an obviously fragmentary character? At present the *Année* is neither fish nor fowl—or better, it may be likened to a splendid fowl, hampered and made less beautiful by the presence of a fish's tail!

If we may venture a word of advice, then, it is as follows: The *Année* should be divided into two volumes, one of which, under another name, might be made the organ of the Paris Laboratory, with other contributions if desired. The *Année* proper could then be restricted to an oversight of the year's work in the various branches of psychology, with greater latitude in the case of 'general reviews.' The introduction of the latter into the present volume is a step in the right direction; with other matter cut out, their number and scope might gradually be enlarged. These changes would give to the *Année* a unity of purpose which it now sadly lacks, and would transform it at once into an encyclopedic work of classic importance.

What has been said above has reference to the appropriateness of a certain class of writings to the *Année*, and is not intended to reflect in any way upon the value of the articles that appear in this particular number. We shall now proceed to examine the contents of the present volume.

Of foremost importance is Dr. A. Forel's paper on the methods of comparative psychology.[1] The author frankly acknowledges his scepticism regarding the value of the results obtained by direct psychological induction. The human mind differs too radically, he thinks, from that of the lower orders, to admit of carrying over to the latter with any degree of assurance the results obtained in the former. When we consider the difficulty in mankind itself of understanding the psychological constitution of individuals differing from ourselves in social grade, intellectual status, or sex, how much more reluctant should we be to assume mental analogies in the case of beings wholly different from mankind in physiological structure!

The author cites in support of his position the case of the social insects—in particular, ants. There is a marked tendency among all writers to describe the mental processes of these insects in terms of our own. But is this comparison warranted? Take the sphere of sensation, for example: the data of the various senses differ not only directly, but also indirectly; the eye gives us accurate notions of space relations, the ear furnishes us with those of time. Both of these senses are well developed in man; in insects the most highly developed sense is that of smell. In man this latter sense gives (explicitly) neither spatial nor temporal data; but in insects it is evidently capable of furnishing 'distinct and rational perceptions' of some sort (p. 44). The sense of smell must then be radically different in insects from what it is in man. Thus we meet, at the very outset, an insuperable obstacle to the direct use of induction in comparative psychology.

[1] *Un aperçu de psychologie comparée.*

Passing to the biological problem, which he believes to lie at the root of comparative psychology, M. Forel traces the phylogenetic growth of the nervous system from the original neuron. For the wave of nerve activity, whether chemical or physical in character, the author proposes the name ***neurocyme.*** The action of neurocyme is comparatively simple within the compass of a single neuron; but when it is called upon to pass from one neuron to another the method of transmission changes: there is now a mass of terminal fibers instead of a single line of conduction. Such an alteration in the mode of transmission, the author argues, must entail a modification in the form of activity—inhibiting it, strengthening it, or causing it to be acted upon by other waves. In this 'interneuronary action of neurocyme,' at present so incomprehensible, is contained, says the author, 'the secret of our mental mechanism' (p. 27).

Up to this point the nerve phenomena are alike for all biological species; but as we proceed further we meet with a distinction. Instinct and reason denote a fundamental antithesis in the realm of mental action. To these correspond, in the physiological sphere, two distinct modes of activity, which the author terms the ***automatic*** and ***plastic activity of neurocyme***, respectively. These are constantly in conflict with each other, and the type of an organism depends upon which has gained the mastery in its race history. Among social insects the automatic activity is well developed, and the neural coördinations are maintained by a long heredity strictly within the same lines, so that the adaptive, or plastic, activity is crushed out. Plastic activity requires far greater complexity of structure than automatic; and hence the brain of the ant, remarkable though it must be considered, is far less wonderful in its complexity than that of a human being. The distinction between automatic and plastic activity, then, is really the key to the situation, and it is only through studying the facts connected with these physiological phenomena that we can reach a proper basis for comparative psychology.

We give M. Forel's views somewhat at length, because they seem deserving rather of attention than of criticism. It is an undoubted fact that psychology is to-day leaning for support more than ever on physiology. Whether psychologists will go so far as wholly to subordinate comparative psychology to comparative physiology, in the way he proposes, we very much doubt. At the same time there is no question but that their own inductions have been too hasty, and that considerable reconstruction of the bases of comparative psychology is necessary. The fact that the critic is a student of biology as well as a psychologist certainly lends additional weight to his conclusions.

A fitting companion-piece to Forel's article is Dr. Azoulay's review of recent theories on the mode of function of the central nervous system.[1] Those who are not familiar with the recent work in the histology of the nervous system will find here a compact *résumé* of the present status of that branch. In a few pages the writer details briefly the state of our knowledge regarding the anatomy of the neuron since the late discoveries of Ramon y Cajal and others. He then proceeds to explain the theories of nerve action which have been founded on these facts. Though fair in his exposition of all, the writer shows apparently no leaning toward any of the theories; he seems personally to prefer a modification of the older view, which held to the activity of the entire nerve—now expressed in terms of the individual neuron. The style of this article is remarkably clear, and it is easily within the grasp of those whose biological knowledge is extremely limited.

Individual, abnormal and child psychology are each represented in the *Année* by a single article. *La psychologie individuelle*, by MM. Binet and Henri, is an original contribution placed (rather inappropriately) among the general reviews. It is a plea for the wider development of anthropological tests, which have hitherto been confined almost exclusively to sensation. Citing the results of Lombroso, Galton and others, the authors conclude that the differences existing among normal individuals in the sphere of the senses 'are very feeble and insignificant compared with the differences in the higher faculties' (p. 416). In all such tests of normal individuals there are two principal objects in view: first, to compare individuals and discover what elements vary and how far; and second, to trace the relations that exist between the different faculties of each individual. Both ends can be attained by a single series of representative tests, if the same series be applied everywhere. The authors examine the series proposed by various writers, and find them all incomplete and more or less impracticable; moreover, they are not fairly representative, since all neglect too much the higher intellectual processes. The real object of these inquiries being to determine not all, but merely the most important individual differences, the writers propose a series of ten tests, from which sensation measurements are omitted entirely. They include memory, the nature of mental images, imagination, attention, understanding, suggestibility, æsthetic sensibility, moral sense, muscular power and will power, and quickness of movement and of glance. These tests are described fully in the latter part of the article.

[1] *Psychologie histologique et texture du système nerveux: les récentes théories du fonctionnement du système nerveux central.*

M. Th. Ribot's *mémoire* on abnormal and morbid character[1] is suggestive rather than complete. The author discards the historic four-fold division of temperament, and adopts the three-fold classification proposed by Seeland—into strong or positive, neutral, and weak or negative, each including some sub-types. Abnormality of character consists in the union of two or more of these in the same individual. There are three cases. The first consists in the complete transformation of the individual at some period of his life. This type approaches most nearly to the normal. Paul, Augustine, Diocletian and others are given as examples. In the second we find two opposite tendencies present at once in the same person. The third is represented by great instability of character and a rapid alternation between conflicting tendencies. This is the true pathological type.

In an article on *Fear among Children*[2], Prof. Binet gives the results of a series of questions circulated among some 100 school teachers and others. He finds five principal classes of phenomena with which fear is associated: 1. Night, solitude and mystery. 2. Loud noises. 3. Objects which inspire repugnance. 4. A possible danger exaggerated by the imagination. 5. A past experience whose recurrence is dreaded. The state of the child's health is always an important factor in determining his liability to fear; on the other hand, there appears to be no relation between fear and the degree of the child's intelligence, except in so far as a highly developed imagination is more liable to furnish objects for fear. Prof. Binet notices further the effect of heredity and ill-treatment, and alludes to the well-known fact that fear is contagious. The signs of fear begin to be manifest at the age of two or three, and increase till the ninth year, when they begin to come under control, and the emotion itself tends in normal cases to be suppressed. Some of the replies are conflicting: the proportion of children susceptible to fear is variously estimated, and M. Binet's own deduction (10%) is admittedly a mere assumption. It is scarcely within our province to speak of the author's remarks on the pedagogic treatment of fear in children, but what he says may be recommended to those interested in that subject as both timely and instructive.

Along the line of experimental psychology a number of contributions appear in the *Année*. Prof. Th. Flournoy describes a new treatment of association time. In a list of 24 words, 12 belonged to some well-defined class, while the remainder had no conceptual relation with one another. Given two such lists, the subject was asked to

[1] *Les caractères anormaux et morbides.*
[2] *La peur chez les enfants.*

read in the one case all the A's, in the other all the non-A's. The time of the latter reading was considerably longer. M. Bourdon gives a variation of an old experiment on the comparative frequency of various kinds of association, and M. Xilliez brings forward a method for calculating the influence of the ordinary serial association of numbers upon our memory of a list of figures chosen at random. Prof. Van Biervliet adds a chapter to the recently developed literature on illusions of weight.

M. Victor Henri's two articles on tactile localization may be classed together as a single monograph. In an original contribution, the author describes a series of experiments which substantiate his view that the exactness of localization on the skin is independent of the exactness of two-point discrimination. Taking a number of normal subjects, he finds that the errors of localization are large out of all proportion with the sensory circles; in many cases an impact on one finger was assigned to a closely symmetrical position on another. The paper on the Sense of Locality on the Skin (*Sur le sens du lieu de la peau*) is a review of the work along the same line from Weber down. Though its outline is somewhat influenced by the author's position, just referred to, it is in every respect typical of what a general review in a work like the *Année* should be. M. Henri reproduces tables of figures from the more important authorities, which enable us to compare the results obtained by different methods of research. At the close of the article is a bibliography of 156 titles. The author promises next year a review of the theoretical side of tactile localization.

Owing to the poverty of the data, M. Passy's review of investigations on the olfactory sense is necessarily less extensive. He takes up successively the physiology of smell, olfactometry, the properties of odors, their compounds, and the reaction time for smell, giving in each case a *résumé* of the principal results so far obtained. He neglects to furnish a bibliography of the subject; but the works actually cited are put in reference form in the footnotes. In an appendix, M. Passy sums up the results of an experimental investigation by Prof. Binet and himself on the comparative psychology of smell.

In the department of physiology, MM. Binet and Courtier contribute an article entitled *Circulation capillaire de la main*. They use the graphic method to investigate the relation of respiration, etc., to circulation. The work includes experiments on a number of problems; the tracings, many of which are given, show the changes in form of the respiration and pulse curves due to different positions of the hand and to various physiological and mental disturbances. The writers discuss at some length the errors incident to different kinds of apparatus, and the best means of avoiding them. The study is long and exhaustive, and the authors promise further researches on several additional points necessary to render it complete. The principal conclusion reached is that 'there exist, in respect to the excitability of the vaso-motor system, important individual differences' (p. 164); these differences are too great to be attributed to the apparatus, and too constant to be due to the disturbing effect of such an experiment upon the emotions of untried subjects.

Our space will permit only a passing reference to the remaining contents. M. Henri gives a *résumé* of the well-known mathemetical methods employed in the calculation of probability and error. MM. Binet and Courtier describe an apparatus for recording the intensity of impact with one or more fingers in piano playing. M. E. Gley compares the physiology of hypnotism with the action of stimulants and narcotics, and concludes that all these effects are attributable to a paralysis of the higher centers, rather than to exhaustion of the entire nervous system.

In the analytic portion of the volume the summaries are generally limited to two or three pages. More extended notices (of ten pages or more) are allotted to Delage's book: *La structure du protoplasma*, Exner's *Entwurf*, Merkel's articles in the Philosophische Studien on *Reiz und Empfindung*, and Baldwin's book on *Mental Development*.

The general bibliography at the end of the volume is this year, by arrangement, identical with that compiled for THE PSYCHOLOGICAL REVIEW.

H. C. WARREN.

PRINCETON UNIVERSITY.

A Study in the Psychology of Religious Phenomena. Parts I. and II. JAMES H. LEUBA, Fellow in Psychology, Clark University. The American Journal of Psychology, Vol. VII. Pp. 309–385.

Mr. Leuba happily avoids the common blunder of attempting to frame a definition of religion which will cover all that the word connotes. He recognizes that it was 'in early societies a complex product made up of all the fundamental needs and aspirations of man,' many of which are now clearly differentiated and are known by their several names. The noetic impulse was one of these, but not the chief one and consequently the essence of religion survives many changes of creed. Even the belief in a supersensible world and personal immortality may pass away without affecting it, for they are not of its essence. It is not based upon a theory of life of any sort, but upon one of the most universal facts of human experience: "the feeling of unwholeness, of moral imperfection, of sin, accompanied by the yearning after the peace of unity.......The reality of this subjective treasure transcends all possible belief concerning the origin and end of things, because it is the psychic correspondent of a physiological growth, and consequently can in no wise fail except with that growth. ......It may be defined in the favorite terms of Herbert Spencer as the unification by coördination of the parts segregated by differentiation of the homogeneous."

This sense of inner discord is the fundamental postulate of the religious consciousness and its resolution into harmony is its end. In the popular religions of our day we find these truths crystallized in the doctrines of sin, conversion, justification, regeneration and reconciliation with God. But our popular religious faiths are declining to their fall, and these are facts of the inner life; it is then high time for psychology 'to accept the succession which falls to it by right.' If it would, "a new creed would be born; the wings of youth would no longer be clipped in the spring of life by a scholastic dogmatism and the soul midwifery now extensively but ignorantly practised by our revivalists and pastors could be based upon a positive knowledge of the psychology of regeneration.' This, Mr. Leuba thinks, 'is the sure conquest of a near future.' May it come within the days of the present writer's life! He would journey many miles to see 'soul-midwifery' practiced in the well appointed laboratories at Worcester, and souls regenerated in accordance with the sound principles of the new psychology, without reference to the conceptions of God and immortality!

Mr. Leuba's hope that the psychologist will assume the function of the spiritual teacher will seem, as he admits, 'a fantastic dream' to many besides myself, but that does not affect the solid worth of his inquiry into the phenomena of conversion. He has collected and published in an appendix seventeen new cases at first hand, and has searched religious literature for others. Upon this material he bases his analysis: the leading stages of conversion are conviction of sin, self surrender, faith, joy and appearance of newness, especially in external nature. The phenomena are nearly constant in all ages and countries and among men of all creeds. The Christian doctrines of Justification, Faith, Grace and Depravity are attempts to formulate some intelligible theory of the basal facts of the religious consciousness. The most interesting outcome of this analysis is the exhibition of the passive attitude of the convert. The guerilla warfare which his fundamental instincts and tendencies constantly wage with one another has developed into a formal battle between two groups for final supremacy over his life, while his will is in abeyance and his accredited beliefs stand in the background.

Mr. Leuba's analysis stops short at a most interesting point. One wishes to see these facts brought under more general conceptions and that he has promised to do in Part III. of his monograph, shortly to be published. It will include 'a genetic theory of sin, of moral resistance, of consent, of self-surrender,' and will especially endeavor to bring to view their possible physiological correlates.

WILLIAM ROMAINE NEWBOLD.
UNIVERSITY OF PENNSYLVANIA.

A New Factor in Evolution. J. MARK BALDWIN. American Naturalist, XXX., 441–457, 536–554, June and July, 1896.

Professor Baldwin has here summarized, enlarged and unified several of his recent papers, especially those printed in *Science* (August 23, 1895, March 20, April 10, 1896). It appears that when Professor Lloyd Morgan was in America last winter he, Professor Baldwin and Professor Osborn found that they had independently reached somewhat similar conclusions regarding certain relations of ontogeny and phylogeny or to use Huxley's distinction and avoid technical words—between *development* (of the individual) and *evolution* (of the animal series). As we all know, the biological questions most eagerly discussed at present are those concerned with the inheritance of acquired characters and the causes of the variations which have resulted in evolution. Professor Baldwin has approached the problem from a new standpoint, and has, I think, formulated ideas which have hitherto had somewhat shadowy contours.

An individual can adapt itself to new conditions. For example, a carnivorous animal, such as a dog, can live on cereals. It learns new habits, and certain adaptations take place in its digestive mechanism. Now if flesh were permanently withheld from the race of dogs those individuals would get on best whose congenital variations fitted them to live on cereals, and these variations being hereditary we might get ultimately a race of graniverous dogs. It would look as though the effect of use in the individual had been inherited, whereas this need not really be the case. We only have individual adaptations preceding in time race adaptations. It is thus possible that many of the cases quoted by Neo-Lamarckians as examples of use-inheritance are invalid.

Professor Baldwin applies this principle, which he calls 'organic selection,' especially to adaptations in which consciousness is concerned. If conscious guidance can produce useful adaptations in the individual organism, these adaptations may become hereditary in the manner described above, and we have the course of evolution directed by consciousness, but without the need of assuming the hereditary transmission of acquired characters.

The clear statement of the fact that new traits may appear first as individual adaptations, and later through the occurrence of suitable congenital variations as hereditary modifications, is important. I am not quite sure how far it may be found in earlier writers. Darwin holds that the taste of the female, an individual trait, modifies organic evolution, and it is the essence of natural selection that under changed environment those individuals will survive who can best adapt themselves to it. If organic selection is itself a congenital variation, as Professor Baldwin indicates, we are still in the *status quo* of chance variations and natural selection. We have not found 'a new factor in evolution,' still less as Professor Osborn claims (cf. *Science* April 3, 1896) 'a mode of evolution requiring neither natural selection nor the inheritance of acquired characters.' We remain ignorant as to why the individual makes suitable adaptations, why congenital variations occur in the line of evolution and why they are hereditary.

Professor Baldwin's paper is by no means confined to this one point, 'organic selection,' 'social heredity,' 'circular reactions,' etc., are commingled in a manner that will prove confusing to many readers. Indeed, I venture to say that I find the author's vigorous thinking too often obscure to an unfortunate degree. For example, I am not sure whether or not Professor Baldwin claims in this paper that the principle of 'organic selection' is set forth in his book on *Mental Development*, nor does my memory after a careful reading of the book enable me to decide the question. Or to take a more serious problem, I do not understand whether or not Professor Baldwin wishes to use consciousness—pleasure, pain, intelligence, etc.—as a *vera causa* in individual adaptations. The average reader will take it for granted that he does, and I admit that it seems to me that he runs with the hare and hunts with the hounds.

J. McKeen Cattell.

Columbia University.

VISION.

Oscillations rétiniennes consécutives à l'impression luminense. AUG. CHARPENTIER. Comptes Rendus. 13 Jan. 1891.
Nouvelle forme de réaction négative sur la rétine. id. 27 Jan.
La réaction négative et la centre de la rétine. id. 3 Fév.
Strosboscopie rétinienne. id. 10 Fév.
Irradiation ondulatoire de l'impression luminense. id. 17 Fév.

M. Charpentier pointed out, some five years ago, that the starting up of a light-process in the retina is followed at a very brief interval by a process of the opposite character, that is, by something which causes an instantaneous sensation of extreme blackness. Under favorable conditions there are several alternations, less striking than the first one, of light and dark, before the continuous sensation of a bright surface establishes itself. So marked is the first sensation of blackness that it has been named the *black band*, when it is formed just after the advancing border of a white sector upon a rotating wheel. The whole penomenon is referred to as *retinal oscillations*. In the series of papers in the *Comptes Rendus* whose titles are given above, M. Charpentier discusses both this subject and the 'recurrent image.' The latter was first noticed by C. A. Young, and has been best studied by Shelford Bidwell. According to Charpentier, it is not well named; it is not properly called an image, for it does not always reproduce the shape and size of the luminous object which it follows; if that is feeble in intensity, its ghost is smaller; if that is very bright, the ghost may be five or six times as large as its original. Moreover, what has been seen by many observers as a definite recurrent image is in fact merely a maximum phase of a sensation which, in its waxing and waning, lasts for a considerable time. But as M. Charpentier does not propose another name, we shall continue to use the one which has become somewhat familiar.

His method for producing the phenomenon consists in a rotating black disc with a window in it which constitutes the moving luminous object. This window is lighted up by a piece of ground glass which is itself illuminated by rays which have passed through a plano-convex lens and which come from a source of light the intensity of which can be regulated. Thus the rapidity of the moving object, its intensity and its size can all be varied at pleasure. When these conditions are all happily chosen (the author does not state what they should be, except that a single revolution of the disc should take place in from one to three seconds) the bright object is followed first by a very black interval and then by a re-vivescence of itself, usually of no definite color, but bluish when the preceding light is very feeble, and of a greenish-yellow color after blue. Some have seen it only after blue, in which case it is in fact always most distinct. Charpentier finds it after all colors; others have failed to find it after red, but Charpentier used red glass and Shelford Bidwell succeeded, with an ingenious arrangement, in using spectral light. Red glass, while it is red in a very different sense from that in which any other colored glass is of its color, is usually rather more yellowish than the extreme limit of the spectrum. The black interval may have a duration of one-fourth of a second if it follows a feeble and short excitation; otherwise it lasts one thirty-sixth of a second. Here also an oscillation may be seen under favorable circumstances. These oscillations in sensation at the beginning and at the end of an excitation by light are very suggestive of the oscillations in the direction of the electrical current which are the objective effect of the action of light upon the retina.

There is an additional observation upon the black band to the effect that it may still be detected when the preliminary excitation is so short that there is no white surface for it to appear upon; it may then be seen as a band of extreme blackness *upon the recurrent image*. This would seem to be equivalent to saying that the negative reaction of the shock of the impinging light is so strong as to mask the negative reaction of the shock caused by sudden darkness. To show this it is only necessary to make a very narrow window in the revolving disk; one degree is a convenient width.

Since there is a negative reaction at the beginning and at the end of an excitation, it seemed possible that a sudden change of intensity would produce the same effect, and this was found to be the case.

The black band had been found to propagate itself beyond the place on the retina which had been effected by the original excitation, in two directions and with a definite velocity, which had been calculated. With the arrangement just described it is very easy to exhibit this phenomenon; the persistent image has attached to it a larger or smaller luminous zone of diffuse light, and (after a very short preliminary excitation) two black streamers may be seen upon this, one proceeding towards the fixation point and the other in the opposite direction, the latter resembling the tail of a comet, with its convexity turned in the direction of the movement, the other being perhaps slightly concave in the same direction. They both begin to appear at the same moment with the black band, and on either side of it; they consist, therefore, of a propagation of this negative reaction in a definite direction and with a definite velocity, a velocity of about 77 mm. a second upon the retina. It is believed that these streamers also exhibit oscillations.

By a stroboscopic method, the oscillations are found to take place at the constant rate of 36 or 37 a second, for a mean intensity of the illumination; if the intensity is much greater or much less, the rate may be from 40 to 34 per second. Another circumstance brought out is that the diffuse spot surrounding the recurrent image changes its shape, becoming sometimes more circular and sometimes more elliptical, and that this change of shape has also a rhythm corresponding to that of the successive black bands. The subject is extremely interesting. It is to be hoped that these new observations will be confirmed and extended.

C. L. FRANKLIN.

BALTIMORE, MD.

Light Intensity and Depth Perception. T. R. ROBINSON. Am. Jour. of Psych., VII., 518–532. 1895.

Admit to one eye less light than is admitted to the other; then

I. a: If but little light is excluded from the second eye, to close that eye will darken the total field, to increase its light will brighten it;

b: If more light is initially excluded, an 'indifference point' (referred to below as limit A) is reached where increase or decrease of the light admitted to the second eye produces no effect on the brightness of the combined field (Robinson, in a previous article);

c: Starting with the initial proportion below the indifference point, to close the second eye produces the same effect as to increase its light; *i. e.*, a decrease of intensity of physical stimulus results in an increase of intensity of sensation (Fechner's paradox). This effect increases from the indifference point downward, until at a certain degree of obscuration of the second eye occurs the maximum darkening of the common visual field, hence a maximum brightening upon closing it or increasing its light (Aubert's minimum point; referred to below as limit B).

II. a: If the eyes are directed by lenses to separate fields, upon which are drawn figures for stereoscopic combination, complete stereoscopic combination occurs down to a certain degree of obscuration of the second eye (called limit C below);

b: With greater obscuration, the combination is only partial, or confused, down to a certain second limit (limit D);

c: Below this second limit no stereoscopic combination occurs, but only a binocular combination of the two fields, where the objects combine as a single surface, the lines of each being distinct, with no depth effect perceptible.

Robinson points out the facts under II., and attempts to determine, for different intensities of the total light admitted to the free eye, the proportion of this which must be admitted to the second eye at limits A, C and D; the relation of C and D to A and B; and the causes of the results found.

He establishes the following facts: The amount of light required for the second eye to produce the stereoscopic effect is, especially with high intensities, very small, and it varies with the absolute intensity. There is a considerable range between the lowest point where the objects combine (limit D), and the point where the complete stereoscopic effect is obtained (limit C). At high intensities, C is $\frac{1}{100}$ or less, D is too small for measurement; at lowest intensities, C is about $1/2$, D is $\frac{1}{8}$ to $1/3$.

The amount of light for the second eye inefficient for the total brightness (limit A) corresponds to the amount required for the stereoscopic effect (C) only at a very low intensities; at higher intensities it is much greater. It varies with the intensity and the observer from $\frac{1}{8}$ to $2/3$.

The minimum point of Aubert (established by him as .122 of the full light) corresponds to the limit D only at lowest intensities. The coincidence here may be accidental, or it may be that Aubert's measurement of B cannot be relied on as applicable for all intensities, and that the apparent non-coincidence at higher intensities may be thus explained.

To account for these facts, Robinson supposes an intimate coöperation of the two retinas, such that where one retina is not stimulated sufficiently to enable it to play its part in bringing about the binocular combination its energy may be supplemented by that of the other. Then the greater the amount of light admitted to the free eye, the greater will be the energy which can be spared by it to supplement that of the partially darkened eye, and consequently the smaller the proportion of light required in the second eye for the binocular combination. For complete stereoscopic combination, however, the free eye cannot aid the other, hence much more light must be admitted to the second eye to produce stereoscopic than to produce binocular combination. When part of the energy is subtracted from the free eye to aid in the binocular combination the common visual field is darkened and the paradox produced; and this is true, both at low intensities, where the free eye cannot give enough energy to produce the complete stereoscopic effect, and where A and C coincide, and also at higher intensities, where complete stereoscopic effect is produced while yet the paradox remains, C being much below A, because the free eye still supplies some of the energy for the binocular combination, and the common field is darkened.

BROWN UNIVERSITY. E. B. DELABARRE.

LOCALIZATION OF TOUCH.

Ueber Raumwahrnehmungen im Gebiete des Tastsinnes. CHARLES HUBBARD JUDD. Philos. Studien. XII Band, 3 Heft, p. 409.

These experiments investigate our threshold judgments of the separateness of points, of direction and of continuous lines. They comprise four series. In the first a pointed bone needle (diameter not given) was set upon the skin for three seconds, then raised and placed upon the same or a neighboring point. The results show the minimal distance at which correct judgments were given of the *direction* of the second from the first point—*i. e.*, whether up, down, left or right. The second series was like the first, save that two needles were used; the first was applied, then after three seconds the other needle was applied to a neighboring point without the first being removed from its place. In the third series tests were made with lines, from 1 to 50 mm. long, cut from thin cardboard and set on the skin in four directions—vertical, horizontal and the two diagonals at 45°—the subject *to say if he felt a line or a point.* For the fourth series solid card-edges like the above were used, together with others from which the card was cut away so as to give two end points (1 mm. long), thus leaving an 'empty' distance to be compared with the 'filled' distances of the other cards; the subject *to say if he felt a point* (below threshold for twoness), *a line, or two points.*

The results of the fourth series, when compared with each other, show: (1) That the threshold distance for judging the direction of two points is less when the needles are applied successively (0.70 cm.) than when simultaneously (2.64 cm.) (2) Unfortunately the distance is not given at which the points appeared merely separate; yet it is declared—apparently a pure assumption, although probably correct—that the threshold for separateness is also less for successive than for simultaneous applications. (3) The thresholds obtained by the second series were greater than those by the first, but less than those for wholly simultaneous application. (4) The threshold distance for judging a line not to be a point (0.88 cm.) is greater than that for 'direction' by successive stimulation, and less than 'direction' by simultaneous points. (5) Threshold for 'lines' is same for vertical as for horizontal, but greater for diagonals than for either. Only one locality was investigated throughout the four series—the volar side of arm between wrist and elbow. Five subjects. Method, that of Minimal Change.

Theoretically the author claims to show that Weber made a fundamental error in not distinguishing between judgments of 'Distanz' and of 'Grösse'. Precisely what is meant by the last term is not made very plain, but perhaps the two could be translated as judgments of 'twoness' and of 'extension,' or of 'number,' and of 'space.' He inclines to found both upon Lotze's 'local signs', but is as classically vague as Lotze himself regarding what this definitely means.

The work is not without technical faults (for example, the tests upon each person were far to few), the results can scarcely be called new, and the paper is not weighty—save in bulk. It fills 55 pages and should have been put in 5.

CAMBRIDGE, MASS. HERBERT NICHOLS.

MEMORY.

Mémoire et Reconnaissance. H. BERGSON. Revue Philosophique. XLI, 225—248; 380—399. March, April, 1896.

M. Bergson begins by distinguishing memory as habit, the organized mechanism by which we repeat, for instance, something learned by rote, and pure memory, the memory of a particular event, say the third reading. The latter he regards as an independent function, and the sharpness of this dissociation and the insistance on this independence are the characteristic features of his discussion. Memory, as ordinarily treated, is a compound, habit illumined by memory; in reality, the past is retained not only in the form of a sensori-motor habit but also in the form of particular images with all its details localized in time. In the present articles, which are part of a forthcoming work in which a full treatment is promised, an attempt is made to illustrate the independence of the memory function by showing the part it plays in the phenomena of recognition. The sense of familiarity is held, with great probability (see especially p. 241), to rest primarily, not on association of the perception with an image, but on an organized motor reaction. Where the recognition is inattentive, it completes itself in useful movements which, though inhibiting the play of the imagination, may nevertheless be accompanied by the appropriate images selected by means of the nascent movements habitually connected with them. But in active attention the recognition is completed, not by useful movements, but by a further defining of the object. Here the intervention of images is more pronounced. M. Bergson represents the process as follows: In the first place, attention, commonly regarded as an attitude of consciousness, is, as recent discussion has shown, consciousness of a bodily attitude. The general features of the perception are determined by movements set up by the perception, its special features by images from past experience. The selection of these images is due to successive movements of imitation. The process is compared to that of a telegraph operator who controls a message by re-transmitting it. The successive attempts at analysis of the object are thus at the same time attempts at the synthesis with it of images resembling it. These movements serve as the common *cadre* both for the perception and the images. Looked at in this way, the essential element in recognitive perception is held to be a centrifugal process. It is thus—by movements from within outwards—that the images are incorporated in the perception. There are no special centres for images: what are regarded as such are merely centres for grouping sensorial impressions; but there are in the brain substance 'organs of virtual perception influenced by the intention of the reminiscence (*l'intention du souvenir*), just as at the periphery there are organs of real perception influenced by the action of the object' (397 *n*). M. Bergson devotes the whole of his second article to illustrating this theory from the perception of language and particuarly from the facts of sensorial aphasia. He shows thorough familiarity with the literature of the subject and makes skilful use of it, drawing from the very sources which have been held to furnish the strongest evidence that the memory has no existence, except as a cerebral trace, independently of the act in which it is localized, precisely the opposite conclusion. The discussion contains valuable matter, and is certainly convincing in two points: the difficulty of localizing the images and the importance for the recognition of words of their motor accompaniments. Also to be commended is the attempt to be true to the dynamic quality of consciousness by exhibiting the elements in the process of recognitive perception as connected by imperceptible transitions and as supporting one another in a function; the process is compared to an electric circuit, in which all the elements are in mutual tension. On the other hand, the conception of the memory itself, influencing by its 'intention' (whatever that may mean), the centres of virtual perception, is obscure; the assumption of a sensory efferent process transmitting the images to the periphery (245) is a very questionable mode of interpreting the value of the motor side of the process; and quite unproved by any of the facts here adduced—though something more may be expected from the volume—is the assertion (239) of the actual existence of a memory containing every detail of the past life in all the particularity of its temporal setting, a conception which strongly savors of the 'subliminal consciousness' of so-called psychical research.

SMITH COLLEGE. H. N. GARDINER.

On Muscular Memory. THEODATE L. SMITH. American Journal of Psychology, Vol. VII., pp. 453–490.

This paper is divided into two parts, to the second of which the title more particularly applies. In the first part the motor element present in the process of memorizing of nonsense series syllables is studied; in the second 'muscle memory proper, *i. e.*, memory of movements.'

In the first set of experiments series of ten syllables were presented to the subject by means of an automatic shutter, the time of exposure of each series being 20 seconds; at the close of the exposure the subject repeated aloud as many of the syllables as he could remember. The experimental conditions were kept as uniform as possible and observations were made in all the experiments of the physical and mental condition of the subject. In order to bring to light the muscular element involved in memory of this kind, the results gained where the subject was undisturbed in memorizing were compared with those gained when the subject had to repeat 'one, two, three' continuously during the experiment. The value of the memory under the two sets of conditions was tested by enumerating the number of errors committed by the subject in the process of reproducing the syllables, the errors being classified under three heads, as (1) displaced, (2) wrong, (3) forgotten syllables.

The general result was that there were more errors when the subject counted 'one, two, three' than when he read undisturbed, the in crease of error varying from 12.6 per cent. with one reagent to 17.7 with another; there were no marked differences in the proportion of the three classes of error under the two different sets of conditions. That the increase of error was due to disturbance of the motor processes, which are present when memorizing is undisturbed, and not to distraction of the attention, was rendered highly probable by another set of experiments. It was found that the memory of all the five subjects was improved when they read the syllables aloud during the act of memorizing.

In the second part of the research the printed characters of the manual alphabet were employed, the characters being formed into series of five and ten, and presented to the subjects in the way described above. In the first group of experiments the subject merely saw the characters; in the second he imitated the characters muscularly in addition; in both cases he was required at the close of the memorizing to reproduce the characters muscularly. The addition of the motor imitation was found with all the seven subjects to cause a diminution of the number of errors (classified as in the first group), the diminution varying from 10.5 per cent. to 20.7 per cent.[1] In a third group the subject was required to count 'one, two, three,' while learning visually as in the first group. The result of this modification was, in general, to diminish the errors and to make the numerical values more constant.

As the author remarks at the close of the paper, the experiments give no exact measurement of motor memory; the investigation in fact consists in the comparison of the memory in cases where the motor element is more prominent with the memory where it is less prominent. But this admission does not deprive such experiments of their value. Every attempt to give a more definite statement of the nature and extent of the motor function in mental life is to be welcomed.

SMITH COLLEGE. W. G. SMITH.

SYNOPSIA.

Entstehung und Bedeutung der Synopsien. RICHARD HENNIG. Zeitsch. f. Psychol., X., 183–222.

One will search in vain for anything new in Hennig's discussion of synopsia. The paper contains definition and classification, following closely upon the lines of Flournoy, who is frequently quoted. References to the work of other writers are very incomplete, but the illustrated descriptions of the writer's forms are given with elaborate detail which is sometimes wearisome and which seems unnecessary in the present state of our acquaintance with the subject. The most valuable part of the paper consists in the facts which it brings to bear upon the disputed question of the 'psychological origin' of synopsia. Hennig believes that many instances of 'colored hearing,' and that all 'forms' occur through personal experiences of their possessors, dating back so far in childhood that they are naturally often forgotten. A possible source of such forms is suggested by Hennig's account of his own number form which follows the line of the houses on a very irregular street of his childhood acquaintance; these houses had interested him

[1] The percentage for the subject J. P. H. should be 11.1 instead of 22.2.

chiefly through their numbers. His form reproduces not only the line of the houses, but the characteristic lights and shades of the street. Hennig also expresses very unequivocally his belief that forms are of great utility, giving at length an account of the experience of a friend who consciously refers his unusual memory for dates to elaborate mental forms.

WELLESLEY COLLEGE. MARY WHITON CALKINS.

PSYCHICAL RESEARCH.

Address by the President before the Society for Psychical Research. WILLIAM JAMES. Proc. Soc. for Psych. Research, XII., 2–10, June, 1896. Science, III., 882–888, June 19, 1896.

The Society for the Psychical Research is fortunate in its leaders. The strongest argument it can offer in behalf of the phenomena it investigates seems to me not the anecdotes and other evidence it has been able to collect, but the fact that men such as Professor James and Professor Sidgwick take an interest in these things and are partly or wholly convinced of their importance.

The presidential address of Professor James, admirably written as a matter of course, reviews the work and claims of the Society with skill and moderation. He finds that the hypnotic wave has subsided and that experimental thought transference has yielded a less abundant return than at first seemed likely. But he thinks that solid progress has been made by the report on the Census of Hallucinations and in the investigation of clairvoyance. Ghosts also should not be ignored. "Though the evidence be flimsy in spots, collectively it may nevertheless carry heavy weight." It is 'a faggot not a chain.' This, however, is an argument that can be turned both ways. When we have an enormous number of cases, and cannot find among them all a single one that is quite conclusive, the very number of cases may be interpreted as an index of the weakness of the evidence. The discovery of a great many gray crows would not prove that any crows are white, rather the more crows we examine and find to be black or gray, the less expectation have we of finding one that is white.

The 'faggot' argument, intended for the 'rigorously scientific' disbeliever, will not be so likely to affect him as the fact that Professor James has found in Mrs. Piper his 'own white crow.' This is an argument difficult to answer except by referring to the continuity of history, which, as the author says, is maintained by the Society. The ablest of men have followed alchemy and astrology, have worshiped strange gods, have consulted witches and burned them. Geese have before now been mistaken for swans, and often to the honor of those who made the mistake. One white crow is enough, but its skin should be deposited in a museum.

COLUMBIA UNIVERSITY J. MCKEEN CATTELL.

THE EMOTIONS.

Character and the Emotions. ALEXANDER F. SHAND. Mind, N. S., V, 203–226. April, 1896.

The first part of Mr. Shand's article deals with the method and problem of Ethology, the idea of which, as a science, seems first to have been formulated by Mill, but the beginnings of which have perhaps only recently been made by psychologists in France. Ethology is regarded by Mr. Shand as a special psychological science, the statical part of which is concerned with the classification of types and circumstances and the dynamical part with the more difficult 'deduction' of types—their genesis and development and the changes produced in them by circumstances. A 'type' is broadly conceived, not merely as a dominant tendency, but as a complex of qualities possessing inner psychical connection: which connection it is the business of the science to show. And the classification of types must take account not only of the strength of tendencies, but also of the degree of their association, of the rapidity and relative persistence of the mental processes and of the intensity of the feelings of pleasure and pain connected with the emotions and sentiments: all which determinations the analysis has to render precise. The classification of circumstances, which, relative to character, are, strictly, an abstraction, must follow their 'objective and universal meaning'. This is insisted on as a principle, but no suggestions are given as to how the principle would be carried out. It may be said, however, that a complete inventory of circumstances, in the sense required by a science of types, as distinct from the biography of individuals, is not, theoretically, impossible.

The remainder of the essay is an interesting contribution to the theory and classification of the feelings. The point of cardinal importance is the distinction between the emotions and the sentiments. Emotions and sentiments differ, not primarily in respect of intensity, but in regard to growth of organization. The sentiments are highly organized habits; the emotions are relatively isolated and simple. The latter, however, tend to develop into more stable and complex feelings and to build themselves, into modifying and modified by, the sentiments.

The sentiments are substantival, the emotions adjectival; the sentiments relate to relatively permanent objects, the emotions to events. This thought is skilfully worked out, it being shown how love for an object gives rise, under varying circumstances, to a large number of emotions; how the converse effects are produced when the object is one of hatred or dislike, and how new modifications are introduced, new emotions and sentiments developed, where the object of regard is another human being or a lower animal or one's self. The classification of the feelings follows, then, the degree or character of their organization. First come the relatively unorganized feelings, including certain emotions, all the appetites and the pleasures and pains of sense. All these may, however, form one of the two subdivisions of the organized feelings, the other being the sentiments and interests. The principle for the classification of an emotion is thus its function in the sentiment. The classification of the sentiments is a more difficult matter, and its consideration is deferred. It is to be hoped that Mr. Shand, who shows fine talent for psychology of the analytic sort and writes well, intends by this to develop the essay here presented into a larger Prolegomena to Ethology. The conception of the organization of the emotions in the sentiments, the matter of special interest to the general psychologist, has in it that quality of pursuasiveness that, once grasped, it seems to have been one's own thought always.

SMITH COLLEGE. H. N. GARDINER.

EPISTEMOLOGY.

Wirklichkeitsstandpunkt. DR. RUDOLF WEINMANN. Verlag v. L. Voss, Hamburg und Leipzig. 1896. Pp. 37.

This little book, which the author calls an 'epistemological sketch,' is in three parts. Part I. is entitled 'Orientierung.' The author gives a brief statement of two fundamental ideas of Kantism which he calls 'subjective realism.' Kantism is realistic because it recognizes a world of objective reality independent of our consciousness, and which lies at the basis of our phenomenal world. It is subjective because it makes an absolute cleft between this world of 'things in themselves' and our world of phenomena. The author next affirms a realistic position. He says that out of the original psychic state, the simple 'da sind,' unfolds the world of objects independent of the subject, and also the world of ideas belonging to the *ego*. This distinction, he says, is based on the one hand, on the immediate experience or feeling of being compelled by objects in forming representations, and on the other hand on that of the mastery over objects. This, he claims, leads to a realistic position. Realism is next contrasted with positivism and idealism, but space does not permit us to give the discussion of these points. The conclusion is that we must accept realism, and the question is as to whether or not we are to accept it in Kant's subjective form.

This leads to Part II. which is entitled 'Apriorität und Subjectivismus.' In the first section, space, time, and causality are discussed, and Kant's position stated. Then follow some arguments against Kantism. His doctrine of the 'thing in itself' is shown to involve a contradiction, since if it is a really unknown 'x' we cannot determine it negatively by denying of it space, time, and causality. The author goes on to say that the Kantian claim that the *a priori* character of these ideas involves also their subjectivity, is a mere assumption, and then he gives some arguments for the objectivity of these categories. First, the fact that we are utterly unable to represent a world without these ideas, shows that they are most real and in fact the necessity attaching to them is just as strong as that of Kant's postulates of the practical reason. Second, the fact that our knowing consciousness is in the closest relation to a part of the objective world, viz., our body, would lead us to believe that there is a harmony between the organ of knowledge and its object. Third, the doctrine of development would indicate a close relation between the subject and the object of knowledge, and shows that our psyche has arisen in dependence on the outer world, and is therefore formed to know this world. Weinmann concludes that notwithstanding all this, the *a priori* character of these categories remains untouched, only it is merely a relative *a priori*, and if our representation of the world is conditioned by our organization, this in turn is and has been conditioned by the world.

In the second section of this part the author takes up the secondary qualities, and criticizes some arguments which have been advanced for their subjectivity. It is claimed by some that we know only the effects of things on us and not the things, and that the effects have no likeness to the things. Weinmann says that the upholders of this argument have fallen into materialistic conceptions. The argument could have force only if the process in the brain was that which is given in sensation. This, however, is not the case, and the sensations which supervene upon the nervous process are to be regarded as ideal reproductions of the outer occurrence. It is no absurdity then to say that this reproduction is adequate, and in fact the burden of proof lies with him who denies this. Further, the doctrine of 'specific energies' has been urged in support of the theory of the subjectivity of our sensations.

This doctrine, however, is physiological, and proves nothing for epistemology. Arguments from the *a priori* nature of our various senses, and from physics are also discussed, but must be omitted here. The author concludes that our sensations are subjective in the sense that they refer directly to our own body and indirectly to the outer world of which they are ideal reproductions. Thus, he says, we get for our general epistemological standpoint, a 'dualistic realism,' in which we have the world in space and time, and subject to the law of causality, and on the other hand the consciousnesses which have been developed in dependence on these categories of their environment, and which, therefore, bring them as *a priori* forms for the cognition of this world.

Part III is entitled 'Wirklichkeitsstandpunkt.' In this part the author states certain advantages of his position, which constitute, he claims, an indirect argument for it. He says that we escape the doctrine of 'chance' in reference to the relation of subject and object, and also the Kantian 'thing-in-itself.' We are also in touch with common sense and physical science, while the various philosophical disciplines, such as psychology, ethics, etc., presuppose the realistic standpoint. Even idealistic metaphysics in its greatest representatives holds that the given world is real, while the doctrine of the 'thing-in-itself' baffles all interpretation. The author closes by endeavoring to set aside two methodological objections.

PRINCETON. C. W. HODGE.

Zur Psychologie der Metaphysik. RUDOLF LEHMANN. Archiv f. system. Philosophie. Bd. II., 38–70. 1896.

The *origin* of metaphysics—the reference throughout is to dogmatic metaphysics—is to be found in two impulses, one intellectual, the impulse to solve the problems left unsolved by the special sciences; the other emotional, the impulse to get rid of certain misgivings and to justify the expression of certain natural tendencies. The first is but a special form of the general cognitive impulse and takes characteristically its rise in the contradictions involved in the fundamental conceptions of the empirical sciences; the essentially æsthetic impulse to systematic totality also leads, when strong, to speculations which seek to overcome the incompleteness of their systems. The emotional impulse, however, is the more primitive and controling. The phenomena by which it is awakened are such contrasts as life and death, natural law and will, altruistic and egoistic impulses, contrasts which are connected with the deepest interests of human life and form the really vital subjects of speculation. The *conceptions* of metaphysics are all constructed on analogies of experience. Here, too, the experiences which furnish the analogies are either intellectual or emotional. The first supply the rationalistic, the second the mystical elements. In illustration of the first, the author refers to the influence of conceptions derived from the observation of nature on the metaphysics of the Ionians, Democritus, the French materialists, Schelling (magnetism), Hartmann (biological phenomena), and Spencer; to the influence of mathematical conceptions on the Pythagoreans and Spinoza; of logical on Plato and Hegel; of psychological on Empedocles, Fichte, Schelling (theosophical period) and Schopenhauer. But of greater influence than all these scientific conceptions are religious ideas, especially the idea of a personal God which supplies the analogy for every system of metaphysical teleology. From religion also come the mystical elements, the basal root of which is the sexual instinct, a truth which Plato finely recognized in his doctrine of the philosophical Eros.

SMITH COLLEGE. H. N. GARDINER.

NEW BOOKS.

An Outline of Psychology. EDWARD BRADFORD TITCHENER, New York and London, The Macmillan Co., 1896. Pp. xiv+352.

Herbart's A B C of Sense-perception and Minor Pedagogical Work. Translated, with introduction, notes and commentary, by WILLIAM J. ECKOFF. New York, D. Appleton & Co., 1896. Pp. xv+288.

Schopenhauer's System in its Philosophical Significance. WILLIAM CALDWELL. New York, Charles Scribner's Sons, 1896. Pp. xviii+538. $3.00.

The Child, Its Spiritual Significance. HENRY KING LEWIS. London and New York, The Macmillan Co., 1896. Pp. viii+222. $2.00.

L' Education intellectuelle des le berceau. BERNARD PÉREZ. Paris, Alcan, 1896. Pp. 340. 5 fr.

Manuel pratique des méthodes d' enseignement spéciales aux enfants anormaux. HAMON DU FOUGERAY and L. COUËTOUX. Paris, Alcan, 1896. Pp. xv+288.

Recherches cliniques et therapeutiques sur l' épilepsie, l' hystérie et l' idiotie. DR. BOURNEVILLE. Paris, Alcan, 1896. Pp. lxxi+250.

NOTES.

THE third International Congress of Psychology met in Munich from the third to the seventh of August. Of the 174 papers announced in advance, the following were presented before the general sessions, in addition to an address of welcome by Prof. C. Stumpf, the President: 'Pain,' by Charles Richet; 'Criminal Responsibility,' by Franz von Liszt; 'On the Localization of the Emotions,' by Guiseppi Sergi; 'On the Association Centers of the Brain, with Anatomical Demonstrations,' by Paul Flechsig; 'The Theory of Sensation,' by Franz Brentano; 'The Psychology of Genius,' by Frederic W. H. Myers; 'A Genetic Study of Primitive Emotion,' by G. Stanley Hall; 'A New Method of Testing Mental Ability and its Application to School Children,' by Herm. Ebbinghaus; 'Individual Psychology,' by Alfred Binet; 'On Memory for Sensations,' by W. von Tschisch, 'The Conception of the Unconscious in Psychology,' by Th. Lipps. We hope to give a full account of the Congress in the November number of this REVIEW.

WE are glad to note that experimental psychology has been included by the International Bibliographical Conference in London among the fifteen leading sciences to be catalogued.

THE chair of mental philosophy and logic established sometime since in the University of Cambridge has never been filled, owing to lack of endowment. £700 annually has now been appropriated for the chair, £200 of which is due to generosity of Prof. Sidgwick, and it is expected that a professor will soon be appointed.

PROF. E. B. TITCHENER, of Cornell University, will translate into English Wundt's *Physiologische Psychologie* and in coöperation with Mr. W. B. Pillsbury Külpe's *Einleitung in der Philosophie.* Miss Julia H. Gulliver, of Rockford College, will translate Wundt's *Ethik.*

THE University of Chicago has laid the corner stones of four Biological Buildings, the cost of which is to be defrayed by the $1,000,000 given by Miss Culver to the Biological Department. The Laboratory of Psychology will be located in the Anatomical Building, which will also include the work in neurology under Professor, now Head Professor, Donaldson.

MR. G. F. STOUT, Fellow of St. John's College, Cambridge, and editor of *Mind* has been appointed to the Anderson lectureship on comparative psychology, recently founded at Aberdeen.

DR. C. v. EHRENFELS, of Munich, has been appointed assistant professor of philosophy in the University of Prague.

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THE PSYCHOLOGICAL REVIEW.

THE THIRD INTERNATIONAL CONGRESS OF PSYCHOLOGY.

BY DR. EDWARD FRANKLIN BUCHNER.

Yale University.

It is very significant that psychology can claim the attention of an international gathering. Being a science, the most subjective and individual—at least in its subject-matter and fundamental method—it is semi-paradoxical that it should attain this high degree of objectivity. One could see ethnologists, philologists, jurists, sociologists, epistemologists and pedagogues along with anatomists, zoölogists, physiologists and pathologists, mingling with *Psychologen von Fach*, offering their contributions towards the fuller knowledge of mental phenomena. In the words of the genial and energetic president, "the apt title 'Congress of Psychology,' signifies that there is a welcome extended to every one who communicates or discusses any fact, whatsoever, standing in relation to psychology, in a manner instructive for psychological study."

The history of psychological congresses shows a decided and gratifying growth in the interest for this science. The first international congress in Paris, 1889, under the presidency of M. Ribot, carried the title 'Congress of Physiological Psychology.' Its chief occupation was the study of hypnotic phenomena and telepathic hallucinations. The second congress in London, 1892, under the presidency of Professor Sidgwick, showed by its title, 'Congress of Experimental Psychology,' that a wider circle of phenomena was to be regarded from the inductive method of investigation. The congresses of 'Rational Psychology,' and of 'Experimental Psychology in Education,' under the auspices of the 'International Congress of Education of the World's Columbian Exposition,' Chicago, 1893, form a chapter in this account, although the controlling theme was chiefly pedagogical. The 'Third International Congress of Psychology,' which sat in Münich, from August 4th to 7th, 1896, under the presidency of Professor Stumpf, of Berlin, revealed a further development. The wide interest in things psychological is indicated by the membership of over five hundred, of whom more than four-fifths were present at the opening session. The value of the sessions, and the contribution of the congress for the science of psychology, may be richly found in the one hundred and seventy-four addresses announced, which were arranged for three general sessions and five sections, the latter being as follows: 'Anatomy and Physiology of the Brain, Physiology and Psychology of the Senses, Psycho-Physics,' 'Psychology of the Normal Individual,' 'Psycho-Pathology and Criminal Psychology,' 'Psychology of Sleep, Dreams, Hypnotic and Related Phenomena,' and 'Comparative and Pedagogical Psychology.'

Graced by the presence of Dr. Prince Ludwig Ferdinand and Princess Therese, of the royal house of Bavaria, the Congress opened with the 'Eröffnungsrede' of the president, Professor Stumpf, of Berlin, in the great *aula* of the royal university. In sketching the previous congresses, the speaker referred to the new title of the present one, which was ascribed to the suggestions of the Executive Committee. "The adjective 'experimental' appears to me always necessary as against certain ratiocinative, abstract, deductive tendencies which have not entirely died out in Germany. While omitting the adjective 'experimental,' the Committee do not wish to deny its right. We agree that the necessity of experiments is almost universally admitted, and that herein is dependent the avoidance of all appearance of one-sidedness, and harmonious promotion of all tendencies. The disciples and friends of the new psychology are bound together by a common conviction as to method: the decisive importance which all of us attribute to the increase and refinement of our knowledge of facts. By refinement, I mean especially the treatment of enumeration. Where one has, heretofore, been satisfied with indefinite quantitative designations, we will now count and measure, just as far as it is possible."

Having expressed the most general principles of the methods 'which, in spite of divergencies, bind us in unity,' the speaker gave "utterance also to the most general convictions as to fact; and to what other question can these be attached than to the relation of mind and body, of the psychical and physical? The effort of every epoch concentrates itself in the attempt to win a satisfactory solution for this question determining the entire view of the world. If we all are agreed that the relation to the physical realm penetrates our entire mental life, and, as we make daily progress in the knowledge of the details of this relation, then it will be quite possible to find a more accurate formula, in which our common views as to the nature of that relation may be expressed.

"Fechner, the founder of psycho-physics, influenced by the speculative theories of Spinoza and Schelling, defended the monistic theory, according to which psychological and physiological processes are really one and the same process, body and mind being only the external and internal modes of the appearance of one being. Unfortunately, as everything else in the world, this two-sided theory has its two sides; it is magnificent, poetical, enticing—but dark. The unitary substance, which should 'express' itself in both physical and psychical attributes, is nothing but a word, which only expresses the necessity of escaping dualism, without actually bridging the chasm for our understanding.

"At the present time, it is the custom to repel such turnings and comparisons, and to regard the secret of the relation as inextricable, handing it over to metaphysics (which generally means the same thing), while maintaining, on the contrary, that the processes in both series are parallel throughout, without acting upon each other or uniting themselves in reciprocal action. There are two forms of the theory of parallelism. According to one view, the physical series is causally bound together, while the psychical possesses no causality in itself, as little as shadows or images act upon each other. 'Consciousness in itself is absolutely nothing!' The second view regards the psychical series as an unbroken, causally-connected development.

"But this doctrine of parallelism is the old dualism once more, which has never appeared under a coarser form. In opposition to this we must ask the question whether the consequences of the investigation of nature, and especially the doctrine of evolution, does not impel us to conceive the world as a totality, causally connected in all its parts, in which everything performs its task, none being excluded from the general reciprocal action. It may be inquired still farther, whether the grounds on which the total psychical world should be excluded from the actual, or from the universal reciprocity, are so constraining as they appear to many. Hume pointed out that cause and effect need not be homogeneous. Experience can only teach what belongs to each other as cause and effect. The law of the conservation of energy is in agreement, since it is a law of transformation. Certain psychical functions are united with a continuous consumption of physical energy, others, likewise, with a continuing production (of such energy). So far as I can see, a psycho-physical mechanics is conceivable, which sets the spiritual processes in a universal, lawful, causal connection, and thereby establishes a monistic view in the true meaning. For, it is not so much the similarity of elements or processes as the universality of the causal connection and the unity of the last and highest law which we must demand of a unitary world.

"For those who are thus not satisfied, another way remains open in which to set the physical in the universal causal connection without violating the law of energy. We might say, a definite nerve process in a definite region of the cerebral cortex is the indispensable pre-condition of the rise of a definite sensation. This follows out of the neural process as a necessary sequence along with the physical effect (so much to distinguish it from the theory of parallelism). But this part of the sequence absorbs no physical energy, and its relations to the conditions cannot be expressed by mathematical concepts and laws.

"I may briefly allude to a shifting of the entire question, which attempts to clear away the difficulties more radically by pointing out the separation of the two realms as a mistake from the very beginning. The physical may be only the sum of sensations or ideas of our mind, as, on the other hand, the mental life may arise out of sensuous representations. Hence, one can not speak of a difference between the two realms. I bow respectfully before the epistemological height which here discloses itself, but refer to the fact that even from this point of view one group of sensuous representations, which possess mathematical-physical properties, is distinguished from another group which does not possess them. I cannot discover that dualism is really overthrown by this so-called epistemological monism. It only changes the position.

"In the future we shall continue to regard our sense perceptions as the effects of the external world, and our wills as the cause of our actions, without being compelled to look upon this manner of expression, which obtrudes itself upon the ordinary consciousness, as a figure of speech. Since the time of Descartes and Spinoza investigations on body and mind have attained extraordinary precision. The philosophical analysis of the concepts of substance and causality, the discovery of the law of energy, the rise of psycho-physics, the victorious permeation of the theory of evolution, the progress of the anatomy and physiology of the central organs, especially the investigations on the localization of mental activities—all has contributed to dissecting the one question which lay before us in a lump. It is our one problem to remove every tendency to dogmatic stiffness, and not, as the common man, speak most easily and confidently about things most difficult."

This critical review of the culminating psychological problem opened the labors of the Congress, whose members were welcomed, in the name of the royal government of Bavaria, by his Excellency, Ritter von Landmann, on behalf of the city of Münich by Vice-Mayor Brunner, and to the royal university by Professor von Baur, Rector *magnificus*, whose greeting was especially cordial towards the members from America.

In the polished address, 'Etude biologique sur la douleur,' Professor Richet, Paris, presented views which are quite different from those which he put forth in 1877. "Considering pain as a chapter in experimental physiology, the first question which presents itself is this: What are the nervous excitations which produce pain? The electrical stimulus, of which the intensity can be gradually increased, is employed. From the point of view of our sensibility there are three phases in the excitation: the first phase, in which the stimulus is too weak to call forth any sensation; the second phase, in which there is painless sensation; the third phase, in which the sensation is painful. With all other kinds of stimuli upon the various senses, we find exactly these three phases. The normal state of the nerve is in a certain mechanical, electrical, chemical, and thermic condition. Pain is produced by all causes which profoundly modify the state of the nerve. Not only in the case of peripheral excitations, but internal stimuli of an organic or pathological sort also produce pain when the excitation reaches a certain stage. The local effect of a strong excitation is always the same. It is a disorganization of the nerve, and the impossibility for this organ to perform its normal function during a certain length of time. As a guard against disorganizing and destructive excitation, all organisms have two kinds of defense, the immediate defense, which is reflex action, and the subjective defense, which is pain. From a first superficial examination it appears that pain has no utility. There is a vast number of lower organisms which defend themselves by reflexes, without any knowledge of pain. But, besides the defensive reflexes, the higher organisms perform a specal reaction, absolutely subjective, which is pain.

"The persistence in memory of a painful excitation is one of the fundamental characteristics of pain. We wish to insist on this one of the essential conditions of pain, which is its duration. The scholastic axiom, *sublata causa, tollitur effectus*, is absolutely false. We forget pleasures much more easily than pains. We are organized in such a way that we fly from all causes of the destruction or perversion of our tissues. Pain is considered as the supreme evil, and thus the function of pain is that of utility in accordance with the end of nature. I wish to make a plain confession. It is this: the principle of finality, which formerly appeared very ridiculous to me, seems to me to-day, after long reflection, absolutely necessary in physiology. The purpose of nature is to keep alive the greatest number of beings, the longest time possible.

"Pain may well be regarded as one of the bases of intelligence, since it is the memory of pain which rules the conduct of beings which are more than pure automata. In the case of man, each pain modifies his psychical structure, forcing him to reflect. It is by pain, as much as by sensation that we appreciate the external world; our conduct is immediately modified by the pain which external objects have provoked. A single perception does not have an intimate influence on our sensibility, but a painful sensation provokes an emotion which continues a long time, and exercises a great influence on us by its force and the vivacity of its persistence."

An opportunity, where the scientific study of abnormal mental phenomena may be of aid to the jurist, a field which has so far been almost entirely overlooked by psychologists, was shown by the critical address, 'Die Strafrechtliche Zurechnungsfähigkeit,' by Professor von Liszt, Halle a. S. Starting with the definition: 'Accountability is the capacity of being punished for past actions,' the speaker proposed the question, 'How must this state of mind be constituted?'

The penal codes of the present attempt to answer the problem in various ways. While the oldest group emphasize the freedom of the will, another finds the accountability in the intellectual factor, *viz.*, insight into the consequences of the deed, and a third limit themselves to the enumeration of the circumstances through whose presence responsibility is excluded. The German penal code adopts the first position, against which the criticism was offered that the 'right of punishment, as every other form of right, must remain removed from the unending discussion on the freedom of the will.' The 'intellectual moment,' also is insufficient, since the individual may, indeed, distinguish right from wrong, and, at the same time be abnormal in feeling and volition. 'The mind of the criminal must be conceived of as a unity, a totality.' Accountability can be regarded only as the normal (not free) determinableness through motives, and is conditioned by mental maturity. While insisting, in spite of this position, that the irresponsible, habitual criminal should be punished, the speaker only made a concession to the ruling ethical judgments. Without giving a positive determination of accountability, the lecture closed hoping to find the agreement of psychologists with the view that 'a punishable deed is not present when the actor, at the time of the performance of the action, was found in a condition of unconsciousness, of morbid checking or impairment of mental activity.'

The chief interest during the Congress was incited at the opening of the general session on Wednesday, by the address 'Ueber die Associationscentren des menschlichen Gehirns,' in which Professor Flechsig, Leipzig, presented anew, with demonstration, the results of investigations which are already somewhat known.

After a rapid survey of the development of the theory of localization of mental functions in the cerebral cortex, he defined his relations to his predecessors, especially Munk and Hitzig, and, in opposition to the clinical, pathological method, described his own as the historical method of anatomical development, in so far as it traces the growth of the nerves successively appearing in a normal way. "The various kinds of tracts which enter into relation with the cortex do not arise simultaneously. But few medullary fibres are found in the cerebral bundles of the ripe fœtus. The first to develop are the sense tracts, the centripetal nerves, which unite the peripheral organs and the organs within the body, with the cerebral cortex. The anatomical method traces this early development of the sense tracts in the fœtus and newly born much more clearly and sharply than any other. The peripheral organs are not united at the same time with the cortex. Their order is rather that of a series, which is started by the development of the tracts which unite the posterior roots of the cord and their continuing nerves. They may be called the 'bodily-feeling' nerves, and contain fibres serving the organic functions of pain, hunger, thirst, etc., and those which contribute to a feeling knowledge of the body. The first impressions which the cortex receives are conducted by these bodily-sense nerves, from which it is seen that the consciousness of the body precedes that of the external world.

"The olfactory tract appears about the same time. Considerably later the optic tract develops, and is found already sheathed to the cortex in the mature fœtus. Finally the auditory tract appears, but only the portion of it which is connected with the cochlea, and, more particularly, only that part which is imbedded in the cerebral lobe.

"The following fundamental propositions may be formulated respecting the extent and arrangement of the cortical sense centers, the 'sense zones' of the cerebrum: 1. In man these zones fill about one-third of the cortex. 2. They do not present a *continuum*, but are separated from each other by cortical circuits in which neither sense nor motor tracts appear. 3. They form four distinct spheres of varying extent; bodily-feeling (which includes the tactile center), olfactory, optical and auditory centers. (A particular gustatory center cannot be pointed out. It unites with the first or second.)

"The continuation of the posterior roots collects in a region which is in the middle of the total cerebral cortex, particularly about the central fissure. The olfactory fibres enter the basal region of the brain, partly in the frontal lobe (reaching to the *gyrus fornicatus*), partly in the temporal lobe. The optical tracts end in the region of the occipital lobe which is especially marked off by the fissure *calarina*. The auditory tract enters the first temporal convolution, especially its two roots, and lies concealed in the depths of the fissure of Sylvius. These conclusions, based entirely on the method of anatomical development, are brilliantly confirmed by the evidence of cerebral pathology.

"Comparing the finer structure of the cortical centers, it is discovered that the chemical senses, at least, possess a special structure, conditioned by the appearance of peculiarly formed cells, and a special arrangement of the layers of ganglion cells.

"All the motor tracts of the cortex proceed from sense centers. By far the greatest number take their rise in the bodily-feeling center, while scarcely one-fifth arise in the field of audition. As respects the sensory functions of these centers, it is undoubtedly true that the destruction of a center in the cerebrum puts an end to the corresponding peripheral sensations.

"Can the newly born associate the perceptions of the various centers? In reference to the anatomical condition discovered in the infant's brain, this question is to be answered negatively. The cortical fields of the special senses are almost completely destitute of tracts which bind them together. There are wide regions between those fields, in which matured, medullated fibres are absolutely wanting. Single, scanty fibres, which appear to be developed sufficiently to transfer an excitation from one sphere to another, run only between the olfactory and bodily-feeling centers. The infant has, presumably, a great number of separated circles of consciousness, corresponding to various sense centers.

"What is the significance of this great complex of undeveloped regions between the centers of sense? Following the anatomical development of the cerebral tracts, we secure a satisfactory explanation as to the function of these blank, intermediate regions. As early as the second month after birth, a multitude of medullated fibres begin to appear, pushing out from the sense centers into the intermediate regions, to be lost in the cortex. With the farther growth of the infant, millions of such associational fibers stream into these formerly blank regions. Each center is the starting point of innumerable associational systems which meet, in the convolutions, like systems springing from the other centers. With reference to these anatomical facts, the intermediate regions may be called 'associational centers.' Rather than separating the sense centers from each other, they bind them together—to be sure, only several months after birth and later.

"There can be scarcely any doubt that the fusion of the activity of the various sense centers is a 'higher' mental function than the formation of single sense perceptions. That which we call thinking, first begins with the association of the several sense activities. That neural condition which makes man the psychical being that he is, is given chiefly in his 'associational centers.' The most convincing proof of their relation to those psychological processes, which we designate collectively under the term 'association of ideas,' is furnished by pathological evidences from the impairment of those regions by disease and the consequent mental disturbance.

"Investigations on a basis of anatomical developments show three groups of these centers, which are completely separated from each other. The 'posterior associational center,' which is the largest, lies in the region between the tactile, visual and auditory fields, and partly between the last two and the *gyrus hippocampi*. A considerably smaller region forms the point of the frontal lobe, especially the base, and is the 'anterior center.' The 'central associational center' is the smallest and lies between the others, corresponding exactly with the island of Reil. It is now the problem of pathology to establish the significance of these single regions for the mental processes. Pure and experimental psychology alone cannot accomplish anything here. Many of these functions are already known from the pathological cases, as, *e. g.*, the different forms of *aphasia*, *amnesia*, etc. Many clinical cases show that the knitting of various perceptions and their memory images takes place in these centers. This combining is, presumably, a consequence of specially extensive groups of cells, whose function consists exclusively in 'associating,' and this is the point where the speaker departs entirely from the usual views as to the mechanism of association, as they have been formed by Meynert, Wernicke and others.

"Since there is no proof that the injury of the associational centers influences sense perceptions, in the restricted meaning of the term, these centers can be said to take a part in perception, in the wider sense, by conducting the memory images to the bare sense impressions. It is highly probable that we are to look for the memory traces of impressions chiefly in the ganglion cells of these centers. The single convolutions of the associational centers are in no wise similarly related. The regions bordering the sense center, which might be called 'marginal zones,' are united with more numerable systems than those farther removed. The collective central regions are united by 'long' associational systems (*fasciculus arcuatus*) with the sphere of bodily-feeling, which may be regarded, from its comprehensive combinations, as the truly central point of the entire cerebrum. Thus only is there an actual unity of psychical mechanism, and not by extensive associational systems which unite the great centers with each other directly. The entire cortex is a powerful, associative organ, in certain fields of which the peripheral tracts stream in, and in which the motor tracts take their rise.

"Is the quality of consciousness, mediated by the sense centers, actually different from that which is released in the associational centers? This is a problem for the future.

"The fields of sensibility and of association are spatially separated; but in the anatomical and functional elements they are so closely connected that a sharp separation between them, in a fully developed organ, is impossible. Without the associational centers it would be absolutely impossible for us to fabricate, into a unitary totality, the information which the several senses give us of one and the same external object." The discussion which followed was the most lively and largely participated in during the entire congress.

In an address, 'Dov' é la Sede della Emozioni,' Professor Sergi, Rome, communicated the results of investigations which were published in a large work, 'Dolore e Piacere, Storia naturale dei Sentimenti, Milano, 1894.' The location of the feelings is not in the brain, in the restricted meaning of the term, where the phenomena of consciousness show themselves, but in the medula. The location of the emotional stimulus, however, is peripheral, since these are only changes in the circulation of the blood, in the food supply and respiration, *etc.*

The light thrown by genetic psychology upon the hidden relations between the somatic and psychical conditions of experience, and upon the profounder philosophical problems of mental life, was traced in 'Die Psychologie des Kindes,' by Professor Preyer, Wiesbaden.

The third and last general session, with which the labors of the congress were ended on August 7th, brought together the following addresses, whose interest lay not alone in the variety of themes and treatment: 'Zur Lehre von der Empfindung,' by Dr. Brentano, formerly Professor of Philosophy, Vienna; 'L'influence somnambulique et le besoin de direction,' by Professor Pierre Janet, Paris; 'Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkin dern,' by Professor Ebbinghaus, Breslau; 'Ueber das Gedächtniss für Sinneswahrnehmungen,' by Professor von Tschisch, Dorpat; and, 'Der Begriff des Unbewussten in der Psychologie,' by Professor Lipps, Münich.

Almost every phase of theoretical and applied psychology was considered in one or more of the scores of *Vorträge*, which were assigned to the several comprehensive sections. The presentation and discussion of this varied material occupied the four sessions for each section. Among many others of interest and importance was the address by Dr. Ehrenfels, Vienna, 'Ueber ethische Werthgefühle,' which critically modified the fundamental tenets of Utilitarianism, in its attempt to explain the ethical feelings of approbation and disapprobation from the knowledge of utility or harm. Whether the social ethics of Utilitarianism offers a satisfactory explanation of individual ethics is a remaining question. The discussion of 'Psycho-physische Principienfragen,' by Dr. Cornelius, Münich, developed a set of propositions which do not agree with Müller's 'psycho-physical axioms,' but essentially simplifies psycho-physical theories by the adoption of certain assumptions.

The demonstration by Professor Sommer, Giessen, of a skillfully arranged apparatus for registering the finer bodily movements, *e. g.*, of the hand, which are three dimensional, upon a surface, excited no little interest. The sensitiveness of this 'micro-motorgraph' approaches the so-called mind reading. The members of the congress highly appreciated the successful demonstration of the 'Röntgen rays' by Professor Graetz. The exhibition of psychological apparatus showed that the 'new' psychology is rapidly enlarging its equipment, and increased one's faith in the importance of the dexterous psychological mechanic.

The ultimate value of the congress for psychological science remains to be seen. The scientific consciousness was intensely astir, but the patient sifting of facts and theories can only come later. That psychology possesses such a grand army of investigators is the *Hauptsache*. Their coming together sends abroad the inspiration of the elbow-touch. The plans of the Executive Committee, in connection with the labors of the contributors, could not be excelled, either in spirit or scope. The pusillanimity of prejudice was conspicuously absent. More liberality could not be shown than that all facts and every theory were given a hearing. Struggling with the supreme psychological problem, with which the president launched the Congress, it might be said that the tendency of its labors was against that ideal subjectivism which has, for the most part, been the historical product in every philosophical age. There was a tempering mindfulness that scientific psychology must bravely meet the problems growing in its own soil, before turning them over either to physics or philosophy.

The magnificent entertainment provided for the members of the Congress has endeared the city of Münich in the psychological hearts of many nations. The reception given by the city of Münich in the hall of the old *Rathhaus*, the visit to the brewery '*Zum Spaten*' under the guidance of the cordial proprietors, Sedlmayr Brothers, and the special rendering of 'Don Giovanni' in the Royal Theatre, were social *fêtes* unchecked by the unfavorable weather which set aside other efforts to stimulate pleasurable psychoses in the psychological guests. To these were added the enjoyment of the scientific and art collections thrown open by the state to the members of the Congress.

The official labors of the Committee modified the organization to a certain extent, the details of which had not been fully completed. As officers of the fourth international Congress, Professor Ribot was elected President, Professor Richet, Vice-President, and Professor Janet, Secretary, all of Paris, where the session will be held in 1900, during the international exposition.

[It is hoped that many more of the papers presented at the Congress can be noted in these pages when the 'Proceedings' of the sessions reach us.—EDS.]

RICHARD AVENARIUS.

BY J. KODIS.

Chicago.

On the 18th of last August Professor Richard Avenarius of Zurich died after a long and painful illness affecting both heart and lungs. He was still a middle-aged man, but he destroyed his health through the enormous mental effort which he made to raise modern philosophy from its present passive state to the high rank of the science of sciences, capable of directing, as of old, the thoughts and actions of humanity. In Richard Avenarius philosophy loses one of its most sincere, most devoted students, whose whole life was a sacrifice on the altar of science. For, having a high ambition, he was one of those few men who are capable of sacrificing small vanities and easy successes to a far purpose, which they do not expect to see realized during their own life. Thus he worked for the future, having for his own share nothing but disappointments and disillusions.

To read the works of Avenarius, and especially his chief book, 'Kritik der reinen Erfahrung,' is not an easy task. His terminology presents an almost insurmountable difficulty for most students of philosophy. In spite of this, Avenarius formed a school—a small one, but composed of men devoted to his ideals. He produced a complete system of philosophy, new methods of investigation of the laws of knowledge, and consequently he grouped around him a number of students, who are working in the field which he explored. The terminology that he used was partly necessary for the denomination of the new phenomena that he pointed out, but partly resulted from the extreme care which he took to prevent all possible changes as well in physiological as in psychological theories; and in consequence of this last-named peculiarity it becomes a real burden to read his books. This was, perhaps, the chief reason why his theory did not have at once a great success. His philosophy is not to be read, but to be studied like a treatise on mathematics or physics. But any one who undertakes this hard work will be sufficiently recompensed by the enormous wealth of ideas, new perspectives and methods, which are contained in this work.

The first philosophical paper that Avenarius published was in 1868. It was an investigation of Spinoza's system: 'Uber die beiden ersten Phasen des Spinosischen Pantheismus.' From the time of his study of the system of this philosopher he maintained the tendency to seek for one single principle in the multiplicity of our experiences. This principle Avenarius believed was to be found in the laws of knowledge. Therefore, it was not an objective but a subjective principle on which he based his monism. Philosophy became for him a means to obtain 'a central position toward the world.' Therefore one strong and closed system of ideas, subordinate one to another, must necessarily result from this point of view.

His next paper, which was published in 1876, 'Philosophie als Denken der Welt, gemass dem principe des kleinsten Kraftmasses,' shows three most important developments:

1. Being brought up in the psychological theories of Herbart, he endeavors to give to the facts discovered in psychical life by Herbart *a biological basis*. He explains the laws of assimilation of the new groups of representations by the older ones, the laws of subordination of notions one to another, etc., by the vital processes of the organism, which processes consist in the preservation, as far as possible, of the state of equilibrium, or, in other words, in the economy of the organism.

2. The *general notions* being formed, according to Avenarius on the same biological principle, he considers them not as entities, but rather as *means directed toward the formation of our knowledge of the world*. In so far as they fulfill this purpose, they are good; when they do not serve this end, they have to be transformed to correspond to our experiences. He undertakes the analyses of some of the notions considered as most fundamental in modern philosophy, such for example, as

notions of substance, matter, 'Ding an sich,' etc., and finds them constructed on a false basis and rather obstructive than helpful to the development of knowledge. The notion of movement and the notion of sensation are alone sufficient to explain all phenomena. This is a kind of objective idealism, which Avenarius afterwards abandoned for realism, conserving always his critical attitude toward the general notions and trying to find the laws of the 'natural history' of their development.

3. The most important point in this paper is the *subordination of psychical phenomena, as a part of life-phenomena, to general mechanical rules.* Therefore it considers the biological fact of self-preservation of living organisms within certain limits as being a case of the law of stability (Beharrungsprincip). The whole psychical life is considered by Avenarius from this point of view, namely, as a function of the self-preservation of the organism, or, in case of its becoming disorganized, as the function of the self-preservation of a partial system of the organism.

'Philosophie als Denken der Welt' was announced to be the prolegomena of a larger work which followed twelve years later, in 1888. This was the 'Kritik der reinen Erfahrung. Avenarius was impressed by the helplessness of the modern idealism, which ends with the affirmation that all we know about the world is only our sensations, *i. e.*, subjective states of mind. On the other hand, he saw that in spite of those negative results of philosophical investigations the sciences increased their discoveries and human life went farther in its development, *based on the belief in the reality of the objective world.* Therefore he came to form the opinion that the theory of knowledge was on the wrong path, and that it was just as capable of a positive development as any other of the sciences, if only it rejected its speculative and rationalistic methods. It had only to limit itself to the facts of knowledge, to investigate them in their relations to each other, their development corresponding to the individual environment, and modifications depending upon processes of the physiological states of the organism. His critique of experience is a supreme effort to found such a science. As it is difficult to characterize his methods in a few words, it is proper to give here, as one example, his theory of the fundamental problem of philosophy, namely, *the theory of reality*. In place of throwing himself immediately into a discussion as to *what is reality*, Avenarius seeks for other states of the human mind, having much in common with this peculiar state, which induces us to ascribe to certain phenomena *the character of reality*. He finds three groups of such characters, namely: the characters of *existence* (which includes the character of reality), the characters of *security* and the characters of *the known* (Bekanntheit). He joins them all under the common name of 'Fidencial-charactere,' which characters he considers as depending upon the exercise of the corresponding nervous processes. He explains this by the following examples:

"The exercized value '*Fatherland*' is for individuals the conception of something '*existing*' in the full sense of the word; and this is more exclusively the case when they spend their lives in the same place; the world at large, of which they have only '*heard*' not being in this respect on the same level with their 'fatherland.' The 'fatherland' is at the same time the '*known*' place of the earth on which the individual feels himself '*sure;*' *i. e.*, the same complex of elements are characterized by '*security*' and by being '*known*.' Therefore, the 'fatherland,' which is something '*known*,' is in addition something '*sure*' for its inhabitants, even when its situation was on the shore of the sea, like Halligen, or at the foot of an unquiet mountain, as formerly were Plurs, Goldañ, and now Elm, etc. What follows may serve as an example of the primordial unity of the three characters:

"The 'known' path and guide, the 'known' guide-book and hotel, the 'known' newspaper and authority are also characterized as 'sure' ones. The money of the fatherland is the 'sure' money, because it is 'known,' and the longer it is 'known' the more 'sure' it is."[1]

Then follows an investigation of the transition of the 'Fidencial-charactere' from the *positive* to the *negative* direction, the change from 'familiarity' into 'strangeness.' Each of the

[1] P. 30 and following, Vol. II.

three characters of this group can go through a line of diminishing values until it passes into a negation.

"'So passes the character of 'existence' (Sein) into 'appearance' (Schein), the character of 'security' into a 'lesser security,' 'the known' (Bekanntheit) into a 'lesser known'; to end in the corresponding negative characters of 'existence,' 'security' or 'the known.' On the way those characters must pass through a point of *indifference*."

Among the examples, given by Avenarius in such abundance as to form in reality sufficient material for a scientific induction of the laws, are the following:

"Those natures whose 'habits of life' are directed toward the continual exercise of 'realism' in the bad sense of this word, *i. e.*, toward seeking for gain and pleasures, or in pursuing a vicious life, or in crawling and pushing (Kricher und Strebertum); such individuals give the maximum 'existential' values (maximale Existential-Werte) to the corresponding mental processes." —(E Werte, in the terminology of Avenarius). "If they are brought to think upon so-called 'ideal' values by their experiences, or through some communication (Mittheilung), the 'existential' difference appears proportional to the exercise. Self-forgetfulness, simple honesty, purely objective devotion, appear to them as 'less true,' 'less real,' 'more apparent,' and further are characterized as 'untrue,' 'unreal,' 'non-existent.'" "So also the type of 'present,' which is the most exercised in life, possesses the strongest 'existential' characteristics; while the 'past' and the 'future' possess, in a degree corresponding to their dependence upon less exercised processes, smaller existential characteristics; the 'present' is the 'existent,' the 'past' is the 'apparent,' which has lost its 'existence.' The 'future' will yet obtain its 'existence.' This is why the Eleatics could say of their 'being' (Sein) that it neither was nor will be, but only *is*, that only the unmovable and eternal being *is*; the 'different,' the 'becoming' (Werdende) and 'passing' is 'apparent.'"

"As an especial case we obtain the expression: This color under ordinary conditions *is* dead-black, but on a white background it *appears* darker."

It is impossible in this place to deal with the whole treatise on the characters of reality. We will add, only as an especially interesting point, that Avenarius considers the *idea* as possessing a character of reality which is in its modifications very near to the zero point, but on the positive side. This plain statement explains in a very simple way some of the most important philosophical misunderstandings.

The theory of knowledge given by Avenarius is, as we can easily see by the above examples, a descriptive one, but at the same time, and, perhaps even because of this fact, it is a *general theory of knowledge*. While formerly every theory of knowledge sought to explain how our thoughts grasp the 'existing world,' and therefore could be considered as a *special theory*, deriving from the individual disposition of the author, the theory of Avenarius gives a description of all those special cases of the 'explanation of the world,' and tries to induce the general laws from these facts of knowledge. The whole work is started from this point of view, that the first things given to the human mind are not abstractions, such as *sensations*, *self-consciousness*, etc., but simply *things* and *ideas*. Therefore *things* and *ideas* should be the starting point of every philosophical investigation of the basis of our knowledge. All that we know consists only of *things* and *ideas* relating to them, and modifications of the latter more or less removed from the facts. Consequently everything in human knowledge is in some way *an experience*, but possessing different degrees of purity. The ideal knowledge is the *pure experience* which contains nothing but elements relative to the facts given by experience. Science possesses already some general notions of this purely experimental character, as, for example, the notion of energy in physics. But it is the tendency of the whole mass of human knowledge to become *pure experience*, in other words, to become in the highest degree adapted to the surrounding world. Humanity represents, in the whole, a kind of ultra-human organism, following the same rules of self-preservation, and consequently of adaptation as any individual organism.

This last statement brings us into the very heart of the theory of Avenarius. He considers that every psychical state in living beings is a result of the self-preservative processes of the organism. Psychical processes accompany the physiological processes of the restoration of equilibrium in living organisms. The elementary physiological processes, consisting of a state of disturbance of the equilibrium of a partial central nervous system, and in restoration of the difference, is called by Avenarius a 'Vital-reihe,' a 'vital train.' Psychical states correlative to these physiological states are called the 'Abhängige Vitalreihe,' 'dependent vital trains.' When the equilibrium of a nervous system is disturbed it is always on account of a difference arising between the nutritive functions of the system and its work. The whole first volume of the theory of experience is devoted to a kind of general biology, describing the laws of the evolution connections, changes, etc., of such 'vital trains' in individual and social organisms. The second volume is a description of the 'dependent vital trains' as single 'psychical states,' and their especial characters, whole trains of thought, and such social products as sciences, religions, ethics, etc., being nothing else than socially developed 'dependent vital trains.' It is the first attempt in modern psychology, so far as I know, to discover the laws of the origin, development and termination *of trains of thought*; the theory of association explaining only (and then not entirely) the origin of thought. It is impossible to give the whole contents of the 'Kritik der reinen Erfahrung,' which furnishes indeed the outlines of a new philosophical science. The few ideas that we can speak of here can give only a very feeble impression of this, the most concise and many-sided philosophical work which has appeared since the time of the great authors of the past.

The little paper 'Weltbegriff,' which followed in 1891 contains most of the things already known to students of the Critique. It is a rather popular exposition of the chief ideas of the Critique, namely, of the critical realism, which consists in the critical and conscious acceptation of the facts first given to a naïve mind, namely, that the world consists of 'things' and 'ideas,' in opposition to those idealistic theories, which consider the world as 'representation' or 'will,' or 'will and representation,' etc. An especially new point in this paper is the theory of '*Introjection*,' by which Avenarius explains the growth and formation of the theory that a fundamental difference exists between the 'inner' and 'outer' experiences. Avenarius does not find in these two kinds of experience any 'incomparability' or any 'fundamental dualism.' The idea of their essential difference has been derived, according to his opinion, from a kind of false materialism, which believed in the enclosure of the soul in the body or in a part of it, and later, in the enclosure of the faculties of the soul in the soul's substance. From this belief sprang the notion that the soul was something enclosed from the 'outer world,' into which enclosure every impression from without could come only through a putting-in, or 'introjection.' The whole modern psychology, psycho-physics and most of the philosophical theories, contain such opinions and therefore serve to strengthen the artificial wall between the 'inner' and 'outer' experiences which makes the sciences of the 'inner world' always more inaccessible to exact methods of investigation, and consequently more sterile.

Besides these chief works and a few short papers published in magazines, Avenarius was founder and editor, for 21 years, of a quarterly very well known in the philosophical world, namely, 'Vierteljahrschrift für die Wissenschaftliche Philosophie.' This magazine was founded by Avenarius and his friends, in order to develop a philosophy which would not be opposed in its chief statements to the final results of science, but would follow the same way of investigation, and conform to the growth of human experience, as the sciences have done. Among the best known authors who contributed to this journal are: Riehl, Goring, Wundt, Laass, Heinze, Windelband, Paulsen and others.

SOME PRELIMINARY EXPERIMENTS ON VISION WITHOUT INVERSION OF THE RETINAL IMAGE.[1]

DR. GEORGEM STRATTON.

University of California

Two important theories of upright vision hold that the inversion of the retinal image is necessary for the perception of things as upright. According to the first, which we may call the projection theory, objects are projected back into space in the directions in which the rays of light fall upon the retina. And the crossing of these lines of direction within the eye requires that if the object is to be projected right side up the retinal image must be inverted. The second theory, which may be termed the eye-movement theory, holds that the movements of the eye and our perception of the direction of such movements are the means by which we judge of the spatial relation of objects in the visual field. Upper and lower, according to this theory, mean positions which require an upward or downward movement of the eye to bring them into clear vision. But an upward movement of the eye brings into clear vision only what lies below the fovea on the retina. So that here too the perception of objects as upright requires that their retinal images be inverted.

The purpose of the experiments, of which only the preliminary ones are here reported, was to throw some light, if possible, on the correctness of this assumption. *Is* the inverted image a necessary condition of our seeing things in an upright position? The method of approaching the problem was to substitute an upright retinal image for the normal inverted one and watch the result.

This was done by binding on the eyes a simple optical con-

[1] Read at the Third International Congress for Psychology, Munich, August, 1896.

trivance constructed on the following principle: If two convex lenses of equal refractive power be placed in a tube at a distance from each other equal to the sum of their focal distances, the eye in looking through the tube sees all things inverted, but in other respects the image remains unchanged. The image cast on the retina is as if the whole field of view had been revolved on the line of sight through an angle of 180°. All light other than that which comes through the lenses must, of course, be carefully excluded by making the instrument fit exactly the inequalities of the face by means of black linings and pads. For if light were permitted to enter the eyes otherwise than through the lenses, the observer would be subjected to both upright and inverted images, and the purity of the experiment would be lost.

The size of the visual field was a matter requiring some care. The size and refractive power of the lenses are the determining factors here, and in the desire to obtain a reasonably large visual field one is tempted to use large thick lenses. But they are soon found to be too heavy to wear on the head for a considerable length of time. I found it best, therefore, to modify the instrument above described, by substituting *two* double convex lenses (placed close together on the same axis line) for each of the lenses in that description. I had thus for each eye a short adjustable tube, and at either end of the tube a pair of good lenses of equal focal length. The instrument by this means gave a clear field of vision with a compass of 45°, and at the same time was light enough to be worn without discomfort.

At first I hoped to use the two eyes together in the experiment; but without automatic convergence of the two tubes the strain in reaching a superposition of the two optic images was found to be too severe. The distress in the eyes made it seem best to experiment on monocular vision alone, which could be done without interfering in the least with the principle or purpose of the research. The lens for the left eye was consequently covered with dull black paper; the eye could then remain open and the disadvantage of bandaging be avoided.

In the preliminary experiment here reported, I bound the instrument on my face at 3 o'clock in the afternoon, and wore it without interruption until 10 o'clock in the evening. The instrument was then removed, with closed eyes; the latter were thoroughly blindfolded, until with closed eyes again the next morning the apparatus was replaced in position. From 9:30 in the morning until about 10 o'clock in the evening of this second day, the instrument was again worn continuously, and then the eyes blindfolded as before. The third day the instrument was worn from 10 o'clock in the morning until noon, and then removed. The time during which the experience under the artificial conditions actually lasted—the total time less that in which the eyes were blindfolded—was therefore about 21½ hours—a time, of course, altogether too short from which to expect very pronounced results in undoing a life-long habit of interpreting visual signs, but which, nevertheless, gave interesting indications of what would result if such an experience were considerably extended.

The time was spent entirely indoors, watching the scene on the street below, watching the movements of my feet and hands, experimenting on the changes which occurred in the visual field in connection with particular movements of the head or of the whole body, grasping and handling seen objects—in short, trying to crowd as varied an experience as possible into the brief time at my disposal.

The course of experience was something as follows: All images at first appeared to be inverted; the room and all in it seemed upside down. The hands when stretched out from below into the visual field seemed to enter from above. Yet although all these images were clear and definite, they did not at first seem to be real things, like the things we see in normal vision, but they seemed to be misplaced, false, or illusory *images* between the observer and the objects or things themselves. For the memory-images brought over from normal vision still continued to be the standard and criterion of reality. The present perceptions were for some time translated involuntarily into the language of normal vision; the present visual perceptions were used simply as signs to determine how and where the object would appear if it could be seen with restored normal vision. Things were thus *seen* in one way and *thought of* in a far different way. This held true also of my body. For the parts of my body were *felt* to lie where they would have appeared had the instrument been removed; they were *seen* to be in another position. But the older tactual and visual localization was still the *real* localization.

All movements of the body at this time were awkward, uncertain, and full of surprises. Only when the movement was made regardless of visual images, by aid of touch and memory alone—as when one moves in the dark—could walking or movements of the hand be performed with reasonable security and directness. Otherwise the movement was a series of errors and attempts at correction, until the limb was finally brought into the desired position in the visual field. The reason for this seems partly to have been that the reconstruction of the visual field in terms of the normal visual experience—the translation before spoken of—was never carried out in all the details of the picture. In general, or in the main outlines, things might be referred to the positions they would have in normal vision, but the new visual field was in many of its details accepted just as found, and was acted upon without any translation whatever. So that when movements were made as if the visual signs meant just what they had meant in normal vision, the movements of course went astray. The limb usually started in the opposite direction from the one really desired. Or when I saw an object near one of my hands and wished to grasp it with that hand, the *other* hand was the one I moved. The mistake was then seen, and by trial, observation, and correction, the desired movement was at last brought about.

As I moved about in the room, the movement of the visual images of my hands or feet were at first not used, as in normal vision, to decide what tactual sensations were to be expected. Knocks against things in plain sight were more or less of a surprise. I felt my hand to be in a different position from that in which I saw it, and could not, except by cool deliberation, use its visual image as a sign of impending tactual experience. After a time, however, repeated experience made this use of the visual image much less strange; it began to be the common guide and means of anticipation. I watched my feet in walking, and saw what they were approaching, and expected visual and tactual contact to be reported perceptionally together. In this way the limbs began actually to feel in the place where the new visual perception reported them to be. The vivid connection of tactual and visual perceptions began to take away the overpowering force of the localization lasting over from normal vision. The seen images thus became *real things* just as in normal sight. I could at length *feel* my feet strike against the *seen* floor, although the floor was seen on the opposite side of the field of vision from that to which at the beginning of the experiment I had referred these tactual sensations. I could likewise at times feel that my arms lay between my head and this new position of the feet; shoulders and head, however, which under the circumstances could never be directly seen, kept the old localization they had had in normal vision, in spite of the logical difficulty that the shape of the body and the localization of hands and feet just mentioned made such a localization of the shoulders absurd.

Objects lying at the moment outside the visual field (things at the side of the observer, for example) were at first mentally represented as they would have appeared in normal vision. As soon as the actual presentation vanished, the new relations gave way to the old ones brought over from the long former experience. The actual present perception remained in this way entirely isolated and out of harmony with the larger whole made up by representation. But later I found myself bringing the representation of unseen objects into harmonious relation with the present perception. They began now to be represented not as they would appear if normal vision were restored, but as they would appear if the present field of vision were widened or moved so as to include them. In this way the room began to make a whole once more, floor and walls and the prominent objects in the room getting into a constant relation to one another, so that during a movement of the head I could more or less accurately anticipate the order in which things would enter the visual field. For at first the visual search for an object outside of the immediate sight was quite haphazard; movements were made at random until the desired object appeared in sight and was recognized. But now the various lines of visual direction and what they would lead to were more successfully held in mind. By the third day things had thus been interconnected into a whole by piecing together the parts of the ever-changing visual fields.

As to the relation of the visual field to the observer, the feeling that the field was upside down remained in general throughout the experiment. At times, however, there were peculiar variations in this feeling according to the mental attitude of the observer toward the present scene. If the attention was directed mainly inward, and things were viewed only in indirect attention, they seemed clearly to be inverted. But when, on the other hand, full attention was given to the outer objects, these frequently seemed to be in normal position, and whatever there was of abnormality seemed to lie in myself, as if head and shoulders were inverted and I were viewing objects from that position, as boys sometimes do from between their legs. At other times the inversion seemed confined to the face or eyes alone.

On removing the glasses on the third day, there was no peculiar experience. Normal vision was restored instantaneously and without any disturbance in the natural appearance or position of objects.

The experiment was of course not carried far enough to see the final aspect the experience under these conditions would assume. But the changes which actually occurred, even the transitory feelings the observer at times had, give hints of the course a longer experiment of this kind would take. I might almost say that the main problem—that of the importance of the inversion of the retinal image for upright vision—had received from the experiment a full solution. For if the inversion of the retinal image were absolutely necessary for upright vision, as both the projection theory and the eye-movement theory hold, it is certainly difficult to understand how the scene as a whole could even temporarily have appeared upright when the retinal image was *not* inverted. As was said, all things which under the conditions could be seen at all repeatedly appeared to be in normal relation; that is, they seemed to be right side up. Only certain parts of the experience (*i. e.*, head and shoulders), upon which under the circumstances vision could give no report at all, because these parts could not be brought directly into the visual field, seemed to be in abnormal relation to the scene. That these parts of the body should have stubbornly refused to come into harmony with the new arrangement is easy to explain. The only visual experience I had had of them was the *normal* visual experience, and this remained firm in memory without the possibility of displacing it by repeated contradictory visual perception under the new conditions. But of those parts of the body which could be seen, the new appearance and localization was able to drive the old from the field, because the new localization by sight showed a perfect and constant relation to the reports by muscular and tactual perception. No doubt the merely tactual experience of the unseen parts of the body and of their relation to the seen parts must inevitably have produced in time a new indirect visual representation of these unseen parts which would displace the older representation brought over from normal vision. The gradual organization of the whole experience would certainly produce this result, although it would undoubtedly require more time in the case of the unseen parts of the body than in that of the parts plainly visible.

In fact, the difficulty of seeing things upright by means of upright retinal images seems to consist solely in the resistance offered by the long-established previous experience. There is certainly no peculiar inherent difficulty arising from the new conditions themselves. If no previous experience had been stored up to stand in opposition to the new perceptions, it would be absurd to suppose that the visual perceptions in such a case would seem inverted. Any visual field in which the relations of the seen parts to one another would always correspond to the relations found by touch and muscular movement would give us 'upright' vision, whether the optic image lay upright, inverted, or at any intermediate angle whatever on the retina. Only after a set of relations and perceptions had become organized into a norm could something enter which was in unusual relation to this organized whole and be (for instance) upside down. But a person whose vision had from the very beginning been under the conditions we have in the present experiment artificially produced, could never possibly feel that such visual perceptions were inverted.

PHYSICAL AND MENTAL MEASUREMENTS OF THE STUDENTS OF COLUMBIA UNIVERSITY.

BY PROFESSOR J. McKEEN CATTELL AND DR. LIVINGSTON FARRAND.

Extended measurements have been published of certain traits of soldiers, of school children and of the defective classes, more especially of their height, weight, eyesight and defects of body. Single tests of a psychological character have been made on school children and on groups of adults, and we have the many researches from our psychological laboratories giving the results of experiments on a few individuals. As it is not our object to give a detailed historical sketch[1] of the statistics and experiments hitherto published it will suffice to refer espe-

[1] There have been at least four series of mental tests proposed in which methods have been discussed without the communication of results: 'Mental Tests and Measurements': J. McK. Cattell, with an appendix by Francis Galton, *Mind*, 1890; 'Zur Individual Psychologie': Hugo Münsterberg, *Centralblatt f. Nervenheilkunde und Psychiatrie*, 1891; 'Der Psychologische Versuch in der Psychiatrie': Emil Kraepelin, *Psychologische Arbeiten*, 1895; and 'La Psychologie Individuelle': A. Binet et V. Henri, *L'Année psychologique*, 1896. One of the present writers was perhaps the first (1885 and subsequently) to publish experiments on individual psychology made in the laboratory, its introduction having, probably, been delayed because Professor Wundt was not favorable to it. Recently the individual variation in some special psycho-physical or mental trait has been frequently investigated. This has been encouraged by Galton in England (to whom we owe the method of the *questionnaire*), by Kraepelin in Germany, and by Binet in France, but by far the most numerous contributions to the subject have come from American Laboratories—Harvard, Yale, Clark, Columbia, Princeton, Pennsylvania, Chicago, Cornell, Wisconsin and others. Two papers which describe several tests made on a number of individuals deserve special mention in connection with this paper: 'Experimentelle Studien zur Individual Psychologie: A. Oehrn; Dissertation (under Kraepelin), Dorpat, 1889, reprinted with slight alterations, *Psychologische Arbeiten*, 1895; and 'Researches on the Mental and Physical Development of School Children': J. A. Gilbert, *Studies from the Yale Laboratory*, 1895, reported also by E. W. Scripture, *Zeitschrift f. Psychologie*, etc., X, 1896, and THE PSYCHOLOGICAL REVIEW, III, 1896.

cially to the two undertakings most similar to our own. Mr. Francis Galton recommended in 1882[1] the establishment of anthropometric laboratories, and subsequently carried his plan into effect by placing a laboratory in the South Kensington Museum, London, which was continued until last year, when the apparatus was removed to the Clarenden Museum, at Oxford. Visitors could there have certain tests made on payment of a small fee. The tests included, in addition to several purely physical measurements, keenness of eyesight and hearing, color-sense and highest audible note, dynamometer pressure, reaction-time and errors in dividing a line and angles. At the World's Columbian Exposition, Chicago, 1893, Professor Joseph Jastrow arranged a psychological laboratory in which a considerable number of tests strictly psychological in character were undertaken.

The early publication of the results obtained by Mr. Galton[2] and by Prof. Jastrow may be expected, but without awaiting these we shall proceed with the description of our work. We are lead to do this at the present time more especially because at the Philadelphia meeting of the American Psychological Association (December, 1895), a committee, consisting of Professors Cattell, Baldwin, Jastrow, Sanford and Witmer, was appointed to consider the feasibility of coöperation among the various psychological laboratories in the collection of mental and physical statistics. As a report from this committee is to be expected at the next meeting of the Association, it is desirable that the members have before them such tests as have already been made. It may also be mentioned that at the meeting of the American Association for the Advancement of Science (Buffalo, August, 1896), a standing committee, consisting of Messrs. Brinton, Cattell, McGee, Newell and Boas, was appointed to organize an ethnographic survey of the white race in the United States. It is important that psychological tests be included in

[1] *Fortnightly Review*; *cf.* also *Inquiries into Human Faculty*, London, 1883.

[2] Since the above was written, Mr. Galton has informed one of the writers that the people who came to his laboratory were so mixed that no homogeneous group can be extracted out of them that is both large and interesting. Still it is to be hoped that the large mass of data collected under Mr. Galton's direction will be published.

this survey, and that the work be coördinated with that proposed by the Psychological Association.

One of the present writers began the collection of physical and mental measurements of students of Cambridge University, the University of Pennsylvania and Bryn Mawr College in 1887–8, and some description of the tests was published in 1890 (*op. cit.*). The methods have been gradually revised and we shall confine our present account to experiments made on students of Columbia University in 1894–5 and 1895–6. These have been described by Prof. Cattell before the New York Academy of Sciences, May, 1895, and the American Association for the Advancement of Science, August, 1896, and by Dr. Farrand before the American Psychological Association, December, 1895.

Our chief object in the present paper is the description and discussion of methods rather than the communication of results, but we give the averages secured from 100 students. This is a comparatively small number, but it suffices for our present purposes. For the study of the distribution of variations extended statistics are needed, but in that case it would not be necessary to make a large number of different tests. The average of a group of 100 homogeneous individuals has a relatively small probable error, and suffices to determine the place of the individual in the group and for the comparison of this group with other groups. Differences that can be established as the result of 100 measurements should be investigated before we undertake the study of minor or inconstant deviations. The 100 measurements at our disposal cannot, however, be subdivided, and about 1,000 measurements will be needed in order to arrive at the end we have more especially in view, namely, the study of the development and correlation of mental and physical traits. We want to know how a man who has, for example, a large head, a short reaction-time or a good memory, is likely to vary from the average in other directions, and how likely he is to vary to a certain extent. As in other scientific work these tests have two chief ends, the one genetic, the other quantitative. We wish to study growth as dependent on environment and heredity, and the correlation of traits from the point of view of exact science.

Before proceeding with this difficult undertaking it is necessary to learn what tests are the most typical and useful, and what methods are the best and most feasible. It is important that coöperation be secured in deciding what tests shall be used, and in studying and eliminating the numerous drawbacks and sources of error. We do not regard it as necessary or desirable that each laboratory should undertake the same tests. It would, however, be useful to select a few tests made in exactly the same manner, and for different investigators to undertake to extend the measurements in the direction in which they are most interested.

We give on the following page a reduced (the original sheet apart from the margin is about 23 x 18 cm.) fac-simile of the blank used in recording our tests from which their general character may be seen. The tests can only be made individually, one recorder having charge of one student, and, unless the apparatus is duplicated, only three or four records can be made simultaneously. It is consequently essential that the tests should be such that the records can be taken quickly. Our series contains 10 records and 26 measurements (several consisting of from two to five separate determinations), which can be completed in from 40 min. to one hour, varying within these limits according to the skill of the recorder, the intelligence of the student and the degree in which the apparatus is in order.

In selecting the tests, the time required to make them must be especially considered, and some attention should also be paid to the time taken in collating the results. The student would, in nearly all cases, be willing to submit to a longer examination, but this requires a considerable expenditure of time on the part of a skilled observer. Our object has been to form a series that can be made within one hour, and but little can be added to this series without omitting something to make place for it. We suggest below several additional tests of psychological interest, for which time might be found when the series is made

Laboratory of Psychology of Columbia College,

PHYSICAL AND MENTAL TESTS.

Name........................ Date of Birth........................

Birthplace........................ of father........................ of mother........................

Class........................ Profession of father........................

Color of eyes........................ of hair........................

Perception of size........................ Memory for size........................

Height........................ Weight........................

Breathing capacity { 1........................ 2........................ Size of head........................ Right handed?........................

Strength of hand, right { 1........................ 2........................ Left { 1........................ 2........................

Keenness of sight, right eye........................ Left........................

Keenness of hearing, right ear........................ Left........................

Reaction-time	1	2	3	4	5	Av.

After-images........................

Color vision........................ Perception of pitch........................

Perception of weight 1........ 2........ 3........ Sensation areas 1........ 2........ 3........ 4........ 5........

Sensitiveness to pain { right hand........................ left hand........................ Preference for color........................

	1	2	3
Perception of time			

Accuracy of movement........................ Rate of perception and movement........................

Memory........................

Imagery........................

Are you willing to repeat these tests at the end of the Sophomore and Senior years?........................ Do you wish to have a copy of these tests sent you?........................

Date of measurement........................ Recorded by........................

under favorable conditions. It might be desirable to place at the end several tests (we have done this in the case of *mental imagery*), which could be made or omitted as time might require. We give below additional observations which can be made by the recorder without much expenditure of time, and a series of questions which can be answered by the student at home.

We fully appreciate the force of the arguments urged by Professor Münsterberg and by MM. Binet and Henri in favor of making tests of a strictly psychological character. For the psychologist these are, of course, the most interesting and important. But we are at present concerned with anthropometric work, and measurements of the body and of the senses come as completely within our scope as the higher mental processes. We can determine in thirty seconds whether or not a man is color-blind, and thus secure a fact of great personal interest to him, and a typical and sharp variation which can be studied in relation to other traits. If we undertake to study attention or suggestibility we find it difficult to measure definitely a definite thing. We have a complex problem still requiring much research in the laboratory and careful analyses before the results can be interpreted, and, indeed, before suitable tests can be devised.

In addition to the writers several graduate students acted as recorders. A large number of records were taken by Mr. Franz, fellow in psychology, and by Mr. Houston, scholar in psychology, and some records were taken by Mr. McWhood, now fellow in psychology, by Mr. Lay, lately fellow in philosophy, by Mr. Schneider, lately fellow in botany, and by Mr. Kingham. All the recorders had had training in making the tests, but it must be remembered that the results depend somewhat on the methods used by the recorder, and it would be desirable to collate the results for the different recorders and to have the same students tested by different recorders, in order to learn what variations may be due to this source. The methods should be, as far as possible, automatic, and it would perhaps be best to let the recorder read written instructions to the student. Still a certain amount of latitude is inevitable, as students vary greatly in the quickness with which they understand what is to be done.

The attempt was made to follow the order given on the blank (except that memory for size was tested at the end), but this could not be done exactly when 2 or 3 students were tested simultaneously. It would, however, be desirable to test all observers in exactly the same order, as some skill is acquired in the course of the experiments. The five rooms of the laboratory were used, and we tried to leave the student alone with the recorder in cases where the test depended on the attention.

We requested the Freshmen of the School of Arts and of the School of Mines to come by appointment. About one-half of them came, and all were interested in the tests and agreed without hesitation to repeat them at the end of the Sophomore and Senior years. The repetition of the tests will be one of the best criteria of their validity, and we hope the results will be of interest in showing the development of the student during his college course, more especially when taken in connection with the nature of his course, his standing in his studies, etc.

The 100 records used were taken alphabetically, none being omitted. They include 60 Freshmen from the School of Arts, 20 Freshmen from the School of Mines and 20 more advanced students. The records were arranged for these groups alphabetically in sets of ten, and the individual variation from the average of each set calculated. Then the average variation of the sets of 10 from the average of the 100 records was taken. We give these two variations in addition to the average, denoting them by v and V, respectively. We have not omitted any record unless it seemed to contain an error on the part of the recorder. In a few cases tests were omitted by accident, and certain of the tests were added the second year, it being found that more could be made within the hour than we had expected. Some of the sets thus contain less than ten records, the total number made in the group being given.

We shall not at present undertake to discuss in detail the distribution of the deviations, or whether the average, or the median, or the limits within which a certain percentage of the records fall, is the best standard. When the records are arranged in small groups the average is most convenient. If an individual varies from the group by an amount not more than the average variation he may be regarded as normal. This would include about one-half of the students. Those coming above may be regarded as hyper-normal and those coming below as sub-normal. The best method of adjusting the observations must be worked out with a larger mass of material than we have as yet at our disposal.

We shall now proceed to the discussion of the separate tests.

PRELIMINARY DATA.

The student was required to write his own name, the date of his birth, his birthplace and the birthplaces of his parents, the profession of his father, his class and course in college.

Handwriting. It is desirable to let the student write in ink his own name and the other data. 'Graphology' has fallen into disrepute because too much has been asked of it. The handwriting, however, is certainly characteristic of the individual and may prove interesting when collated with the other tests. But we are not prepared to communicate any results based upon our present data.

Age. The average age of the Freshmen, School of Arts (59 cases), in their first term was 18. The age of our college students has often been discussed and our records are of value only in connection with subsequent tests. It may be worth while to call the attention of those who compare statistics of the age of students to the fact that while there are no students whose age is considerably below the average there are sometimes a few older men in the class. For most purposes it would consequently be better to use the median than the average. There were no men over 23 among the Columbia Freshmen, and it appears that the average age is younger than at Harvard or Yale.

Birthplace. The nationality was, in percentages (which are also the actual numbers), as follows:

	Student.	Father.	Mother.
North America,	94	64	81
New York City,	(29)	(10)	(17)
Foreign,	5	34	17
German,	(2)	(20)	(7)
Irish,	(0)	(5)	(2)
English,	(1)	(5)	(2)
Not Given,	1	2	2

As we have already stated all our data have their chief interest in their correlations with the others, and we shall not be able to work out these relations for some years. It will, for example, be of interest to compare the physical and mental traits of students of American parentage with those of German or of English parentage and to study the effects of heredity and environment. For this purpose it would undoubtedly be desirable to record the nationality of at least the grandparents (see the supplementary set of questions given below). We may, however, call attention to the large percentage of foreign parents, especially of fathers. It is a characteristic sexual difference that twice as many men as women should have emigrated.

The profession of the fathers was as follows:

Business,		56
Profession,		26
Lawyers,	(6)	
Physicians,	(6)	
Clergymen,	(4)	
Farmers,		3
No Calling,		1
Not Given,		14

A majority of the students of Columbia University come from the business classes, and the father in most cases did not have a college education.

Supplementary data. Further details regarding the heredity, interests, habits and condition of the student, such as he himself could give or such as could be secured from the impressions of the recorder would undoubtedly add greatly to the value of these tests. The limitations are due to the need of completing the series within one hour and additional records should not lengthen this time.

We suggest the two following series of records, the first of which should be filled up by the recorder and not seen by the student, while the second blank should be given to the student to be filled up at his convenience at home and returned in an addressed envelope. These series are only provisional, and have not as yet been used by us. We shall, however, use them this year, and should be glad to have suggestions regarding them.

Supplementary Observations by the Recorder.

[To be filled in while the student is writing his name.].

What is his apparent age? ( ), 17 ( ), 18 ( ), 19 ( ), 20 ( ), ( ).
Is his apparent state of health good ( ), medium ( ), poor ( )?
Is he tall ( ), medium ( ), short ( )?
Is his head large ( ), medium ( ), small ( )?
Do you think his physical development good ( ), medium ( ), poor ( )?
Do you think him likely to be as a student good ( ), medium ( ), poor ( )?
In these mental tests do you think him likely to be good ( ), medium ( ), poor ( )?

[To be filled in during or after the tests.]

Hair: dark ( ), medium ( ), light ( )?
Complexion: dark ( ), medium ( ), light ( )?
Complexion: clear ( ), medium ( ), blotched ( )?
Eyes: dark ( ), medium ( ), light ( )?
Hair: straight ( ), wavy ( ), curly ( )?
Nose: convex ( ), straight ( ), concave ( )?
Elevation of nose: high ( ), medium ( ), low ( )?
Ears: large ( ), medium ( ), small ( )?
Ears: projecting ( ), medium ( ), close ( )?
Mouth: large ( ), medium ( ), small ( )?
Lips: thick ( ), medium ( ), thin ( )?
Hands: (in relation to size of body,) large ( ), medium ( ), small ( )?
Fingers: (in relation to width of hand), long ( ), medium ( ), short ( )?
Face and Head: note symmetry or asymmetry, also any abnormality as malformation of ears, squint, etc.

[To be filled in after the tests have been completed. The recorder is expected to use any suggestions that he may obtain from having made the records, but not to examine these with a view to using the information.]

Do you think his state of health good ( ), medium ( ), poor ( )?

Do you think his physical development good ( ), medium ( ), poor ( )?

Do you think him likely to be as a student good ( ), medium ( ), poor ( )?

Do you think that in the mental tests he has done well ( ), fairly ( ), poorly ( )?

In understanding what was wanted, was he quick ( ), medium ( ), slow ( )?

Was he talkative ( ), medium ( ), quiet ( )?

Do you judge him to be accurate ( ), medium ( ) not accurate ( )?

Do you judge him to be straightforward ( ), medium ( ), not straightforward ( )?

Do you judge him to be intellectual ( ), medium ( ), not intellectual ( )?

Do you judge his will to be strong ( ), medium ( ), weak ( )?

Do you judge his emotions to be strong ( ), medium ( ), weak ( )?

Would you call him well-balanced ( ), medium ( ), not well-balanced ( )?

Would you call his temperament choleric ( ), sanguine ( ), melancholic ( ), phlegmatic ( )?

Name, ...

Recorded by, ...

Date,..

SUPPLEMENTARY DATA TO BE FILLED IN BY THE STUDENT.

[Place a check (√) in the proper parenthesis; use a question mark (?) when you are unable to answer a question or would prefer not to do so. If you can only answer a question approximately do so and add *ca.*]

	Father.	Mother.	Paternal Grandfather.	Paternal Grandmother.	Maternal Grandfather.	Maternal Grandmother.
Living? (if so, give age), ...						
Deceased? (if so, give year of death and age at time of death)						
Cause of death, if deceased						
Most serious diseases from which they have suffered.						

	1	2	3	4	5	6	etc.
Your mother's children. } born . . . deceased. .							

[Write B for brother and S for sister in the order of age and in the proper column. Include yourself designated by X. After B, S or X write date of birth thus, B. Feb. 10, '84. In case any brothers or sisters have died, write date of death after 'deceased.']

How many brothers did your father have? ( ), how many sisters? ( ), was your father his mother's 1st ( ), 2d ( ), 3d ( ), 4th ( ), 5th ( ), 6th ( ) or what ( ) child?

How many brothers did your mother have? ( ), how many sisters? ( ), was your mother her mother's 1st ( ), 2d ( ), 3d ( ), 4th ( ), 5th ( ), 6th ( ), or what ( ) child?

[In answering questions such as this one, think of the people you know as in three classes equal in number and decide to which class you belong.]

Do you regard your general health as good ( ), medium ( ), not good ( )?

Do you regard your present health as better than usual ( ), same as usual ( ), not as good as usual ( )?

Indicate such of the following diseases as you have had by writing in the parenthesis the approximate age at which you had them: convulsions in childhood ( ), measles ( ), diphtheria ( ), scarlet fever ( ), pneumonia ( ), brain fever (meningitis) ( ), malaria ( ), nervous prostration (neurasthenia) ( ).

Do you have headaches often ( ), seldom ( ), never ( )?

Do you have colds often ( ), seldom ( ), never ( )?

Are your teeth good ( ), medium ( ), poor ( )?

Have you consulted an oculist? ( ), If so, at what age for the first time? ( ), Do you wear glasses? ( ), Give the nature of the defect if you know it. ( ).

How many hours do you usually sleep ( )?

Do you dream much ( ), little ( ), never ( )?

Are your dreams as a rule pleasant ( ), commonplace ( ), fearful ( )?

As a child were you subject to bad dreams which you have since outgrown? ( ).

Is your appetite good ( ), medium ( ), poor ( )?

At what time of day do you feel in the best spirits? ( ).

At what time of day can you study best? ( ).

Do you drink coffee ( ), tea ( )? If so, how many cups daily, coffee ( ), tea ( )? At what age did you begin? ( ).

Do you smoke? ( ). If so, how many pipes ( ), cigars ( ), cigarettes daily ( )? At what age did you begin? ( ).

Do you use alcoholic drinks? ( ). If so, occasionally ( ), daily ( )? If daily, how many glasses of beer ( ), wine ( ), spirits ( )?

About how many hours or minutes daily on the average during the month of October do you spend in study ( ), in reading books other than text and reference books ( ), in playing sedentary games ( ), in playing athletic games ( ), in other physical exercise, as walking, riding a bicycle, etc. ( )?

Do you play a musical instrument? ( ), if so, what one or ones? ( ), how long daily? ( ), what musical instrument do you prefer to hear played? ( ), which opera that you have heard do you prefer? ( ).

What novelist do you prefer? ( ), what poet? ( ), what painter? ( ), what play that you have seen acted? ( ).

Supposing the following ten ways of spending an hour give to you pleasure, write numbers after them in the order of amount of pleasure they give. Eating dinner ( ), playing your favorite athletic game ( ), playing your favorite sedentary game ( ), working with tools as in a garden ( ), reading a novel ( ), hearing music ( ), talking to a friend ( ), day dreaming ( ), learning something ( ), writing something ( )?

What profession or business do you propose to follow? ( ), in what calling would you prefer to succeed if you had your choice? ( ).

Send with this, if possible, your most recent photograph (with date at which it was taken,) and if you have them, or can have them taken, send photographs both in full face and in profile.

Name, (in full)..

Date of Birth,................................ Place of Birth, ...

Class and Course in College,..

Physical Characters.

The colors of the hair and of the eyes were recorded with the results given below. The figures are both percentages and actual numbers.

Hair.		Eyes.	
Black,	8	Gray,	33
Dark brown,	56	Blue,	30
Light brown,	34	Brown,	31
Flaxen,	1	Green,	1
Red,	0	Not given,	5
Not given,	1		

In making these records it would be well to confine the designations to those given above, and it would be an advantage to have standards by which the recorder could make the comparison.[1] A lock of the hair might be preserved with the record. Unless the recorder has been carefully trained such descriptions do not have great value. The same eyes may be called gray, blue and brown, respectively, by different recorders. In a population so mixed as that of New York City, it is questionable how far these records are of use. If taken at all the description should be made more complete by giving the traits enumerated in the supplementary blank printed above. The finger prints could be taken for purposes of identification.

Height and weight can be measured with comparative ease. We had a Fairbanks' scale with an upright adjustable measuring rod graduated for the metric system. The averages give:

	Av.	v.	V.
Height in cm.	175.1	4.9	1.7
Weight in kg.	66.2	6.0	1.7

Both height and weight are above the average of the population and above the averages for the freshmen entering Yale University.

It so happens that the subdivisions of the metric system are not well suited for these measurements. It is not quite accurate enough to measure to kilograms and centimeters, whereas to measure to tenths of these, especially in the case of weight, is needlessly exact. In these measurements the weight was taken in ordinary indoor clothing, and the height of the heel was subtracted. The record should be written, *e. g.*, 162.7 cm. — 1.4 cm. = 161.3 cm. In some cases the height of the heel was not subtracted, and the average given above is slightly too large.

The size of the head was measured with the *conformateur* used by hatters. This was placed horizontally above the temples, giving approximately the largest horizontal area of the head. The diameters are given below together with the ratio of length to breadth.

[1] Such standards are sold by the Cambridge Scientific Instrument Co., but at a very high price.

	Av.	v.	V.
Length in cm.	19.3	0.5	0.2
Breadth,	14.9	0.4	0.1

The measurements are not sufficiently accurate to study growth, but would serve for comparison with the other data. The method has the advantage of being easily carried out and leaving a permanent record. It also measures irregularities in the shape of the head that would not be shown by the perimeter. There is a slight inaccuracy owing to the hair being included in the measurement, and a more serious one in the difficulty of placing the instrument in the proper position. This latter difficulty indeed holds for all measurements of the head which can only be made with exactness by a skillful observer.

On the whole we think the conformateur in its present form is less accurate and (in the subsequent calculations) more troublesome than the perimeter and expect hereafter to use the latter instrument.

The breathing capacity was measured with a fluid spirometer, two tests being taken, the averages in liters (98 cases)

	Av.	v.	V.
Capacity in liters,	3.73	0.45	0.19

In a determination such as this it is desirable to take two records, as one, especially the first, is sometimes faulty. We think it best to record both measurements, but to use not the average, but the maximum. The averages of the maxima are: are:

	Av.	v.	V.
Capacity in liters,	3.83	0.41	0.19

The maximum of the two trials is thus 0.1 liter greater than the average. If time permit it would be desirable to continue a test such as this until the maximum has been reached. If on a sufficient number of observers a larger series of trials were made we could determine how likely it is that the maximum be reached in the first trial, the first two trials, etc.

In addition to these measurements Mr. Galton proposes taking the span of the arms, the height sitting, the height to the top of the knee, the length from elbow to finger-tip and the length of the middle finger of the left hand. These measurements would probably prove useful for purposes of identification, but do not seem otherwise advisable unless a more thorough physical examination is undertaken than that proposed by Mr. Galton.

It would indeed be highly desirable to make a thorough physical examination, but for this purpose the recorder would need some special training. The most important tests would be of heart (including pulse tracing), lungs (including rate and tracing of breathing), temperature and urine which could be made in a few minutes by a practiced physician. There are many other physical data, such as deformities, peculiarities, stigmata, tendon reflexes, etc., which it would be desirable to have, and we may hope that coöperation between physicians, students of criminology and of the defective classes and those interested in anthropometry may be obtained to select the most important determinations and devise the best means for carrying them out.

Vision.

Color blindness was tested by letting the observer select the four green shades from the woolen skeins supplied by the Cambridge Scientific Instrument Company in accordance with Mr. Galton's instructions. Three per cent. of those tested (71 cases) were color blind and three per cent. appeared to have defective color vision.

The method of selecting colors suffices to show whether or not color vision is normal, if the recorder have sufficient skill to note hesitation on the part of the student. In the case of those color blind or having defective color vision it would of course be desirable to investigate more carefully the nature of the defect.

The Galton instrument is needlessly expensive, as the yarns could be matched for a few cents. If the instrument is used the four pointers should be removed, as the observer should not know that he is expected to find just four shades of green.

Keenness of sight was tested with Mr. Galton's instrument. This gives the distance in cm. at which diamond numerals can be read by each eye singly. We made the test in a room lit only by an electric lamp of 100 candles at a distance of 1 m. from the type. We determined the distance at which at least 8 letters out of 10 could be correctly read, making sure that all letters could be read on the card one step nearer. The percentages (94 cases) for the different distances and for each eye are:

Distance in cm.	Right Eye.	Left Eye.
72	1.06 %	2.02 %
61	29.9	16.00
52	26.6	29.80
44	18.09	31.99
37	10.64	7.49
31	6.38	7.49
26	3.19	3.19
22	1.06	0.
19	1.06	0.
16	2.02	2.02

The right eye is thus better than the left, the 'normal' for the right eye being a distance of about 52–61 cm., and for the left eye of 44–52 cm., or a little more.

It is perhaps a needless precaution to use a dark room and standard illumination, but we have found great variations when test-types are illuminated by ordinary daylight. Test-types of varying size at a distance of 5 or 6 m. will do as well as small type at varying distances, but it is easier to have a selection of lines in small type than in large and to expose them for a fairly constant time while the observer is in ignorance of their nature. The tests used by oculists are as a rule defective from a scientific standpoint. The near as well as the far limit ought perhaps to be taken and astigmatism tested.

The test in any case is not very exact, but perhaps as good as any that can be made quickly. It would, however, be desirable to compare various methods, such as counting dots placed at a distance, or drawing a series of figures, and determine which gives the most accurate results in the least time. The test requires atropin to be accurate and an objective examination of the eye such as can only be carried out by a skilled oculist.

It is, however, a great advantage for the student to know whether his eyesight is normal, sub-normal or abnormal, and, if desired, a more careful determination of the nature and amount of the defect can be made either in the laboratory or in the office of an oculist.

The least light visible cannot be readily measured owing to the variations accompanying adaptation; and the least noticeable difference in intensity cannot be measured quickly. A series of shades of gray nearly alike can, however, be sorted by the observer on the plan recommended by Mr. Galton for weights. We must, however, admit that the least noticeable difference in intensity cannot be determined in two or three minutes, and that vision is one of the most difficult senses to test.

Preference for color was tested by showing rectangles (about 5×3 cm., the 'golden mean') of the following colors in irregular order on a black field and asking the student which he liked best. The preferences (66 cases) were as follows:

Blue, 34.9 %; red, 22.7; violet, 12.1; yellow, 7.5; green, 6.1; white, 6.1; no preference, 10.6.

The student was asked to define his degree of preference, in four grades, but our data are not sufficient to warrant a discussion of the results.

HEARING.

Hearing was tested by determining the distance at which the ticking of the laboratory stop-watch could be heard with each ear singly. The results (86 cases) were in percentages:

	Normal	Subnormal	Abnormal
Right ear	86	13	1
Left ear	84	13	3

We did not undertake to measure sharpness of hearing exactly as the laboratory was too noisy. There is unfortunately no good method for measuring the intensity of a faint sound, and one cannot do much more than determine whether the hearing is normal, sub-normal or abnormal.

The accuracy of the perception of pitch was determined by giving twice on a monochord the *f* below the middle *c*, the observer being required to find the sound by adjusting the bridge which had been in the meanwhile shifted (which should have been done to about *c'*). The average variation (48 cases) was 7.5 cm. (v, 5.9 ; V, 1.9) or nearly one whole tone. Of those tested 10% could adjust the monochord within about $\frac{1}{10}$ tone, 61% came between $\frac{1}{10}$ and one tone, and 29% had a greater error.

The observer was not allowed to hum the tone. Perhaps a simpler method would be to strike a key on the piano and let the observer find it. In this case three notes could be struck—high, middle and low. The highest audible note is probably a good test and one not difficult to make.

Dermal and Muscular Sensations.

Sensation areas were determined by using an æsthesiometer in which the points were 2 cm. apart, the instrument being applied longitudinally on the back of the left hand between the tendons of the fingers. Five tests were made, the subject being touched with one or two points in the order, 'two, two, one, one, two' and being required to decide in each case whether he were touched with one or with two points. The percentages of the men (49) who were correct a given number of times is as follows:

Correct.	5 times.	16 per cent.
"	4 "	38 "
"	3 "	20 "
"	2 "	22 "
"	1 "	2 "
"	0 "	2 "

The answers were correct in 67% of all the trials, and were correct in 60% of the cases with two points, and in 75% of the cases with one point. It is difficult to determine sensation areas exactly, as there are many sources of error, both in the decision of the student and the way in which the points are applied by the recorder. Perhaps a method of equivalents in which, say, the observer were touched by points 5 cm. apart, and were then required to indicate the distance on the skin, or were touched and required to touch as nearly as possible the same point, would give more satisfactory results. The data given above determine the sensitiveness of the group, but not of the individual.

The perception of the force of movement was measured by letting the observer make with a dynamometer two pulls in succession as nearly as possible alike, and measuring his error. He was instructed how to make a pull about 4 kg. in strength, and then required to make three pairs of pulls. The average error, from the average of the differences in the three pairs of pulls, was (48 cases):

	Av.	v.	V.
Error in kg.	0.63	0.45	0.12

The method of average error always gives results more quickly than the method of right and wrong cases, and it is consequently an advantage to use a dynamometer rather than weights for this test. It is not possible to determine the least perceptible difference in weight by lifting two weights without a large number of experiments. A series of, say, five weights differing by small increments can be arranged in order as suggested by Mr. Galton. In this case, however, it is difficult to find the series that can be just arranged correctly, or to calculate the probable error from the mistakes.

Sensitiveness to pain was determined for the ball of the thumb of the right and left hands. An algometer was used in which the surface applied was of rubber 1 cm. in diameter and rounded at the corners. The instrument was applied with gradually increasing pressure by the student himself or by the recorder (it should be done always by the recorder to secure exactly comparable results), and the student was told to say as soon as the pressure became disagreeable. If he showed signs of discomfort the pressure was stopped. Two tests were made on each hand in alternation, beginning with the right hand. The averages (95 cases) are as follows:

Pressure in Kg.

	Av.	v.	V.
Right Hand,	6.90	2.90	0.96
Left Hand,	6.70	2.64	0.94

The strength of the right and left hands was measured with the ordinary oval dynamometer. Two tests were made with each hand in alternation, beginning with the right hand. The averages (99 cases) of the two trials with each hand are as follows:

Strength in Kg.

	Av.	v.	V.
Right Hand,	38.8	5.7	2.4
Left Hand,	34.6	5.3	2.6

In this test it would save time to make two trials and use the maximum. The dynamometers ordinarily sold are not very accurate, and the amount of pressure measured depends largely on how the instrument is held. We believe the maximum pressure of the thumb and forefinger would be a better test if it could be generally introduced.

Accuracy of movement and tremor were measured by allowing the observer to join two points distant 10 cm., the line being drawn as straight as possible with the free and unsupported hand. The observer was shown at about what rate the movement should be made, the line being drawn in about two seconds. A calculation of the results quantitatively would require much labor, but they could be readily classed for comparison with the other data. Three or five classes could be used, say: straight, medium or crooked; and tremor, much, medium or little.

We think it desirable to add at least one further test of movement and fatigue, and expect this year to try the following: Let the observer make with a spring dynamometer maximum contractions of the thumb and forefinger as rapidly as possible for fifteen seconds. The rate and force of the movements must be recorded on a kymograph. A dynamogenetic test might be added by giving, say, at the end a loud sound and determining its effects on the curve. This experiment would require expensive and complicated apparatus, but there is no special objection to using such apparatus so long as the test itself can be easily and quickly made. The trial should be made with both right and left hands, and perhaps twice, the second record only being used. This determination would make the ordinary dynamometer test unnecessary, except for purposes of comparison.

Professor Jastrow includes a number of other tests on movement. The number of movements that can be made in 15 seconds is a good test, though we think that the one recommended above is better. It can be carried out by tapping a telegraph key and recording the taps on a kymograph. The counting instrument which records the number of pressures made could be used. It is cheap and does get out of order, but the amount of pressure is a variable factor.

The maximum rate of movement is also a valuable test and one easily made after the apparatus is in order. The accuracy with which a movement of given extent can be repeated may be measured, as also the accuracy with which movements can be made in different directions and with the right and left hands. Tremor and involuntary movement can be recorded with the planchette, and the whole field of dynamogenesis offers opportunity for interesting tests if time permit.

TIME MEASUREMENTS.

The reaction-time for sound was measured 5 times in succession with the Hipp chronoscope giving the following results (97 cases):

	Time in σ.		
	Av.	v.	V.
Reaction-Time.	174(v=29[1])	30	13

It is possible that more regular and typical results might be secured if, in place of a sound for stimulus, an electric shock were applied to the fingers with which the reaction is made. Sound is, however, better than light. We do not regard it as desirable to use several senses when time is limited. We be-

[1] This variation is the average of the variations of the five reactions made by each observer. In several cases five valid reactions were not recorded.

lieve that the Hipp chronoscope is the most convenient instrument for measuring reaction-times. When once in order it can be used by anyone, and the times are written immediately on the record blank. But the method is immaterial, as it would suffice to measure the times to 0.01 sec. For the well fitted laboratory nothing more suitable than the Hipp chronoscope (in the form in which we use it) can be wanted, but there is urgent need of a simple and portable instrument that will measure times to 0.01 sec. In measuring the reaction-times of an unskilled subject it is not desirable to place him in a separate room, as he must be watched and instructed by the recorder, but a screen should be used to hide the apparatus.

The observer was told to lift (not press, which is a slower and more complex movement) his fingers as quickly as possible after the occurrence of the noise, and was allowed to direct his attention as he found most convenient. It might be desirable to ask the observer after the experiments have been completed as to the direction of attention, but it would scarcely be possible to investigate 'sensory' and 'motor' reactions.

As stated above, we let each observer make five reactions. When all the first reactions, all the second reactions, etc., are averaged together the following results are obtained:

Times in σ.

	Av.	v.	V.
I	196	55	16
II	178	46	19
III	170	43	16
IV	169	40	19
V	166	35	19
Av.	176	44	18

The first reaction is thus likely to be about 25 σ and the second about 10 σ longer than the subsequent ones, which show only a slight decrease.[1] To get an observer's reaction-time, therefore, it might be well to make five reactions and use the averages of the last three. The variations, however, show that the first two reactions, though longer, are not more irregular

[1] The average of the first reactions is lengthened by two of over 500 σ which were not true reactions and have been excluded from the averages given at the beginning of the section.

than the subsequent ones, and for purposes of comparison the five first reactions can be used.

The reaction-time is one of the tests naturally thought of first in a series such as this, but we do not regard it as one of the most satisfactory. To make reactions quickly and regularly is something of a 'trick' and the variations in time which occur with unpracticed observers depend on complex causes.

Both Prof. Jastrow and Prof. Münsterberg recommend a number of psychometric tests for a limited series such as this. Those used by Prof. Jastrow, which consist of discrimination and choice are even more difficult to carry out and interpret than reaction-times. The results vary greatly with the apparatus used, with the instructions of the recorder and with the attitude of the subject.

The plan first used by one of the present writers of giving lists of colors, words, etc., and measuring the total time required to name them, to form associations, etc., is recommended by Münsterberg and indeed makes up about one-half of his tests. We have used one test of this character, and doubtless others would be useful if time permitted. We gave the observer a blank containing 500 11-point capital letters, of which 100 were A's. Each of the other letters occurred 16 times, and the whole series was arranged in an order drawn by lot. The observer was required to mark as quickly as possible all the A's. We thus have the time (93 cases) required to recognize and mark 100 letters and to discriminate cursorily 400 more.

	Time in Secs.		
	Av.	v.	V.
Marking 100 letters	95.0	12.8	6.4

The average number of A's omitted was 2.6. It was but seldom that a wrong letter was marked. It would be desirable to correlate the rapidity with the number of mistakes. A rough correction could perhaps be made to the rate by adding to the total time the time that would be required to discriminate and mark the letters omitted or wrongly marked. This would increase the average time to about 97.5 seconds, which is very nearly one second per letter. The order of the individuals would be somewhat changed by such a correction, as there are a few who make a great many mistakes. This itself is typical; some will do a task quickly and well, some quickly and ill, some slowly and well, and some slowly and ill.

If time permit the making of other psychometric tests we should recommend reading as rapidly as possible a list of 100 words, and 100 similar words making sentences;[1] naming 100 (or 20) colors (say red, yellow, green, blue, violet, gray and black, 1 cm. sq. arranged in a chance order on a white ground), which is useful in determining color-blindness as well as quickness of perception and speech, and lastly giving 100 (or fewer) words and requiring the student to write as rapidly as possible the suggested ideas.

This last test would of course be useful in the study of association of ideas, which has not been included in our series. We regret that this has been the case and may try to take up some study of association in our subsequent work. The difficulty is that this subject (like imagery and memory, which we have included), requires more psychological investigation before a test can be conveniently applied and properly interpreted.

Perception of Space and Time.

The observer was given a standard line, 10 cm. in length, drawn near the top of a piece of paper, and was required to place this on the left-hand side of a sheet of letter paper of the same width, and draw in a corresponding position a line of the same length. His line was then folded under and he repeated the trial. The results (93 cases) were:

	Error in mm.		
	Av.	v.	V.
Average Error,	6.5	3.4	0.9

The constant error was on the average + 0.08 mm., that is, there was in the group no appreciable tendency to over-estimate or under-estimate the line.

[1] The rate at which a foreign language can be read is a good test of familiarity with the language.

As the sheet of paper was only 20 cm. wide, the observer may have guided himself by the distance from the edge of the paper. It would save time (especially in the subsequent calculations) to make only one trial. We expect hereafter to amplify this test as follows: Give the student a sheet of letter paper (about 25 x 20 cm.) with a line 5 cm. in length drawn horizontally 20 cm. from the bottom of the sheet. The student is required to reproduce this line in the same position on a similar sheet, and afterwards to draw from the middle of the line he has drawn a vertical line of the same apparent length, and then to bisect the left-hand angle and, perhaps, tri-sect the right-hand angle and divide the vertical line in the middle. The test can be made quickly, but it would be somewhat tedious to measure the errors.

The accuracy with which intervals of time can be judged was measured by giving the student an interval of 10 seconds., marked at the beginning and end by taps, and letting him make a tap when an apparently equal interval had elapsed. The results (90 cases) were:

	Time in Sec.		
	Av.	v.	V.
Average Errors,	1.57	0.81	0.26

The constant error for the group was on the average—0.18 sec. The errors are almost too small to be measured by the method used (an ordinary watch or stop chronoscope), and it would seem desirable either to increase the time to 30 seconds or to use chronographic methods of giving the signals and measuring the times. This test is one easily and quickly made, and strictly psychological in character. But the interpretation of the results is not obvious, and it might perhaps be omitted by those not specially concerned with psychology or amplified by those who are.

Memory.

The experiment already described in which we required a student to draw twice a line as nearly as he could the same length as a standard line of 10 cm. was made at the beginning of the series. About three-quarters of an hour later when all the tests had been completed, he was reminded of the line he had drawn and told to draw from memory a line of the same length. We have thus a good test of recollection (the observer not knowing at the time that he would be asked to remember) easily made and giving a quantitative result. The average error of recollection was 7.3 mm. (21 cases only), and the constant error under +0.2 mm., practically none. The error is but slightly larger than in the case of immediate comparison of the lines, but the number of students tested was small.

Like all tests of memory, the results are somewhat complex, and cannot readily be compared with other work not made by exactly the same method. But it is desirable that a test of ordinary or casual memory be made and the conditions fixed by agreement. As in some of the other experiments on our list, the average result of this test gives the accuracy of the class tested, but the place of the individual in the series is very inadequately determined by a single trial.

We also tested immediate memory by reading aloud eight numerals and requiring the student to repeat them, making the determination three times with different numerals. The average number correctly given (without regard to order) was 6.92.

The errors can be counted in three ways, with regard to omissions, substitutions, and mistakes in order or position. It is tedious and difficult to count up the mistakes in order or position, and we give only the total number of numerals remembered. This test can be made in various ways; one can use numerals, letters, words or nonsense syllables, read them or show them, etc. We prefer numerals in spite of the elaborate work with nonsense syllables undertaken by Ebbinghaus and by Müller and Schumann. Jastrow uses additional tests of memory, and it would certainly be desirable to compare auditory and visual memory. It would also be useful to test memory by reading aloud a paragraph (say 200 words) and requiring the student to reproduce it. The experiment is easily made, but it is somewhat difficult to calculate the errors. Perhaps the papers might simply be graded on a scale of 10 with regard to verbal and logical memory.

AFTER-IMAGES AND IMAGERY.

After-images were tested by allowing the observer to see in a dark room for fifteen seconds a white light of determined area and intensity. The area was a cross with arms 1 cm. square, 30 cm. distant from the eyes, and the intensity (light through ground glass which absorbed about one-half) was from a 100 candle power incandescent electric lamp at a distance of 30 cm. Of the 75 students tested 73.3% saw an after-image. The average total duration from the disappearance of the light to the disappearance of the first image, and the duration of the latent period before any image appeared, were as follows:

	Av.	v.	v.
Duration of image in secs.,	44.2	25.2	3.0
Duration of latent period (34 cases),	16.2	9.4	0.2

The latent period is long because the student is not likely to notice the first positive image and oscillations. With 61.8% of the students the after-image, after disappearing, reappeared. With 29.1% it appeared three or more times; with 7.3% four or more times, and with 3.8% five times. The after-image, when first seen, was sometimes positive and sometimes negative, and the colors varied greatly, being distributed in the first phase noticed as follows:

Negative or dark, 33.3%; light or white, 29.4: blue, 13.7; purple, 9.8; green, 5.9; yellow, 3.9: red, 2.0; miscellaneous, 2.0.

We included after-images in our series in part because it was a subject being especially investigated in the laboratory. We think it an advantage for each laboratory to undertake, in addition to certain tests made everywhere, some special tests, so that a larger field may be covered, and the best tests selected by survival of the fittest. Our results with after-images seem to show that the test is a good one. We get definite results, combined with great individual differences. The differences depend on attention, power of observation, etc., and, perhaps, on inherent differences in the nervous system, which may prove typical when correlated with our other determinations.

Imagery was tested by letting the student fill in a blank containing the questions printed below. The answers are given in percentages (95 subjects) after the questions.

Think of your breakfast table as you sat down to it this morning; call up the appearance of the table, the dishes and food on it, the persons present, etc.

Then write answers to the following questions:

(1) Are the outlines of the objects distinct and sharp?

Yes, 86.5%; No, 6.2%; miscellaneous, 7.3%.

(2) Are the colors bright and natural?

Yes, 83.3%; No, 10.4%; miscellaneous, 6.3%.

(3) Where does the image seem to be situated? In the head? Before the eyes? At a distance?

In the head, 28.7%; before the eyes, 36.2%; at a distance, 33%; miscellaneous, 2.1%.

(4) How does the size of the image compare with the actual size of the scene?

Same, 53.7%; smaller, 45.3%; miscellaneous, 1%.

(1) Can you call to mind better the face or the voice of a friend?

Face, 75%; voice, 14.6%; miscellaneous, 10.4%.

(2) When 'violin' is suggested, do you first think of the appearance of the instrument or the sounds made when it is played?

Appearance, 76.8%; sounds, 23.2%;

(3) (*a*) Can you call to mind natural scenery so that it gives you pleasure? (*b*) Music? (*c*) The taste of fruit?

	Yes.	No.	Miscellaneous.
Scenery,	94.6%	4.3%	1.1%
Music,	89.1%	9.8%	1.1%
Taste of fruit,	68.1%	28.6%	3.3%

(4) Have you ever mistaken a hallucination for a perception, *e. g.*, apparently heard a voice or seen a figure when none was present? If you answer 'yes' describe the experience on the back of this sheet.

Yes, 74.7%; No, 25.3%.

As we have already had occasion to state those tests that are of special interest to the psychologist are often ones with which it is difficult to get definite results. The student has had no practice in introspection and even a trained psychologist may find it difficult to fill in such a blank. For this reason we have added to several of the questions proposed by Mr. Galton others admitting of more definite answers. On the whole we think it desirable to make this test. A discussion of results would lead us beyond the limits of a general article.

CONCLUSIONS.

Our experience with these tests leads us to recommend that they be made a part of the work of every psychological laboratory. When used with freshmen on entering college the record is of interest to the man and may be of real value to him. It is well for him to know how his physical development, his senses, his movements and his mental processes compare with those of his fellows. He may be able to correct defects and develop aptitudes. Then when the tests are repeated later in the college course and in subsequent life the record of progress or regression may prove of substantial importance to the individual. The making of the tests brings the psychological laboratory into relation with a large number of students and with other departments of the university, shows the modern methods of anthropometry and experimental psychology, and may lead to a more serious study of these on the part of a larger number of students.

The psychological laboratory can also be brought into mutually helpful relations with the community by extending the tests to any who wish to have them made. Children in the schools might be tested with special advantage. For this purpose tests are especially useful which can be made simultaneously on a large number of observers. Physicians might find it an advantage to have records made of their patients. The tests are well suited for civil service examinations. If a small fee were charged in these cases it might suffice to support an assistant, the larger part of whose time would be spent in scientific work. In any case the making of the tests is good practice for advanced students preliminary to, or in addition to, special research. By bringing the laboratory into relations with the community we add to its influence and at the same time secure the material needed for research.

We have only studied 100 individuals and regard this paper rather as an investigation of methods than as a summary of results. We think that an hour used in tests should be divided between physical, psycho-physical and mental measurements. We regard it as important that work in physical anthropology, which is a subject sure to be recognized before long by all our universities, should be intimately associated with the work in experimental psychology. We are not able to suggest any radical improvement in the tests selected or in the methods of making them; but in reviewing the individual tests we have called attention to difficulties and suggested improvements. The work is one now only begun, but likely to develop and requiring investigation and discussion from diverse points of view.

We do not at present wish to draw any definite conclusions from the results of the tests so far made. It is of some scientific interest to know that students entering college have heads on the average 19.3 cm. long, that 15% have defective hearing, that they have an average reaction-time of 0.174 sec., that they can remember seven numerals heard once, and so on with other records and measurements. These are mere facts, but they are quantitative facts and the basis of science. Our own future work and that of others must proceed in two directions. On the one hand we must study the interrelations of the traits which we define and measure. To what extent are the several traits of body, of the senses and of mind interdependent? How far can we predict one thing from our knowledge of another? What can we learn from the tests of elementary traits regarding the higher intellectual and emotional life? On the other hand we must use our measurements to study the development of the individual and of the race, to disentangle the complex factors of heredity and environment. There is no scientific problem more important than the study of the development of man, and no practical problem more urgent than the application of our knowledge to guide this development.

DISCUSSION AND REPORTS.

PSYCHICAL RESEARCH.

'Psychical Research' has so many enemies, fair and foul, to elude before she gets her scientific position recognized, and is moreover so easily vulnerable in her present stage of development, that I may be excused, as one of her foster-fathers, for uttering a word that may turn the edge of Prof. Cattell's amiable *persiflage* in the last number (p. 582) of this REVIEW. He seems not quite to have caught the argument of my presidential address. The inquiry, I said in substance, still remains baffling over a large part of its surface, for the evidence in innumerable cases can neither be made more perfect, *nor*, on the other hand, be positively explained away. It *may* be malobservation, illusion, fraud or accidental coincidence; it *may* be good and true report. One can only go by its probabilities and improbabilities; and the scientist, who goes by the *presumption* that the usual laws of nature are superabundantly proved, feels the improbability of 'occult' phenomena to be so infinitely great that he is practically certain that the evidence in their favor must be bad, even though he can't show in the particular case where the badness comes in. The issue between Prof. Cattell and myself is as to the general logic of presumption here. I urged that the force of the scientist's presumption, quâ presumption, might some day be worn out by the accumulation of 'psychic' cases, long before his doctrine of nature was radically overthrown, as it would be were a single case conclusively proved. Prof. Cattell says: "When we have an enormous number of cases, and cannot find among them a single one that is quite conclusive, the very number of cases may be interpreted as an index of the weakness of the evidence;" apparently holding the scientist's presumption to be actually strengthened by the quantity and quality, taken together, of the psychical research reports. It would indeed be strengthened if, *pari passu* with the accumulation of reports, there went for each concrete type of case a parallel accumulation of demonstrations of its erroneousness. And as this is just what happened in the 'physical mediumship' type, the work of the S. P. R. in that field has been mainly destructive. But it has happened practically nowhere else. In the veridical apparitions, in the chief thought-transference experiments, fallacy has been assumed, but not clearly demonstrated. The presumption has remained presumption merely, the scientist saying, "I can't believe you're right," whilst at the same time he has been unable to show how or where we were wrong, or even except in one or two cases to point out what the error most probably may have been. In such a state of things people trust their instincts merely, while waiting for a final proof. Many naturalists, for instance, consider the evidence for the sea-serpent practically sufficient. In others it provokes a smile. Meanwhile a single sea-serpent dragged up on the beach would settle the matter forever. I spoke of my own final proof or psychical sea-serpent-corpse, under the name of a 'white crow.' Professor Cattell says: Can the exhibition of any number of gray crows prove that any crows are white? But our reports are not of gray crows; at the very worst they are of white crows without the skins brought home, of sea serpents without the corpse to show; and where there are such obvious reasons why it must be easier to see a wild beast than to capture him, who can seriously maintain that continued reports of merely seeing him tend positively to decrease the probability that he exists? In the case of telepathy, ghosts, death-apparitions, etc., the reasons why the evidence is always likely to be imperfect rather than perfect are equally obvious, and the logic is the same as in the wild beast case. Continued reports, far from strengthening the presumption that such things cannot exist, can only detract from its force.

Both here and in my address I have played into the hands of the scientist, and granted him every conceivable concession about the facts for the sake of making my point as to the logic of presumption all the more clear. But there is such a thing as being too fair-minded, so that one wades in a very bog of over-reasonableness. For, in point of fact, the concrete evidence for most of the 'psychic' phenomena under discussion is good enough to hang a man twenty times over. The scientist's objections, on the other hand, are either shallow on their face (as where apparitions at the time of death are disposed of as mere 'folk-lore,' or swept away as a mass of fiction due to illusion of memory), or else they are proved to be shallow by further investigation, as where they are ascribed to chance-coincidence. May I add a word to illustrate this?

On page 69 of Vol. II. of this REVIEW, I summarized the elaborate Sidgwick's report on the Census of Hallucinations. That paper concluded that the stories of apparitions occurring on the day of the death of the person appearing were 440 times too numerous for the phenomenon to be fairly ascribed to chance. I said that the chief objection practically to this conclusion was that the census, covering only 17,000 cases, was still too small. Last spring I wrote a letter to Professor Sidgwick, giving, for quotation at the Munich Congress, the results of my American census of 7,123 cases. They prolong and corroborate his own. The 'yes' cases were 1,051 in number, or 14.75% of the whole. I cite part of my letter:

"Of these yeses 429 were without particulars, and in 36 the percipient had not signed the account. Only 586 subjects thus remained for statistical treatment.

"Of these, eliminating all who had the experience before they were 10 years old; and all who gave vaguely plural experiences, there remain 62 subjects with *71 cases of visual hallucination of some recognized living person. Of these, 12 are reported to have occurred on the day of the death of the person seen.*

"These numbers are so small that I have not ventured to reduce by any elimination of 'suspicious' cases, as you did, but as a correction for oblivion have multiplied the whole lot by your figure 6½.

$$71 \times 6\tfrac{1}{2} = 462 \quad \text{(in round number).}$$

"*Let this 462 represent the probable whole number* of visual hallucinations of living persons really seen by the percipients since their tenth birthday. The 12 veridicals are in round numbers $\frac{1}{39}$ of 462. Therefore $\frac{1}{39}$ is the probability induced from facts, and due to the unknown cause of apparitions, that if a man 'appear' at all it will be on his death-day.

"On the other hand (the U. S. death rate being practically the same as that of England) the *pure chance* that if any one appear on a certain day it will be one who is dying on that day is only $\frac{1}{19000}$. But $\frac{1}{39} = \frac{1}{19000} \times 487$; so that *apparitions on the day of death are, according to our statistics, 487 times more numerous than pure chance ought to make them.*

"The details will be sent later, but I append now a few remarks. Of the 71 cases, all but the 12 that were death-apparitions are treated as insignificant in the statistical result. But this, though inevitable, is unfair to an occultist theory of their origin, since 16 of them, though not veridical of death, were coincidental in other ways. *E. g.*, 6 were collective, 2 were reciprocal, 1 was voluntarily produced by the distant agent, 2 were premonitory and 3 were veridical, but not of death. But let this pass. There remains another unfairness to occultism in our systematic rejection of all vaguely plural cases. I rejected 19 percipients in all for this reason, but 7 of these percipients gave us coincidental cases, 2 of them being apparitions at time of death.

"We can afford to be very generous. Suppose we throw in these 19 subjects as if each stood for one non-coincidental case. Suppose we multiply for oblivion by 10 instead of 6½, making 900 cases in all. Suppose we take only ½ of our 12 veridicals. We shall still get $\frac{6}{900} = \frac{1}{150} = 126$ times $\frac{1}{19000}$, the chance-probability."

The objections to be urged are:

"1. *Smallness* of numbers. But the agreement of our figures with yours goes against this.

"2. *The collectors packed their sheets with veridicals.* As a matter of fact, they say they knew the answer beforehand in 3, possibly in 4 cases. In 5 cases they state their ignorance. In 3 they say nothing. From the warning against packing with yeses and the very large number of veridicals that the collectors furnish *separately*, this objection is probably not very important.

"3. *The veridical cases are not strong.* They are not. Only 5 have any corroboration, and in no case is it first-rate. Our best cases are not among these. But this is an argument at any rate in favor of the sincerity of the Census; and since coincidentals and non-coincidentals are treated homogeneously (at least all the deliberate treatment going against the statistical result, where they are treated otherwise than similarly), the ratio of the surface figures is perhaps a fair one.

"But I never believed and do not now believe that these figures will ever conquer disbelief. They are only useful to rebut the assurance of the scientists that the death-warnings, if not lies, are chance coincidences. Better call them lies and have done with it."

I make this quotation, first because of the facts themselves, but mainly because I have above too easily granted the ambiguity of the evidence for such phenomena, and I wish to show, by a new example, how, when two interpretations are possible, it is not always the scientist's which has the greater numerical probability in its favor, or which is the more carefully or conscientiously weighed.

WILLIAM JAMES.

PSYCHOLOGY AND LOGIC—FURTHER VIEWS.

The discussion opened in the May number of the REVIEW, by Dr. G. M. Stratton, on the proper statement of the relation between psychology and logic, is one that may profitably be followed up; and let us hope that it will be. Dr. Stratton's paper is marked by a large lucidity that we have now learned to expect in what he writes; and, from the point of view likely to be held by the majority of his readers, it will probably appear conclusive. To some, however, and of these I confess myself one, it will be thought-provoking rather than satisfying, and its value will lie rather in the graver questions which it suggests than in its settlement of those nearer to the surface which it directly discusses. In the hope of leading to a still fuller comparison of views, I wish in the present article to bring out some of the more prominent queries that Dr. Stratton's paper has stirred in my own mind. I do this the more willingly, because the view he advocates, while strongly opposed to that which has generally been current, is one which (I believe) has also been advocated by Professors Strümpell and Münsterberg; very likely, therefore, it may be shared by many of the experts in the new psychology; so that Dr. Stratton's independent defense of it, so clear and forcible, will probably have the effect of fixing in the convictions of the younger psychologists (and logicians, too) a doctrine of which I am persuaded we ought at any rate to say that it does not reach the bottom of the question, however truly it may supply a needed advance from the view earlier prevalent, that logic is adequately described as simply a province of psychology, and of psychology regarded as a science of observation.

It must seem plain, I think, that Dr. Stratton has made his propositions out unanswerably, if psychology is to be defined as he evidently assumes that it is, and, indeed, expressly declares it to be—as the science whose distinctive and ultimate problem is to explain mental phenomena, in the sense, solely, of determining their 'natural causes;' that is, the chain of regular and systematic antecedents that are found on critical observation and experiment to attend them in consciousness. Logical *norms*, imperatives over thought-values, Dr. Stratton rightly says can get no footing by an observational science; that is, none *as* imperatives. He admits, of course, that psychology as an observational science cannot avoid taking note of the logical forms, as facts of consciousness; these belong, in short, to *descriptive* psychology. His point is, that they cannot properly belong to *explanatory* psychology, when explanation is defined as the determination of merely 'natural' causes; and he implies that explanation, in this sense, is what constitutes the gist of psychology—is, in fact, what makes psychology psychology. To put his case in a different way: Logic, like ethics and æsthetics, is a normative or legislative science—a science of mandatory standards of value. Consequently, it cannot be made out by any inquiries into the natural causes of conscious facts; nor, on the other hand, can it contribute at all to the settlement of what such causes in any specific case specifically are. Logic, as a conscious fact to be explained, must accept its explanation, so far as any may be forthcoming, from explanatory psychology; and, *per contra*, psychology must accept from logic all the canons of thought-integrity, precisely as it must accept from ethics the canons of moral integrity, and from æsthetics the canons of taste.

On this view, one thing is noticeable that Dr. Stratton perhaps overlooks, or at any rate has understated. He admits that logicians are in the habit of trenching on the ground which he has reserved to psychology, and thinks this is not seriously reprehensible, provided the offenders, and others concerned, clearly understand where and when the trespass is committed. The logic people, he implies, are in such cases dabbling in descriptive psychology, and it would help things if they clearly knew and acknowledged the fact. But he omits to say, and thus prevents us from knowing whether he notices, that it is beyond their power to do otherwise. Yet is it not plainly the truth? For how in the world is the logician to make any statement of his science, without, for instance, drawing the distinction between conceptions, judgments and syllogisms, and describing accurately in generalized definitions what these forms of conscious fact are? This *inevitable* trespass of the logician upon the psychological preserves, even if it be only in their outer border of description, and, because of this inevitableness in the trespass, the reciprocal participancy of psychology in an *essential* act of the science of logic, stirs thoughts in one which I confess I do not know how to get rid of consistently with stopping at Dr. Stratton's doctrine; and I find myself wondering whether he, and those who share his view to the full, have reckoned with it to the bottom. If psychology and logic are really so clear of each other as the new doctrine implies, then why can they not be expounded in entire separation? Why *must* the logician take a hand in psychology, willy-nilly, and *perforce* sin against his own canons of division? I have my suspicions that the trouble comes from the very definition of psychology with which this view sets out, and that the very conclusiveness with which the view follows from that definition should be a warning to us that something is wrong in the definition itself.

In this brief discussion I shall not even attempt to reach any final solution of the question here involved; much less to vindicate it. I shall be satisfied if I can make it clearly apparent that a defect of clarifying view exists, and give some hint of the direction in which we are to look for a view that is more comprehensive. I must say, too, that in these suggestions I am aiming at a school of views rather than at Dr. Stratton's own, and with an eirenical rather than a polemical motive. For I suspect that the discussion of the apparently superficial question raised by Dr. Stratton, if pushed to its depths, will expose a clue to the dispute between the so-called old psychology and the new, and indicate the way to its reasonable solution.

The view of the relation between psychology and logic presented by Dr. Stratton admits that the province of conscious fact covered by logic is also covered by descriptive psychology, but excludes it from explanatory. But what justification can there be for this abrupt arrest of the chief function in the new psychology? If psychology in its function of description must take cognizance of our apprehension of the logical norms, as a psychic fact, why must it suffer sudden arrest of its function of explanation in presence of that fact? Is it not, on the contrary, bound to explain the norms, if they are facts that it can describe? Or, is the difficulty this, that the very description which it gives of them shows them to be of such a nature as passes its powers of explanation? The latter is the manifest fact; as Dr. Stratton notices, when he says, correctly, that the contents of logic supply us with a *canon of criticism*, and that this canon must be accepted *ab extra* by observational psychology. But, I insist, why should a *science* of mind accept *anything*, merely *ab extra* and as sheer, dead, unintelligible fact? Dr. Stratton would very likely answer, that an all-embracing and entirely thoroughgoing science of mind would not do so, but that psychology, as he understands that term, and as the new school understands it, lays no claim to being a science of mind all-embracing and entirely thoroughgoing. Rather, his contention is that there is no *one* science of mind that is thus comprehensive and profound, but that our knowledge of mind, such as the knowledge is and can be, is only possible through several collaborating sciences; and that the exact discrimination of these, and in general a careful observance of their boundaries, is an important aid in the best performance of their separate and their collective tasks.

I would not be thought to deny the truth of the last proposition, nor its relative importance. But I incline to insist that its importance is *only* relative, and that its truth is not absolute. Moreover, what is of greater import is this: The partial and relative truth brought out in the undeniable proposition that a merely observational psychology, with its explanation (so-called) by means of unvarying antecedents accurately determined, is incapable of explaining anything canonic in consciousness, forces us to ask: What, then, is the source and the authority of such canonical forms? To say that nobody can possibly tell; that they must be accepted 'from without,' absolutely; that there is no conceivable psychology which can ever throw any rational illumination on their legislative authority—this is the same as saying that they have no rational worth at all; that their operation in our consciousness is just the dead pressure of an impenetrable necessity, and that therefore they are no guide to *truth*, but simply express the brute fact that we are as we are, and are forever incapable of knowing what our judgments are worth, or whether they are worth anything; that, at best, we can only register the processes of our being, and describe the connections of its mechanism.

But if this is so, let us not forget to draw the fact out to its full conclusions. For if there is within our powers no capacity to warrant the objective worth of our canons of judgment, then we are not capable of any psychology at all, even in the humble sense of description and 'natural' explanation; we are not capable of *any* science, however modest in its aims; nay, we are not capable even of that last apparently fatal judgment, that we can only register our own mechanical, meaningless processes. For the judgments of psychology not only have to accept, as Dr. Stratton says, the canons of logic 'from without,' but they have to *submit* to them; they *depend* on them, and all their results are vitiated by them if once we admit that they have no ascertainable worth. If they, too, are only mechanical facts, untransparent to intelligence, then their operation in us can lead to no real explanation, even of a partial and relative sort; our psychology ceases to be a *science*, in anything but the name, and even our professed registration of dead facts dissolves into illusion; everything becomes the seeming of a seeming, the dream of a dream.

But when we seriously ask for the source of logical canons, for the source and credentials of their authority, what possible answer can we really get but this: That they rest on the simple witness of the mind, on the testimony of self-consciousness? And what name can we give to the account of this last possible court of appeal, unless we call it, in some proper and inevitable sense of the word, psychology? It is not a merely observational, much less an experimental psychology, doubtless. But it seems none the less to be a fact that can neither be escaped nor evaded. It is, rather, the Rational Psychology, necessary and unconditional, free from all contingency, which, in no way hostile to the psychology of observation and experiment, but demanding this as its indispensable aid and supplement, furnishes the indispensable presuppositions and conditions without which no experimental science, and not even experience itself, would be possible. It is true enough that logic is no part of simply observational psychology, any more than ethics—I mean, of course, an ethics of Duty—or any more than æsthetics is. But as an observational psychology is only a partial psychology, which depends for its methods and for the validity of their results on the validity of logical laws *as* laws, and, like these must finally go back for its warrant to the rational psychology of an absolutely real self-consciousness, completely autonomous, it would appear that the true answer to the question of the relation between logic and psychology is found by denying, indeed, the inclusion of logic in empirical or observational psychology, but by including it, along with all the sciences, normative or explanatory, in the comprehensive whole of rational psychology. This whole is organic and genetic (rather than simply generic) relatively to these sciences, and, among them, to the new or experimental psychology. Rational psychology, as the account of the conditions in pure self-consciousness for experience in every form, is the heart and real meaning of the old psychology; and the new psychology, while rightly correcting the error of the old in attempting to extend the authority of direct self-consciousness over the details of experience, and justly disputing this intrusion into fields where pure thought unsupported by perception would be fruitless, must acknowledge its reciprocal dependence upon this heart of the new as well as of the old—this soul, in fine, of all science whatever. Without the recognition of this organic psychology, the secret of *truth* in the judgments of all psychology, there would be no solution of the question how logic is related to psychology or to any other science; nor, above all, how logic can be an Organon of science—a law of physica things as well as a self-legislated law of mind.

I know how easy it will be to feign a discredit of all the foregoing by affecting that it is all a mere dispute about the use of a word. But in the somewhat current employment of the word Psychology in the meaning of the new psychology only, there is an ignoring of a real fact—the fact of self-consciousness and its pure constituents that are the bases of all science, as they are likewise of all possible experience —a fact which must be recognized, by whatever name it may be called. In that great fact lies the real being and vigor of the soul; and it would be a strange and irrational victory that should strip from the authenticating account of that fact its time-honored and legitimate title of Psychology—the Science of the Soul in the highest and most significant sense of the words.

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THE PSYCHO-SENSORY CLIMACTERIC.

In following the results of recent studies in the visualizing powers of various classes and individuals one is struck by the predominance of this power, according to the reports, at least, among naïve classes and conditions. By visualization is here meant the power of actually reproducing the object of memory with its color and outlines, as contrasted to the remembering of something about the object. It is doubtless true that the latter is frequently mistaken for the former and is so reported, and the writer has been inclined to believe that the difference above referred to, as appearing in the cases collected by Galton and Preyer, and particularly by American observers, is largely due to the greater discriminating power of those who confess themselves to be non-visualizers. The difficulty of properly interpreting and describing these experiences is quite like that which perplexes the blind who have had no experience of vision.

Non-visualizers may frequently have a strong visual memory in the sense of recalling accurately the judgments based on the perception. One curious and important form of such memory is that which may be called dynamic and consists in the translation of the data of visual sensation into terms of latent muscular contraction. Thus one may find himself able to reconstruct the outline or image when, with pencil in hand, he attempts to draw it. Doubtless such minds, when attention is directed to an object, instinctively go over it dynamically. The writer when desiring to fix the outlines of an object is often obscurely conscious of a mental tracing of the outlines in which the movements of the eye are associated with vestigial reproductions of the effort sensations which would have been called out in the manual tracing. It seems to the writer that there is a vast substrate of dynamic vestiges beneath what is called memory. 'Trying to impress a thing on one's mind' is simply the revival of dynamic vestiges. It would be impossible to a mind which should really be a *tabula rasa* and reproductive power would increase with the increase of experiences. The commonest forms of these dynamic vestiges are connected with speech, but we should not fail to recognize that memory is not dependent on language.

It is a familiar fact that many experiences have a fringe of spatial association. Thus it is common to find that one has located the events or objects in a narrative (quite unconsciously to himself at the time) in some part of his spatial sphere—to the right or left, above or below —or perhaps with reference to some prominent preëxisting element in spatial consciousness. In attempting to recall the object one finds that the 'clue' belonging to the unrecalled but not forgotten object is a locality. "It *does* seem as though, if I could fix my attention on the upper left corner of the visual field intensely enough, it would reappear!" Here again is a dynamic vestige. It is not necessary to illustrate further, but it will be admitted that these and many other elements in reproduction substitute in mature experience for a visualized reappearance of the impression.

It may be safely assumed that the materials of the visual image are infra-cortical, while the really vestigial elements are generated within the cortex. If this is correct there is a very useful distinction to be made between the two classes of elements in the reproduction. It will be remembered that one class is not in consciousness. When one recalls the image with its colors and form *objectively* to mind, as good visualizers claim to do, this objectivity is due to the same cause which gives to the actual object its outwardness. The materials come to the cortex and are there construed exactly as in the case of primary vision. This may amount to actual hallucination or may be so slight as to but faintly tinge the reproduction. On the other hand, when a poor visualizer, like the writer, recalls any object it is by a marshalling of cortical vestiges and judgments and thus the result has none of the objectivity just described.

The writer has elsewhere insisted on the necessity of discriminating sense content from sensation and precisely this distinction is here required. The good visualizer reproduces the sense content along with the cortical vestige, while the non-visualizer only requires to revive the cortical or conscious equivalents of this content to have what serves for him the purposes of a complete reproduction. This difference is like that seen between the sophisticated and the naïve individual in the act of portrayal to others. The latter finds it necessary to reproduce by gestures and mimicry as many as possible of the events described, while the former is content to rely solely upon his repertory of verbal sounds. In exactly the same way the naïve mind requires to reproduce the actual pictures which called out the conscious states of a previous experience in order to live over the latter, while the trained consciousness disdains such mediation. The more one is accustomed to live in the world of abstractions the more complete does this independence of the sub-conscious mechanism become.

Upon the theory of consciousness elsewhere advanced some interesting suggestions may be hazarded. If consciousness depends upon fluctuations in the equilibrium of concentric forces in the brain, and if the anatomical mechanism for the supposed complicated balance of interdependent forces is primarily within the cortex, it does not follow that reactions of other centres do not affect the equilibrium. On the contrary, it is of course chiefly the stimuli from lower centres which constitute the material for consciousness. Ordinarily the impact is from without, but its *form* is determined by the cortical intermediary mechanism. Yet the distinction between the force and its form probably does not lie wholly in the cortex. Even in mature life these limits are undoubtedly more or less shadowy. In naïve and primitive states it may be supposed that the equilibrium of consciousness is still less limited and the *form* of conscious reaction may be more largely influenced by direct participation of lower centres in the equilibration which determines the nature or 'content' of consciousness. The progressive limitation of the sphere of consciousness may be part of the evolutionary process by which a diffuse somatic consciousness has been concentrated and freed of corporeal limitations, or, to speak broadly, 'spiritualized.'

However all this may be, a series of very interesting practical problems in pedagogy associate themselves with the change in method of reproduction which we may call the *psycho-sensory climacteric.* It will be admitted that the undoubted gain in efficiency and promptness afforded by the habit of abstract reproduction is accompanied by a distinct sacrifice in objective independence and clearness, just as the narrative of the savage is likely to be more forcible and vivid than that of the 'Cultur-mensch.' It becomes a serious question therefore whether the premature attempt to hurry children into abstract topics, such as may require recollection of symbols for the effects experiences rather than the simple data of experience, and especially such as call for introspective study, may not deprive the child of a precious store of concrete data which ought to form the substantial foundation for later thinking.

There are also several professions where the power of objective memory is of the highest possible service. The artist and word painter particularly must see the object before his mind's eye and it cannot be doubted that the creations of fancy partake of the same character as the actual reproductions of sense.

It may be urged that more attention should be given to symmetrical mind training in secondary schools. It is a grave mistake to suppose that memorizing of a text is an all-round training in memory. The formation of dynamic vestigial associations other than speech are necessary. Thus, training in drawing and music are of the highest importance quite independently of any interest or value attaching to the arts themselves. They serve to reënforce the memory with powerful dynamic associational elements which arm the thought with vigor and persistence. The practice in composition and description, especially the description of objects and events actually in experience, is of the highest importance. Abstract mathematics should come later than natural history. Physics especially can hardly come too early, while chemistry is far less adapted to an early stage. Descriptive botany and zoölogy are among the most important means for serving the end sought, *provided* the instructor have a vital acquaintance with the subjects to enable him to discriminate in presenting the data and to clothe them with flesh and blood. The average 'general lesson' in natural science has been a frank failure and has done vast harm. It was my privilege (?) to hear a bevy of school teachers cramming for their general work and hustling 'waders,' 'swimmers,' 'scratchers,' etc., into captivity in a most heterogeneous fashion with many a groan and sigh, and it was little surprise to discover that their pupils a few days later were echoing the sighs and groans, while the 'waders,' 'swimmers' and 'scratchers' reappeared in motley never known to nature.

There is one class of associations which is of still greater importance for the fulness and happiness of life; it is the subtile connection between visual and auditory reactions and the circulatory centers and their reflexes. What the association with the motor reflexes does for the life of action and thought, that with the vasò-reflexes does for feeling and emotion. A certain nuance or intensity or contrast of colors produces in a sensitive nature a distinct circulatory change. To a much greater extent is this true of sounds. It is certain that this change is not a secondary result of an emotion, but a direct physiological result, though a very important part of the substructure of feeling. To one who frequently yields himself to the touch of these fairy fingers and permits the fibres of his being to pulsate to the preëxisting harmonies of his own being there comes a ripeness and richness of experience casting a glamour over prosaic drudgery and keeping fresh the springs of thought. Nor is this in any purely sentimental sense, for when a familiar psychosis is clothed with a pleasing or effective feeling tone it has the cogency of a novel sensation; it has the freshness which makes it a power in reproduction and dominant in association. Plato's ideas of the influence of æsthetics in education are found to be sustained by the best results of modern neurology. No man can afford, even from the standpoint of intellectual efficiency, to permit the premature advent of the psychical climacteric.

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PSYCHOLOGICAL LITERATURE.

An Outline of Psychology. EDWARD BRADFORD TITCHENER. New York, The Macmillan Co., 1896. Pp. xiv+352.

The prominence given to quantitative determinations, the division of sensations into peripheral and central, and the definition of consciousness as *process*, are some of the features that mark the present work clearly as coming from a disciple of Wundt. Putting the question of standpoint aside, Professor Titchener's book is a most valuable addition to the literature of experimental psychology. Indeed, it is not too much to say that it is the best digest of that subject that has yet appeared in English. The style is clear and the arrangement of the subject logical. The author starts with a three-fold problem: to analyze mental experience into its simplest components; to discover how these elements combine, and to bring them into connection with their physiological conditions. The third problem is brought up here and there throughout the work, while the other two form the basis of the main discussion. The elementary processes of sensation, affection and conation are first treated, in order; then the complex processes of ideation, feeling and voluntary movement; finally the higher syntheses —memory, self-consciousness, reasoning, etc., on the intellectual side, the sentiments on the affective, and the reaction problem on the active. This progressive scheme is admirably worked out, though unfortunately the casual reader is not likely to trace it through the latter part.

As might be expected, considerable space (three chapters) is devoted to sensation, and the treatment of this element is most thorough. The author emphasizes *quality* as the distinctive mark of the sensation, rather than its intensity, extent or duration. He spends some time in discussing the number of distinct qualities and the experimental methods of testing them. It is a great desideratum in a general survey of the field to distinguish clearly the various minor sensations —joint, tendon, static, sexual, alimentary, pain, etc.—and in this Professor Titchener has succeeded very well, considering the limitations of our present knowledge and the confusion that has prevailed regarding some of them. He classes physical pain, about which there has been so much discussion of late, as a common sensation, due to the excessive stimulation of any sense-organ or the injury of a sensory nerve. Weber's Law is discussed at some length; he gives it a purely physiological interpretation, and believes that the deviations observed near the limits of sensibility are due to variations in the excitability of nervous substance with different degrees of stimulation (p. 89). The treatment of affection is interesting because of the prominence which the author gives to it as an element of consciousness distinct from sensation. He ascribes it to the general anabolic and catabolic bodily processes, rather than to the action of special stimuli, and devotes several pages to showing the difference between pleasantness and unpleasantness (as he terms pleasure and pain), and pleasant and unpleasant sensations. Professor Titchener comes out squarely against the theory of a third conscious element corresponding to activity. The two elementary 'active experiences' are conation and attention: conation is 'the experience of effort or endeavor,' but its conscious elements are all either sensation or affection; and attention reduces to the same terms. Yet although they have no direct conscious equivalent, the physiological processes which make up what the author terms *bodily tendency* are important factors (he thinks) in determining the direction of psychic life.

Part II. deals with the complex processes which arise from the union of elements, and constitute the real elements of adult life. The perception and idea are treated as practically identical, although only peripheral sensations are concerned in the formation of the former, while 'central sensations' always enter into the latter. A chapter is devoted to the association of ideas—a term, by the way, which the author regards as inaccurate and misleading, and only adopts on historical grounds. It is difficult to see the force of some of Professor Titchener's distinctions between association classes. For example, after distinguishing simultaneous and successive association, he divides the former again into *associative supplementing* and *word-association*. Psychologically speaking, these two are quite similar, and neither of them is very different from the primary idea, since the name, *e.g.*, often forms as essential a part of our idea of an object as its odor, or some other sense element. Here, as in one or two other places, the author seems to leave the psychological standpoint for the metaphysical. Under successive association he recognizes two forms: The *train of ideas* and *association after disjunction*. The latter includes judgment, which is disposed of in a single paragraph (p. 207). The term *association after disjunction* itself is open to criticism. It is defined as 'the coming together again of ideas which were originally together, but have somehow become separated' (p. 205). But the judgment: 'This house is a hotel' (to use the illustration given, p. 207), may consist in adding to a certain house-idea elements which have never been associated with *this particular complex* before; and if the author simply means that *some* elements in the complex have been previously associated with the (hotel) idea, this is equally true of all kinds of association,—according to his own formula: *ab–bc*,—and cannot be the mark of any particular class.

Passing to the affective side, Professor Titchener makes a neat distinction between affection as an element and feeling as the complex which we experience—a distinction corresponding to that between sensation and idea. Emotion is a still higher complex and 'stands upon the same level of mental development as the simultaneous association of ideas.' His classification of the emotions as present and future on the one hand, and subjective and objective on the other, will probably meet with criticism from several quarters, the most obvious objection being the omission of the *past*. A chapter on voluntary movement follows, and shows the change which has come over the treatment of this phenomenon in the past five years. The innervation-sense theory is cast aside. Action is arranged in an ascending series of classes, from impulse to reflex and instinct, thence to the more complex forms of selective, volitional, and finally automatic action.

Part III. treats of still more complex processes. It is not altogether clear why memory should be placed here, with self-consciousness and reasoning, rather than with ideas. On the affective side, the analysis and classification of the intellectual and æsthetic sentiments is especially able. The chapter on synthesis of action furnishes a good summary of the reaction-time experiments; unfortunately Professor Titchener follows the Leipzig view implicitly, and ignores the type theory of reaction which has been established independently by Baldwin, Flournoy and Angell.

The concluding chapter is on the nature of mind. The author is content to assume the principle of psycho-physical parallelism and leaves the ultimate question to metaphysics. In the final section he quotes Lotze, who speaks, of course, from the metaphysical standpoint. This quotation might better, perhaps, have been omitted, as it is rather beyond the ordinary reader, and may lead him to believe that the author is endeavoring to dodge the issue, while the rest of the chapter is an earnest attempt to show that the question really does not belong to psychology to settle.

In estimating the value of Professor Titchener's work, it must be borne in mind that it is expressly designed to be a résumé of *experimental* psychology (see Preface and p. 19). Unless this is clearly understood, we may be apt to protest against the summary way in which certain mental processes are dismissed. Judgment, *e. g.*, is a highly developed and specialized process, and as such deserves, like speech, some extended notice in a work on general psychology; in experimental work it is scarcely distinguishable from several other forms of association, and may properly be treated under the same heading as they. It would have been better if Professor Titchener had qualified his title by inserting the word *experimental*, and avoided the chance of misconception.

An outline work cannot, of course, be expected to take up every disputed point; but in order to be reasonably thorough it should certainly mention the more important differences of opinion. Professor Titchener's book fails in this respect. The author says nothing about alternative theories of physical pain (p. 65), or emotional expression (p. 227); in discussing conation he does not mention the 'innervation-feelings,' so that when the term comes up later, in another connection (p. 237), it is quite without explanation. The names of those associated with prominent theories are withheld in many cases. Thus the three-color theory of color perception is adopted (p. 49) without any reference to the names of Young or Helmholtz, and there is no mention of Hering's theory or the retinal vibration theory of Charpentier. The reader of a *scientific* text-book has a right to know the prevailing views on important points, whether they agree with the author's or not; if there is no room for discussion, the principal literature on the subject should be cited, at least. Moreover, it is not too much to ask that the sources be cited for the experimental results that are given. The description of experiments is necessarily very condensed in the present work, and references to the originals might prevent misunderstanding in many cases; or readers might easily wish to pursue the matter further,—*e. g.*, to inquire about the various complications of conditions in reaction-time experiments, to which Professor Titchener refers (p. 327). Careful search fails to reveal a single reference to modern psychological literature in the entire book. This is certainly a most singular omission and is much to be regretted. The book is, in a word, too self-complete. It lacks thoroughness, and while it is extremely suggestive, it takes no pains to direct into proper channels the desire for further reading which it will undoubtedly provoke. The failure in this respect is apparently not due to any real dogmatism on the author's part, for the general treatment is broad, and there is no attempt to slur an issue. It seems to spring, rather, from too great a desire for condensation, or an under-estimation of the reader's capacity.

In the way of minor criticism may be mentioned a slight tendency to alter accepted terminology, which is scarcely in place in a book of this character: cognition is made a special kind of recognition (p. 266); the terms *pleasantness* and *unpleasantness* are used instead of pleasure and pain, etc. However, this is not so marked as in the translation of Külpe's work. The author occasionally ventures upon the 'etymological argument;' *e. g.*, in speaking of the principal colors (p. 49), and in discussing the origin of association, etc. (p. 301). This kind of argument is best left to the old-school psychologists.

In spite of its omissions (and minor commissions), Professor Titchener's work is an able presentation of psychology viewed from the experimental standpoint. The analysis is sharp and thorough, and in this respect the book will be of value to every 'school.' As a text-book it has a wide field before it, and we may hope, besides, that it will find its way into the hands of the 'laity,' and help to dispel some of the grotesque notions that are prevalent about experimental psychology.

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Lehrbuch der allgemeinen Psychologie. Dr. Johannes Rehmke. Hamburg and Leipzig. 1894.

The work before us represents a field of labor of which every psychologist must recognize the importance. In proportion as a science develops, it becomes more and more reflective, and the need of questioning and of restating its fundamental assumptions becomes more and more keenly felt. The appreciation of this need has evidently prompted our author to his present task, and the thoughtful, painstaking tone of his effort gives it a claim to respectful attention.

In a preface devoted to a discussion of the nature (1) of science in general and (2) of special science (Fachwissenschaft) he sets forth the aim of the former to be the attainment of unquestionable clearness (p. 1). This ideal of science is only to be reached through a continual questioning of given experience. But the answers to the questions that arise within the science militant are only to be obtained by an appeal to the object (p. 4). We find here the old assumption that the subjective (pp 1, 2) clearness and objective truth must ultimately correspond. The discussions of the past, of Descartes, Leibnitz and Spinoza have not demonstrated the necessity of such an assumption, and Rehmke does not appear to be conscious of a problem.

Science then asks questions and appeals to an object for answers. This object Rehmke defines in his somewhat peculiar terminology as a 'concrete,' *i. e.*, an element of experience which is a unit including changing phases. A special science has for its problem the laws of change of its 'concrete' object (p. 5). The object of psychology is the mind (Seele) (p. 10). In the work following the three parts are devoted respectively to (1) the essence of mind, (2) the momentary state (Seelenaugenblick), (3) the mental life (Seelenleben). The first part appears as a philosophical preparation; the second and third parts together fulfill the foregoing definition of a special science.

The philosophical standpoint worked out in the first part, stripped of much that is individual in Rehmke's way of expressing it, may be simply stated as follows: The world of experience presents two concrete forms of being—the material thing, and the self or mind (p. 40 seq.). These two concrete individuals, although completely different from each other, do not belong to two worlds that are separable from each other. Separateness (Geschiedenheit—by which Rehmke means numerical distinctness) (p. 72) of concrete individuals implies that the difference (Verschiedenheit) between them is not complete. Thus physical objects are separate because they have a common space quality of which they can represent different particularizations. But between physical thing and mind there can be no generic connection; they are 'schlechthin verschieden.' Hence they cannot be separable. On the contrary, in any momentary consciousness they are absolutely identical. "The possibility of a concrete consciousness existing at all depends precisely upon the condition that one and the same element of experience can be at the same time physical and mental" (p. 70).

The discovery of the paradox that the physical and the mental are at once totally different and perfectly identical, so far from discouraging Rehmke, furnishes him with a ground for congratulation. Thus we find, as the expression of his final position, the following: "The difference between concrete mind and physical thing * * * is so complete that the physical can be at the same time the mental; the otherness of thing and mind is so fundamental that, just for that reason, that which belongs to the concrete thing can belong at the same time to consciousness" (p. 73). The swing of the passage cited would seem to indicate that Rehmke enjoyed this paradox, and as accompanying the statement of a final position his complaisance suggests a little the picture of Nero fiddling while Rome burns. And yet the present reviewer does not wish in the least to deny the fundamental truth contained in the foregoing paradox considered as a stage in the development of an ultimately consistent position. The dialectic through which Rehmke arrives at this position, and convicts others of errors in failing to recognize one or the other element of the paradox, is quite skilful and on the whole appears sound. Still, as a final position, few, I suppose would remain satisfied with Rehmke's statement, and, indeed, I venture to think that it rests upon a misapprehension easily discoverable, to wit: a failure to distinguish between what is immediately given in a moment of consciousness, and the context to which a larger 'reflective' experience finds it to belong. That dangerous abstraction, the immediacy of the moment, will contain, if the abstraction be complete, no distinction between physical thing and mind. Reflection may, to use another dangerous phrase, consider this immediate in different relations, one of which makes it a part of the history of a concrete thing, the other a part of the history of a concrete mind. In proportion as we perfect the abstraction (really highly reflective) of the momentarily immediate we do not obtain two totally different things that are identical (a meaningless paradox), but we lack the material out of which to construe two things at all.

We must pass over two exceedingly interesting discussions on the origin of the mind and on the interaction of mind and body, to consider for a moment the classification of mental states to which Rehmke's general position leads him. He regards mental life as a whole as made up of a series of momentary mental states (Seelenaugenblicken) following a temporal order; but not necessarily continuous. The momentary state is made up of subject-content (Subject-moment) and mental attribute (Bewusstseinsbestimmtheit). There are three such attributes: the object-consciousness, state-consciousness and causal-consciousness (Gegenständliches-, Zuständliches-, Ursächliches-Bewusstsein). These elements are immediately given in consciousness, although an extended experience is necessary before the attributes can be distinguished in thought (gedacht, p. 489). The above classification is selected in place of the old division of mental states into 'thinking, feeling and willing,' on the ground that these latter terms do not imply immediately given mental characteristics, but process in time and are definable by relations to the external world (pp. 145, 349). On this ground also Rehmke rejects the dual division of Brentano and Münsterberg. The 'relation of consciousness to an object' is not the basis of classification that pure psychology can adopt; it belongs to physiology, to logic, or to ethics (p. 349).

It is to be presumed then that subject, object, state and causal consciousness are not definable, and on the whole Rehmke does not attempt to define them. Yet, the object-consciousness is defined as consciousness of an 'other' (p. 144). 'Other' namely than the mental state of the moment, a relation surely that cannot be immediately given in the momentary-consciousness. Still more striking is Rehmke's further subdivision of object-consciousness, presumably from the same standpoint of 'pure-psychology,' into perception (Wahrnehmung) and representation (Vorstellung). One is surprised to find that the distinction rests on the ground that the representation is conditioned only by a cerebral state, the perception by a peripheral nerve excitation (p. 158). But suppose Rehmke were perfectly consistent, it is still true that either this subject-consciousness and these mental attributes are definable or are they not. If they are to be defined it might well 'gravel a philosopher' to discover how this might be done without involving relations that go beyond the moment and include the 'external world,' as the psychologist ordinarily uses the term. If they are not definable why call them by different names? for they have become wholly inarticulate. It is impossible to harmonize Rehmke's later and more able treatment of 'Denken' (§44) with what he here takes to be the 'pure psychology' standpoint.

In his general style Rehmke shows himself to be possessed of that kind of courage (in which the Germans are frequently not lacking) which does not fear to be dry. Add to this that he is technical and diffuse, and his book will be seen to offer little charm to the lover of beautiful style. But these very faults speak in his favor among those who prefer consistency and clearness to beauty of form. The use of technical terms lends the author far-reaching categories of criticism and of construction. The diffuseness reveals a conscientious struggle to be clear. The utility of these two faults goes far to excuse their homeliness. And then—if one is to traverse a desert, why not ride a camel?

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Psychology and Psychic Culture. BY REUBEN POST HALLECK. Instruction in Psychology, Louisville Male High School. New York, American Book Co., 1896. Pp. 366.

In psychology, as in the early development of other sciences, books were at first written for other scientists rather than for students, but now the time has come when we may expect psychology, which is at present studied in so many different grades of educational institutions, to be presented in the form known in other sciences as 'science-made-easy' text-books, a term descriptive of unwise attempts to make subjects artificially easy. The author of this book has certainly thought of students in preparing his work and has avoided technicalities interesting only to specialists, and he has not given what to the student are only meaningless classifications and empty generalities, as have so many writers of psychological texts, but the book is so full, not only of illustrations drawn from every day life and from literature, but of analogies and comparisons, that it is certainly open to the charge of belonging to the type of text-book named above. The author has read considerably in recent psychology and usually states the results of research with approximate correctness, but he knows nothing of true scientific method, and his treatment of Weber's law displays shallowness and misconceptions that would be a disgrace to our ordinary high school student, hence none of his statements can be relied upon by readers as correct unless verified by reference to standard works.

The chief defect of the work is the prominence given to the interesting, in the treatment of every topic, both in the space devoted to the different parts of the topic and in the character of the illustrations used. For example: the chapter upon consciousness and attention is a very interesting *introduction* to the subject, but the important part of the discussion given under the head of 'Laws of Attention' occupies only about a half page, which is only half the space given in another chapter to the comparison of reflex-action to a barrel hoop, and in the chapter treating of presentation. Although many of the important truths of modern psychological research are incorporated into the discussion, yet there is nothing to help the student to distinguish between the absurd exaggerations of a French rhetorician to the effect that it is possible for epicures to distinguish by taste 'which leg a partridge has been accustomed to sleep on,' or to tell 'under what latitude a wine was produced as accurately as an astronomer can predict an eclipse,' and the generalizations made by a scientist after thousands of careful experiments. It is altogether probable that students in this and other cases will note and remember the striking statements and illustrations rather than the important facts stated and truths illustrated, unless the teacher using the book takes special pains to emphasize what the author has drawn attention from by his sensational treatment of less important parts.

The remarks thus far made apply more particularly to the psychological portion of the work. In the discussions of 'psychic culture' that follow each general topic the author gives some good practical suggestions, but the treatment is in general shallow, showing a lack of knowledge of fundamental principles of the science or philosophy of education. This is indicated by his somewhat rambling treatment and by the apparent basing of his directions upon such absurdities as that practice in perceiving one set of qualities or objects will educate one in the perception of all kinds of objects and qualities, and that whatever association will enable a person to remember particular facts will be good for his memory, without reference to the injurious or helpful effects upon the thinking powers resulting from the habits of association that are thus formed.

There can be no doubt but that the author has consistently carried out the views expressed in the preface. "Especial effort has been made to enliven the hard dry facts of the science by employing illustrations and anecdotes to elucidate them. No one knows better than the psychologist that it is of little use to present the best of subjects in an unattractive way, because facts devoid of interesting features will not secure attention." If the author were more of a student of education he would also know that the only interest worth cultivating is a direct interest in the subject itself. It is very doubtful whether the method of treatment adopted in this work will in a very large proportion of instances lead to such a result, and it is certain that no student of this book will get any practice in earnest careful study, unless he gets it from study outside of the text. Hence, although the book has many merits, especially for general readers, it cannot be recommended as a text by those who believe in making *students* of their pupils.

E. A. KIRKPATRICK.

WINONA, MINN.

New Essays Concerning Human Understanding. LEIBNITZ. Translated by ALFRED G. LANGLEY. The Macmillan Co. Pp. xix+861.

The translator could hardly have chosen a better work to put into the service of English students in the history of philosophy. Interest still centers in the questions about human knowledge, scarcely less so than in the age of Locke and Kant; and our age has not outgrown the need of a rediscussion of those problems that engaged the great minds of that period. To have, then, in his own language, the New Essays of Leibnitz is for the English student an almost inestimable service. For one can hardly appreciate the significance of Locke's philosophy, its strength or its weakness, who does not read the Essay Concerning Human Understanding in conjunction with these critical essays of Locke's great contemporary.

Familiar as I had thought myself to be with Locke's Essay, my reading of Leibnitz not only led me to a profounder apprehension of the problems raised by Locke, but it opened also to my mind new aspects of those questions with which these men were engaged. The Essay of Locke becomes a new book when read along with the New Essays of Leibnitz.

Of the character of this book as a mere translation I am not qualified to give a critical judgment; the translation has every appearance of being carefully and conscientiously done; the English is certainly good, as good as it could be according to the design of the translator, which was 'to represent as faithfully and as accurately as possible, and in as good English as its form and expression admitted, Leibnitz's exact thought.'

Professor Langley has, however, done more than to give us a very good translation of an important part of the philosophy of Leibnitz; he has done a piece of fine, scholarly and most valuable editorial work; he has enriched his volume with notes and annotations which, by their comprehensive character and their judicious selection, should be of the greatest help to the student; he has seemingly spared no effort in putting this work of Leibnitz into its historical setting; passing over no name or circumstance without some note adapted to make his author's thought more intelligible. To be commended also is the translator's incorporation of the selections which form the appendix of this volume. These pieces serve admirably to acquaint the student with the position which Leibnitz occupies in the historic development of philosophy; they constitute a good orientation in the philosophy of Leibnitz.

As to contents and scope, the book contains the following: Gerhardt's excellent introduction to his Edition of Leibnitz's New Essays; this is followed by Leibnitz's earliest published thoughts upon Locke's Essay in 1696; then follow two fragments published by Gerhardt for the first time; a sketch of Locke's Essay, published in the *Monatliche Anzug* in 1700, with a supplement which appeared a year later. Then follow the New Essays entire, which occupy the body of the volume. The appendix of about ninety pages contains chiefly essays which exhibit Leibnitz relation to the Philosophy of Aristotle and to the Cartesian Philosophy.

The long list of additions and corrections, fifty pages in all, is inserted in this place, owing to the circumstance that most of the important matter contained in them was not available until that part of the translation of which this matter relates was already in type.

An exhaustive and well constructed index of nearly one hundred pages completes this rather massive book, but in which there is really no superfluous matter, when the translator's design and the excellence of his work are taken into consideration. JOHN E. RUSSELL.

WILLIAMS COLLEGE.

La psychologie des sentiments. TH. RIBOT. Pp. 444. Paris, Alcan. 1896.

This book is without doubt the most important of Ribot's works. He has summarized in it his lectures at the Collège de France, and it is surprising to find what a large amount of material he has been able to place in this volume extending to not more than 450 pages. The work is divided into two parts of equal importance, but of very different character. The first part is devoted to the simple elements of emotional life, physical pleasures and pain, moral pleasure and pain, the inner conditions of emotion, memory for emotions, and the relation of the association of ideas to emotion. Throughout this part the author most frequently makes use of physiological observations and experiments, drawing especially from the psychological laboratory. In the second part he reviews the special emotions—fear, anger, affection, love, the social, moral, religious, æsthetic and other feelings, and here he has made use of anthropology, the history of customs, of the arts, of religions and of the sciences. He himself has well described this change of method. He says: "Some have an unshaken faith in laboratory experiments, but the evolution of the feelings in time and space, through the centuries and among the races, is a laboratory whose operations have extended through thousands of years and on thousands of men, and of which the historical value is very great. It would be a serious loss to psychology to neglect these records. * * * Though mental life has its roots in biology it only develops in society." It seems to me that this second part is even more interesting and original than the first. We find treated in it, in a manner to which psychologists are not accustomed, questions of great importance, such as that of the religious feelings. The chapters on the instinct of cruelty and on the moral feelings are models of clearness, conciseness and good sense. On the other hand, the first part suffers somewhat from the fact that systematic psychological investigation has not yet covered the field of the emotions. The account, for example, of the physiological effects of joy and sorrow, is injured by the confusion of the author, which indeed he shares with all his predecessors, between true and false vaso-constriction. I believe that this entire subject will soon be remodeled, thanks to the great extension in the use of the plethysmograph.

Let us now review briefly the author's chief theses. He has carefully described the effects of pain on the organism, holding that pain is a quality of sensation and not a sensation. He argues forcibly that pain does not consist in a state of consciousness; all the effects of pain may be observed in cases where consciousness is absent. There is not only an analogy between physical and moral pain; they are identical and the innumerable modes under which physical and mental pain are presented depend on the sensory or intellectual elements which accompany it. Psychological states include simultaneously elements of pleasure and of pain, and according to circumstances the one predominates over and inhibits the other. The product in consciousness is the result of the difference. Pleasure is not, as is often maintained, the opposite of pain.

In the following chapters, M. Ribot studies the pathology of pleasure and pain, including the enigmatical case of pleasure taken in suffering. A special discussion is given to neutral states, states of complete indifference, which are admitted by Wundt, and given an intermediate place between pleasure and pain, as transition states. Ribot, without expressly committing himself to one point of view or the other, holds that individual differences should be specially studied. Neutral states would seldom occur in nervous people who are in a state of perpetual excitement, they would doubtless occur much more frequently in the case of apathetic characters of limited intelligence. In concluding this general discussion of pleasure and pain Ribot takes up the two questions of the *how* and the *why*. As regards the former he maintains the general formula that the cause of pleasure is an increase of activity, and of pain a decrease of activity, but he also points out that this formula is very vague, and that the exact details of Meynert are highly hypothetical. In discussing the second question Ribot is equally cautious. Why is there a relation between pleasure and utility and between pain and what is injurious? The theory of evolution provides without doubt the best answer calling to its aid the theory of the survival of the fittest, but there are many exceptions to the rule which are difficult to explain. The relation between pleasure and utility and between pain and the harmful is a formula which owes its origin to philosophers. That is to those who always and before all else seek for unity.

After pleasure and pain the emotions are taken up, the general characteristics of which are depicted with care. Ribot accepts the

James-Lange theory, but in developing it, interprets it in a particular way. "James and Lange," he says, "adopt a dualistic point of view, like that of the theory they seek to refute, the only difference being in the inversion of cause and effect. In the one the emotion is the cause of which the physical manifestations are the effect, in the other the physical manifestations are the cause of which the emotions are the effect. In my opinion it would be a great gain to eliminate from the question all idea of cause and effect, all reference to causality, and to substitute for the dualistic point of view a unitary or monistic conception * * * * No state of consciousness should be dissociated from its physical conditions; they form a natural whole which should be studied as such. Each kind of emotion should be considered from this point of view; what movements of the body, vaso-motor disturbances, respiration, the phenomena of secretion, express objectively, the correlative states of the mind express subjectively. It is a single event translated into two languages." This is not the place to discuss this opinion, suffice it to say that it completely changes the conditions of the problem.

Under the name 'inner conditions' of emotion the author studies their physiological processes and under the name 'exterior conditions,' their signs and expressions. Darwin's theory is discussed but preference is given to that of Wundt. A very interesting chapter is devoted to the classification of the emotions. Ribot has selected a score of classifications made during a period of fifty years by well known authors, and divides these into three groups according to their character. The first group is a classification of the emotions as pleasurable and painful only; under the second group they are classed according to their empirical characters or according to their origin. The third group is an intellectual classification. Purely intellectual states are classed, and thus the emotional states that accompany them. Ribot rejects all the classifications because they are purely hypothetical and because the complex emotions cannot be arrranged in a linear series. Two chapters conclude this first part; the one on memory and the emotions had previously been published in the *Revue Philosophique*; the other is on the rôle played by the association of ideas in the development of the emotions and in the production of complex emotions.

With the second part the special analysis of a certain number of the more important emotions is taken up. Three emotions are correlated with the instinct of preservation: 1st, the emotions and instincts relative to nutrition; 2d, fear and its variation, repugnance; 3d, anger. Of each of these psychological states the author gives a very complete picture. He first indicates the physiological side of the subject, the possible localization and the organic effects; he then gives a description of the emotions based on the testimony of consciousness; he traces their origin and development and concludes with their pathology. We may note, in passing, that the *phobies*, which constitute the pathology of the emotion of fear, are of two principal forms, fear, properly so called, and repugnance. The evolution of anger is traced with great felicity. It is made up of three necessary stages, a reflex of defense and of attack; anger, which is only a differentiation of this reflex; and hate, resentment, in which the same reflex is delayed and sometimes concealed. Hate is not the opposite of love, as has been so often maintained; hate cannot be a primitive emotion, because it includes the phenomena of inhibition, and inhibition is a complex and a late development.

The chapter on the affections contains a number of subtle and pertinent observations. The author treats the affections and sympathy together; he defines the latter as the keen representation of the emotional states of others and shows that this representation, if the affections are not included, does not suffice to constitute what in common language is called sympathy, or in other words, altruism. It is thus the affections which, added to the sexual instinct, constitute the foundation of love. In short, the greater part of the emotions are complex, they are derived from simple emotions, by evolution, by arrest of development and by the combination of several simple emotions.

Of the complex emotions the author reviews first the social and moral feelings. This chapter is well worth reading. It contains a classification of the principal kinds of societies, and a sketch of the feelings to which they give rise and the stages of their evolution. Ribot does not agree with many authors that the family is the primitive form of social union from which the clan and the tribe have arisen. He thinks that the tendency to live in society is irreducible and inherent and has developed independently of the family. There follows a complete exposition of moral feelings which do not arise, as claimed by the intuitionists, from an idea, from a formula (the categorical imperative). It is from the outset a spontaneous instinct, finding its expression in customs which later become conscious and reflective and are expressed in written laws and in the abstract speculations of moral philosophers. Further, this instinct of morality has two aspects, the first positive, corresponding to feelings of benevolence, the second negative, corresponding to those of justice.

The second part of the work closes with some chapters on religious emotions—treated with unusual wealth of detail—on the æsthetic sense and on intellectual feelings, and lastly, two chapters (which had already been published elsewhere as articles) on normal and morbid characters and on the decay of emotions. A last chapter summarizes the leading ideas of the book. These are as follows: emotional manifestations are neither qualities of sensation nor of a confused intelligence; they are primitive facts prior to intellectual life. In the emotional life two elements should be distinguished, sensations of pleasure and pain, and the tendencies we call desires when they are accompanied by consciousness and appetites when they are unconscious. These are incipient movements prior to all experience of pleasure and pain. It is a blind force; "and this blind force, when it attains its object, experiences satisfaction and seeks for it anew because it is pleasant."

In conclusion I may say that in my opinion it matters little whether the reader can agree or not with the views of the author. Even those who dissent will find in this book what, at the present time, they will seek for vainly elsewhere, a place where all researches hitherto made on the emotions are brought to a focus. It is a fine testimony to the activity of French psychology.

A. BINET.

PARIS.

The Florentine Painters of the Italian Renaissance, with an Index to their Works. By BERNHARD BERENSON. G. P. Putnam's Sons. 1896.

This little book is a companion to the author's *Venetian Painters* and forms the second of a series of handbooks intended chiefly as guides to travelers in their artistic pilgrimage through Italy. In approaching the Florentine School, however, Mr. Berenson has not been able to avoid some philosophical discussion of the nature of the appeal made by these painters, and has given us a little sketch of an æsthetic theory, not without psychological interest.

The Florentines, he tells us, were preëminently figure-painters, and in this figure-painting they devoted their attention, not to color or sentimental expression or symbolic meaning, but to pure form. Now form has three dimensions, and to render the third dimension upon a flat surface is the chief technical problem of this art. Until this problem is solved the figures are merely decorative or symbolical, and painting remains, so to speak, a literary art. It has the value only of an illustration. But when the painter, by his rendering of *values*, produces the illusion of bodily existence, and creates an imaginary space in which his figures live, he affords us a truly artistic pleasure. This pleasure may be greater than that of perceiving an object in actual space, because the indications of form, the values, may be emphasized in representation. Instead of the confused impressions which the actual object would probably send us, the painter strives to give us only the significant data, only those sensations which will help us to conceive the form, in all its complexity, as real and solid. The painter thus gives us a lesson in perception, and teaches us to appreciate bodily form and to enjoy it.

In the course of this analysis Mr. Berenson advances two opinions which, at least as presented here, without evidence to support them, must seem arbitrary and hasty to the psychologist. One is that the third dimension is perceived by association of the visual image with 'tactile' sensations, or 'muscular sensations inside my palm and fingers.' The influence of feelings of movement, apparently in the arms, is once mentioned, but the other possibilities in the case are ignored. The second opinion advanced is that æsthetic pleasure consists in stimulating to "an unwonted activity of psychical processes . . . which here, free from disturbing physical sensations, never tend to pass over into pain." A work of art, for those who are capable of enjoying it, heightens the intensity of the act of perception. It "overwhelms them with the sense of having twice the capacity they had credited themselves with; their whole personality is enhanced" and they 'feel better provided for life.'

It would be manifestly unfair to criticise these opinions as if they represented the author's complete theory of æsthetic values. But his views are worth considering as indications of the direction in which an intelligent connoisseur looks for an explanation of his own judgments. He looks for it in the act of perception itself, in an acceleration of the process by which the conception of a physical reality is gained. While we may pass over the illustrations of this principle which Mr. Berenson comes upon, and which are chosen, perhaps, somewhat at random, we must welcome the attempt, on the part of a professional critic of art, to trace æsthetic pleasures back into the primary processes of sense and imagination. Such an attempt is a proof of directness and vitality in the author's criticism and at the same time it is an encouragement to the psychologist who might fear to miss the essence of the higher artistic feelings while digging in the psychophysical field. It is there, Mr. Berenson tells us, that those feelings have their roots.

The painters whom he reviews would generally have agreed with him; for it is not the artists themselves, or those who have a technical appreciation of art, that repel an interpretation of its effects as immediate and physical. The opposition comes rather from those who, without specific training or sensibility, find in art only a general stimulus to their vague, heterogeneous emotions. To such persons the significance or use of art lies in the ideas, moral, religious or sentimental, which it suggests to them and which alone they are capable of feeling strongly. But the artist, in whom perception is vivid and accurate, and who is ready to understand its marvelous complexity, finds meaning and value in the forms themselves, apart from extrinsic associations.

The opposition between these two points of view is, indeed, not fundamental. A man like Michael Angelo may well combine them, since he had capacity enough to feel to the utmost both the beauty of bodily form and the tragic and religious burden of life, so that he could give his visions the greatest plastic reality while he kept his soul strained towards the highest moral ideals. But these interests are independent, and it was perhaps the desire to identify them, and the despair of doing so, that made the art of Michael Angelo in a way swollen and sad. For, as Mr. Berenson says, the Florentines were not merely painters; they were men of varied gifts and general interests who found in painting only an occasional and partial means of expression.

G. SANTAYANA.

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Manuale della Semejotica delle Malattie Mentali. Guida alla diagnosi della pazzia, per i medici, i medici-legisti e gli studenti. Vol. II. Esame psicologico degli alienati. ENRICO MORSELLI. Milano, Vallardi [1896]. 12°. Pp. xviii, 852.

Of this thick volume perhaps five-sixths of the pages are in fine print. It forms, consequently, the most thorough and minute analytic symptomatology of insanity in existence. I say analytic, because, although the author divides it into 'synthetic' and 'analytic' halves, it yet deals solely with separate and elementary symptoms, and nowhere touches on those complex aggregates of symptoms that make up the various types of insane personality. The result is a book rather for reference than reading. Whoever wishes to find *everything that can possibly be said* about a given function, such as physiognomy, language, conduct, perception, memory, will, etc., in the insane, can do no better than consult its pages. At the same time the very completeness, largely brought about by filling to their utmost all the compartments of an exhaustive scheme marked out in advance, is more mechanical than practical. We doubt, for example, whether such an experimental examination of 'consciousness' as that for which directions are given on pp. 735–765 can ever be applied by an asylum physician to a single patient. It includes determinations of the acuteness and range of the various senses, and of Weber's law as applied to each of them; chronometric determinations of the rhythmic oscillations of the attention; *ditto* of the simple and the variously complicated reaction-times, with their disturbing conditions, again applied to all the senses; measurements of the area of the conscious field by the Wundt-Dietze method; observations on automatic movements subconsciously performed when the attention is distracted; exploration of the patient's suggestibility under hypnosis; and finally, of his subjective consciousness of altered personality, or the reverse. First and last we get almost the whole of Wundt's Physiological Psychology, and the author may well speak in his preface of the great labor he has thrown into his work. An Englishman or a Frenchman would have lightened the burden by throwing out much of the only hypothetically practical matter. Prof. Morselli's book is, in fact, only one more instance to add to the number which prove the affinity between the Italian and the German turn of mind. His style is better, but his learning is as ponderous, and his multiplication of Greek terms as great as that of any Teuton—*e. g.*, *hyperpraxia* and *hypopraxia* for the over-activity and inertia of mania and melancholy, and no end of *dis*-es, such as the various species of *disnoesia*, namely, *disesthesis*, *disgnosia*, *dismnesia*, *disfantasia*, *dislogia*, etc., etc.

But all this does not detract from the solid value of the matter contained in the volume, or from the author's good judgment when, instead of enumerating facts, he pronounces opinions. His pedantry entirely breaks down, *e. g.*, when speaking of the methods of the 'exact' anthropological school. Except as a disease of central organs involving the *conscious self*, insanity is unintelligible. "What has so far been explained with respect to the genesis and forms of mental disease by all the measurements of cranium and stature, by all the sphygmography, the urine-analysis, even by the dynamometry and æsthesiometry, of which so many of the followers of objective empiricism boast, and which they confound with the true experimental method? I have read with the greatest serenity all the histories of cases that come coupled with this address. But, arrived at the end of the somatic and physiological inquest, and at the beginning of the psychological examination, I have always had, when it was a question of the primary forms of mind-disease, the impression of an absolute cleft and utter lack of connexion between the two examinations * *

* * I conclude that, whilst still granting to anthropology and nerve-pathology the confidence they well deserve, we must restore psychology proper to its rights" (p. 21).

One of the things that most strikes me in Prof. Morselli is his contempt for the absoluteness of the accredited 'types' of psychosis ordinarily named and recognized. Individuals are types by themselves, and enslavement to conventional names and their associations is only too apt to blind the student to the facts before him. "The more I study and examine the insane, the profounder grows the conviction in me that the purely symptomatic forms of our classifications are based on the expressive appearances which insanity assumes according to the temper and pattern of the subject whom it affects. In short, individual subjects operate like so many lenses, each of which refracts in a different angular direction one and the same ray of light" (p. 143). Elsewhere (p. 53) Prof. Morselli writes: "Many forms of insanity which the nosographs distinguish and circumscribe within sharp limits are, despite their apparent divergence, only *clinical varieties* or *different stages* of a probably *unique malady* which is *modified diversely according to the personality of the individual whom it affects.*"

Unfortunately we are carried no farther by the author along this curiosity-exciting path. W. J.

I Sogni e il Sonno nell isterismo e nella epilessia. DOTT. SANTE DE SANCTIS. Roma, Sociéta Dante Alighieri, 1896. 12°, Pp. 216.

An inquiry into the manner of sleeping and dreaming in 98 cases of hysteria, 45 being of the light, and 53 of the grave variety; and in 91 cases of epilepsy, of which 25 were inveterate and showed intellectual decay, whilst of the remaining 66 fresher cases, 45 had 'classical' attacks, whilst 21 were of *petit mal.* The amount and depth of the sleep were noted, as well as the frequency and character of the dreams, and their relation to the phases and incidents of the malady. The work is carefully done, and contains a very complete reference to the literature of dreaming and sleep. The minuter statistical details must be seen in the original. The main results are that hysterics and the lighter epileptics sleep badly, but the better the older the case. In epilepsy with *grand mal* the sleep is good. Sleep-walking (contrary to a common opinion) is rare in both diseases; sleep-talking is frequent. Abrupt awakening, and hynagogic hallucinations, are common in both diseases. Nightmare (*incubus*) also; but the more so in epilepsy, in which it tends to disappear with age. As for the dreaming, age and repeated epileptic attacks seem to make it less frequent as well as less easily remembered. The dreams of epileptics are simple, those of hysterics complex and dramatic, and often 'macrozooscopic.' One of the most interesting points connected with the dreams of hysterics is their influence on their waking life and course of the symptoms. Dr. de Sanctis found this influence; but only in 6 of his cases did it seriously aggravate the disease. In more than half the cases the dreams of the previous night influenced the humor and conduct of the following day. W. J.

SUBLIMINAL CONSCIOUSNESS, ETC.

Subliminal Self, or Unconscious Cerebration? ARTHUR H. PIERCE. Proceedings of Soc. for Psych. Research. Vol. XI., pp. 317–325. (1895.)

Reply to the same. FRANK PODMORE. Ibid., pp. 325–332.

Ueber Spaltung der Persönlichkeit (Sogenanntes Doppel-ich.) DR. FREIHERR VON SCHRENK-NOTZING. Wien, Holder, 1896. 8°. Pp. 23.

Die Mehrheit geistiger Persönlichkeiten in einem Individuum. Eine Psychologische Studie. DR. S. LANDMANN. Stuttgart, Enke, 1894. 8°. Pp. 186.

The well-known observations made on hypnotic and hysteric subjects and automatic writers by Gurney, Janet, Binet and others, and which by their authors are supposed to prove that mutually disconnected currents of conscious life can simultaneously coexist in the same person, are subjected to critical reinterpretation by Messrs. Landmann, von Schrenck and Pierce. All these writers deal with theory, no one of their essays bringing out any new kinds of facts.

Mr. Pierce thinks that the performances, such as the executing of orders, answering of questions in writing, etc., that may go on whilst the subject's upper consciousness ignores what happens and is otherwise occupied, are all due to unconscious cerebration. Educated to certain aptitudes, the brain is now able to perform them whilst its consciousness is altogether engrossed with other conduct simultaneously going on. The notion of multiple consciousness has no limit if we begin to use it. There is no *direct* proof of the supposed split-off consciousness, for by the hypothesis, if split off it is never known to the 'person,' and if remembered later it was probably not split-off.

Mr. Podmore objects that Mr. Pierce talks as if consciousness and brain-processes formed an alternative. He himself favors the parallelistic theory and considers some consciousness to accompany all processes, its degree fluctuating; he disbelieves in two disconnected systems of consciousness forming a definitely *dual* control, and thinks the facts best covered by the conception of a conscious field with a single bright center and a margin stretching indefinitely away into twilight.

Baron von Schrenck holds somewhat similar views. He believes that only those processes that form the 'crest of the wave' of cerebral excitement give rise to full consciousness. But the wave-crest is always shifting its place; and a system of cerebral operations, A, *started* with full consciousness, can run on for a certain time, even although the wave-crest may forthwith have proceeded elsewhere and started another system, B, which latter then in its turn may run on sub-consciously, whilst the wave-crest reverts to the now subsiding system A, and with a stroke of full consciousness starts it up to activity again. We have only to suppose, now, that the pulses of conscious attention that accompany the A-process and the B-process severally, as the wave crest oscillates to and fro, fail to combine into a united memory system, and we have, according to von Schrenck, all the phenomena of simultaneous double self, so-called, or split consciousness, explained on the type of *alternation of systems of ideas* with the memory-bridge between them gone. The theory of *simultaneous coexistence* of fully conscious systems thus falls to the ground.

Dr. Landmann accounts for the facts by assuming three levels of brain-operation, only one of which has *self*-consciousness attached to it. This latter is the consciousness of psychic activity as such. It is attached exclusively to certain (undesignated) processes in the cortex, and only he who has it can say 'I.' The second level is that of ideation and association without this self-consciousness (*unselbstbewusste Vorstellungen*); whilst the third level belongs to the 'subcortical centers' and is often spoken of as 'unconscious' by Dr. L., though he also repeatedly speaks of the *Vorstellungen* and *Gefühle* that go with the subcortical centers. Whole groups of cortical cells can fall into isolated activity; the subcortical cells can act by themselves, and the cells of self-consciousness can either coöperate or not coöperate with the rest. But the self-consciousness is either wholly where it is, or else not there at all; so that the ordinary talk about fractioning of the personality, upper and lower selves, etc., is absurd, 'personality' and 'self' being indivisible elements of the mental life. The only possible doubling of the self is where it acts in alternation, first with one and then with another system of ideas.

Where one self appears to be writing automatically whilst another self converses at the same time through the mouth, the latter self is the sole real self engaged; the automatic performances being the work of the 'non-self-conscious' parts of the cortex, and of the 'unconscious' basal ganglia. Dr. L. applies these principles in an intolerably rambling style and with tedious minuteness to the elucidation of Janet's and Binet's observations, thinking (strange to say) that their merely descriptive phrases about 'dissociation of the personality,' etc., consitute a 'theory' irreconcilable with his own.

The really urgent problem in these phenomena of split or uncoupled mental life is that of the conditions of splitting and coupling-again, be they cerebral conditions or physical conditions, or both. *What happens* when any one system of ideas or of brain activities get so thoroughly shunted off and ignored by the consciousness that goes with the rest? On this problem no one of our three authors can be said to throw any more positive light than Mr. Myers or Janet. Myers would be the first to say that his phrase 'subliminal self' is only a temporary noun of designation for a certain group of facts. Janet would say the same of his phrase 'defective power of conscious synthesis.' But their three critics, each with his own notion of a unique activity of self-consciousness which cannot be split, seem to me to carry matters backwards rather than forwards, and to tend, if anywhere, towards a somewhat pre-Lockian and non-empirical point of view.

W. J.

Introduction to Philosophy. F. PAULSEN. Translated by F. THILLY. With an Introduction by W. JAMES. New York, Henry Holt & Co., 1895. Pp. xix+437.

Professor Paulsen's Introduction has been in the hands of the students of philosophy in the original long enough to have become familiar. To those who have not known the original, Professor James' preface will be sufficient recommendation. The features of the book which strike the present reviewer may be briefly indicated. First, the readable character of the author's expositions is noteworthy. Then the comprehensiveness of the book is surely a great recommendation of it for class-room work.

As to doctrine, several things are striking. Professor Paulsen's 'voluntaristic' psychology gives character to his philosophical views all the way through (see pp. 313, 320 f.), and it is this standpoint, possibly, that leads him to subordinate the problem of epistemology, as he does, to that of philosophy in general (pp. 349, 353). But the tendency of the book which gives it its most prominent character is what may be called its 'animistic' view of nature (99 ff.)—in a good sense. Paulsen goes the length of finding a 'world-soul' to be more than a figure (107 ff.). His arguments for it seem to be inconclusive as other arguments recently urged in the same direction (*e. g.*, the interesting theorems of Professor Royce). The argument of Paulsen, based, as it is, on analogy, for some sort of subjectivity in connection with the life functions, has great force; but when a similar argument is carried over into the inanimate world it gives occasion for a good deal of stumbling. Then, when Professor Paulsen goes on to appropriate the term 'pantheism' for his doctrine, he seems to open himself to a sort of criticism which Lotze avoided by avoiding this term, although his view was perhaps as near traditional pantheism—or as far from it—as this of Paulsen.

It is curious, but there seems to be in many a tendency to a sort of mysticism in conceiving the sort of 'world-ground' which modern philosophy is reaching after. We go the length of a 'monism,' call it theism, hope the absolute is 'personal,' and yet shrink from an animistic view of nature. Perhaps Professor Paulsen's frank acceptance, both of the latter doctrine and of a much abused name for it, will tend to convince some readers that this course is better than the sort of vague mysticism in which we have been resting. But yet it seems to me that the final doctrine of the absolute will have to accept the distinction between consciousness with its experience, and mechanical nature with its law, and find a more profound way of justifying an ultimate monism than the simple way of reading into the minerals a form of experience which directly contravenes the distinction. In other words, the final synthesis of metaphysics would seem to be rather logical, as going beyond the distinctions of experience, than material, as being justified by positive agreements in experience. And just for this reason, the older method, which makes a critique of experience a preliminary problem, would come in to get its justification.

The book is the best thing we have in English, its matter is very modern, its historical expositions wonderfully illuminating, its divisions flexible, and its style direct. The only criticism I should make as a teacher is that foreshadowed above, that for an introduction it teaches a philosophy too directly. But then, that is what the author set out to do. The translation is accurate and idiomatic, but possibly rather too literal.

J. MARK BALDWIN.

Schopenhauer's System in its Philosophical Significance. WILLIAM CALDWELL, M. A., D. Sc., Professor of Moral and Social Philosophy in the Northwestern University, U.S. A., etc. New York, Chas. Scribner's Sons, 1896. Pp. xviii+538.

If anything can justify philosophical scepticism it is the present status of Schopenhauer. To some his thought represents the highest flights of speculation, its nearest approach to that ultimate essence of Nature which has been the *ignis fatuus* of philosophy since the days of Thales. To others his system, if system it may be called, is merely the futile attempt of a brilliant but ill-regulated mind to comprehend the world in which it lived and to evolve from its own discord practical principles for the guidance of more happily constituted souls.

With the latter position Professor Caldwell has no sympathy and, although in his preface he says that he has tried 'to strike a mean in the matter of the connection of Schopenhauer's philosophy with his personality,' he seldom recurs to the topic in his subsequent chapters, and when he does so fails to call attention to the most salient peculiarities of Schopenhauer's very peculiar temperament. Schopenhauer is, to him, a philosopher of profound significance. In shifting the object of philosophic contemplation from thought to will, from the logical necessities of the Hegelian dialectic to the concrete sequences of nature and the terrible realities of human passions and ungratified desires, he has given philosophy a status in the modern world which it never had before and which it will never lose. So deeply is Professor Caldwell impressed with the importance of this step that he tacitly ranges himself in general on Schopenhauer's side and speaks with sympathy of his views even when he feels compelled to differ with them. Yet he is in no sense a schoolman. Thoroughly as his own thought has been modified by reflection upon Schopenhauer's teaching, he shows no tendency to adopt without careful criticism and appreciation, and this very fact, which gives his book its chief philosophic value, makes it difficult for one who, like the present writer, possesses only a general acquaintance with Schopenhauer's writings, to discriminate the elements which are drawn from Schopenhauer from those which are due to the author's own reflection. The form of the book greatly increases this difficulty. Professor Caldwell makes no attempt to give a clear and adequate view of Schopenhauer's doctrines as he himself apprehends them and then to indicate the points in which they stand in need of revision or completion. In the opening chapter, 'A General View of Schopenhauer's Significance,' he touches upon the chief points of contact between Schopenhauer's thought and that of his age;

Chapters II. and III. deal respectively with his 'Idealism' and his 'Theory of Knowledge;' Chapter IV. with the 'Bondage of Man;' Chapters V. and VI. with his 'Philosophy of Art;' Chapters VII. and VIII. with his 'Moral Philosophy' and 'Philosophy of Religion;' Chapter IX. with his 'Metaphysic;' Chapter X. with 'the Positive Aspects of the System,' and these are followed by a brief 'Epilogue,' or resumé of the leading conceptions of the book. This arrangement gives rise to an amount of repetition and an expansion of relatively few thoughts into scores of pages which might have been desirable in the series of lectures upon which the book is based but is most unfortunate in a book. Chapters II. and IX., Chapters III. and Chapters IV., VII. and VIII., deal with approximately the same material respectively and might have been condensed into smaller space with advantage.

Professor Caldwell frankly adopts Schopenhauer's fundamental conception that the essence of Nature is will, striving, or effort, but, instead of following him in his assimilation of the ultimate Will to the blind forces of Nature, he tends to assimilate it to the highest manifestations of self-conscious will as found in man, or rather *vice versa*, man's will is, of all that lies within the range of his experience, the most faithful representative of the archetypal essence. Man's thought and discursive reason can be understood in a teleological sense only. It can serve to mirror his present environment and to throw a feeble and flickering light upon his path, but it cannot portray to him his true being or that of the Universe, nor can it enlighten him as to the ultimate end towards which the World Will, as manifested in the phenomena of Nature, and in his own blind longings and inner strivings as well as in his deliberate volitions, is leading him. With Schopenhauer's 'illusionism and confusionism' Professor Caldwell has no sympathy. Schopenhauer had himself only half learned the lesson which it was his mission to teach the world. He had grasped the familiar truth of idealism that there is no ultimate difference in essence between the subjective and objective sides of experience, but he had failed to see that reality is not to be sought outside experience, although it is not to be found in all its fulness within experience. Since Will is the essence of reality, the most complete revelation of reality in experience must be sought in those forms of experience which at once most fully satisfy the cravings of man's will and presage a still fuller satisfaction yet to be found. Such are the realities of Art, of Ethics and of Religion. With that recognition Schopenhauer's unwavering conviction of the truth of those realities finds a justification which he was never able to provide, the disappointments inherent in human life find their place in philosophy, but philosophy does not thereby become a system of pessimism and illusionism.

It would seem therefore that, although Professor Caldwell seldom or never uses the word 'God,' his interpretation of Schopenhauer brings us back to the familiar conceptions of philosophical theism, save that the Divine immanent in things is to be conceived rather in an active than a passive aspect. The life of the Ultimate Being is not a mere contemplation of its own perfection, as the older philosophers thought, but a constant endeavor towards the perfection of its creatures.

If I have failed to grasp the essence of Professor Caldwell's thought it is not from any lack of grace in its expression. Schopenhauer himself never wrote more charming pages. Like him, Professor Caldwell has caught the secret of good style; his reader's attention is spontaneously arrested by the transparent clearness of his thought and is free to follow and enjoy its development without voluntary effort and without fatigue.

Altogether the book is one of the most attractive and interesting that has appeared in recent years. The author, it is true, takes Schopenhauer somewhat more seriously than some of us are inclined to do, but such fundamental differences of opinion as to the relative value of philosophic methods, and their probable fruitfulness in the production of sound knowledge, are not profitable subjects of discussion and should not be made grounds for criticism.

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Moral Evolution. By George Harris. Boston and New York, Houghton, Mifflin & Co., 1896.

The title of Mr. Harris's book, 'Moral Evolution,' shows his main idea, viz.: that there is no conflict between evolution and ethics. Positively, he traces the harmony of the two along three lines: (1) evolution is recognized only when its results are known, and ethics is essentially a science of ideals; (2) both have the same material—self-regarding and other-regarding feelings—which are equally natural and are harmonious though not identical; (3) both are alike in method, there is gradual progress (Chaps. I., VII.).

Historically, this progress has consisted in the development of personality, that is, in the increasing participation of the individual in social functions. Pain and struggle lead to this; perversion or wrong is only an incident. Theologically, the ideal is the same as that given by history—"the person having the powers with which he is endowed and cultivating them in their true proportion and symmetry into the perfect character" (p. 71). This ideal is not identical with happiness, but in the long run ensures it.

The ideal of the good is the content or dynamic side, which determines the right or the ought, the formal or static side. The sense of obligation is what distinguishes man from animals. Its origin, Mr. Harris does not know. He has leanings towards some kind of instantaneous creation. But however it got here it will stay, he is sure, as long as man has ideals. But again, the distinguishing characteristic of man is said to be his 'recognition of the relative worth of the higher and lower goods of persons.' I do not see the relation between these two 'distinguishing characteristics.'

The transition from morality to religion is made by the reflection that since the outcome of evolution is rational man the process must be rational, and therefore implies God both historically and ideally. Further, if God is rational, He must be perfectly righteous, for if He were not, and yet imposed good on man, He would be arbitrary, which is contrary to the assumption.

The bearing of the last half of the book, on religion and theology, is not clear to me. The author seems to have abandoned his starting point of moral evolution and to be engaged with the idea that evolution having got us on so far it may now be dispensed with (as a principle) and a fixed moral content substituted.

The principal criticism to be made on the book is that its fundamental terms are either not defined or the definition is arbitrary. Thus, when the personal ideal is defined, the crucial phrase is 'cultivating man's powers in their *true* proportion.' What this true proportion is we are left to surmise, until in the last of the book we run across the statement that Christianity alone gives the true proportions. But Christianity in turn needs defining. Is it that of the gospels or of the churches of to-day? And if any special period is taken, how do we know of its finality?

Again, such terms as 'higher' and 'lower' goods are used, but are defined only by implication. I infer that by 'lower' goods are meant such things as food, clothing and shelter, but in an ideal of unified activities, such as has been given here previously, such distinctions are not valid, and if 'lower' is used in opposition to 'higher' it ought not to be labelled good.

The use of the terms good and bad is also as unsatisfactory as usual. In the terms of evolution, good is the normal, and bad the abnormal. But the question comes up, as always, how in any specific case can we *know* which act tends to the normal? As a rough and ready rule past experience may serve, but history never repeats itself. Life is a series of experiments. There is always a new element which makes the outcome of each venture uncertain, and hence the judgment of good or bad can be passed only after the act is done, and no standard is final in advance.

This element of newness is so characteristic in the idea of evolution that questions again arise concerning the relations of the moral and religious parts of Mr. Harris's teaching. He asserts, for example, the finality of the *contents* of the life of Jesus as the moral ideal, as well as of its method or spirit. Given evolution as a moral, not simply a physical, fact and it would seem to follow that if the spirit of Christ's life was perfect when lived, then it would require a different setting and content in order to be perfect if lived to-day. Or, again, and this is the point which we should expect Mr. Harris to insist on more, Christ may have been perfect in the sense that He embodied the law of all development. In that case the specific acts of His life are of no importance whatever, and would vary infinitely according to time and place. The unity of His life with that of the world constitutes His divinity and oneness with God.

This leads us to a consideration of Mr. Harris's idea of God—another term which is not defined. At times the term is used as if God were apart from the world, molding it to His will, perfect before it existed; and, again, as if God were inseparable from the world. It is argued that because the outcome of evolution is rational man, therefore the process must be rational and therefore it implies God.

I will merely point out here that the process might be rational without implying God, unless by God is meant simply reason in the world, without regard to whether this reason is or is not distinct from matter; and further, that logically there is no more need of imagining a creator of mind or reason than of imagining a creator of the creator of reason. The category of causation can not in any case be applied to the totality of experience, because it is a helpmeet within experience.

On the positive side, the book is valuable for the emphasis which it lays on the inter-action between the individual and society and their mutual dependence. The favorite illustration of the ellipse whose foci are the individual and society, both of which determine the curve at every point, is very striking. Another point also worth mentioning is the constant assertion that self-regarding impulses are just as moral as other-regarding ones, and that the two are not antagonistic. I can not help regretting, from the standpoint of ethical science, that Mr. Harris did not work out these points more fully instead of devoting his energies for more than half his book to an exposition of the truth of Christianity which is so generally granted that it is not needed.

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ETHICS.

The Relation of Intuitionism to the Ethical Doctrine of Self-realization. HENRY CALDERWOOD. Phil. Review, V., 4, pp. 337-351.

Intuitionism claims that the principles of conduct are given immediately by the reason, and are not the product of induction. Opposed to it are Utilitarianism and the system of Self-realization. Mr. Sidgwick, for the former, while criticising it for its lack of scientific precision, is yet forced to admit that an intuitive operation of the practical reason seems to be somewhere assumed in all moral systems. Does the theory of Self-realization offer us a better explanation of the facts? There are two phases of this theory, the high idealistic position of Hegel and Green, and the more humble position of the rational psychologists. They must be tested with reference to the *knowledge* of the law and to the *end* of action. The former phase of the theory is mainly metaphysical, and according to Green's own confession can give us no adequate account of what man's true self should be. How do we know what is right? The divine mind 'reproduces itself in the human soul,' says Green. This is really Intuitionism. The rational psychologists give us no clearer account of the process by which we reach a knowledge of moral truth. They insist that Self-realization is the end of action, but do not tell us clearly how we know what the true self is. We learn it, they say, by considering the process through which the institutions and rules of life have arisen out of the effort after an ideal, and have in their several measures contributed to its realization. But conscience is superior to institutions, and we need a philosophy of our knowledge of the *inner* law, without which, institutions and rules, and the objective ethical world itself, are inexplicable. Considered with reference to the *end* of action the theory of Self-realization is also inadequate. Thought must be self-centered as belonging to our consciousness, but the law of right conduct, and the motive for well-doing, and the end for which we live, all out-stretch self-satisfaction.

Morality the Last of Dogmas. ANTONIO LLANO. Phil. Review, V., 4. pp. 371–394.

The thesis of this article is that "in the course of time all moral feelings (those, that is, involving such ideas as obligation or compulsion, duty and the like) will disappear from the human mind and cease to have any influence upon the further development of the race." The basis for this belief is to be found in a knowledge of the nature and origin of conscience, and in the modern scientific conception of the world. Conscience is merely an abstract feeling of fear of punishment, and its origin is to be sought in the primitive conception of nature as an aggregate of superhuman beings, to whom man was responsible. Morality arose from this fear of external power. The modern tendency is toward individual freedom, hence the idea of compulsion or obligation must pass away from morality. There can be no reason why my individual feelings should form a standard for any one else. Moreover, the naturalistic or deterministic conception of the world must work toward the same end. Man's conduct is only a phase in the transformation of an infinite and eternal energy, and is no more subject to praise or blame than is the course of the stars. We cannot demand that a man should be other than his conditions have made him. Moral good and evil are meaningless terms.

Determinists have shrunk from these conclusions, and this has been urged as an argument against their theory, but two psychological laws explain the inconsistency between their theory and practice. First, action ultimately depends upon feeling, not upon judgment alone; second, a feeling which has become organic through heredity cannot be suddenly eliminated, even though reason has destroyed its basis. Hence we cannot expect the deterministic theory to change our moral feelings even after several generations have accepted it. Nevertheless, the growing sentiment of tolerance in religious and political matters is in reality a sort of movement towards what may be called moral indifference—toward the time when no man will condemn another for thoughts, feelings and conduct which are the necessary product of his organization and environment. NORMAN WILDE.

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VISION.

Ueber den Einfluss von Lichtstärke und Adaptation auf das Sehen des Dichromaten (Grünblinden). J. v. KRIES und W. NAGEL. Ztsch. f. Psych. u. Phys. der Sinnesorgane. XII., 1–38. 1896.

It has long been known that by mixing in the proper proportion light from the two extreme ends of the spectrum an absolute match can be obtained, for the eye of the partially color-blind, to every color sensation which it is capable of receiving, and in particular to every homogeneous light throughout the spectrum.[1] Soon after the obtaining of the first exact results of this nature, it was announced by König that the equations in question are not independent of the absolute intensity of the lights employed, that an equation which has been obtained at a high intensity no longer holds when the lights are turned down. The same variations were found to hold for the color-equations of trichromates as well, and they are summed up under the phrase 'departures from Newton's law of color-mixture;' the facts were absolutely denied by Hering, who said that if they were established it would be equivalent to an entire upsetting of the constitution of the universe, but they have been fully confirmed by other observers and are now admitted by Hering also.

The facts here referred to have lately won an additional interest on account of the present theory that the cones are the bearers of the color-sense and that the rods convey the colorless sensation only. König's observations have been criticised by Hering on the ground that he worked with too large a field, and hence that he did not avoid an irregular effect of the yellow pigment of the macula, and also that he did not give sufficient attention to the adaptation-condition of the eye. In order to meet these objections, and also for the purpose of having the observations confirmed by one more observer, Nagel, who

[1] The quality of the entire gamut of sensation throughout the spectrum for the color-blind is either yellow, or blue, or gray, and nothing more. It is not true, as is commonly supposed, and as is stated, for instance, in the Century Dictionary and in Johnson's Cyclopædia, that there are some who are red-blind, but have the sensation of green, and others who are green-blind, but have the sensation of red; this erroneous belief had its origin purely as a deduction from the Young-Helmholtz theory of color-vision (if there were three fundamental color-processes, red, green and blue, a partial loss of color-sense would naturally consist in a defect in one of these processes), and it is a lasting monument to the folly of making deductions from unproved hypotheses and then forgetting that the deduction has the same hypothetical character as the premises from which it was deduced. The true state of the case was first discovered by William Pole, F. R. S., in 1857, by reflections upon his own sensations (he was himself a dichromate, who did not discover his own defect until the age of thirty), and his result, for which he has never received due credit, is one of the most brilliant products of the application of pure reasoning to an apparently hopelessly confused mass of facts that has yet been witnessed. His conclusion has been abundantly confirmed by the cases of color-blindness in one eye only that have since been detected, the first being that of Becker, in 1879.

is a dichromate, has repeated the experiments, with various modifications, upon himself. The splendid color-mixing instrument, originally designed by Helmholtz and perfected by König (made by Schmidt & Hänsch, of Berlin) was employed; this is the second instrument of the kind, of any value, that has been constructed. It is of much less complexity than that used by König himself, but it was found to answer the purpose for these experiments. The field offered to the eye of the observer was two degrees in diameter ; its middle point was fixated, and the eye was kept constantly adapted for brightness. The principal difference between the yellow and the blue curve obtained by these observers and those given by König for the same colors is that in the present case the blue curve extends only to $\lambda 536$ instead of to $\lambda 600$,that is, overlaps the yellow curve much less, which means, in other terms, that the yellow in the immediate vicinity of the gray line of the dichromate's spectrum was much more fully saturated than König found it to be. This difference is readily explained by the fact that the field was here small and constantly central. By this means the participation of the rods in the sensation produced was almost wholly excluded, and the curves represent more exactly the visual process as it takes place in the cones alone. The discrepancy was greater here than in other parts of the spectrum because the cone-sensation reaches here a maximum of intensity. Another criticism which Ebbinghaus has brought against König (that the blue curves do not show sufficient coincidence for the dichromate and the trichromate) is fully met by the same consideration. At the same time it should be remembered that whether the addition of a small amount of blue is or is not necessary to effect equal saturation in the two halves of the field is a difficult observation to make; the 'equal amounts' of red and blue which go to make a pure gray are not equal in respect of brightness, but equal in color-quenching power, to use the appropriate phrase of Helmholtz—the red unit is in fact as bright as twenty times the blue unit—and such small quantities of blue as this are naturally difficult to measure.

The next step was to redetermine the distribution, throughout the spectrum, of the colorless sensation of the dichromate in a faint light, the twilight sensation, as v. Kries has happily named it. This was found to be sufficiently in coincidence with the same curve as found by König, after making allowance for a possible slight difference in the quality of the gas used, and Hering's curve is also the same, after reduction from daylight to gas light. The red end of the spectrum was found to be faintly visible as gray, if it was looked at sufficiently at one side of the fovea.

Since v. Kries' results agree with those of König as regards the two elements of the comparison, they naturally agree with them as regards the conclusion. According to the ideas of Hering, two lights which are equivalent at an ordinary intensity must have an equal white valence, or, since that is the same thing as their twilight-values, these also must coincide. But that is very far from being the case; the mixture from the two ends of the spectrum, which is for the dichromate absolutely indistinguishable from an homogeneous light in the yellow green at an ordinary intensity, needs to be made more than a hundred times brighter to match it in a faint light. And what Hering has considered to be possible sources of error in the experiment have here been entirely done away with. It follows that it is an absurdity, upon practical as well as theoretical grounds, to speak of a brightness of a color as being due to the brightness of its colorless component.

Ueber die Wirkung kurzdauernder Lichtreize auf das Sehorgan. J. VON KRIES. Ztsch. für Psychologie u. Physiologie der Sinnesorgane, XII., 81–101. 1896.

A large number of observations have been made lately on the subject of the secondary image which follows a brief excitation of the eye by a rather strong light. The phenomenon was noticed by Purkinje, who noticed everything; it was rediscovered by Professor C. A. Young, and the most detailed experiments upon it have been made by Hess, and especially by Bidwell, by whom it has been called the recurrent image. The observations upon it have been of a very conflicting nature; it is usually stated to occur one-fifth of a second later than the primary image, but Exner found no interval at all. In color it has been described as complementary, except by Hess, who found it to be of the same color. Bidwell and v. Kries discovered at about the same time that it fails to occur after excitation by red light, and v. Kries has now observed that it is also altogether wanting in the fovea; these two circumstances point strongly to the influence of the now commonly accepted difference of function of the rods and the cones of the retina, and consequently a study of the effect upon it of adaptation—a change of condition which takes place chiefly in the rods and which is without doubt a function of the visual purple—was very desirable. This v. Kries has now carried out; his method was similar to that of Bidwell, and consisted in allowing a spot of spectral light to fall upon a mirror which rotated upon an axis not perpendicular to its surface, and from that to be reflected to the eyes of the observer, who perceived a bright spot moving about a central point of fixation. Under these circumstances the ghost was very distinct, of complementary color to the primary image, and at a distance from it which translated into time was equal to one-fifth of a second, so long as the eye of the observer had not been adapted for darkness; the constant darkness adaptation of the eye was maintained, since the walls of the room were black, by frequently looking out of the window. The first image is sharply defined, not quite circular, but rather cylindrical, with a concave edge behind; the second gradually fades off in a faint trail, and its head is surrounded by a circle of more than ordinary blackness. But if the light is very intense, so much so, for instance, that the trail stretches out through the entire circle, then the first image is also much longer (this is, no doubt, the ordinary positive after image), and becomes joined on to the secondary image; this is the form in which Exner saw the phenomenon—without any interval.

If the eye has first suffered complete darkness adaptation (that is, has been kept in the dark for two hours, at least), the appearance presented is very different; the secondary image is of a brilliant white, and it appears almost immediately after the first image, which is consequently in shape more like a slender crescent. The secondary image, in one or the other of its two forms, Professor v. Kries very properly takes to be at least the principal cause of the phenomenon of the fluttering heart. Both forms alike vanish when the real image goes through the central part of the retina. One observer said that it seemed as if they slipped into a tunnel. The area of this ineffective space was about 35 by 38 mm. at a distance of 1 m. from the eye, which corresponds very exactly with the size of the space which is practically free from rods.

From various attendant circumstances, Professor v. Kries is forced to assume that there are two distinct reactions of the rods, not simply one reaction which takes place after adaptation both with greater force and with greater promptness. He suggests that one may be due to visual purple in the rods, and the other to that outside of the rods, assuming in both cases that the visual purple is a true visual substance, whose product of decomposition excites the nerve end.

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Theorie des Talbotschen Gesetzes. Von KARL MARBE. Wundt's Studien, XII., Heft 2, pp. 279–296.

The general statement of Talbot's Law is as follows: If two light stimuli successively and periodically excite the same point on the retina there will result either a series of separate sensations or one single sensation of a constant intensity and quality. This latter is identical with that sensation which would be excited if the light acting through one stimulation were distributed uniformly over that entire stimulation period.

Under these conditions there are four factors which promote the production of this constant sensation:

1. The decrease of the stimulation period.
2. The increase of the difference of duration of the two stimuli.
3. The decrease of the difference of the intensity of the stimuli.
4. The strengthening of the mean intensity of both stimuli.

If the succession of stimuli be given by means of a rotating disc, then a fifth factor enters, viz., the rate of movement. The slower the movement the less do the stimuli fuse. That the influence of these five factors applies to the fusion of colored light is proved in an experimental appendix to this paper.

The theory of Talbot's Law must explain both the general fact of fusion and also the influence of these five factors. This is found in a general photo-chemical principle. The photo-chemical action in the retina is not a summation effect, for if we fixate a white surface for two seconds the sensation is no more intense after the second second than after the first. Nor can it be limited to the 'elementary effect' of the corresponding time element, for then the series of stimuli would never fuse into a constant sensation. There remains the view that it is a function of the elementary effects immediately preceding and simultaneous with the sensation, these forming a 'characteristic effect group.' The excitation in the retina grows with the duration of the stimulus until the duration reaches a determined critical value.

We see, then, that as the equality of light dispersion progresses the 'characteristic effect groups' become more similar not only to each other but also to the 'effect group' produced when the light is uniformly distributed.

With this theory the explanation of the first four factors is not difficult.

1. The shorter the stimulation period becomes the more evenly the light is distributed over the whole period and the more nearly the 'effect groups' approach the 'elementary effects.'

2. By the increase of the differences of duration of the two stimuli the mean variation of the 'elementary effect' is lessened.

3. This also takes place when the difference in intensity is lessened.

4. By increasing the intensity of the whole series, the single 'ele-

mentary effects' will of course be increased. But with this there must be an increase of the difference which 'characteristic effect groups' shall have in order to produce a notable difference in sensation.

The fifth factor, the movement of contour, requires some further explanation. Suppose we fixate a black square on a white ground. One part of the retina will be affected by the light coming from the square and another by the neighboring white ground, and we see the boundary of the square sharply outlined. Now let us suppose that the square moves very slowly while the eye remains in absolute rest. Under these circumstances every 'characteristic effect group' will be determined by its own time element. There will no longer be a sharp boundary between the white surface and the square, for each point of the retina here will have a different time element, thus giving rise to sensations of proportional intensity. This will cause a gradual shading of the two fields, as the time elements gradually shade into each other in the direction of the movement. With light of a given intensity the width of this shaded portion will be proportional to the swiftness of the movement. If, instead of one dark surface, a series of them be moved before the point of fixation, their shaded portions will gradually widen with the rapidity of the movement until finally they overlap and fuse into a constant sensation. This is the state of affairs when the sections of the color wheel finally fuse. As this fusing process is a function of the movement of the edge of the surface it follows finally that a surface *a* with movement *b* is less favorable for fusion than surface *2a* with movement *2b*.

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A Preliminary Study of some of the Motor Phenomena of Mentat Effort. ERNEST H. LINDLEY. Am. Jour. Psy., VII., 4., July, 1896.

This is an experimental study of those peculiar automatic movements which one is apt to execute more or less unconsciously when one's attention is concentrated; as, for example, in reading, writing, conversation, study, 'trying to remember,' etc. The material was obtained partly from responses to President Hall's syllabus on 'Some Common Automatisms,' and partly from observations made in the kindergarten and primary grades of the Boston Normal Training School. Something over 600 cases were observed, and the results are tabulated so far as may be. The first table classifies automatisms according to the part of the body involved, and compares children with adolescents, not only as regards proneness to automatisms in general, but also with respect to the relative frequency with which the different parts of the body are employed by each. In both children and adolescents the fingers come first in the order of frequency, with the feet second. Children not only manifest more automatisms than adolescents on the whole, but are surprisingly more prodigal in the use of certain parts. For example, children are ten times as prone to head-automatisms as adolescents. The latter, on the other hand, are more given to automatisms of the eyes, the jaw and the forehead. The second table classifies automatisms according to the activities which they accompany. In writing, automatisms of the lips and tongue; in reading, those of the body, head and hands; while in difficult recollection, those of the eyes, hands and lips, were most frequent.

The number of these movements was found to increase with the age of the child in the kindergarten, to decrease greatly in the primary grades, and to be more marked in the execution of the smaller movements. The large number of these movements among young children is due to their great activity, their defective inhibition and their proneness to imitation. Many automatisms are 'sympathetic,' *i. e.*, they belong to muscles whose center lies near to that of the muscle in use at the time. Those automatisms which persist among trained thinkers (*e. g.*, twirling a watch chain while speaking), seem to be accessory to the concentration of attention or contributory to the stimulation of the brain-cells. Others again seem to be due to excitations which have been prevented by close attention from entering the higher centers, and must find an outlet by lower channels. Finally, many automatisms of posture, especially in children (*e. g.*, bending the body forward, with the head much too low and on one side in writing, and the feet turned in and resting on their sides, or the soles of one foot pressed against the other leg), suggest a return to the fœtal posture, or even to that of 'man's more remote ancestors.'

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Das Gefühl und der Alter. S. Ottolenghi. Zeitsch. f. Psychol. IX., 321. 1895.

In this paper the writer gives the results of electrical tests of sensibility made on 321 male observers of different classes, and from 9 to 75 years of age. The tests were made with the faradic current, but of the other conditions of the test we are left in ignorance. The tests of what the writer calls general sensibility were, we infer, determinations of the threshold of electrical sensations, the stimulus being measured in volts. The most sensitive on the average were students and university graduates of 19 to 40 years; the least sensitive were the oldest group tested, men of 65 to 75. There seemed to be a decrease of sensibility with increasing age after middle life.

Similar results were found for pain sensibility, the percentage of subjects having the highest of 4 grades of sensibility (90 + volts), being as follows: School children (9 to 14), 6 per cent.; older school children (14 to 18), 31 per cent.; students (19 to 24), 17 per cent.; graduates (24 to 40), 7 per cent.; workingmen (20 to 40), 5 per cent.; older workingmen (40 to 65), 65 per cent.; very old workingmen (65 to 75), 45 per cent.

Ottolenghi concludes that the sensibility increases from childhood to manhood, and then decreases. His observatious were not sufficient to justify such an induction. He tested but 18 school children of 9 to 14, and but 16 of 14 to 18. Then the men of 18 to 40 were of a different class from his other adult observers. The only conclusion that the figures warrant, that sensibility decreases in old men, is therefore not entirely acceptable. The writer fails to state the results for very young children, since, as he says, they objected to the completion of the test. It is not evident why the stimulus at which the children objected to its continuation cannot be taken as a pain threshold.

HAROLD GRIFFING.

Beiträge zur Psychologie des Zeitbewusstseins. ERNST MEUMANN. Philosophische Studien, Bd. XII., Heft 2, 1896.

The author interrupts the systematic course of his announced investigations in the psychology of time-consciousness, to publish a cyclus of experiments concerning the illusions of the same. Two cases of time-estimation are distinguished: first, where the interval of time is simply limited by comparatively sudden sensations (judgment of the rapidity of succession of the limiting sensations); second, where the problem is the comparative lengths of continuous stimuli. The apparatus used is the well known 'time-sense apparatus' of the Leipzig Institute. A few details are added by the author to the elaborate description of the apparatus in an earlier article.[1] The method is esentially the same as in the earlier article on the Influence of the Intensity of Stimuli on the Estimation of Small Time-intervals; the inter-

[1] *Philos. Studien.* Bd. IX., p. 270 ff.

vals, whether 'filled' or 'empty,' are produced by the electric current, the subject being in a dark room separated from the apparatus, and the length of the comparison-interval being gradually varied from shorter to equal and longer than the constant or normal interval, and reversed. The subject adds, as he did not in the author's earlier experiments, the degree of sureness of the judgment, as, *e. g.*, clearly, very clearly, doubtful, or very doubtful. In the comparison of 'filled' with 'empty' intervals it proved advantageous to let the latter precede the former and to retain the 'filled' interval constant or normal. The point is made clear that the 'empty' interval is, however, not empty, but filled with such sensations as the pressure of the clothing, of the chair on which the subject sits, the rising and fall of the breast from breathing, etc. Both the 'filled' and the 'empty' intervals are produced by sensations of sight, hearing and touch. The number of sensations entering the filled interval is also varied. Further, the author gives a number of experiments to show the effect of artificial 'aids' in estimating intervals, *e. g.*, tapping the finger, breathing, nodding the head, etc.; and another series in which the one interval is 'filled' with mental work, such as reading.

In the first group of experiments the stimuli are sounds; first with the filled interval, and then with the empty interval preceding. The first two tables present experiments where the 'filled' interval includes, beside the limiting sparks, only one sensation. The scheme is
'filled' interval, 1 2 3
'empty' interval, 1—2. In Table I. the filled interval precedes.
The result may be stated as follows: Where both are very short the 'filled' interval is much over-estimated; as the intervals are increased in length the deception disappears in an 'indifference-zone;' if the intervals are still lengthened, the 'empty' one becomes much over-estimated. The experiments show that these transformations of the illusion occur with all subjects used, but that they occur at different lengths of the constant interval with different subjects. "The length of the interval by which indifference enters is by no means constant." In the next following experiments the 'filled' interval follows, instead of preceding, the empty one, and remains constant. The result is the same, excepting that the indifference-zone lies higher, *i. e.*, by a much longer constant interval than in the former series. In the immediately following experiments the number of sensations increases to 5, 6 and 9, the arrangement of the two intervals being varied the same as before. As a result the over-estimate of the 'filled' interval becomes more marked than before, the indifference-

zone being again raised; but the transformation of the deception from over to under-estimate of the 'filled' interval, with the lengthening of the constant interval, remains obvious.

In the second group of experiments the influence of artificial 'aids' in estimating time-intervals is investigated. First, the beginning and ending sensations of the filled time are more strongly marked than the intervening ones. It is comparatively indifferent whether the former are objectively strengthened or merely rhythmically emphasized by the subject; in either case, the deception, although still manifest, is very much reduced. The indifference-zone appears by a much shorter interval than before. In the next experiments the subject was practiced before each hour in accompanying the six impressions of the 'filled' interval with six tappings of his finger, the tapping being continued to the close of the empty interval; the latter is, in this case, compared with the former by means of the number of taps. As a result the deception became greater than in normal experiments, *i. e.*, without the tapping. It was sought to investigate the effect of periodic breathing; but only a disturbing influence appeared, in consequence of which the difference-threshold (U E) was very much raised. Finally, the 'filled' time was made to follow the 'empty' one, the motor 'aid' continuing through both. The deception of the normal arrangement continued unreduced. Experiments in the rhythmical execution of 'filled' intervals were conducted as follows: The subject made in one case, two, and in another, three hammer-strokes within a given interval; the rhythmical execution adopted in the former case is 1́ 2 and in the latter 1́ 2 3. The strokes are registered on a kymographian cylinder. The two subjects execute the middle stroke of the triple interval somewhat quicker than either of the other two, indicating, among other things, that the triple interval is shortened to make its length *seem* (in compliance with the deception of filled intervals) to the executor the same as that of the double one.

In the third group of experiments the illusions of filled intervals in the different senses are compared, viz., sight, hearing and touch. The experiments already conducted in the domain of hearing are here repeated in sight and touch, with the same general results as before.

The fourth group deals with the illusion resulting from filling the one interval with a continuous sound, the instruments being the Wagnerian hammer and the tuning-fork. The sound produced by the former, after being telephoned to the subject in the dark room, is a peculiar whirring noise. Where the 'filled' time follows the 'empty' one, the result is in general the same as before; but in this arrangement the difficulty involved in letting the filled interval be varied and placed before the 'empty' one is not present. Where the sound is discontinuous, and the interval inconstant, there arises a momentary uncertainty as to the last hammer-stroke or other stimulus, which disturbs the judgment. The result of varying an interval filled with continuous sounds, while the 'empty' one is constant or normal, is, in general, the same as before; but the quantity of the illusion is much less than before, showing that the two cases are in fact very different.

In the fifth group the effect of filling the same interval differently is investigated. The stimuli are the already mentioned varieties of light and sound sensations. The first interval chosen is short, viz. 0, 4 s, and the result is in every case an over-estimation of the filled interval. When the stimulus of the 'filled' interval is continuous, the over-estimate is less; the application of the tuning-fork showing the least illusion. The second interval chosen is of medium length, viz. 1, 0 s. Here the over-estimation of the 'filled' interval is confined to the cases of discontinuous stimuli, the continuous stimuli producing here an over-estimation of the 'empty' interval. When the stimulus of the 'filled' interval is rhythmical, the deception is reduced but not eliminated. The third interval chosen is comparatively long, viz. 8, 0 s. Here the empty interval is clearly over-estimated.

In the sixth and last group the one interval is filled with mental work, such as reading a series of letters on the revolving cylinder of the kymograph and combining the same into a word, the apparatus being so arranged that only one letter at a time was visible; and again, the counting of a number of lines which appear in successive groups simultaneously on the cylinder. In this case the 'filled' interval is more or less under-estimated and the 'empty' one over-estimated.

Merely the general tendency of the author's explanation of the different illusions of time-judgment can be mentioned here, viz., the direction of the attention either to the time-relations themselves, or to the content of the intervals. In the last group of experiments, *e. g.*, in reading letters and combining them into a word, the attention is at first absorbed with the letters themselves (*i. e.*, with the content of the interval) and the interval is estimated too short. As the letters become better known, the attention is directed more to the time relations of the two intervals which are as a result more correctly estimated; finally, the letters become familiar, the 'work' is pleasant, and the 'filled' interval seems shorter, owing to the feeling of pleasure which accompanies it. At the close of the article the author gives about 15 or 20 short statements of results of the experiments which cannot be reproduced here. The article is rich in detail which we have not touched upon and which the student will do well to read in the original.

GUY TAWNEY.

NEW BOOKS.

Grundriss der Geschichte der Philosophie zum Selbststudium und für Vorlesungen. DR. JOHANNES REHMKE. Berlin, Carl Duncker. 1896. Pp. 308. $1.35.

Yoga Philosophy. SWÂMI VIVE-KÂNANDA. London, New York and Bombay. 1896. Pp. xi+224.

Infallible Logic. A Visible and Automatic System of Reasoning. THOMAS D. HAWLEY. Lansing Smith Printing Co., Lansing, Mich. 1896. Pp. xxviii+659.

Sense of Beauty, being the Outlines of Æsthetic Theory. GEORGE SANTAYANA. New York, Charles Scribner's Sons. Pp. ix+275. $1.50.

Leibnitz's New Essays concerning Human Understanding. Translated, with notes, by A. G. LANGLEY. New York and London, The Macmillan Co. 1896. Pp. xix+861. $2.25.

Education of the Central Nervous System. REUBEN POST HALLECK. New York, The Macmillan Co. 1896. Pp. xii+258. $1.00.

The Power of Thought. JOHN DOUGLAS STERRETT. With an introduction by J. MARK BALDWIN. New York, Charles Scribner's Sons. 1896. Pp. xiv+320.

Elements of Psychology. GEORGE CROOM ROBERTSON. Edited from notes of lectures by C. A. FOLEY RHYS DAVIDS. New York, Charles Scribner's Sons. 1896. Pp. xiii+268.

The Life of James McCosh. Ed. by W. M. SLOANE. New York, Charles Scribner's Sons. 1896. Pp. vi+287. $2.50.

Grundriss der Psychiatrie. C. WERNICKE. Th. II. *Die paranoischen Zustände.* Leipzig, Thieme. 1896. Pp. 178. M. 1.30.

Gustav Theodor Fechner. K. LASSWITZ. Edited by R. FALCKENBERG. Frommann's Klassiker der Philosophie, I. Stuttgart, Frommann's Verlag. 1896. Pp. viii+204. M. 1.75.

Hobbes' Leben und Lehre. F. TÖNNIES. Frommann's Klassiker, II. Stuttgart, Frommann's Verlag. 1896. Pp. xiii+232. M. 2.

S. Kierkegaard. H. HÖFFDING. Frommann's Klassiker, III. Stuttgart, Frommann's Verlag. 1896. Pp. x+170. M. 1.50.

Geschichte des Unendlichkeitsproblem. J. Cohn. Leipzig, Engelmann. 1896. Pp. vii+261. M. 5.

NOTES.

The American Psychological Association will meet at Boston on December 29, 30 and 31, which are also the place and time of the meeting of the Society of American Naturalists and of the affiliated Societies. It is proposed to hold a discussion on the morning of December 30. In the afternoon President Fullerton will deliver his address, and the business of the Association (including the reports of committees) will be transacted. It is proposed to group the papers of an experimental and physiological character on December 29, and those of a philosophical character on December 31, so that members wishing to attend two days only can do so. An effort will be made to keep the sessions short and to allow ample time and opportunity for social intercourse. The program promises to be of special importance, and part of the proceedings of the other Societies are such as to be of interest to psychologists.

A prize of £50, to be called the 'Welby Prize,' is offered for the best treatise upon the following subject: *The causes of the present obscurity and confusion in psychological and philosophical terminology, and the directions in which we may hope for efficient practical remedy.* Competition is open to those who, previously to October 1, 1896, have passed the examinations qualifying for a degree at some European or American University. The donor of the prize desires that general regard be had to the classification of the various modes in which a word or other sign may be said to possess 'meaning,' and to corresponding differences in the conveyance or interpretation of 'meaning.' The committee of award will consider the practical utility of the work submitted to them as of primary importance. The essays, which may be written in English, French or German, must be type written and extend at least to 25,000 words. Each should be headed by a motto, and accompanied by a sealed envelope containing the name of the writer. Manuscript from America should be sent to Professor E. B. Titchener, Cornell University, Ithaca, N. Y., and must reach its address not later than October 1, 1897. Other members of the committee are Prof. James Sully, London; Mr. G. F. Stout, Aberdeen; and Prof. O. Külpe, Würzburg. A French member will be added.

A COMPLETE edition of the works of Descartes, in honor of the third centenary of his birth, will be published under the auspices of the French Ministry of Public Instruction. It will contain not only his philosophical and scientific publications, but also five volumes of correspondence. The scientific works will be edited by Prof. Ch. Adams, of Dijon, and the scientific works by M. P. Tannery, of the Collège de France. The edition has been planned by the editors of the Revue de Metaphysique et de Morale, 5 Rue de Méziêrs, Paris, and subscriptions sent in their care will be filled at a large reduction in price.

THE Paris Academy of Moral and Political Sciences has awarded the Bordin prize of 2,000 fr., the subject for which was this year Kant's Ethics, to M. Cresson, professor at Besançon.

A NEW life of Kant by Dr. M. Kronenberg is about to be published by Beck, of Munich, and Prof. Fr. Paulsen has also in preparation a volume on Kant for *Frommann's Klassiker der Philosophie.* Volumes in this series on Fechner by Prof. K. Lasswitz, on Hobbes by Prof. F. Tönnies, and on Kierkegaard by Prof. H. Höffding, have already been published.

DR. H. T. LUKENS, of Clark University, has been appointed professor of education at Bryn Mawr College, and Dr. Colin A. Scott to the chair of experimental psychology and child study at the Chicago Normal School. Mr. J. H. MacCracken has been made instructor in philosophy in New York University. Prof. W. M. Warren has been promoted to a full professorship of philosophy in Boston University. Dr. Guy Tawney (Leipsig) has been appointed demonstrator of experimental psychology in Princeton University.

WE record with regret the death of Dr. M. W. Drobisch, professor of philosophy in the University of Leipzig, who died on September 30, at the advanced age of 94 years.

ALL communications for the editors of THE PSYCHOLOGICAL REVIEW, together with books, reprints, etc., intended for review, should be sent during the year beginning November 1, 1896, to Professor J. Mark Baldwin, Princeton, New Jersey.

INDEX OF NAMES.

The page numbers are italicised in the case of contributors; they are in heavy Roman type in the case of authors reviewed; they are in thin Roman type in the case of mention in the notes.

INDEX OF SUBJECTS.