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Psychological Review

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# THE PSYCHOLOGICAL REVIEW.

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## SOME CURRENTS AND UNDERCURRENTS IN PSYCHOLOGY.<sup>1</sup>

BY PROFESSOR JOSEPH JASTROW.

*University of Wisconsin.*

In surveying the addresses of my several predecessors in this office, I am pleased to find that the precedents which they have set for the tenor and purpose of the presidential address to this Association are sufficiently diverse to enable me with propriety to follow my personal inclinations on the present occasion. Though various in trend and content, the several addresses embody two tendencies: the one accepts the obligation of presenting, in some comprehensive outlook, the status of a general problem, or of a sphere of psychological influence, or a portrayal of the bearings and relations that may most wisely pertain between different divisions or aspects of psychology, or between it and other departments of learning with which it has natural affiliations; the other utilizes this occasion for the presentation of a more specific theme representing the outcome of the reflection and investigation of the specialist. My topic introduces both types of considerations. I fear, however, that my representation will not be presidential in character, but rather that of a secretary who reports, with what skill he can command, the contributions of others, and describes, albeit with personal comment and as he sees it, the status of affairs within his chosen province.

That which may be spoken of as the most cherished interest

<sup>1</sup> President's Address, American Psychological Association, Baltimore, December, 1900.

of the American Psychological Association—the progress of psychology in this country—has unquestionably fulfilled the anticipations alike of the cautiously critical and of the buoyantly sanguine. There have been difficulties and obstructions; there have been misconceptions of our purposes and antagonism to what many of us consider the soundest direction of advance; there have been unwise and profitless discussions; there has been a loss, in some quarters, of the significant perspective of things born possibly of a youthful enthusiasm unripened by the experience of scientific intercourse and not yet mellowed by the composure of intellectual responsibility. But there has been pleasingly little of personal controversy, of pedantic insistence of individual points of view, of distortion of motive or purpose. All this we may look upon with complacency—nor is it necessary to intensify this pleasing state of consciousness by introducing the odium of comparison with conditions elsewhere. If we look upon our prospects from a material point of view—a point of view which I desire neither to emphasize nor to overlook—we may contemplate our achievement with a modest but proper satisfaction. So far as I am aware, those “large three-storied palaces erected for the study of psychology, with laboratories equipped with the utmost lavishness, in which companies of workers by the dozens take up the problems in hand”—with all of which our esteemed transatlantic colleague Kraepelin credited us some years ago—still exist only upon his generous pages. But the equipment and spirit of investigation for experimental work are, in the main, keeping pace with our needs and our ability to direct them to useful ends. It is particularly satisfactory to observe that the unfortunate antagonism formerly emphasized as existing between what was termed experimental and introspective, or physiological and analytical psychology, or was characterized by less pertinent designations, has to a large extent given place to a generous and rational comprehension of the inherent propriety of both aspects, indeed of the inherent necessity for both forms of study in all of the essential problems of psychology. We have little time to spare for the discussion of whether the shield is gold or silver; we have no excuse for one who travels so narrow a path that he cannot ob-

serve the several aspects of many-sided phenomena. One must not overlook the real distinctions that exist in the interests and the methods and the modes of approach and the purposes of different psychologists, as also of the various departments of our common pursuit. Indeed I am about consciously to betray my interest in certain of these aspects, and to pursue my way along my favorite highways and at my own pleasure. Such specialization and such consistency of purpose are necessary to a logical advance.

Having referred to aspects of the status of psychology about which approval cannot justly be withheld, and having for the present omitted reference to many other phases—such as the position of psychology in the educational curriculum, its growing prestige among the sciences, the recognition of its practical functions in education, in medicine, and its general contributions to the influences that make for civilization—in regard to which commendation would have been pertinent, I may all the more readily give expression to a feeling of disappointment that, in regard to many significant and sufficiently definite problems, the total result of recent research has not been larger. I recognize that this state of affairs could have been foreseen by a far-seeing prophet; and perhaps I am in the comprehensible state of mind of the farmer who, when questioned as to the condition of the crops, replied, “Well, they’re purty good. But they ain’t as good as I ’xpected,—and I didn’t ’xpect they’d be either.” Still, I am ready to admit that on many problems upon which much time and ability have been expended the psychological stock-taker would not have very impressive, positively available, assets to enter. As I have hinted, there are sound reasons for this state of affairs—reasons which I shall not further discuss; but we must remember that we are no longer an infant industry that may be expected to thrive only by the condescending protection of the powers that be. We have passed the stage at which we can offer immaturity as an excuse that covers all sins; we have entered upon or are certainly in rapid transition to a subsequent period of development, in which we must hold our own with the advancement of the other sciences, and expect to be judged, not as a favored



youngest member of the family, but by the same standards as the rest. For this purpose we shall have to show, after reasonable periods, material progress which the recorder of the advancement of the sciences may enter in his inventory book to our credit.

We may, at all events, take comfort that psychology is no longer in the plight of the sciences so graphically described and deplored by Bacon, which "have certain general positions which are specious and flattering," but in regard to particulars, "where they should produce fruit and works," give rise to "contentions and barking disputations," and "stand almost at a stay, without receiving any augmentations worthy of the human race." It still remains in part inevitable that, as Bacon further deplores, "what was a question once is a question still, and instead of being resolved by discussion is only fixed and fed"; but we have certainly become alike more skilful and more rational in our questioning, and fix and feed our opinions by more logical discussion, calling to our aid, not precedent and authority, but ingenious experiment, pertinent demonstration and living illustrations. So far has the condition of things altered that the accumulation of works, if not of fruits, in regard to particulars—the absence of which Bacon laments—seems to one of our own generation positively alarming. We recognize the geniality of James in the description of the modern method of "patience, starving out, and harassing to death," a method "which could only have arisen in a country whose natives could not be *bored*," a method wielded by "these new prism, pendulum and chronograph-philosophers," who in a material age stand for business, not for chivalry. None the less this method is credited with containing the promise of accomplishing even if "by spying and scraping," by "deadly tenacity and almost diabolical cunning" what generous divination failed to secure. But for successful triumph over the problems of psychology we desire no more brilliant example than that which James embodies, using the generous divination upon a material woven through and through with the outcome of experimental and experimentally incited data. There is no danger that divination and insight born of rare gifts will ever find their occupation



gone. We recognize their worth and their place; yet even they cannot build bricks without straw. And so we continue to cherish, with Bacon, that characteristic of a living science that finds its daily food in accumulations albeit small but worthy, and in the end produces fruits and works. If the harvest be not so large nor so rich as our hopes or our wishes led us to anticipate, let us learn how to fertilize the soil, or to improve our methods of cultivation. That as a result of such improvement a future president of this Association will be able to report in regard to many lines of research, such definite and demonstrable advance as I have indicated as a desideratum, is equally my belief and my hope.

The title which has been prefixed to my remarks is intended to be suggestive not of a systematic disquisition upon the nature of currents in general and of currents psychological in particular, but is designed to create a mood for a stroll along the shores of psychological waters, with the stroller's privilege of lingering to note what catches his eye and his interest, to watch the procession of the waves and the deposits which they roll up at one's feet, to follow the retreating rush of the waters, to note the action of the tide and the shifting of the sands, and with it all the building of permanent deposits and the shaping of continents. Or if we prefer to cruise in the waters themselves, we may sail with interest as our compass, and a course not too rigidly set. Yet he who cruises to advantage must neither drift nor go as the wind listeth, but follow the invitations of shore and bay, keep in mind, yet not too consciously, the headlands and reefs, touch at harbor and port, and be not unmindful of the attractions of home and the fireside. In some such way I shall attempt to cruise in and out among the currents and the undercurrents of contemporary psychology.

The conception that will serve as the background for my survey will be that of psychology as the science of mental functions. The attitude towards mental phenomena which this conception emphasizes came into prominence in the wake of the evolutionary wave that so wonderfully and so permanently altered the face of the waters. The conception of mental endowment as a growth-process and as a response to complex en-

vironmental conditions at once cast a blinding light upon an area of dimly-lit chaos in which dogmatism and misconception and neglect held a confused occupation. Not alone the illuminating truth that human mentality was intimately and significantly related to the mental endowment of animals was thus realized as a living and momentous reality, but, like the discovery of a new planet in the solar system, there entered our universe a new pivotal force. It brought us to realize that human history from its embodiment in the stone axe or the bone amulet, from the sculptured rocks of ancient America and the picture writings of ancient Egypt, from primitive cult to old-time science and to modern superstition, from savage myth to medieval legend and present-day folk-lore, is of a nature all compact. These and all other spheres of human achievement became suffused with the glow of a new interest, took on a richer coloring and shone with a fuller meaning in the light of this supremely significant conception. Into the dry bones was breathed new life, and the thoughts of men were widened by a keener insight of the increasing purpose that through the ages runs.

The functional aspect of psychology enters into a considerable portion of the contributions to modern psychological literature; it more often serves, however, to give the color-tone and the setting of the painting than it appears as the central subject of the composition. From its accepted recognition as an undercurrent I am desirous of seeing it brought forward as one of the main currents along which the burden of psychological knowledge comes freighted. And particularly should it assume this position as an educational highway. For collegiate instruction and as an introduction to the study of the nature of psychological problems and psychological thought, I place no aspect of psychology higher in appropriateness than the functional aspect; I regard no topics in psychology better suited and more pertinent and profitable for introductory study than those topics and those chapters in psychology in which this aspect naturally comes to the front. For the comprehension of the nature of sensation, of the powers of discrimination, of intelligence in general, of emotional and intellectual expression, of

movement and conduct, a consideration of these processes in reference to their functional phases is fundamental, and is a prerequisite to their profitable consideration in other aspects.

I shall stop to illustrate my point of view more in detail, even at the risk of dwelling upon what is familiar or commonplace—characteristics, let it be noted, of the possible importance, though not necessarily of the interest, of what is illustrated. By the functional aspect of vision, for example, I mean not merely the uses of the several portions of the visual apparatus—physiological functions in some part<sup>1</sup>—but more especially the utility aspect alike of the more generic and of the more specific characteristics of human vision. The existence of vision itself represents a psychological response to a form of energy which comes streaming in upon our planet; the color-sense is a more specialized response to certain properties of that form of energy; both, in common with other senses, have the function of bringing us into relation with our environment, of enabling us to appreciate differences in our environmental conditions, and thus to regulate conduct according to circumstances. The sight of what is comes to be prophetic of what is to be, and forewarned we act forearmed. All this is of personal and immediate advantage to the individual, and in such advantage lies the clue to its constitution and to the understanding thereof. And similarly for detail: the nature of foveal vision is to be presented in regard to the advantages it possesses over a more diffuse non-foveal vision; the marvellous coördinations of binocular vision—representing as they do a most highly evolved

<sup>1</sup> In the discussion of this matter it becomes important to form as definite conceptions as possible of the relations of psychology and physiology. On this point I find myself in entire sympathy with the position taken by Fullerton, that much of what passes for and is incorporated in the text-books of physiology is really psychological in character. The absence of physiological knowledge is supplied by the introduction of relevant psychological material. Even the physicist indulges in the same procedure. There is no harm in this, if the psychological character of what is thus introduced is recognized, and it is not posed as physiological. As Münsterberg, endorsing the same position, says, "brain-physiology without psychology would have been perfectly blind"; psychology is perfectly willing to serve as eyes to those whose retinae are adapted to other fields of activity, but it is proper to credit the results of this operation to the leader and not to the led.



product of development, and one reaching its perfection only in man—must again find an explanation from a consideration of their utility, the increase in the scope of psychological power which such an arrangement makes possible. Serving as a background to such considerations should be an appreciation of the primitive conditions under which the existing state of our mental endowment was moulded in the rough. The functional rationale of human vision as it is now constituted, is to be found in advantages that were real and significant in the life of cave-dwelling, forest-roaming, food-hunting man and of his more remote ancestors. Vastly as our sensory endowment has been modified, in structure and more certainly in the modes of its employment, by the increasing complexities of civilization, and especially of modern civilization, such variations are but refinements of finish that may obscure but do not obliterate the rough-hewn contours to which they have been added.

Another example pertinent to my present purpose, and an equally obvious one, may be found in a fundamental motor activity, speech; this, following the normal relations of things, is so intimately connected with and guided by a sensory process as to be more properly spoken of as a sensori-motor process. In this case the functional avenue of approach answers a question which otherwise might remain unasked: namely, as to the why and wherefore of speech as a vocal auditory process. Why did the ear and voice assume this all-important function, and not, for example, the eye and hand? In reply we must consider homely advantages of a primitively useful type; that in our ability to make sounds we have a mechanism which, unlike signs that appeal to the eye, we have always at our service; that in primitive times the darkness of night that would cut off the direct address to the eye in no wise detracted from the serviceability of auditory impressions; that the thicket and the jungle placed men out of sight but not out of hearing, and so on. Even the exception to the first of these statements proves the rule; for the visual sign-making apparatus which we have constantly at command—the making of gestures—was used as a most primitive mode of communication, antedating in some part even articulatory utterance. It is by considerations of this type

that the functional advantages of spoken speech, and the appreciation of the nature of its development and origin become possible. Other instances of this general type could be added, important and trivial, obvious and obscure, general and specific, from human endowment and from the animal world. They would serve to illustrate the variety and the scope of the functional aspect of mental phenomena, and by their aggregate extent and explanatory power would reveal the importance of this psychological current—a current increasing in its sphere of influence and destined to shape in considerable measure the contour lines of future psychological maps.

Still holding in mind the functional aspects of psychological processes, I shall next call attention to the threefold manner of approach to many of the significant problems of psychology—a contribution in my judgment that represents one of the most interesting and valuable deposits which the waves of recent investigation have washed upon our shores. This trinity may be spoken of as the genetic or comparative, the normal, and the decadent or abnormal phases of mental phenomena. I shall select for illustration of these phases the study of intelligence. Useful and legitimate criteria of intelligence are derivable from each of these phases, and each completes and corroborates the others. A conception of intelligence, to be adequate, must be founded upon the recognition of the comparative status of the reactions, simple and complex, which in their composite extent are properly brought within the connotation of this term. The exact fixation of the boundaries of intelligence, both above and below, need not detain us; the middle ground is well enough indicated by the ordinary psychological usage of the term, while the method of more precisely determining the scope and criteria of intelligence may in some measure find illustration as we proceed. Intelligence must first be realized as an advantage-gaining factor in the evolutionary struggle; that struggle is not merely, and indeed in all the stages that here come into consideration, not mainly a conflict of tooth and nail, a contest of strength of claw and fleetness of foot, but a war of wits, an encounter of skill and cunning, a measure of strategy and foresight. The reaction of bodily structure and psychological utilization of struc-

ture forms one of the most interesting phases of the study of intelligence. In this field lies the problem of instinct versus intelligence, with all its many ramifications; it is this that gives zest to the study of animal life, from the painstaking account of the courtship of a spider or the experiments upon the visual powers and the memory of an ant, to the dramatic and idealized narratives of Kipling's jungle, and Seton-Thomson's forest and prairie. In brief, an adequate conception of intelligence is not derivable from the contemplation of human activities; its validity must be further justified by its consistent applicability to the phenomena of animal intelligence. Nor is this the only application of the genetic or comparative point of view; two other developments are of equal theoretic importance, though each of the three differs widely from the others in the actual conditions of its applicability. The genetic growth of the individual in the unfoldment of infancy and childhood and youth and maturity offers another and an indispensable aspect of the study of mental endowment. The same is true of the growth of the race from savagery to civilization. Whatever criteria a scientific investigation shall decide as the proper criteria of intelligence must with due modifications and allowances find corroboration when applied to these three genetic series. It is the growing recognition of this requisite that I look upon as a significant current in modern psychology. Let me point out, also, that not only does this comparative aspect furnish a widening outlook over the range of mental processes, but that a truer insight into the nature of human activities is as likely to be the outcome of investigation in this field as of the direct study of the data of human psychology. This mutual illumination and suggestiveness of animal and human psychology contributes at once to the charm and the profit of this mode of approach. As a concrete instance I may cite Lloyd Morgan's exposition of the focal and marginal elements in perception. That lucid and acute analysis was the result of a study in regard to the differentiation of human and animal mentality; it leads Professor Morgan to regard the perception of relations (thus necessitating the serial focal apprehension of marginal details) as the essential criterion of and prerequisite to rationality. It may indeed be said that the



valuable contributions which Lloyd Morgan has made to our conception of psychological functions, and also to their analysis and their internal economy, are the result of his exceptionally able utilization of the comparative point of view, in the manner which is here emphasized.

The study of intelligence with reference to its status and the method of testing it in the normal individual is, to my thinking, one of the lines of investigation most deserving of increased attention, and one which I had in mind when deploring that in some directions progress had not been so definite nor so satisfactory as was to have been expected and as remains desirable. The functional endowment of the average man is a worthy object of the psychologist's devoted attention. It may contribute little or nothing to an analysis of mental processes, but it stimulates that branch of psychological investigation because its more detailed progress depends upon such analysis. We cannot test a process properly and discriminately until we have analyzed and discovered its relation to other processes, with some of which it usually combines to make a functional unit. One of the chief difficulties in the designing and execution of such tests is the ambiguity attaching to them by reason of an insufficient analysis of the mental attitude and of the processes contributing to the result. If consequences were always preceded by the same antecedents, if recognizable objective results were the infallible indices of determinate subjective processes, the path of the psychological investigator would be, not indeed strewn with roses, but certainly less beset by thorns. In the study of mental functions, important and trivial, nothing should be kept more constantly in mind than the truly complex and intricate logical relation that pertains between process and result. I am pleased to have occasion to cite once more Professor Morgan's researches for their excellent illustration of the essential connection and organic unity of human and animal psychology. Lloyd Morgan emphasizes the dangers of disregarding processes and accepting results—ends accomplished—as criteria of the status of mental actions. The example of Tony the dog and his learning to carry a stick in his mouth in passing through an open place in the rail fence

has come to be quite familiar. The passer-by, who happens to see the dog take the stick up properly by one end and draw it through after him, remarks: "Clever dog that, sir; he knows where the hitch do lie." But the experimenter, who has recorded the stages of trial and error, of haphazard holding and tugging at the stick in all possible directions, has a different and a more accurate conception of the dog's mental processes, which are seemingly indicated by the result accomplished. *Ab uno disce omnes*. Let no one look upon the study of mental functions and the determination of mental standards as a matter of technique and a facility in the use of apparatus, or as a process unrelated to a painstaking, analytical investigation.

An adequate set of tests of normal functional efficiency, that shall receive a considerable authoritative sanction, is a great desideratum for present-day needs, and an end by no means beyond the goal of properly directed endeavor. Its starting point is a correct analysis of the most distinctive modes of exercise of the several elementary components of our mental functions; the next step is the devising of tests that shall most simply, naturally and definitely measure the functional efficiency of a selected factor or process; this accomplished, the way is prepared for the extensive utilization of such standards or norms of efficiency, by (a) their correlation with one another, (b) by a comparison with similar results obtained upon children at different stages of their development, thereby gaining an insight into the order and nature of genetic unfoldment, (c) by a comparison with irregular, undeveloped, defective and decadent forms of such processes, as they occur in connection with individual variations, with the consequences of mental stimulation, or in disease. This programme, which could readily be expanded, is even in outline a most extensive one—rich in detail, fertile in mutual suggestiveness of its parts, possibly momentous in its practical consequences. The conclusion is obvious that for a host of comparative purposes the determination of norms or standards of functional mental efficiency is indispensable. That such determination involves conventions and artificialities is true and proper and inevitable. But neither is a foot, nor a meter, nor a candle-power, nor a horse-power, nor a volt, nor an



ohm a natural and predestined *ding-an-sich*. Yet the arbitrary and conventional character of these units does not interfere with their utility. I am not advocating a ready-made mental yardstick which shall show in what measure all men are not equal, and how each may discover the thumb-marks of his individual success or failure. All this has been attempted before, and with necessarily futile results. The problem is recognized to be one of a general statistical nature, freighted doubtless with practical consequences, but the application of which must always be uncertain and dependent for its success upon judgment and insight. There is no serious danger of being misunderstood in this respect. Some other forms of objection, however, to what I have advanced I must not seem to disregard. Having these in mind, I can only say that I cannot bring myself into an attitude of sympathy with those who, on grounds possibly legitimate in themselves, but not pertinent to the functional aspect of mental processes, question the possibility or the utility of mental measurements. A searching critique of the fundamenta of such measurements, of the assumptions which they involve, is helpful; it clears the atmosphere and brings out the perspective of things with sharpness and precision; it distinguishes between the true goal of such investigation and unattainable ideals; it points out sources of error and prevents misconception. Such criticism is wholesome and pertinent; but a transcendental skepticism in regard to the possibility of such measurements itself transcends its proper sphere when it impugns the value of the scientific study of mental functions by quantitative methods. I have equally little sympathy with those who admit the possibility and even the utility of such investigation, but question its psychological character. They speak of it, and somewhat disparagingly, as mental anthropometry, and thus in a measure restrict psychology to that which yields definite and, if possible, original analytical results. In part the difference between this attitude and the one I am advocating is one of terminology, and thus unimportant; in another aspect it is a difference in the interpretation of the scope of psychology—a subject certainly capable of supporting differences of view. I prefer to cast my lot with the wider conception of its scope, and mainly for the

reason that the relations between mental problems are so intricate and manifold that one can never anticipate from what sources further illumination may come. Call it mental anthropometry if you will, but do not disregard the valuable contributions to other divisions of psychology and to the general conception of this science, which such investigation has the possibility and, in my opinion, the probability of contributing.

But I find myself drifting along a current tending seaward, and I must turn about and regain my course. The study of the normal efficiency of that composite group of processes which contribute to our common humanity has, I confidently believe, an important and a practical future. Its progress is dependent upon careful analysis, upon systematic investigation, upon the coöperative and the coördinate labors of many, upon interpretative skill and psychological insight. An auspicious start has been made; the day of the production of works and fruit cannot be far off.

I have still to consider the abnormal aspect of typical mental functions. The familiarity of this point of view, the prominence it has assumed through various causes, render unnecessary any extensive consideration of it in this connection. It is almost sufficient to cite the vast increase in definiteness and value of our knowledge of the factors of speech that has come from a study of speech-defects, as in itself a convincing endorsement of the value of this aspect of study and of the significant message which it bears for the psychologist. I wish also to refer to the group of studies that owe their origin to the enthusiastic devotion and ingenuity of Kraepelin as the most systematic and successful expression of the point of view here emphasized. So close is the relation of the abnormal to the normal, and so defective our knowledge of the latter, that, in many instances, for the sake of the determination of what is abnormal the establishment of the normal is first undertaken. Several of the Heidelberg studies are equally studies in normal psychology, and develop methods by which academic laboratories may profit. Thus, in many converging ways do we approach the central problem of the determination of the nature, conditions and fluctuations of psychological functions.

The mention of the abnormal suggests a presence in contemporaneous thought that offers a seductive opportunity for comment; and, as I am privileged to stroll, I shall yield to the temptation. There is a noisy undercurrent in psychological circles which affects us professionally in a pronounced and unfortunate manner. It does so, in the main, indirectly by affecting public opinion in regard to our aims, our purposes, our methods, our needs and our *raison d'être*. For the reputation of psychology in the hands of its professional representatives I have no fear; and I am convinced that this undercurrent can make no headway against the true interests of our science, and certainly not in this country. But the public does and does rightfully take an interest in psychological matters; and we cannot be blind to the fact that under present conditions such interest is likely to take its clue from the unusual and the obscure, that the current conception of psychology is becoming distorted, and the true interests of psychology are jeopardized, that its encouragement and advance are hampered, by the unfortunate confusion of psychology with what is unfortunately termed psychical research. If I may repeat words used elsewhere: "In the face of this situation, the professional psychologist cannot but take heed of the dangers which imperil the true appreciation of his labors and his purpose on the part of the sympathetic layman. It is a matter of serious concern that the methods of genuine psychological study, that the conditions of advance in psychology, that the scope and nature of its problems should be properly understood. It is a matter of importance that the dominant interest in psychology should center about the normal use and development of functions with respect to which psychology bears a significant message for the regulation of life."

I am well aware that it is in the modest retreat of study and laboratory that the real advancement of science goes forward; but science also progresses in the glaring light of publicity. Even newspaper science has a power for evil that the protectors of science cannot afford to let go unrecognized. And the layman, however discerning and sympathetic, cannot be expected always to distinguish between the sterling results of authoritative research and that which bears some of the outward traits of



such genuineness ; and therein lies the danger. Let me repeat : against the professional and technical investigation of the legitimate problems unfortunately grouped together as psychical research (true psychological problems, although not always studied as such) I have no word of complaint. Some of these problems appeal strongly to my interests, and others do not ; and particularly in regard to the latter class do I admire the patience and the devotion and the skill with which research has been carried on. I admire it the more because of my personal incapacity to contribute to it. It is to this aspect of investigation that I would transfer James' remark : that it could only have arisen in a country whose natives could not be bored. True psychical research in the hands of true psychologists may not always be profitable ; but it is in no sense a menace, and may prove, indeed has proved, in several directions to be an aid of great value. But psychical research as it has quite generally been pursued, and the spirit and attitude towards psychology which it has bred, have been productive of harm to our profession and to the reputation that we cherish. By this insidious undertow many have been carried to sea ; their intellectual footing seems to have been lost by reason of a single or a few startling experiences, and their conceptions of our mental nature seems never to have recovered its normal status. Against such disaster and equally against the unfortunate connotation which has been scattered broadcast as to what psychologists are and do, I wish to utter a word of warning and of protest. I shall bear with what grace and resignation I can command the tiresome tales brought to me with an air of satisfying my most intense cravings, of coincidences and hallucinations and haunted houses and thought-transference ; but I cannot permit the injury to the fair name of psychology, which this misrepresentation of its message abuses, to go unprotected.

I cannot formulate to my own satisfaction how this form of interest came about. Was there always a widely disseminated interest in psychological problems which the Societies for Psychical Research simply diverted into their own peculiar channels ? Or is this but the old-time interest in the supernatural assuming a more modern garb ? Does the popular

mind that speaks glibly of the psychological basis of education, that absorbs notions about mental influences in disease or in hypnotism, make any consistent connection between these interests and that in telepathy or the performances of a trance-medium? The parentage of the intellectual craving that psychological research satisfies is indeed obscure. The relation between psychology and psychical research is clearly not that of mother and daughter; and yet I am reminded of one of the several conflicting opinions regarding Astrology to which Kepler gives expression. "Astrology," he apologetically writes, "is indeed a foolish young daughter, but bless me, where would the all-wise mother, Astronomy, be if she had not this daughter! The world is more foolish still, so foolish indeed, that the folly of the daughter must be speciously presented and be cried to the public for the benefit of the old sensible mother." Nor is the critical temper of to-day as lenient as that of Kepler's time; for he could add, "But when guesses are limited to yes and no, one has always about half the chances in one's favor. Right guesses are remembered, failures forgotten, and so the astrologer remains in honor." But the failures of psychology will not be forgotten, nor its indulgence in guesses be condoned; nor will the psychologist remain in honor unless he succeeds in differentiating his goal and purposes and methods from those that strive to connect personally interesting experiences with the movements of the stars and the exercise of transcendent mystic forces. For it still needs to be said, and said emphatically, that the explanation of what may be mysterious is not intrinsically the psychologist's business; nor does it follow that the explanation of what has some resemblance to obscure psychological process will, when found, be of a psychological kind at all. It may be and it may not be psychological; and until we know how mysterious knowledge is gained of other people's private affairs, there is no saying how little or how much or what kind of a psychological material may there be hidden. It is an interesting speculation to imagine what might have happened if the X-rays had accidentally been discovered by some one interested in the display of occult forces, who would have gone about the world describing the contents

of sealed boxes, locating bullets in the bodies of wounded soldiers, seeing through opaque screens as through glass, and posing his success as the result of a telepathic gift, or the intervention of personally interested spirits! With the world at large totally in ignorance of the true *modus operandi* what a career for a modern Cagliostro! How he would have been welcomed and risen superior to so-called expert tests, and what a secondary place all other varieties of 'psychical research' phenomena would have taken! Yet the supposition is not a likely one, because discoveries of this kind demand quite other conditions for their appearance; but it serves to show that one cannot decide whether a process is psychological in character unless one has a fairly clear notion as to what the process is. As we learned before, results and processes are not always mutually inferable. Our X-ray telepathist might have successfully posed as a psychological mystery; but when his methods came to be discovered it would have been obvious that the whole procedure was a physical and not at all a psychological one. *Hæc fabula docet* many things, and most of all that reasoning about results in ignorance of the processes by which those results have been reached, is apt to lead to curious reasonings, and, misery like, to make strange bed-fellows.

But I am reminded once more that the shore is receding from my view and that I must regain my course. A current in the affairs of modern psychology, or rather in those of its waters in which I am cruising, which appeals to my interest, is the study of functional complexes. Easily the best illustration of these are the two prominent speech-complexes, reading and writing. Quite a number of studies have appeared in recent years, and apparently largely in independence of one another—thus indicating the common wave of timeliness which brought them forth—all of which have to do with an attempt to obtain a more intimate knowledge of the processes by which so large a part of our intellectual pabulum is assimilated, and our intellectuality is expressed. Here again the problem of processes versus results meets us. We all read and write; but the reading is not psychologically quite the same thing for different persons. The analysis of these individual differences, their reduction



to their lowest terms, their determination by accurate methods constitute one of the essential phases of the psychology of reading. What are the sensory processes contributory to reading? To what extent are they auditory, to what extent visual, how far are they in terms of sensations accompanying articulation? What are the motor innervations contributing to the result? Do the lips move in reading, is there embryonic articulation? What makes a rapid reader? How are slow readers differentiated from rapid ones with respect to the above characteristics? What are the distinctive reading methods? What are the mechanical and what the apperceptive elements of the process? What are the movements of the eyes in traversing the line and page? What are the time-relations of these various processes? What is the unit of apperception? What is the relation of quickness of comprehension to the capacity to retain and reproduce? What are the differences of method and process that distinguish the novice from the expert reader? These and a host of other problems, all of which enter to constitute the psychological study of reading, have been in part investigated and with very satisfactory though as yet incomplete results. It will soon be possible for some properly equipped investigator to unify and coördinate this scattered series of researches to fill out the gaps in the exposition—not forgetting the abnormal and the genetic aspects of the problem—and thus to present a most useful compilation of the psychological status of one of the fundamental functional complexes. Work of this kind I regard as eminently desirable and as leading to such definite and tangible results as I have indicated as a desideratum. The share of American psychologists in what has thus far been done has been most creditable;<sup>1</sup> and for several branches of the study the opening wedge was driven by one of our own number, Cat-

<sup>1</sup> Among special contributions to the psychology of reading may be mentioned the investigations of Goldscheider, Erdmann, Dodge, Pillsbury, Quantz, Hucy, Bagley and others; while the list would be considerably extended if it were made to include the researches contributing to the subject in the line of studies of some of the processes which enter into the general complex. The relations of reading to thought-processes is a topic of special importance to be pursued in part by other methods. Experimental studies of the genetic and the diseased aspects of reading are destined to yield illuminating contributions to the general topic.

tell, in his investigation of the apperception-time of words, letters and kindred factors of the reading process.

In regard to writing, an equally detailed study is possible. I may, for the sake of brevity, refer simply to the investigation published last year by Diehl under the inspiration of Kraepelin, as a type of this activity. He measured, by specially devised apparatus, the aggregate extent of the writing, its rapidity, the pressure exerted in writing, the pauses in the consecutive process, the effect of practice, individual differences in these respects, the influence of familiarity and the difficulty of the subject matter, and from a correlation and comparison of these factors drew suggestive conclusions valuable in themselves and stimulating to further inquiry. The study of individual handwriting as indicative of the complex psychological traits commonly summed up as character has always been a tempting problem, particularly upon its practical side, and has in this aspect given rise to endless systems of graphology. The form of research just commended will clearly demonstrate the unpsychological nature of such individual application of vaguely derived generalizations. The work of Preyer, one of the few treatises bearing the title, 'Psychology of Writing,' though it incorporates some of the more precise and measurable characteristics of handwriting, is practically devoted to an unprejudiced but equally unsatisfactory examination of characteristics far too complex and far too much influenced by conventional and other extraneous circumstances to be applied to individual cases. A hundred or even a score of such characteristics, mutually corroborative, might be of some value, but to present one as an index of character is both unwarranted and misleading. Such application of a vague glimpse of types and generic differences is an index of an imperfect conception of the logical relations of theory and practice. This is equally the fallacy of phrenology, of palmistry, of graphology. It is the differences between these and the modern doctrine of the localization of brain functions, the study of comparative anatomy and of the psychology of writing as a functional complex that mark the advance of science in method and purpose.

Other functional complexes offer promising outlooks to the



psychological investigator. Vocal speech and auditory comprehension are more fundamental than reading and writing, but are more difficult of investigation because of the difficulty of their record. Yet recent progress in the study of phonetics—which, judging by the increased number of entries in the psychological literature, is another example of the simultaneous pursuit of the same problem by widely separated and independent workers—promises to supply us with an adequate though not yet a simple method of such record; and the physicist may have a surprise in store that will, at a single stride, set the entire method of research upon a different basis. Such research is to no inconsiderable extent the direct result of an interest in the psychology of the speech process. The list of functional complexes does not end here, though those connected with the expression of thought in conventional symbols will always command a leading place. The psychology of the telegraphic language has been ably presented by Bryan and Harter. The study of locomotion has as yet been mainly of physiological interest, but has points of suggestiveness to the psychologist; tests of skill, of mutual guidance of eye and hand suggest another form of complex; while special mechanisms, such as those concerned in the perception of the third dimension of space, or the spacial correlations of sight and touch, offer a further expansion of distinctive yet allied departments of investigation. All of these have an essentially analytical interest due to the fact that their precise investigation contributes to and in part depends upon an analysis of the several factors constituting the complex, and, again, a practical interest in their utility and application to the affairs of life.

By an easy transition I come to speak next and last of the practical currents in contemporary psychology. It is sufficiently obvious that one and probably the dominant bond of connection between psychology and the practical life lies in the aspect of psychology here emphasized, viz., as a science of mental functions. A great deal has been said pro and con of the practical importance and value of psychology; the discussion has in some measure cleared the atmosphere of misconceptions, has tempered extravagant statements on both sides, but has left a weakened

and confused appreciation of the practical import of the study and the results of psychology. The discussion has produced, to use James' words, an "atmosphere of vague talk about our science, which to a great extent has been more mystifying than enlightening." There is no one who has brought to this question a more searching analysis and a more wholesome influence than he whom, notwithstanding his foreign origin, we are able to claim as an American psychologist—Professor Muensterberg. He has properly centered a prominent part of his discussion about the relations that may be most philosophically, and by the same token most truly and advantageously, maintained between psychology and education. The blind infatuation of a certain type of educational theory and practice for a psychological escort he has effectively disillusioned. The psychological utilization of material which the occupations of the school-room offer has been trenchantly differentiated from the training and the talents which the teacher should covet for success in the art of education. And the suggestion of an intermediary between the educationalist and the psychologist, who shall derive inspiration and method from the one and apply them to the investigation of the problems which the other presents, is both pertinent in itself and contributes to the clearer understanding of the functions of each. With these and other positions taken by Muensterberg in regard to the relations of education and psychology, I cordially concur, although not without certain minor differences of attitude. But these differences of point of view are not so great but that the resulting opinions could be united in a stereoscopic combination with a clear and truthful result. But in regard to the more fundamental aspects of theory and practice as applied to psychology, our ways divide. In some measure we believe the same things but for different reasons, and in some measure we do not believe the same things.

It seems safe to assume that psychology is essentially devoted to an analysis and explanation of the phenomena of the mental life; that for this purpose it posits a logical reconstruction of elements, which does not find its counterpart in the world of observable reality; that the connection of psychological with physiological and physical processes does not in the least im-

pugn the distinctively psychological status of these phenomena, still less does it explain them; that, in brief, psychology as a science has its peculiar status, material and assumptions, and above all its special attitude towards its material—a characteristic no more and no less peculiar to psychology than to the other sciences. All this is true and in certain aspects important; but I cannot admit that the realization of these truths in any way militates against the truly intimate practical significance of psychological principles and generalizations. The world that the physicist constructs and the suppositions which he creates to explain physical phenomena are not found in the world of reality; yet the bond of connection that unites the ideal and the actual world of the physicist is different only in type, not in logical status, from the similar nexus between the artificial but justifiable creations of the analytical psychologist and the realities of psychological functional mechanisms. A two-dimensional flatland is a legitimate and interesting mathematical speculation; and those who can may revel in the intricacies of a universe of four dimensions. The purity of mathematics is not impugned by the actual existence of a three-dimensional world in which endless practical applications are made of homely mathematical principles. If we render unto Cæsar those things which are Cæsar's, we shall run no danger either of disregarding the significance of earthly realities, or, philistine-like, of ignoring the necessity of a deeper logical insight into the rationale of our habits of thought and their philosophical justification, than the needs of the practical life demand. But as Lloyd Morgan reminds us, the legitimate reply to the plain man who regards "the psychologist with some suspicion, as one who is over-subtle in his distinctions, too introspective, and not sufficiently objective in his study of mind," is neither an expression of impatience, nor "the assumption of an air of superior knowledge and wisdom, but a quiet determination to justify his procedure by the results which, through its means, he is enabled to reach."

The path that unites theory and practice is always difficult rightly to trace and to follow. Short circuits are as disastrous as other forms of attempt to take by storm the kingdom of



heaven. Like the two parts of the terminal apparatus in a system of wireless telegraphy, so with theory and practice, each responds to each when the oscillations are sympathetically attuned. The messages that theory sends to practice and practice to theory can be interpreted only by those who have learned by technical skill and progressive experience the signal code of the system of communication. The relation of theory to practice in psychology is not essentially different from that which pertains in other sciences and their derivative arts. It may be that as psychologists we are more tempted to preach without strictly examining the authority of our texts; but there are sound texts from which to preach, and they carry with them weighty practical consequences. Psychology and life are closely related; and we do not fulfill our whole function if we leave uninterpreted for practical and public benefit the mental nature of man. If it be urged that, important as this office may be, it is not inherently a psychological one, I can only say that I find the conception of our science that incorporates these declarations of principles within its constitution as alike more inspiring and more consistent than one which, by ignoring them for philosophical reasons, converts a fictitious into an actual barrier.

For the educationalist and for the teacher, for the biologist and for the physician, for the anthropologist and for the sociologist, for the philosopher and for the moralist, for the student of history and of literature, psychology brings appropriate messages, as it also finds in these activities valuable illustrative material for the building of its own structure. Psychology assumes its proper share in the formation of those interests and attitudes which in part are the outcome of and in part pave the way for the newer discoveries and the more illuminating theories that mark the progressive stages of civilization. The emphasis placed upon this function of psychology and the increased recognition of the practical significance of our science I recognize as a prominent and fortunate characteristic of the intellectual currents in which we move and live. That there are in this region dangerous waters, making necessary all the craft and skill which our helmsman can command, I am well aware; that adventurous barks too lightly laden or ambitiously carrying too

much sail for their beam, or both badly rigged and carelessly piloted, have come to grief, I recognize and deplore. None the less through these waters lies a profitable and a proper course—a goal worthy of our earnest endeavor and a fulfillment of our natural responsibilities.

Here ends my cruise. We were not embarked upon a voyage of discovery, and we certainly had no thought of any psychological garden of the Hesperides to captivate our fancy and possibly distract us to the neglect of our rudder. The uneventful, but I trust not wholly uninteresting, excursion which I have offered in response to your complimentary expression of willingness to entrust yourselves to my temporary pilotage, has, at all events, furnished vistas which your eyes can see and interpret more deeply and more truly than my own, and in so far may not have been unprofitable. The varied scenes of activity upon which we have rested our gaze augur well for the vitality of the interests and the intellectual attitude which we, from various sides individually yet in consonance collectively, represent. It is certainly pardonable to note, as we go, that this activity and this vitality find not their least successful expression in the contributions furnished by American psychologists. Indeed, it may be said that some of the currents to which I have asked your attention flow most characteristically along American shores. The currents of the present suggest those of the future. That twentieth-century psychology will offer problems of difficulty and complexity, it requires no prophet to foresee; the currents and undercurrents which we recognize, and also attempt to reënforce or to oppose, will continue to roll along and gather strength or decline, and are in turn destined to be joined by others originating possibly in unlooked-for sources and coming unexpectedly to the surface of the waters. But, though our modern machinery makes us familiar with mills that grind with exceeding rapidity, the forces that direct the natural and wholesome advance of intellectual products grind slowly and grind exceeding fine. Evolution and not revolution is the order of nature. The psychology that we perhaps unwisely call new, if worthily representative of its time, is a natural sequence of former developments, and is destined soon to be merged in those

of the future. Whether it be merely the effect of our near-sighted perspective, or that we see as through a glass darkly or, as we hope, somewhat prophetically, yet we cannot but feel that the impress which the second half of our century has made upon the status of psychology will long be apparent in the currents and the undercurrents of the century to come.

## THE SOCIAL INDIVIDUAL.

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The consciousness of the present is distinctively social and tends to the merging of the individual more or less completely in the social organism. This tendency itself is no doubt one-sided, and there are dangers with which it is threatened. It arose, however, as a healthy reaction against the separative and anti-social tendencies of the thought of the eighteenth century which had embodied themselves in extreme forms and in which the divine right of the isolated individual had become a central dogma. Modern psychology was born on the rising tide of this movement, and has not as yet quite lost the impress of its parentage. Until a comparatively recent date the struggling science might very appropriately and not unjustly have been styled the psychology of the solitary.

Now apart from external motives there may be found in consciousness itself a degree of justification for this separative tendency. If we look into the workings of our own consciousness through the spectacles of the most advanced psychology we will find that it is possible for us to distinguish what we may call our isolated, individual self, from a larger self that includes our social relationships. Thus from the former point of view I conceive myself simply as a solitary ego or a me standing over against the other me of my family and social environment, and, in a sense, as having interests that are distinct from theirs, while from the point of view of the social relationships this dualism seems to be transcended, and the me becomes husband and father in relation to the family, and citizen in relation to the community and nation. It was the solitary ego rather than the me of the social relationships that became the subject of the old psychology, and the new psychology has arisen as the result of an



effort to cure this one-sidedness and to restore the broader social self to its rights. The project of the new psychology thus contemplates the reform of the psychological doctrine of the self as a basis for an ultimate reform of the doctrine of social relations.

How then, we may ask, is this new concept of the self as a *socius* to be achieved? In the first place, it is clear that it will be necessary to deny the validity of the older point of view in which the self stood apart from its social relationships and viewed these as external to its own interests and self-activities. On this point the old psychology can derive little support from the ordinary consciousness of the plain man to which its appeal was so commonly made; for the plain man's self is one that includes all his possessions, so that even an insult to his dog is taken as an indignity to himself. The real self is the concrete self of the social relationships; the self that can say, "nothing that touches any of my possessions can be indifferent to me." It is the business of the new psychology to show the validity of this by exhibiting either analytically, that the stripping off of the social relations leaves a mutilated ego, and when carried far enough nothing that is definable; or, genetically, that it is one and the same consciousness and life history in which are developed the representation of the individual self and that of the social other, and that the distinction between these two representations is *intra* rather than *ultra* to the real self. What then do we mean by the self as a *socius*, and how is the concept of the *socius* to be scientifically grounded? The answer will involve a reflection in two parts, the first dealing analytically, the second genetically with the problem. In the first place, then, we may ask for an analytic answer to the question what the notion of the self as a *socius* is. Let us take as our point of departure the consciousness of an adult, say that of an intelligent man who is at the same time innocent of psychology and not much given to self-reflection. Take, for example, the ordinary man of business and society whose life is absorbed in outer activities, and let our analysis proceed from the standpoint of his own conscious relation to his activities rather than from the aloofness of a mere spectator. His world must be represented, in fact, as one in which



his own aggressive and organizing agency stands central and to which every part of it will be related. Let such a man begin to inspect his own conscious processes; or, what would be still better, let some one who is trained in this species of analysis enter into his point of view as far as may be possible and perform the work of analysis in his behalf. If the man be primarily a man of business and only in a secondary sense a votary of society, it will be found that the standpoint from which he is most accustomed to consider himself and the issues of his life is that of his business relations, and that proceeding out from these he develops a conscious representation of himself as so bound up with a community of other selves of the same type, and whose point of view is identical with his own, as to give rise to a whole system of responses in the form of demands and obligations. This system of responses will constitute what is most real in his life, and were he to attempt to form any construct of himself as he would be apart from these vital relations of the business world, he would either find the enterprise impossible, or the self he would achieve would be hypothetical rather than real. The real self is the self of vital interests, and apart from this there can be no real self. The only resource open to the man in question, if he be not satisfied with his business self, is to transfer his vital interests to some other world. Let this be the world of society. Here it will be found that the same drama repeats itself; his vital responses take on the society order, and when he attempts to dissociate himself from his society relations the self that remains is mutilated and to a great degree divested of reality. This analysis may be carried through the whole sphere of his social relationships so as to include the domestic, civic and religious, and the same conclusions will be found to hold true. The self-consciousness of the family man is that of the individual clothed with a specification, so that the real self is now father, husband or son, and this specification thus modifies and determines the basis of all his conscious responses and consequently the whole sphere of his conscious responsibilities, privileges and enjoyments. Again the civic consciousness by virtue of which he becomes a citizen, a patriot and a member of a political party, is the bearer of a still further

specification of the central self. The citizen-consciousness is that of the conscious self specified and defined in the direction of the civic interests and relationships, and thus becoming the bearer of a larger complex of duties, privileges, responsibilities, rights and enjoyments. Lastly his religious consciousness by virtue of which he becomes a worshipper of God is a still further specification in view of his sense of unique relation to a being that transcends him. The result is a self defined and specified in this particular direction, and responding concretely to a system of motives that are distinctively religious ; in short, a self that is not real apart from its religious relations.

We have only to follow out this analysis into every detail of life in order to reach the conviction that the self which is central in all these activities, and which we may, therefore, call the cardinal self, is not in any sense independent of its social relations, or in any sense complete without them. The social relations constitute in fact the modes by which the self passes from the stage of indeterminateness, where it only vaguely realizes itself, to that of more complete specification and definiteness, through which it becomes more completely self-realized. The *socius* is, therefore, the more fully defined and realized self. James, in his very suggestive chapter on *The Consciousness of Self*, in the second volume of his 'Psychology,' gives an exhibition of this analytic method<sup>1</sup> and shows how the self achieves the various and successive stages of its definition in terms of the social medium. He represents these several stages as so many selves, and maintains that a man has a plurality of selves, each of which has its own characteristic ways of responding to its world. This may be accepted as a striking and, on the whole, appropriate way of stating the case, provided we do not go to the extreme which James avoids, and assert that these selves are not only distinguishable, but also separable. Our doctrine will lose coherence if we do not hold to it that it is the same cardinal self that is central and continuous in all this variation of form, and that the process as a whole is to be taken as the mode in which this cardinal self attains to definite and concrete self-consciousness.

<sup>1</sup> Psychology, Vol. I., Chapter X. The Consciousness of Self. This chapter has marked an epoch in the recent psychology of Self..

Passing to the second method of dealing with the social aspect of the self, the genetic, we find important illustrations of it in the work that is being done in the field of genetic psychology. The general aim of genetic psychology is, of course, to discover and formulate the stages and conditions of the development of consciousness. But a special department of the science has arisen of late in response to a pressing demand for a more adequate treatment of the psychological aspects of the social consciousness. The result has been a group of works which have had for their aim the genetic study of the social individual or self. Taking the work of Baldwin as developed in his *Social and Ethical Interpretations in Mental Development* as representative of the common aim of these works, we may found on it the following representation. The problem of this branch of the genetic enterprise is to show how the social consciousness may be brought under the rubrics of psychological evolution so as to give a demonstration of the solidarity of the social with the consciousness of the individual self. And this aim is achieved by showing in detail how the self in coming to its own clear and definite self-apprehension is brought by the same process to a recognition of its social other. The investigations we have in mind posit, by implication at least, a germinal self or at least a consciousness of the self-type as the inner individual center of response, and the object is to exhibit the method and the environmental forces which lead this germinal self-consciousness through the progressive stages of a development in which the social becomes a corporate part of the very self. Now what is needed in order that this aim may be effected and the development be seen to be real is to determine first what is meant by social environment and heredity; (2) the characteristic form of reaction in this field, and (3) the kind of definition or specification that the self obtains as a result. In short, the categories of the evolution must be defined with reference to the kind of material in which they are supposed to work.

Now it is not difficult to determine the nature of the social environment. If we consider the self as a social unit in a system of interacting units, it will be clear that the environment is simply the social medium in which the organism exists and performs its



functions, and that this medium not only includes the social individuals of the community, but also the social institutions and conventions of the community-life and conduct. Let us represent a child, for instance, as a floating center of adaptation in a medium that will embrace not only other social individuals and institutions, but will also hold in solution the whole current mass of conventions, convictions and tendencies that are characteristic of the time. This complex will represent the environment with which the child's consciousness will be in interactive relation. What, then, shall we designate as social heredity? It is possible, of course, that social modifications may be transmitted in a direct, organic way. But our opinion as to this would be largely determined by the theory of heredity which we regarded as nearest the truth. It is obvious that a Weissmanian could have little sympathy with the notion of the organic transmission of social effects. If, however, we recognize the super-organic character of the social, we will not be disposed to think it strange if we are asked to look in the super-organic field for the principle of the conservation of social effects. In truth, we have been asked, notably by Baldwin, to look into the heart of the social medium itself for this principle of conservation. When we consider this medium carefully we find that it not only contains a mass of what we may call social traditions in solution, but that there is a tendency in this medium for these traditions to embody themselves not only in institutions which perpetuate certain great ideas or trends of the mass, but also to give themselves an unorganized though well-defined form in what may be called the *spirit*, which the past has projected into the present. This spirit will manifest itself most broadly in civilizations, less broadly in national character, so far as it grows out of traditions. It will give itself more and more circumscribed but not less powerful embodiments in the traditional spirit of tribes, cliques, special institutions and families, the spirit of the family, for example, being one of the most potent educators of the child. The tendency of this conserving force is, therefore, toward the fixity of definite types in distinction from that of the environment, which is a medium in which everything tends to become fluent. Now it is to this conserving



force, however it may express itself, whether in the perpetuity of institutions, the conservation of literature and art, or in the hereditary spirit of family, tribe and nation, that the name social *heredity* is to be applied, and it is evident that when we have overcome a little our biological prejudices against the super-organic in general we will be ready to admit that we have a force here which performs a real function of conservation and transmission. We shall take the liberty then to agree with those who have thus defined the principle of social heredity.

The second problem we have to determine is the form which the responsive, adaptive movement takes in this field. The psychologists to whom we have referred develop two lines of investigation which have a bearing on the question, the first of which has for its object the exhibition of the general method by which the subject-consciousness comes to a realization of itself and its world, while the second aims to determine the principle by means of which this result is achieved. Now in regard to the general method by which the subject-consciousness realizes its world, it has been carried almost to the point of demonstration, I think, that the movement is first objective. Consciousness goes out upon its objective world in some pulse of aggressive activity, and in this act is able in some way to penetrate and realize the object. This leads to a return reactive movement in which consciousness as the result of its penetration of its world, attains to a higher and better defined conception of itself. The general movement is thus circular and embraces objective and subjective stages. What then is the principle through which this movement realizes itself? Here again we come upon a super-organic phase of our problem. The principle or category which was first pointed out by Tarde, and developed by Baldwin, Royce and others, is that of *imitation*, a term that is somewhat difficult to define, but whose operation may be definitely conceived. Let us suppose that a boy of say six years, who is the son of a carpenter, after observing his father plane and fit together some flooring boards, procures a plane and some pieces of board and makes the effort to plane them and fit them together.<sup>1</sup> The process is manifestly one of

<sup>1</sup>This illustration is taken from an instance that actually happened a few days ago in connection with the building of my own house.

imitation, and the boy has the representation of his father's action as a copy which he is trying to reproduce. By a series of tentative movements let us suppose that the boy succeeds in a passable reproduction of the copy he has set before him. We have here not only a transaction, but an experience. The transaction is the *imitative* movement or series of movements by means of which the boy has reproduced a certain kind of effect in the objective world. The experience is the subjective reaction of this result, the modification or specification which the self has achieved when it has not only expressed the emotional exaltation which we call the feeling of success, but has also become defined by its knowledge of the feeling of a carpenter when he produces the original of the boy's copy. In other words, the boy has not only produced an effect in the objective world, but he has also defined a consciousness in himself analogous to the consciousness which in his father accompanied the act of carpentry. And it is open to the analyst in this field to point out how this new consciousness becomes, by virtue of the fact that it takes the form of a defined idea, a motive impulse to further activities in the same line. We thus have exhibited the operation of a principle which tends to the repetition of activities on a progressively higher scale, and thus to the perfection of the adaptive result.

Let us now pass on to the third point, and consider the kind of modification or specification which the self receives as the result of this process. Referring once more to the case of the boy, it is clear that the knowledge of the way in which an objective act of skill is to be performed will not be the only respect in which his self-consciousness will become defined. More important than this in its psychological bearings will be the fact that through his activity the boy is able to *enter into his father's consciousness and to realize, in fact, how a carpenter feels in connection with his work*. In short, he has made an important step in the direction of mastering the carpenter's point of view from which he contemplates and reacts upon his world. We have, now, only to change the illustrations to forms that are more distinctively social, as, for example, the imitation by children of family, social or religious functions, in order to be able

to see that this category of imitation stands as a definite mode, whether we regard it as the only mode or not, by and through which the growing consciousness not only makes progressive inroads into the objective, but also achieves a progressive definition and qualification of itself.

If now we take into account both lines of psychological investigation, we find that in both inquiries the social vindicates itself as an essential element in the defined consciousness of self. The analytic inquiry made this clear by showing that to strip off the social modification is also to take away the definitions of self-consciousness, so that where the process has been completed there will remain nothing but the wholly undefined cardinal self which the whole investigation has presupposed. The various social selves are reducible, therefore, in the last analysis to phases of the one central self. The results of the genetic inquiry have been found to be on the whole confirmatory of the results of analysis. The problem here is one of history, and the aim is to show how the self develops its social character. The outcome of the investigation is, as we have seen, not only confirmatory of the result of analysis, but it teaches an impressive lesson in its own way. When we have followed the process by which the social elements gain entrance into the growing consciousness, and have seen that it is the very process also in which the self-consciousness becomes defined, our conviction becomes that of one who has been permitted to be present at a demonstration.

Admitting the truth of the doctrine as thus far developed it is still open to us to ask whether the boy's own subjective consciousness with which he accompanies the progressive stages of the objective activity is not his only immediate experience, and whether he does not learn how his father feels in a given situation, by traveling through that situation, and first learning how he himself feels?

This seems to be a more adequate view, and we are disposed to recant anything we have said to the contrary, and to put in its place the statement that the boy learns the true subjectivity of situations by traveling through them, and that being the model of his father traveling through the same situation in



mind, the interpretation of the father's consciousness is the result of a largely spontaneous application of analogy. This will enable us to define the boy's relation to his model in a way that will save the initiative of his own consciousness, for if it turns out that there is only one way of getting at the inner consciousness of another, and that, by traveling through some objective movement in an imitative way which generates directly a modification in our consciousness which is referred to the consciousness of the other, through the model that connects it with the same kind of activity; if this proves to be true, then we are in possession of a datum that will be important when we come to determine how one conscious self may interact with another.

Analysis of the situation makes it evident that the above statement of the case is correct, and that while the boy seems to be reading his father's consciousness directly through his model, *he is, in the first instance, determining his own consciousness by means of the imitative activity, and reaches the construct of his father's consciousness only by what we may venture to call an immediate analogical inference.* If this be true, the question may arise as to the precise function which the *model* performs in the boy's development. The imitative function is clear enough, and there can be no question that what the boy has in the foreground of his consciousness is not simply a representation of a series of movements, but rather the representation of this series as connected with, and as being the movements of, a definite individual, his father. The whole model is, therefore, a representation of his father performing a series of movements and the boy's attempt to imitate the whole situation. It is clear then that the effort to imitate is in reality an effort on the part of the boy to identify himself with his model, and that this identification involves his reading himself consciously into the standpoint of his model, so that his own consciousness and that of his model, so far forth as that special series of activities is concerned, shall be the same. Now we have here, I think, an instructive example of the typical method by which the self comes into conscious relations with other selves and is able to form constructs of the selves which stand related to it as its social others. We are not dealing here with the practical motives that may



enter into the situation and lead to actual association. Men as a matter of fact associate for all sorts of reasons. The question here is different. Assuming that men do and will associate for a variety of reasons, we ask: What is that fundamental quality of their nature which makes it possible for them to associate and without which association would be impossible? Mr. Spencer has given a general answer to this in the second volume of his 'Psychology,' in which he maintains that in order to sympathize with our fellows we must be able to represent to ourselves their consciousness and their actual mental condition.<sup>1</sup> Now the whole theory of imitation may be regarded as a grounding of this general principle by showing how the representation of another's consciousness is achieved. And the analysis of the imitative situation has led us to expect that in it we have involved the most vital point of relationship between one individual consciousness and another. Let us endeavor then to make this clear. We have seen that a necessary condition of imitation is a model in the foreground of consciousness. The boy's model is his father planing and fitting floor boards. Only a part of this model is, however, an external representation. The most vital part for us is internal and consists in a construct which the boy has formed of the consciousness of his father. If now we scrutinize the situation with sufficient care we will find that the boy's construct of his father's consciousness which he has incorporated in his model is one that is defined just as far as his experience of his father enables him to define it, and beyond that it is undefined, or at least but vaguely guessed at. And the point of vital interest here is the fact that before the imitative activity begins *just that part of the father's consciousness that is directly involved in the series of movements which the boy is trying to reproduce, will be an undefined region for the boy, and that the imitative movement will have as its result its definition.* Let us represent this part of the father's consciousness by  $x$ ; it will be clear then that to the boy  $x$  is an unknown quantity, and that the value of this quantity is to be determined by the experiment itself.

<sup>1</sup>Principles of Psychology, Vol. II., Corollaries; I., Sociality and Sympathy.

That  $x$  shall be an unknown quantity is then an essential condition of the experiment. The boy is doubtless unaware of this, and he is least of all interested in a psychological experiment. All that he is conscious of is the fact that his model is interesting to him, and that there is a, to him, undefined impulse to attempt to realize it. Nevertheless he is taking part in a very profound experiment, and is putting both science and metaphysics under obligation. Let the problem here be to determine the value of  $x$ . Now the known terms are the present consciousness of the boy, which is undefined in its relation to  $x$ ; the model which connects a series of movements with the father's consciousness, which to the boy is also undefined as respects  $x$ ; and thirdly the impulse to imitation—that is, to a reproduction of the model. These are the known data. How then will the boy proceed to ascertain the value of  $x$ ? The answer will be as follows. Obeying the impulse to imitate his model he will, no doubt in a very tentative way, proceed to perform the series of movements involved. He will provide himself with a carpenter's plane and with some pieces of flooring board, and will proceed to use the plane as he has seen it used, and finally to fit the pieces of board together so that the raised part of one will fit into the groove of the other, and he will no doubt prosecute the experiment until he has succeeded in obtaining a satisfactory result. This will represent the whole outward process, and will be all perhaps that the boy could give a very clear account of to his own consciousness. But in the meantime  $x$  has not dropped out of the problem, and some very important steps have been taken in the determination of its value. For the boy has been learning how a carpenter feels in connection with his work, or this part of it, and in doing so has defined his own consciousness as respects the unknown term  $x$ . The value of  $x$  expressed in terms of his own consciousness is the first-hand knowledge he has acquired of how the carpenter-consciousness operates in connection with this particular series of movements. We have then as the first step in the solution the determination of the value of  $x$  *for the boy's own consciousness*. But it still remains to determine the value of  $x$  for the father's consciousness. The peculiarity of the situation here is, of course,

the fact that the father-consciousness is assumed already to know the value of  $\alpha$  for itself, and that the problem is altogether one for the consciousness of the boy. How shall the boy reach the construct of his father's consciousness so that he shall be able to sympathize with him in his work? It is clear that in order to discover the value of  $\alpha$  in the father's consciousness the boy must realize it in his own, and then using his own  $\alpha$ -defined consciousness as a model he will by the use of the analogical reference construct a like defined consciousness for his father, and will assume that his father's conscious relation to his work will be the same as his own. And having thus determined the value of  $\alpha$  for his father's consciousness he will be able, taking the common value of  $\alpha$  as his basis, to enter sympathetically into his father's experience.

The above analysis of the situation has been followed out far enough to enable us to see clearly the modes by which one conscious self enters into and realizes the consciousness of another self. There is no magic involved, nor is the relation purely outward and extrinsic. But we find that, through the stimulus of the model in the foreground of consciousness, the boy, and his experience may here be generalized, enters upon a series of movements which enable him to effect a new definition in his own consciousness, and it is through this self-definition that he is able to form his construct of the consciousness of another. Now it is evident we may broaden out the situation beyond the limits of well-defined imitation, so as to include the direct as well as the indirect methods of interaction, and the principle will be the same. I mean by this that, whether we conceive the father as reacting directly upon the boy, or the boy as reacting directly upon his father, it will be true of these direct reactions, as it is of the indirect reactions in which imitation is overt, that each, in order to reach a construct of the consciousness of the other, must draw it up in terms of his own inner experience in similar relations. This brings the issue to a point where the last and most vital term in the theory of the social consciousness may be brought out and defined. We have seen that every step we take in construing the inner consciousness of another—that is, in conceiving the existence of



another like ourselves—is preceded by the specific definition of our own self-consciousness in just the respect in which we proceed to define the other; and we have discovered this in connection with the fact that we were able to reach this definition, first of self and then of the other, through the medium of some common outer movement or series of movements, which we were able to relate to both self and the other as their common activities. Neglecting this latter feature for the present and taking into account only the inner relation between self-consciousness, and that of the other, it is clear that the condition of being innerly conscious of another self is the becoming ourselves conscious in the definite sense involved, and that it is from this definite self-consciousness that we form the construct or concept from which we read ourselves into the consciousness of the other. The primacy of the self-consciousness is thus secured, and the consciousness of the other is, in this fundamental sense, its function. When, therefore, we ask either how the self comes to ascribe its analogies to another or how the other secures for itself a representation in the consciousness of the self and thus the power to influence it internally, the answer must be one in which this primacy is respected. For, whether we suppose that the consciousness in which the effect is to be produced has before it a definite model, as in explicit imitation, or simply certain outer signs which it interprets, we will find that the interpretation in either case will involve the bringing of the sign to the touchstone of some inner experience. Thus, when the child begins to cry on seeing her companion's finger bleed, the result is no immediate effect of the representation, but acquires its emotional power through some process that associates it with an inner experience of pain of the child's own, arising from an analogous cause. The touch that makes us kin is, therefore, an inner touch, while the objective and outer motive that leads to this touch is either an imitative movement or a representation that is rendered capable of a reference to the inner consciousness of another by means of its prior association with inner experiences of our own.

The conclusion of the whole matter may be stated in the following terms. We learn as the result of certain experiences



to ascribe our inner consciousness, or its analogue, to others. The outward instruments of the development of this social consciousness are, broadly speaking, *association* and *imitation*. But when we pass from the consideration of the external instruments to that of the internal process we will find that we are able to enter into intelligible social relations with our other only because our nature is such that we are able to draw from the inner definitions of our own consciousness, brought about by certain objective agencies, a concept or construct of the consciousness of the other, which we conceive to be a true representation of his inner experience, and it is through this construct or representation that we are able to enter sympathetically into his life and treat him as a *socius*; a being like ourselves. On the other hand, if the question be how my other is able to come into social relations with me and to affect and modify me socially, the answer is very much the same. He can do so only by producing in me a definite representation of himself, and this is possible only through the outer mediation of association or imitation. The conditions are the same on both sides, and the truth is made clear that the only way in which social interaction is possible, or social effects producible, is through the power which each self-conscious individual has of internally representing the consciousness of his fellow; or, putting it from a different angle, the power which each self has of entering into the consciousness of its fellow and producing there an internal representation of itself.

## AN EXPERIMENT ON GETTING AN AFTER-IMAGE FROM A MENTAL IMAGE.

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In the literature of the subject one finds a deplorable lack of detail both as regards methods used and results obtained. Leaving out of consideration the experiments conducted on hypnotized persons, the classic references are to the cases of Féré and Meyers and to the general statements of Wundt. The great difficulty that suggests itself to even the most casual reader of the passages in question is the one upon which Dr. Franz lays stress in his monograph upon after-images, namely, that the subject, who, in the nature of the case, must be an exceptionally vivid visualizer, could very well suggest a sequence of colors corresponding to after-image effects unless this possibility were ruled out by the subject's absolute ignorance of the very existence of after-images.

That the subject should be naïve as regards after-images is the first and paramount requirement for the success of the experiment. Other requirements and precautions suggest themselves, however. The subject must have his mental images under control; he must be able to hold them for a certain length of time; he must, moreover, be to a certain degree a trained observer—that is, able to observe and to report accurately upon color sequences. Usually, in work upon after-images, constancy in result is held to be the test of the trained observer, and yet even under the most unvarying objective conditions variations in the results obtained from any one individual will occur. Naturally one would expect to find this difficulty increased in dealing with mental images where the attributes of quality, intensity and duration could not be kept absolutely constant. Variation in result is usually balanced by frequent repetition of the experiment. But in the problems under consideration even

repetition has its dangers. How during these repetitions is the subject to be kept from suggesting a sequence of color once obtained? Conditions, of course, might be varied, or a sufficiently long time-interval might occur between the experiments to rule out self-suggestion due to conscious memory, but the effect of subconscious memory would remain. How serious the operations of subconscious memory might be the writer is not able to judge.

Since the colored after-images, even in those whose color-vision is classed as normal, vary widely within certain limits the results of any experiment upon the topic could be tested only by a comparison with the after-images obtained by the subject from a series of actually presented colors. Even so, discrepancies could not always be interpreted unfavorably, for variations, as said, may occur in any individual case. Within limits, indeed, variation would be favorable evidence, inasmuch as it would show the absence of self-suggestion.

The writer has been able to carry out a series of experiments on the after-images of mental images under the above-mentioned conditions. The greatest difficulty experienced was the finding a satisfactory subject. A great many students were tested, and although several were found who could visualize colors readily and vividly, yet work with them usually disclosed the fact that their images were not under control, could not be held for any length of time, could not be projected upon outer surfaces, etc. While several trials with different subjects were more or less successful, the experiments could not be carried far enough to justify any conclusions. An entirely satisfactory subject was finally found.

The subject was a girl in her twenty-first year, a junior in college in the classical course, her work being almost entirely confined to the languages. As regards after-images she was absolutely naïve. For the rest she is a good student, and not only an exceptionally good visualizer, but also accurate as to the memory and discrimination of colors.

The experiments were tried carefully. During the experiments, as far as possible, the eyes were kept from all stimulation other than light reflected from a white or black surface. In the majority of the experiments the colors were visualized

upon a white background, and, after they had been held for some seconds, the eyes were closed. Sometimes the background upon which the colors were visualized was changed; sometimes the after-image was projected upon some background other than the retinal light. The subject was asked to observe and to report carefully all experiences. Unfortunately not all the conditions could be kept absolutely constant. It was not possible to try all the experiments at the same hour of the day, nor at the most satisfactory hour. Usually the time given to the experiments was at the close of the day's recitations, when the subject was fatigued to a greater or less degree. During the early part of the morning the subject could visualize more readily and more vividly than later in the day. The experiments seemed to tire the eyes as would working with actual colors.

The aim of the experiment was to test other-colored after-images, and therefore the time of holding the mental images was made long. Nevertheless, positive after-images were noticed on several occasions as the records show. No attempt was made to keep the time of holding the mental image constant. The subject was not always able to hold the color a required time, and was instructed to close her eyes if she felt the image leaving her. The subject was also allowed to choose the form which the color should take. The order in which the colors were visualized in the different series was carefully varied; in particular, complementary colors rarely followed one another. The results, in general, show no error arising from this cause, and so this record, although carefully kept, is not included in the account. Every effort was made to rule out expectation by means of constant variations in the manner of conducting the experiment. When possible the subject matched the color of the mental image and the after-image on the Bradley color-top. No suggestion was ever made as to the object of the experiment and the casual remarks of the subject were interesting. "I can't see what you can get from these experiments, for the results are never the same twice," etc. At the beginning of each series the subject was questioned, as indirectly as possible, as to her memory of the previous results. She usually answered: "I haven't tried to remember." "I can't remember." She had



been instructed not to use effort to recall any of the results. October 23, she was, for the first time, asked to use effort and to remember what she could. "Of course I can't recall all, because the results were very different. I *know* that several times I got a black image after visualizing red. Once I got red after visualizing green. I can't remember what came after orange. Sometimes I got blue after yellow and I believe once I got orange after violet."

The girl is not particularly suggestible, nor, on the other hand, open to contrary suggestion. A trial was made both with visualized and presented colors in which a color sequence was suggested. In the case of the visualized colors out of eight experiments the true after-image came first five times, and had to be suppressed with effort before the color suggested could be visualized. In two experiments there came a perceptible blank interval and then the suggested color. In one instance the suggestion operated to remove all images. These experiments are included in the tables summarizing the results obtained from the different colors. In the case of the presented colors, now used for the first time, suggestion had in five experiments no effect whatever. In two other instances it operated to banish after-images altogether. In the further tests with actual colors the subject was asked if she thought she could control the color of the after-image. She thought not. The writer has never been able to change the color of an after-image in the slightest degree. That expectation could emphasize one color of a compound, the yellow or red of orange, for instance, is, however, conceivable.

In order to test the relative vividness of the color-tones of the mental images as compared with actually presented colors, an experiment was tried in which the visualized images were thrown upon colored backgrounds and after-images obtained from the combinations. The writer's faith in the color-intensity of the images was so slight that she fully expected failure in the attempt. The results did not confirm her expectations. On the contrary, the subject found no difficulty in throwing her mental images upon the most brilliant background. When questioned as to the relative intensity of the colors she maintained that they were about equal. The visualized disc completely hid the color

lying underneath. In three cases, namely, orange visualized on blue, blue on orange, and green on red, the colors fused, giving gray. Out of eleven trials, in only two cases was the subject unable to throw her mental image upon the required background. The after-images obtained from the combination of visualized and presented colors are included in the tables.

Two or three unsuccessful attempts were made to throw the after-images from a mental image upon a colored background. Unfortunately, circumstances made it necessary to bring the experiments to a close and further work along this line had to be given up.

At the close of the experiment the subject was required to get and to match carefully on the color-tops a set of after-images from the six spectral colors. The other conditions were kept constant. No explanations were made, but the subject was encouraged to compare as far as memory permitted both the visualized colors with the actual colors and the results in the two sets of after-images. The comparisons, which were highly instructive, are given below.

It will be noticed in going over the tables that the results obtained from green are practically constant, varying between brownish-red through spectral red to pinkish-violet. This table is the most satisfactory of the six. The results obtained from red are also conclusive, although by no means satisfactory. In the series of black after-images found in this table suggestion was doubtless operative, as remarked by the subject herself. The results from orange and violet were also practically constant, a blue-violet for the one, a yellow-orange for the other. Blue was the hardest color for the subject to visualize and the hardest to hold any length of time. Though a large number of experiments in this series were without result, those giving colors were, for the most part, favorable, showing yellow as the after-image. The results from yellow are unsatisfactory. While a violet or blue occurs in most of the experiments, it often came supplementary, as it were, to some other color.

In summarizing the results by numbering the colors obtained from any given image it is to be remembered that the true after-image, so to speak, was sometimes accompanied by little blotches

of other colors, and these are included in the summaries. A reference to the table will show just the relation between the colors where two or more came together.

In several instances the forms of the mental and after-images were different. In general as regards the disc seen as after-images both of the mental image and the actually presented disc the subject observed that it appeared to lie on an inclined surface; that is, it appeared oval rather than perfectly round.

In considering the results of the experiments the writer was, at first, puzzled by what had been obtained in the case of blue, orange and violet. Her own experience with after-images gives with closed eyes, blood-orange for blue, brilliant blue for orange, bright yellow for violet. The subject visualized a blue that was lighter than spectral, and a yellow-orange rather than a spectral orange. The results obtained from actually presented colors confirmed those obtained from the visualized series. As a matter of interest each member of the writer's psychology class of the present year was required to match carefully on the color-top his other-colored after-images from the six spectral colors. The individual differences were highly instructive. As typical are given the results obtained by four members of the class for spectral blue, the objective conditions being practically the same for all: *A*, cream (19 white plus 1 spectral yellow); *B*, light orange (19 spectral orange plus 1 white); *C*, yellow (14 white plus 6 spectral yellow); *D*, greenish-yellow (12 spectral orange plus 8 spectral green).

In conclusion, it may be said that although no exact quantitative measurements were attempted the writer believes that the experiment, as conducted, offers good evidence for the getting of an after-image from a mental image. It is, however, admitted that possibility of error was not wholly overcome; most serious being the effect of subconscious memory and, consequently, self-suggestion. The subject, who commenced the study of psychology this September, had, at its close, the object of the experiment carefully explained to her and her opinion was asked as to the results. Personally she is convinced of getting after-images from her mental images but, of course, her testimony has no absolute value. The reader may draw his own conclusions from the tables given below.



TABLE I.

MENTAL IMAGE.						AFTER-IMAGE.		
Date.	No.	Color.	Form.	Back-ground upon which thrown.	Time of holding.	Color.	Form.	Back-ground upon which thrown.
Dec. 1, '99	1	Orange	Cross	White	20s	White.	Cross	White
Feb. 7, '00	2	"	"	Black	45s	Green, orange and lavender in sections.	No record	Retinal light
"	3	"	"	"	—	Violet.	Cross	"
Mar. 7, '00	4	"	"	White	30s	Violet (spectral).	"	"
Mar. 14, '00	5	"	"	"	40s	Violet or purple with green edge.	"	"
"	6	"	"	"	40s	Deep violet, little green around edge.	"	"
"	7	"	"	"	40s	Orange with spectral violet around.	"	"
Mar. 22, '00	8	"	Circle	"	45s	Heliotope. Matched on color-top (shade of violet 12, orange 8).	) (	"
"	9	"	"	"	35s	Green (10 sec.) then violet shade.	Circle	"
Sept. 28, '00	10	"	Disc	Black	15s	Violet.	Dis't Disc	"
Oct. 23, '00	11	"	"	White	30s	Blue-violet (then green around the violet. Green had been suggested).	Disc	"
Oct. 24, '00	12	"	"	"	20s	Violet-blue (blue had been suggested).	"	"
"	13	"	"	Blue Square	20s	No result.	"	"
"	14	"	"	Blue square	20s	Big yellow square with little violet disc inside. Around yellow square a margin of blue.	Disc	"

## SUMMARY OF RESULTS FOR ORANGE. (TABLE I.)

Total number of experiments, 14.

Number of experiments without result, 1.

After-color: Violet hues, 12; orange, 2; white, 1; green, 5.

(Note.—In No. 5 and No. 6 the green appeared as a border edging the violet; in No. 2 as a section accompanying orange and lavender; in No. 9 green appeared first, giving way after 10 seconds to violet; in No. 11 green had been suggested as after-effect and it appeared around the violet after the subject had made an effort to visualize it.)



## EXPERIMENT WITH PRESENTED ORANGE.

On the closing of the eyes, after they had been stimulated for 30 seconds by spectral orange upon a white background, the after-image was described as bluish-green and matched by 8 spectral blue plus 12 spectral green. The subject observed that the orange she visualized was not as red as the spectral orange. She matched from memory the orange she had been visualizing as  $9\frac{1}{2}$  spectral yellow plus  $10\frac{1}{2}$  spectral orange. The after-image from this was matched by 2 light green plus 18 spectral violet.

TABLE II.

MENTAL IMAGE.						AFTER-IMAGE.		
Date.	No.	Color.	Form.	Back-ground upon which thrown.	Time of holding.	Color.	Form.	Back-ground upon which thrown
Dec. 1, '99	1	Blue.	Cross.	White.	20s	Yellow.	Cross	White
"	2	"	"	Seen in air.	20s	Red with violet tinge.	"	Retinal light
Feb. 7, '00	3	"	"	Black?	30s	No results.		
"	4	"	"	"	30s	" "		"
Mar. 7, '00	5	Spectral	"	White.	30s	Dark green changed to spectral yellow.	Cross	"
"	6	"	"	"	35s	Green with yellow outside.	"	"
Mar. 14, '00	7	Blue tint.	Disc.	"	35s	Yellow with pink border.	Disc	"
"	8	"	"	"	35s	No results.		"
"	9	"	"	"	35s	" "		"
"	10	"	"	"	50s	Spectral yellow not bright.	Disc	"
Mar. 22, '00	11	Spectral	Cross.	"	35s	No results.		
"	12	"	Circle	"	50s	Spectral yellow.	Disc	"
Sept. 28, '00	13	"	Disc.	Black.	22s	No results.		
"	14	"	"	"	20s	Yellow streak.	Streak	"
Oct. 23, '00	15	"	"	White.	15s	Bright yellow streaks.	Streaks	"
"	16	"	"	Red square.	No Rec.	Big green square with small red disc inside, then blue disc.	Disc	"
Oct. 24, '00	17	"	"	White.	20s	Yellow then red. Red had been suggested.	"	"
"	18	"	"	Orange square.	23s	Greenish-blue square which changed to violet square, then yellow appeared around edges of the violet.		"

## SUMMARY OF RESULTS FOR BLUE. (TABLE II.)

Total number of experiments, 18.

Number of experiments without result, 7.

After-colors: Yellow, 9; red, 3; green, 2; pink, 1.

(Note.—In No. 17 red had been suggested as after-image. It came with effort after the yellow had been suppressed. In No. 7 the pink was matched by 10 orange plus 10 white.)

## EXPERIMENT WITH PRESENTED BLUE.

On the closing of the eyes, after they had been stimulated for 30 seconds by spectral blue upon a white background, the

TABLE III.

MENTAL IMAGE.						AFTER-IMAGE.		
Date.	No.	Color.	Form.	Back-ground upon which thrown.	Time of holding.	Color.	Form.	Back-ground upon which thrown.
Dec. 1, '99	1	Yellow	Cross	White	20s	Red.	Cross	White
"	2	"	"	"	20s	No results (eyes tired).		
Feb. 7, '00	3	"	"	Black	40s	Deep green.	"	"
"	4	"	"	"	40s	"	"	"
Mar. 7, '00	5	"	"	White	30s	Green, gentian pink or violet around edges.	"	"
Mar. 14, '00	6	"	"	"	35s	Green.	"	"
"	7	"	"	"	40s	Green then violet at top.	"	"
Mar. 22, '00	8	"	Circle	"	40s	Green, not vivid, (10 green and 10 white).	Circle	"
"	9	"	"	"	35s	Blue tint, small.	"	"
Sept. 28, '00	10	"	Disc	Black	20s	Yellow.	Disc	"
"	11	"	"	"	15s	Green and violet.	"	"
Oct. 23, '00	12	"	"	White	25s	Blue disc with yellow.	"	"
"	13	"	"	"	20s	Blank interval then violet. Violet had been suggested.	"	"
Oct. 24, '00	14	"	"	Violet square	25s	Big yellow square; inside of it orange and blue discs.	"	"
"	15	"	"	Green square	17s	Big pinkish-red square, no disc. Light blue of no definite shape ar'd the disc. This blue was probably due to contrast and is omitted in the summary.	"	"

after-image was described as yellow and matched by 5 orange plus 15 yellow. The blue visualized was judged to be somewhat lighter than spectral blue.

TABLE IV.

MENTAL IMAGE.						AFTER-IMAGE.		
Date.	No.	Color.	Form.	Back-ground upon which thrown.	Time of holding.	Color.	Form.	Back-ground upon which thrown.
Dec. 1, '99	1	Violet.	Cross.	White,	20s	Orange or deep yellow (yellow 9 and orange 11).	Cross.	White.
"	2	"	Tri-angle.	"	20s	Violet with orange around.	Tri-angle.	"
"	3	"	Cross.	In air.	20s	9 yellow + 11 orange.	Cross.	"
Feb. 7, '00	4	Light Violet.	"	Black.	30s	2 yellow + 18 orange.	"	Retinal light.
"	5	"	"	"	30s	2 yellow + 18 orange.	"	"
Mar. 7, '00	6	Violet (Spectral).	"	White.	30s	Orange in center, green outside. When eyes opened quickly saw violet; when closed, orange-yellow.	"	"
Mar. 14, '00	7	Violet.	"	"	30s	Orange, green for an instant at one corner.	"	"
Mar. 22, '00	8	Light Violet.	Circle	"	45s	Orange.	Circle	"
Sept. 28, '00	9	"	Disc.	Black.	12s	Two squares; one orange ( $7\frac{1}{2}$ yellow + $12\frac{1}{2}$ orange), the other violet with green at bottom. The green and violet square vanished first.		"
Oct. 23, '00	10	Violet.	"	Yellow square.	15s	Large violet square, small yellow disc.	Disc.	"
Oct. 24, '00	11	"	"	White.	No record	Nothing, then blue. Blue had been suggested as an after-image.	"	"
"	12	"	"	"	20s	No result.		
"	13	"	"	Yellow square.	15s	Large violet square, a small yellow disc within.	"	"

## SUMMARY OF RESULTS FOR YELLOW. (TABLE III.)

Total number of experiments, 15.

Number of experiments without result, 2.

After-colors : Violet or blue, 7 ; yellow, 2 ; red, 1 ; green, 7.  
(Note.—In No. 5 and No. 7 the violet edged the green. In

TABLE V.

MENTAL IMAGE.						AFTER-IMAGE.		
Date.	No.	Color.	Form.	Back-ground upon which thrown.	Time of holding.	Color.	Form.	Back-ground upon which thrown.
Dec. 1, '99	1	Red.	Cross.	White.	20s	Black.	Cross.	White.
"	2	Dark red.	"	In air.	20s	Dark green with red around.	"	Retinal light.
Feb. 7, '99	3	Scarlet.	"	Black.	No Rec.	Spectral yellow.	"	"
"	4	Bright red.	"	"	"	Spectral yellow.	"	"
Mar. 7, '99	5	Crimson.	"	White.	30s	Black with dark green around.	Sq.	"
Mar. 14, '99	6	Red.	No Rec.	"	No Rec.	Square, black-blue, changed to black-yellow.	"	"
"	7	"	"	"	40s	Black (probably due to self-suggestion, "I'll see black bordered with red").	No Rec.	"
"	8	"	"	"	40s	Black with yellow center. Subject tried to banish suggestion.	"	"
Mar. 22, '99	9	"	"	"	35s	Dark green.	"	"
"	10	"	Cross.	"	30s	Green.	Oval.	"
Sept. 28, '99	11	"	Disc.	Black.	20s	Red square with light green around edge.	Sq.	"
Oct. 23, '99	12	"	"	White.	25s	Green, no particular form.	"	"
"	13	"	"	"	25s	Green then violet (violet had been suggested).	Disc.	"
"	14	"	"	Green square.	No Rec.	Colors reversed. Green disc on red square.	"	"
Oct. 24, '99	15	"	"	Blue square.	25s	Large yellow square with small green square inside and blue on the outside of it.	Sq.	"
"	16	"	"	Black square.	15s	Large black square with small spot of green inside.	Dot.	"

No. 13 violet had been suggested as the after-color. The number of images with green in them is large. The writer has thought that perhaps the green image was somewhat after the



nature of a positive after-image, the retinal light modifying the yellow to this extent, as mixing black with yellow would do, but is aware that the suggestion has, under the circumstances, little cogency.)

#### EXPERIMENT WITH PRESENTED YELLOW.

On the closing of the eyes, after they had been stimulated for 30 seconds by spectral yellow, the after-image was described as blue and matched by 6 violet tint plus 14 spectral blue.

#### SUMMARY OF RESULTS FOR VIOLET. (TABLE IV.)

Total number of experiments, 13.

Number of experiments without result, 1.

After-colors: Deep yellow or orange, 11; violet, 2; green, 3; blue, 1.

(Note.—The green came each time as a border to the violet or orange. The blue was suggested as an after-effect and came after a blank interval.)

#### EXPERIMENT WITH PRESENTED VIOLET.

On closing the eyes after they had been stimulated for 30 seconds by spectral violet upon a white background, the after-image was described as orange and matched by 5 orange plus 15 yellow.

#### SUMMARY OF RESULTS FOR RED. (TABLE V.)

Total number of experiments, 16.

Number of experiments without result, 0.

After-colors: Green, 10; red, 1; black, 5; yellow, 4; violet, 1.

(Note.—In No. 7 and No. 8 the subject was conscious of expecting black, and this probably accounts for the black. In No. 13 violet had been suggested, and it appears after the green had been suppressed with effort.)

#### EXPERIMENT WITH PRESENTED RED.

On the closing of the eyes after they had been stimulated for 30 seconds by spectral red upon a white background, the after-image was described as green and matched by green tint.

## SUMMARY OF RESULTS FOR GREEN. (TABLE VI.)

Total number of experiments, 14.

Number of experiments without result, 2.

After-colors: Violet-pink, red, and red-brown, 10; green, 3; violet, 1; yellow, 2; black, 1.

TABLE VI.

MENTAL IMAGE.						AFTER-IMAGE.		
Date.	No.	Color.	Form.	Back-ground upon which thrown.	Time of holding.	Color.	Form.	Back-ground upon which thrown.
Dec. 1, '99	1	Green.	Cross	White	20s	Yellow.	Cross	White.
"	2	"	"	In air	20s	Violet-pink.	"	"
Feb. 7, '00	3	"	"	Black	30s	No results.	"	Retinal light.
"	4	Dark Green.	"	"	40s	Red-brown (8 ½ spectral red + 11 ½ dark red).	"	"
Mar. 7, '00	5	Spectral green.	"	White	30s	Gentian violet (10 pink disc + 10 violet) changed to brown.	"	"
"	6	"	"	Retinal light	No Rec'd	Spectral violet.	"	White
Mar. 14, '00	7	Green.	"	White	30s	Brown (6 ½ spectral yellow + 13 ½ red).	"	Retinal light.
"	8	"	"	"	40s	Green in center, brown outside.	"	"
Mar. 22, '00	9	"	Circle	"	40s	Green with red in center.	Circle	"
"	10	"	Sq.	"	30s	Green, then red circle.	Sq.	"
Sept. 28, '00	11	"	Disc.	White	20s	Red blur over all changed back to green.	None	"
Oct. 24, '00	12	"	"	Black	20s	Red, then yellow. Yellow had been suggested.	Disc	"
"	13	"	"	"	20s	Blank interval, then black. Black had been suggested.	"	"
"	14	"	"	Red square	30s	Big green square with small red disc.	"	"

(Note.—In No. 12 the yellow had been suggested and it appeared after the red had been suppressed. In No. 13 the black had been suggested and it appeared after a blank interval.)

## EXPERIMENT WITH PRESENTED GREEN.

On the closing of the eyes after they had been stimulated for 30 seconds by spectral green upon a white background the after-image was described as reddish-pink and matched by 2 violet-tint plus 4 red plus 14 rose-pink. The green visualized in the latter experiments was matched from memory by  $9\frac{1}{2}$  black plus  $10\frac{1}{2}$  spectral green, and the after-image from this was described as red and matched by 5 rose-pink plus 15 spectral red.

## DISCUSSIONS AND REPORTS.

### THE PSYCHOLOGY OF READING.<sup>1</sup>

The increasing prominence of studies in the psychology of language is not due merely to their great practical value, nor to the invaluable sidelight which they throw on the theory of logic; their greatest worth, after all, lies in their direct illumination of psychological principles. Language has long been regarded by the psychiatrists as a sort of psychical microcosm corresponding in the developed consciousness, stage for stage, from simple apprehension to reactive expression, with the larger system of mental phenomena of which it is only a fragment. The monograph of Herr Zeitler must be regarded, therefore, not only as a valuable contribution to the psychology of reading, but also as a test of the general psychological theory which inspired it.

The gravity tachistoscope, which was used by Herr Zeitler as the exposure apparatus in his experiments, was designed by Wundt to eliminate some of the evident faults of the older form of apparatus. The faulty binocular accommodation conditioned by the distance between the falling screen and the object is obviated by sacrificing binocular observation altogether. The object is observed monocularly through a magnifying telescope. This increases the influence of the physiological inequalities in the single retina, but it is doubtless the best way out of the difficulty, if one will still use the falling screen. The tendency to follow the fixation point at the beginning of the fall is eliminated by placing it on a secondary screen, which covers the object until just before the exposure, when it is thrown out of the way by the falling screen. This device, moreover, permits a greater velocity in the screen at the moment of exposure, with the same height of apparatus; the exposing slit is, therefore, wider than in the older form, while the period of total exposure is larger in proportion to the period of uncovering and covering the object.

The noise of the contact between the two screens is a considerable weakness of the apparatus, since it occurs almost simultaneously with

<sup>1</sup>Tachistoskopische Versuche über das Lesen. Julius Zeitler. *Philos. Studien*, Bd. 16, Heft 3, pp. 380-463.



the exposure. It even compelled the subjects to undergo a course of training until they became accustomed to it.

An Atwood machine attached to the falling screen permitted a considerable variation of exposure without changing the distance of fall or the width of the exposing slit.

The universal objection to all fall tachistoscopes, viz., that the exposure is a successive and never a simultaneous one, is reduced to a minimum in the new form of the instrument. It seems probable, however, that the slower movement of the edge of the falling screen across the object is responsible in part at least, for the practice needed by the observers to accommodate themselves to the 'long' exposures of .1" when it took 4σ to uncover the object. The mere increase in exposure does not necessitate so much practice. This is proven by the experiments of Professor Erdmann and the reviewer, as well as by the exposure occurring in every normal reading pause which averages more than .2".

The distinctive feature of Herr Zeitler's article is the attempt to demonstrate that the apparent simultaneous apprehension of a word as a whole is an illusion of 'assimilation'; that the real 'apperceptive' process consists of a successive apprehension of the word, part by part, by a movement of attention from left to right, in which part of the letters of a word are passed over rapidly while the attention fastens upon the dominating letters and letter complexes.

At the beginning the author forces his analysis into Wundtian concepts as he distinguishes sharply between 'apperceptive' and 'assimilative' reading. An act of apperception occurs when a dominating letter or dominating group of letters rouses and fuses with the dispositions of similar complexes previously perceived. A word is assimilated when on the basis of the apperception of its dominating letters a secondary group of dispositions is roused connecting the dominating letters into a whole. By dominating letters the author means such letters as are particularly characteristic and readily perceived. Although his differentiation of them is not as critical as one could wish, they are apparently those letters which project above or below the body of the word and the additional middle-sized letters *x* and *z*.

'Apperceptive' reading is an immediate process conditioned largely by the objective image, and must occur in the shortest possible time, while 'assimilative' reading is a mediate process and needs a longer period for its development.

The author, therefore, endeavors to shut out the assimilative process as far as possible by reducing the interval of exposure to the

smallest practicable value. Experimentation shows this to vary from .01"—.02" for different individuals. An exposure interval of .1" would condition 'pure assimilative reading.' In assuming that the apperception proceeds from dominating letters and letter complexes, while the assimilative reading depends largely on the total word form, the author begs the whole question at issue.

The experimental expedient of reducing the exposure to a minimum in order to isolate the 'apperceptive' process from the 'assimilative,' the reviewer regards as a failure: (1) Apperceptive reading is characterized by the predominance of the objective elements in consciousness as distinguished from the subjective elements or dispositions. The minimum exposure affects precisely those objective elements, making them as faint as possible, while it exaggerates the physiological inequalities of the retina already emphasized by monocular observation. (2) Theoretically, the process of assimilation can not be limited by the duration of the external stimulus whether it is .01" or .1". It is unquestionably longer than either. (3) Experimentally, the author's own results show that the process of assimilation is so wide when the exposure is only .01" as to make his theoretical claim untenable; since the cognition and recognition of letters are both processes of assimilation (Wundt, *Gundzüge*, II., 441-442), while the observer's ability to recall the acoustic-motor name of the visual symbols is the result of an even more remote process of assimilation. (4) Lastly, the apprehension of such words as 'Rücksichtslosigkeit' and 'Aufmerksamkeitsschwaukung,' from one exposure of .01" leaves his interpretation, as well as his experimental method, without adequate foundation.

Coördinate with the author's distinction between apperceptive and assimilative reading is the distinction between reading with fixed and reading with roving attention. In respect to this distinction the communications of the observers are most explicit and interesting, making one of the most valuable parts of the paper. The apparent simultaneity of the apprehension of words as emphasized in the researches of Cattell, Erdmann and others, the author regards as an illusion of assimilation, occasioned by the rapidity of the process and the training in the succession. The roving of the attention is not held to condition a succession of letters, but a succession in the clearness of the dominating letters from left to right in an appreciable temporal succession. This succession 'may occur with the shortest exposure.' It is only subjectively appreciable with an exposure of .1" for words of over 15 letters.

In his unfortunate critique of *Untersuchungen über das lesen auf*

experimenteller grundlage, Professor Wundt asserted that the apprehension of such long words as were read by the authors must be conditioned by a movement of the attention during the 'long' exposure of .1" plus its after-image. It was clearly a desideratum that the present monograph should demonstrate this assertion experimentally. This, however, was not found altogether easy. A movement of the attention was, indeed, discovered by all the observers with an exposure of .1" when reading words of more than fifteen letters, but 'the better the fixation the more the perception (sic!) of this movement of attention was inhibited.' At first the observers were therefore requested to let their attention rove. The report of Dr. M., probably the most accomplished subject, is characteristic and altogether worth translation. He found that "the attention moves slowly over the word image, which is quietly read. It clings longer to the dominating complexes and passes over the unemphasized parts more rapidly. The movement occurs from left to right, corresponding to the connection between the characters; it is not jerky, but is more or less rapid, corresponding with the objective image. It is a succession in that during the fluctuation of the attention from a letter complex at the left to one at the right a subjectively appreciable pause occurs. The longer the time at command the more this movement becomes an elemental one, *i. e.*, it moves from element to element."

Whatever else such reports may prove, they certainly demonstrate that the movement of attention described is not a movement which can occur within the barely perceptible time interval of .1". It becomes evident from the whole discussion that the objective succession and the wandering of the attention actually found in consciousness are in reality not a function of the visual apprehension, but of the motor-acoustic interpretation of the visual presentation. The author has brought forward no facts in his argument for successive visual apprehension of thoroughly familiar words that do not admit of a more natural interpretation through the action of the successive motor-acoustic word idea; while his experimental results demand such explanation.

That long or unfamiliar words like 'Balænoptera' and 'Rot-sämischleder' must be apprehended part by part, no one who has any knowledge of the psychology of reading will question; least of all the investigators criticised so freely by our author. To hold that the form of a word which corresponds to no residua of past experience could be the determining factor in its apprehension would be to hold a self-evident absurdity. That one may direct one's attention to the indi-



vidual letters of a word complex as the author and his subjects admit that they did, no one who has ever read proof can doubt, but this is surely not the same process as the normal reading of familiar words. But to establish the hypothesis of a roving attention in the apprehension of familiar word forms under really normal conditions of fixation during an exposure of .1" one must fly in the face of the overwhelming evidence of the author's own experiments or espouse the hypothesis of unconscious changes in attention, which is about as good psychology as the hypothesis of unconscious ideas.

The author errs in concluding from a group of experiments in which the attention is directed toward the letters and letter groups that the same process occurs when the attention, is directed towards the sense.

Moreover, a movement of attention over the field of vision in reaction to the stimulus of unrecognized parts can certainly not occur during the time when the total stimulus has not yet overcome the inertia of the retina, *i. e.*, within 100 of the beginning of the exposure; that it might occur, according to Wundt's analysis in a somewhat shorter interval than the well-known *Unterscheidungzeit* is possible, but certainly not in less than one-third that interval, while the sharp fixation and total absence of the effort to let the attention wander which characterized the experiments of Erdmann and Dodge, must delay it still longer.

While the author's interpretation of his work in the form of Wundtian concepts is far from satisfactory, the experimentation itself is a valuable piece of work. Moreover, the emphasis laid on determining letter complexes which occur within the general word form is surely not altogether misplaced. The optical difference, however, between a group of letters and the same letters as constituent parts of a well-known word form seems to be as little appreciated as the psychological difference between the successive apprehension of the individual letters as such and the apprehension of that visual complex of contrasts which makes a word.

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## A SCHEME OF CLASSIFICATION FOR PSYCHOLOGY.

The responsible editors of this REVIEW drew up for the purposes of the PSYCHOLOGICAL INDEX the classification of psychological material which, with certain modifications suggested by the compiler of the Index, has been used heretofore in that publication. The coöperation effected with the German and French reviews emphasized the



need of a common scheme, and Professor Warren represented the REVIEW in a conference held in Paris during the recent International Congress. The agreements reached, together with certain alterations made subsequently suggested are now embodied in the following scheme. It is thought that this scheme—having thus the authority of an international committee and embodying the opinions of experienced bibliographers of psychology—may have value for the wider use of public and private libraries, and we recommend it to all those who are interested in psychological classification. That it avoids all difficulties and inconsistencies we do not pretend to say; it embodies compromises and considerations of utility as well as those of logic.

In the division headings two alternative schemes are presented: the 'graded numerals' and the 'decimal system.' The INDEX continues to use the former; but others who wish to adopt the classification may prefer to use the decimal system.

J. MARK BALDWIN.

#### CLASSIFICATION.<sup>1</sup>

- 0. I. General:
  - 0.1 1. Text-books and Systematic Treatises.
  - 0.2-0.5 2. General Problems, Methods, Terms and Apparatus.
  - 0.6 3. History and Biography.
  - 0.7 4. Collections, Proceedings, Dictionaries, Bibliographies.
- 1. II. Anatomy and Physiology of the Nervous System:
  - 1.0 1. General.
  - 1.1 2. Nerve Elements.
  - 1.2 3. Brain and its Functions:
    - 1.21 a. Anatomy of the Brain.
    - 1.22 b. Physiology of the Brain.
  - 1.3 4. Spinal Cord, Nerves and Sympathetic System.
  - 1.4 5. Reflex and Automatic Functions.
  - 1.8 6. Pathological Anatomy.
- 2. III. Sensation:
  - 2.0 1. General.
  - 2.1 2. Sense Organs (General).
  - 2.2 3. Psycho-physics (Weber's Law, etc.).
  - 2.3 4. Psychometry. (See Time Relations, IV7.)
  - 2.4 5. Vision:
    - 2.40 a. General.
    - 2.41 b. Anatomy and General Physiology of the Eye.

<sup>1</sup> In the matter of English terminology the recommendations of Baldwin's *Dict. of Philos. and Psychol.*, now in the press of the Macmillans, are anticipated and followed by permission (with view also to the German, French, and Italian equivalents given in that work).

- 2.42 c. Physics and Special Physiology (Dioptrics, Refraction, Accommodation, Acuteness of Vision, Perimetry, etc.).
- 2.43 d. Visual Sensations.
- 2.44 e. Special Phenomena of Vision (After-images, Contrast, etc.).
- 2.46 f. Eye Movements and Binocular Vision.
- 2.48 g. Pathology.
- 2.5 6. Hearing :
  - 2.50 a. General.
  - 2.51 b. Anatomy of the Ear.
  - 2.52 c. Physics and Physiology.
  - 2.53 d. Auditory Sensations.
  - 2.58 e. Pathology.
- 2.7 7. Other Senses :
  - 2.71 a. Taste.
  - 2.72 b. Smell.
  - 2.73 c. Cutaneous, Pressure and Joint Senses.
  - 2.74 d. Muscle sense.
  - 2.75 e. Static Senses (Position, Equilibrium, and Dizziness).
  - 2.76 f. Organic, Pleasure and Pain Senses ; General Sensibility.
  - 2.77 g. Electric Sense.
- 2.8 8. General Pathology.
- 3. IV. General Characters of Consciousness :
  - 3.0 1. General.
  - 3.1 2. Attention and Apperception.
  - 3.2 3. Association.
  - 3.3 4. Dispositions.
  - 3.4 5. Habit, Accommodation, and Selection.
  - 3.5 6. Work and Fatigue.
  - 3.6 7. Time Relations ; Mental Chronometry.
- 4. V. Cognition :
  - 4.0 1. General.
  - 4.1 2. Perception and Idea.
  - 4.2 3. Perception of Time, Space, and Motion.
  - 4.3 4. Memory and Imagination. (See also Dreams, IX1.)
  - 4.4 5. Judgment and Belief ; Reasoning.
  - 4.5 6. Reflection and Self-consciousness.
  - 4.6 7. Normal Illusions and Suggestion.
  - 4.8 8. General Pathology.
- 5. VI. Affection (Feeling and Emotion) :
  - 5.0-5.2 1. General ; Pleasantness and Unpleasantness.
  - 5.3 2. Emotion and its Expression.
  - 5.8 3. General Pathology.
- 6. VII. Conation and Movement :
  - 6.0 1. General ; Dynamogenesis.
  - 6.1 2. Organs of Movement. (See Muscle Sense, III7d.)
  - 6.2-6.4 3. Instinct and Impulse (Imitation, Play, etc.).
  - 6.5 4. Special Motor Functions :
    - 6.51 a. Speech.
    - 6.52 b. Handwriting.

- 6.53 c. Walking.
- 6.57 d. Other Functions. (See also Vision, III5f.)
- 6.6 5. Volition and Effort.
- 6.7 6. Freedom of the Will.
- 6.8 7. General Pathology.
- 7. VIII. Higher Manifestations of Mind :
- 7.1 1. Logic and Science ; Methodology.
- 7.2 2. Theory of Knowledge.
- 7.3 3. Æsthetics.
- 7.4 4. Ethics.
- 7.5 5. Religion.
- 8. IX. Sleep, Trance, and Pathology :
- 8.1 1. Sleep and Dreams.
- 8.2 2. Hypnosis and Trance States.
- 8.3 3. Psychical Research.
- 8.4 4. General Works on Pathology.
- 8.5 5. Nervous Disease :
- 8.51 a. Developmental and Acquired Forms.
- 8.52 b. Neurasthenia and Paralysis.
- 8.53 c. Epilepsy and Hysteria.
- 8.57 d. Other Neuroses.
- 8.6 6. Mental Disease :
- 8.60 a. General (Insanity).
- 8.61 b. Idiocy, Imbecility, etc.
- 8.67 c. Other Special Psychoses.
- 8.7 7. Medical Jurisprudence.
- 9. X. Genetic, Individual, and Social Psychology :
- 9.1 1. Evolution and Heredity.
- 9.2 2. Comparative Psychology.
- 9.3 3. Mental Development :
- 9.30 a. General ; Adolescence and Senescence.
- 9.31 b. Child Psychology.
- 9.32 c. Pedagogy.
- 9.4 4. Individual, Sex, and Class Psychology.
- 9.5 5. Folk Psychology.
- 9.6 6. Social Psychology.
- 9.8 7. Pathology :
- 9.81 a. Criminology.
- 9.82 b. Degeneration.

## A DISCLAIMER.

The attention of the undersigned has been called to the fact that an organization known as 'The American College of Sciences,' situated in Philadelphia, is issuing circulars advertising a course of instruction in hypnotism as prepared in part by them. These circulars contain many statements about hypnotism and about the advantages to be derived from its study and practice which are not justified by the

articles written by the undersigned, which in their judgment cannot be substantiated by any facts known to science, and which they believe to be in the highest degree misleading. Furthermore, the undersigned are of the opinion that the practice of hypnotism by the general public is attended by dangers which have no compensating advantages, and would in no case countenance any scheme which encourages its practice under such conditions. They feel it incumbent upon them, therefore, to make public a statement of the circumstances under which these articles were written.

Each of them was requested, individually, by 'The New York State Publishing Company,' of Rochester, N. Y., to prepare an article for a collection of such articles. Inquiries made of this company elicited no suggestion that the collection was to be issued by any other than the usual method of publication and sale, and the articles were contributed by the undersigned without any knowledge or suspicion that they would be used as constituent parts of a course of instruction in hypnotism. Had they known that they would be so used, they would have refused to contribute the articles in question. They now disclaim all responsibility for the methods adopted by the American College of Sciences and for all statements made in its publications, excepting only those found in the several articles above referred to, and for them their individual authors are alone responsible.

While the position of the undersigned on these questions is perhaps already sufficiently well known to the academic world, they feel that this disclaimer is due to the general public.

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## PSYCHOLOGICAL LITERATURE.

### RECENT WORKS ON ETHICS.

*Elements of Ethics.* NOAH K. DAVIS. Silver, Burdett & Co., New York. Pp. iv + 288.

*Introduction to Ethics.* FRANK THILLY. New York, Scribners. 1900. Pp. xi + 339.

*Ethics and Religion, A Collection of Essays,* Edited by the Society of Ethical Propagandists. London, Swan, Sonnenschein & Co. 1900. Pp. ix + 324.

*The Ethical Problem.* PAUL CARUS. Second edition enlarged. Chicago, Open Court Publishing Co. 1899. Pp. xxiv + 344.

*Problems in Ethics or Grounds for a Code of Rules for Moral Conduct.* JOHN STEINFORT KEDNEY. New York and London, G. P. Putnam's Sons. 1900. Pp. xx + 252.

Dr. Davis takes the notion of 'a right' as his point of departure for the construction of an ethical system. "The problem before us is: Given the simple idea or notion of a right; to find all forms of obligation." "These rights are grounded in the very constitution of human nature," which is accordingly assumed as the 'basis' of ethics. (pp. 38, 41, 209). Combining these two ideas, we have thus a return to the eighteenth-century theory of Natural Rights as the foundation of moral philosophy. Into a discussion of the doctrine of Natural Rights (obsolete save as advocated by Mr. Spencer) we do not propose to enter; it should be noted, however, that the strong individualism of this doctrine is inconsistent with what the author calls the 'modified altruism' of his own ethical theory. Why it is called *modified* altruism is not quite obvious, since he holds that 'all righteous conduct is disinterested, is unselfish' (p. 151), and that moral obligation wholly 'excludes self as an end' (p. 148); 'self is never, can never be, a moral end.' The moral law, however, "does not call for \* \* \* the extinction of the natural and healthful desire for one's own welfare." Nor does it "prohibit anyone from acting in a way that shall benefit himself, but only from thus acting in order that he may benefit him-

self" (p. 165). Thus it appears that one may desire one's own welfare, but may not seek it. Personal welfare is not prohibited by moral law provided it be unsought, *i. e.*, comes as an accident. It should indeed be sufficiently obvious that the accidental has no moral quality. It is less obvious why that which is a legitimate object of moral desire may not be sought. Nor is it clear how a doctrine based upon the 'claims' which I have upon others and they upon me—which claims compose our 'rights'—can forbid me, in whom certain claims vest, to regard myself as in any sense an end. Nor is this position consistently held. For Dr. Davis, in speaking of the 'right to service,' *i. e.*, to the beneficent action of others towards self, says that "if I myself be used as a mere tool \* \* \* I am indignant" (p. 165), which surely implies that I do regard 'myself' as an end worthy of consideration, and is virtually a falling back into the Kantian doctrine which Dr. Davis repudiates. Thus the claim to the service of others is made to rest upon an egoism which has been cast out naked and disgraced. And indeed it is difficult to see how a theory which is based upon the rights of individuals can completely ignore the individual who unfortunately happens to be myself. According to the atomistic conception of society, it would be more logical to say with Bentham that everybody is 'to count for one,' myself included. Kant, moreover, does not say that I am to 'make myself in mine own person an end' (p. 166), but that I am to 'regard *humanity*, whether in mine own person or in that of another, as an end withal'; which is a different matter.

It may be noticed in this connection that Dr. Davis seems to fail of understanding Kant. In commenting on the passage in which Kant says that "an action done from duty must wholly exclude the influence of inclination," Dr. Davis says: "The implication is that love is not a duty; for this conception of obligation excludes all personal inclination, teaching that an action determined by love alone is not a moral action, and that one wherein love mingles is morally impure, being contaminated by inclination." "We heartily reject a scheme of ethics implying that a man is under no obligation to love his mother or his country, but should purify his character by eliminating all such inclinations; a scheme that clearly, distinctly enacts: *Thou shalt not love thy neighbor.*" (pp. 169-171). Poor old Immanuel Kant must feel that the right to have humanity regarded as an end in his own person is a right which does not extend to immortals; but instead of turning in his grave, we can imagine him only smiling sadly on learning that Christ's parable of the good Samaritan, though

less abstruse than the critical philosophy, also lends itself to misinterpretation. (See note, p. 175.)

Dr. Davis not only misreads Kant, but apparently falls into several of the errors of which he accuses the latter. "An action conforming to moral law is a virtuous action. This qualification implies a contrary inclination overcome by will," etc. When, however, the virtuous desires prevail more and more uniformly until "all struggle, all conflict has ceased, the victor, because of his victory, is dubbed a perfectly virtuous person" (p. 139). That is, in other words, a virtuous action implies contrary inclination, but a virtuous person is one who experiences no such opposition of duty and inclination.

Again, while condemning any theory of morals which looks to self as an end, the author says that the doctrine of the present treatise is that of the Stoics, that 'whatever is natural is right' (p. 151 note). But "it is undeniable that selfishness generally prevails and is dominant" (p. 153). If selfishness is dominant it must be 'natural,' according to the common implication of that word; and if natural, then right. Yet all right conduct must be unselfish. Moreover, if whatever is natural—*i. e.*, native or constitutional—in human nature is right, how can we exclude those impulses and emotions which terminate upon self as the end, and constitute an important part of that 'life' which is regarded as a primary natural right? It is obvious that some deeper hidden meaning must lurk in the word 'natural' as used by our author. Yet even with the qualifying definitions presently introduced, human nature and the reciprocity between human beings prove to be an inadequate basis of ethics. True to his principle that obligation is based upon natural rights, which imply a relation between two or more persons, Dr. Davis holds that the isolated man "has no responsibility, is not a moral being." "With him nothing is either right or wrong" (p. 217). Robinson Crusoe then is not under moral law. Yet on the next page but one it is said: "But should he reasonably despair of a return among men, still he may not neglect his personal dignity, or ever, even under the greatest suffering, take his own life. \* \* \* He is bound by indissoluble obligation to his maker," etc. (p. 219). Thus it appears that human nature and human reciprocity do not after all furnish of themselves a sufficient basis of individual ethics. Nor, even with the help of the conception of the 'unity of mankind,' does Dr. Davis transcend his individualistic standpoint. Thus a curious limit (due to the author's individualistic point of view) is set to the sphere of moral action of the State, which he regards as a 'personality,' with 'a conscience of its own' (pp. 259,



270). Why, it may be asked, if we are to think of mankind as an 'organic unity,' and of the State as an ethical personality, is not the State bound to the same law of 'loving service' as other personalities?

There is a way of transcending eighteenth-century individualism; not that of Professor Davis, but a way foreshadowed by Kant, in whose day the conception of society as an organism and of the spiritual unity of mankind was not as familiar as it is to Dr. Davis. When Kant says that we are to treat humanity as an end, and when further he refuses to find this end in any existing community of personal relations, but in an ideal to be realized by humanity collectively and separately, we have altruism laid upon us as a moral obligation because we 'are members one of another,' while the rights of personality, or the worth of the individual, follow from the fact that 'ye are all of one blood.' We possess individual rights, and both egoistic and altruistic duties are incumbent upon us, not in virtue of our claims as individuals, but in virtue of our all partaking of that common higher life which we all potentially possess and which it is our duty severally and collectively to make actual. It is the presence of this ideal—which Dr. Davis also recognizes—which is not derived from the 'constituted order of nature,' but is imposed upon it by human thought as the norm to which nature should conform, the recognition by man of "an opposition between his desire for the realization of the ideal self and his desire for the gratification for the lower self, an opposition between the life of spirit and the life of nature,"<sup>1</sup> that constitutes the idea of duty. Whoever would preserve an obligatory morality must start with the recognition of this opposition.

It is obvious, therefore, that Professor Davis must in some way modify the doctrine already referred to, to the effect that whatever is natural is right and that the basis of ethics is 'the natural constitution of man.' The words 'nature' and 'natural' are in fact ambiguous. 'Nature' usually means what *is*; but 'natural,' as has frequently been pointed out, is often equivalent to 'normal,' and the latter word implies the notion of a rule or standard by which things are judged and to which they *ought* to conform (vid. Ritchie, 'Natural Rights,' Ch. IV.). It is in this, the ethical sense, that our author uses the word 'natural,' for which he ordinarily, though not invariably, substitutes the word 'normal.' By the 'natural' man he means the 'normal or rehabilitated man'—*i. e.*, 'not man as he is, but man as he should be' (pp. 38, 39). Dr. Davis in fact, in spite of a somewhat ambiguous way of expressing himself, really means to inculcate a sound idealistic

<sup>1</sup> Professor Watson, 'Introduction to Philosophy.'



imperative morality. Rights have their ultimate ground in the desires which motive action; and the primary principle of ethics is that man has a right to gratify his normal desires (p. 45, 46). When it is elsewhere said that "the normal is pleasurable, the abnormal painful" (p. 154); and again, "Normal desires, or such as have an instinctive rise and are in accord with the general order of nature, impel toward the fulfillment of the appropriate functions of the man in a world of persons and things" (p. 46), it is not to be imagined that Dr. Davis intends to make either pleasure or instinct the criterion of right conduct. Since, however, we are not told anything about 'the general order of nature,' nor what are 'the appropriate functions of the man,' we may as well fall back upon the conception of normal.

"A man's malevolent desires," we are told, "are in general abnormal in kind, since they do not conform to the normal principles of the human constitution." This does not help us. Duty consists in the gratification of normal desires. Normality is the standard of duty, but what is the criterion of normality? Benevolence is the primary duty; is benevolence, then, obligatory because it is normal, or is that normal, and hence duty, which is benevolent? Apparently the latter; for the end of moral conduct is welfare, and benevolence is the condition of welfare. It may be imagined that here then we have the ethical standard of which we are in search; and if so, why not forsake 'nature' and abandon the various ambiguous formulæ employed and follow the notion of welfare as our guide? We are attempting, be it remembered, to find some explication of those 'normal desires' in the gratification of which duty is said to consist, and have been led to the conception of welfare. "We are, then," in words of the author, "in great need to know, clearly and distinctly, the meaning of welfare." When, therefore, welfare is defined as 'the gratification of normal desires,' and is said to consist 'in the constant gratification of right desires' (pp. 188, 189), the reader may begin to suspect that he is the victim of some trick. Welfare is defined with reference to normal desires, and the latter in turn by the notion of welfare. Only such gratification of normal desires, however, is allowable as does not interfere with the rights of others. Hence the primary law receives this important modification: Do not trespass. Since a trespass, however, is any violation of the rights of others to gratify their normal desires, this brings us no nearer the definition we seek.

Whether 'the natural constitution of man' or the notion of welfare is to be taken as the ultimate criterion of morality, is not made as clear as might be. We incline to the latter view however, since,

although all desires are natural in the sense that they pertain to human nature (selfishness in this sense would appear to be the most natural, since it is dominant), only some are normal. When it is said that the affections (by which the author means the altruistic or 'giving' desires) are naturally paramount, this must mean that they are normally supreme, and they are manifestly normally supreme in view of the fact that they chiefly further welfare.

Briefly stated, Dr. Davis's ethical theory comes to this: that one should seek with no thought of self to promote the welfare of others and thus involuntarily secure one's own happiness. If happiness be set up as the direct personal end of conduct, it will prove unattainable. "The only possible way to it is through its condition welfare. Hence wisdom disregards happiness as an end, not looking beyond welfare, but seeking this as the end of all endeavor. This attained, happiness results by a benign law of human nature." But surely happiness may be the supreme end, though it be necessary to seek it indirectly. Welfare, which Professor Davis elsewhere (p. 210) defines as consisting 'of liberty and continuous success in the exercise of benevolence and beneficence,' might be the best means to the attainment of the end, and still not the *summum bonum*; and the mere fact that happiness cannot wisely be directly sought would not of itself prove that it is not the ultimate (though it may not be the proximate) end. Dr. Davis, however, clearly intends to make welfare and not happiness the moral end. And although welfare, the 'principal element' of which is virtue (p. 187), is apparently made to include happiness as part of its content, no stress is laid upon this synthetic conception of the end, the happiness element of which is persistently held to constitute an unlawful aim for the individual. Dr. Davis, in short, seems to fall back upon the old 'be good and you will be happy' theory. For theoretical purposes however it is impossible to identify virtue and happiness, and it is necessary to distinguish them as separate ethical ends. It is of course possible, and we think it is correct, to hold that the moral ideal is not simple, but complex, and includes both virtue and happiness, the best expression for this synthetic end probably being self-realization. But Dr. Davis apparently holds that virtue (which is summed up in benevolence or loving-service, in which the individual's welfare is said to consist) is the end for the individual agent and happiness the end for everybody else. This again seems a curious conclusion for a theory professedly based upon human nature, in which assuredly the individual partakes. Or, stated still more individualistically, society, though metaphorically spoken of as an organism, is

nevertheless constituted of individuals, and what is good for the greatest number, or for the whole, must surely be good for the individuals which make up that whole.

Professor Thilly is a pupil and follower of Paulsen. Not only is his ethical position fundamentally the same as Paulsen's, but he has caught much of the master's spirit, and his clearness of exposition, lucidity of diction, and simple and natural arrangement of material, have united to make an excellent introduction to ethics.

Having said this much by way of appreciation, a word of criticism growing out of our inability to agree with the author as to the nature of ethics must not be deemed ungracious. Two of the chief problems of ethics—Conscience and the Highest Good, psychologically considered—the author discusses with sufficient fullness for the purpose of his book. He holds the Spencerian or naturalistic view of Conscience, and the Aristotelian doctrine of the Highest Good. But the question of Obligation, the central problem of modern ethics, is less satisfactorily handled. This defect arises from the author's conception of his subject; for he (wrongly as we think) regards ethics not as a normative but as a descriptive science. "It is the business of a scientific ethics to study the moralities that is, to investigate the rules of conduct which men *feel* as moral, and discover the principle which gave rise to them. If we find that there is such a principle and that men tacitly assent to it, we shall understand the genesis of morals. We shall be able to see where men have bungled in their blind attempts to apply the principle, and we shall be able to distinguish more intelligently between the right and the wrong. After we have found the ideal which is vaguely guiding the destinies of mankind, we of the present time can ask ourselves whether we are realizing it in our own conduct." Morality is means to an end, and its rules are justified when they further this end. But we cannot justify the end itself. Man desires this end *absolutely*; "but why he should desire what he desires is a mystery which we cannot solve" (pp. 139-141).

In other words, ethics, as Professor Thilly consistently holds, is a branch of psychology. This psychology of morals is in itself an interesting and important study, to which Professor Thilly has a perfect right to confine his attention if he choose, but surely it is untrue to say that here (in the determination of the end actually desired) "we have reached the bed-rock of our science, here we have a true categorical imperative which commands absolutely and unconditionally" (p. 141).



For 'desired' end may mean either simply the direction our impulses and desires are consciously or unconsciously taking, or it may mean 'desirable' end, *i. e.*, the end which is to be rationally desired. In the former case, it is obviously absurd to say that man *ought* to desire what he does and *must* desire because the forces of nature have made him desire it. In the latter case this rationally desirable end can only be determined by our conception of the nature of man and his relation to the universe at large, *i. e.*, metaphysically. Suppose, however, this desirable or ideal end to have been determined; it is also commonly conceived of as something which men '*feel that they ought*' to realize. Our ideals change their content, but the changing ideal is accompanied by an ever-present consciousness of obligation to realize the existent ideal. In this sense obligation seems to us the ultimate conception in ethics. At any rate, we must hold on to obligation if we would conserve morality; at least until we reach that stage of 'absolute' morality of which Mr. Spencer speaks. We must also seek to justify the moral imperative, if we would be true to ourselves as rational beings. There is no need to deny the difficulty of this undertaking; nevertheless it would seem to be incumbent upon those who define ethics, even 'roughly,' as 'the science of duty,' as Professor Thilly does, to devote especial attention to this difficult problem.

Now it is obviously one thing to say that men desire pleasure, or the normal development of their powers, or that the end they seek to realize is perfection, and quite another thing to say that they ought to desire and seek any of these. It is one thing to say, if you desire a certain end you ought to observe certain rules of conduct; and another thing to say, you ought to desire and to strive to realize a certain end. It makes all the difference where you put the ought; how you formulate the ethical problem. Professor Thilly's imperative is purely hypothetical, as he elsewhere shows. As he conceives of the science of ethics, his treatment is fairly adequate. But if ethics deals not simply with the Is but with the Ought to Be, as we hold, then his theory is notably deficient. He does not indeed ignore the problem of obligation. He accounts psychologically for the Categorical Imperative by giving a genetic account of 'the feeling of obligation within us.' But even if we should grant the truth of this natural history of the feeling of Oughtness, it may well be asked whether this is sufficient. It is not true that "when we have explained this feeling [*i. e.*, shown how it has developed in us] we have explained the Categorical Imperative" (p. 133). "The mere observance of facts," as Mr. Thilly remarks, "will lead to nothing." Facts are not reasons, history is not philos-



ophy. The natural history of a process does not of itself furnish an explanation of the meaning of the process, either in the moral or the material world. If the moral law has a meaning as well as a history then we *must* justify the end which we say is obligatory, and we must further inquire into the ultimate meaning of obligation. An objective grounding of the consciousness of moral obligation may or may not be possible, but in our view this inquiry falls within the scope of ethics. This of course is one of those *ultimate* problems which Professor Thilly modestly hands over to the philosopher to solve (p. 20), and it may be thought that one should not take exception to this division of labor; but aside from the fact that in our view some of these ultimate problems constitute just the most important part of ethics, it may be added that in our experience the class of readers for whom an elementary text-book is intended are apt to be quite as much interested in the question what the moral law means and why man should obey it, as in the question how this law originated and developed in human consciousness. There is no doubt a plausible excuse for thus avoiding ultimate problems in an elementary text-book, owing to the recognized difficulty of treating philosophical questions in an elementary and popular manner; but the difficulty is inherent in the nature of the subject and cannot legitimately be evaded.

The history of ethics seems to teach that our choice must lie between the naturalistic ethics of evolution and some modified form of the Kantian position; but Professor Thilly does not seem to us to have gone far enough in working out the compromise he claims (p. 171). It is of course possible, in Spencer's words, quoted with approval by Professor Thilly, "to agree with moralists of the intuitive school respecting the existence of a moral sense, while differing with them respecting its origin." But this is not the important point at issue. The fundamentally important issue is: given the sense of obligation, what is its real significance, and how does the question of its origin affect this? In other words, the question of origin is not identical with the problem of validity. The evolutionary account of the origin of the feeling of obligation may or may not be true to fact; and if true, it may or may not destroy the authority of conscience. But the latter question is precisely the one which the theories of evolutionist moralists have made it most incumbent upon themselves and us to answer, and to which they have made but scant contribution. Professor Thilly appreciates the problem, and in a few words (p. 112) indicates what appears to us so true a view of it that it seems a pity he had not made it his own and worked it out more fully. But the

authority attributed to the moral sentiments according to the view hinted at on page 112 is very different from Spencer's view that "they have a coördinate authority with the inductions of utility" (p. 71). The 'inductions of utility' command conditionally, but Kant's imperative is categorical. Professor Thilly indeed says: "It cannot be proved that one *ought* to strive after some highest good; this is matter of feeling." If this be taken to mean that here is a Categorical Imperative which is an ultimate fact of human nature and beyond this we cannot go, we have a falling back into a type of intuitionism, which, while it conserves Oughtness, leaves it 'in the air.' On the other hand, the naturalistic (which is not identical with the genetic) account of the origin of the feeling of Oughtness, is in danger of robbing the latter of its sanctity and authority by reducing it to the level of a useful instinct; and for an ethic based upon this principle there can be no other logical 'end' than the utilitarian one of preservation and pleasure.

If it be denied that these are the supreme ends which all men do actually desire, it is still difficult to see how the psychological determination of what man desires (be it any end whatsoever), or the biologico-psychological history of how he came to desire what he desires, can ever issue in the ethical proposition that he *ought to desire* a certain end (no matter how defined). The highest good is the ideal, and Professor Thilly identifies 'this ideal' with the categorical imperative (p. 145); that is, it is an obligatory ideal. But Professor Thilly elsewhere says (p. 252): "Different persons may have different ideals (meaning by ideals the direction which their impulses are taking, whether they are conscious of it or not)." Could animals, then, be said to have ideals? And if so, what is the difference between the instinctive actions of animals and the moral conduct of human beings? An ideal is surely an *idea* to be realized. But according to the above definition of an ideal, which is farther identified with the categorical imperative, animals would also be under a categorical imperative. If the ideal, the highest good, is what man actually desires and wills, and has no other meaning, it is absurd to say that 'he ought to strive after' it. But if the sense of obligation is an ultimate fact, as Professor Thilly would seem to hold in the sentence quoted, then the ideal cannot be identified with the direction which our conscious or unconscious impulses happen to be taking, for obligation implies the antithesis of the actual and the ideal. There seems to be a lack of clearness in Professor Thilly's thought as to the relation between obligation and man's actual impulses and desires, which prevents us from recognizing the reconciliation of Kant and Spencer as complete.

The fundamental point, however, is not as to the 'derived' or 'underived' character of the sense of moral obligation, but as to its significance; for the obligatory character of the moral ideal depends not upon the time-relation of its origin, but upon its *meaning*.

We reach the crux of the matter, therefore, in Professor Thilly's contention that intuitionism and teleology are not necessarily antagonistic (p. 151 seq.); and this is true. But intuitionism commonly seeks some more ultimate basis of the moral law than the feeling of obligation itself, and teleological morality must do the same. In order to this, teleology must be applied to the universe and not simply to rules of conduct. That is, teleological morality must mean not merely that rules of conduct are good or bad simply as they further or hinder the end actually desired by man, this desired end itself being the product of a natural or non-moral process, but that the universe is proposive, and that the moral law is the subjective expression of a moral world-order, and the Highest Good assumes the form of a Categorical Imperative because grounded in the nature of the Absolute Good. If the moral ideal be regarded as 'prophetic' in character, *i. e.*, as indicating the real inner meaning of the whole evolutionary process and as pointing towards its final goal, then it is ultimately based upon the true inward nature of things, of which it is at present the most perfect manifestation. We may then recognize a certain opposition between the 'cosmical' and the ethical process, while still maintaining that they are both '*part and parcel*' of the general process of evolution,' the real significance of which as a whole is to be interpreted not in terms of its earlier but of its later stages. From this point of view the whole world-process is at bottom a moral order, of which the physical and the ethical are simply higher and lower phases, less and more perfect manifestations. And if this be so, if the natural order is in the last analysis a moral order, if man at his highest and best is made in the image of God, and his Ideal is also the Real, if his 'true nature' is essentially identical with the essence of the All-Real, which cannot be otherwise conceived than as altogether Good, then the ultimate basis and authority of the moral law is not simply in his own nature, still less in external nature, but in the heart of the universe, in the divine Thinker, whose thought is progressively revealed in the whole sphere of phenomenal existence. The physical universe is an unconscious manifestation of this revelation. It is a mirror which reflects what is thrown upon it—it is passive, is acted upon, but not active, is bound fast in the causal chain of natural law, has power of itself neither to further nor to hinder moral ends of which it is not even conscious. Man, however, is both a part



of nature and superior to it. He sees the vision of the Good and may consciously follow it. The vision is a subjective experience, has no prototype in the external world; yet it is no vision merely, but the shadow of the Real. It is this vision, this ideal, which is not born of man's sentient being, but springs from his spiritual, rational nature, and is the image of the heavenly, that becomes a categorical imperative, the law of his life. Thus man is both autonomous and at the same time under law; autonomous in that his self-legislation is in accordance with the demands of his own moral nature; under law in that he reflects or reënacts the absolute law of God.

If it be objected that this is to found ethics upon metaphysics, the fact must be admitted. But we do not first start with a philosophy of the universe, and deduce our theory of morals therefrom. We start with the facts of the moral life. If we can reach no final interpretation of these without the aid of metaphysics, that is a misfortune which has often overtaken naturalistic moralists unawares. For it is not a question of having or not having any metaphysics, but it is a question of *what* theory of the universe one prefers.

We therefore agree with Dr. Paul Carus ('The Ethical Problem') in criticising the societies for ethical culture because they do not furnish any 'basis of ethics.' The book entitled 'Ethics and Religion' consists of twelve essays, the majority of them written ten years ago, by men who have been founders or influential friends of ethical societies. These essays are all as readable as one would expect from the distinguished names of their authors—J. R. Seeley, Felix Adler, W. M. Salter, Henry Sidgwick, G. von Gizycki, Bernard Bosanquet, Leslie Stephen, Stanton Coit, J. H. Muirhead. As stated in the preface: "They are unanimously insistent upon one point," viz., "that an ethical society should hold itself uncommitted to any theory of the universe, and should not be primarily interested in the metaphysic of ethics." "Coöperation for moral ends is the aim of the societies." "An ethical society is an institution not for the advancement of ethical theory only, but also, and preëminently, for the improvement of ethical practice" (Felix Adler, pp. 31, 38). "The aim of the ethical movement is moral regeneration" (*Ibid.*, p. 58).

With this aim everybody must surely be in profoundest sympathy; and for the greater efficiency of a society which claims this high function it is perhaps wise that the bond of its members should be not a common doctrine, but a common practice, a common enthusiasm for moral improvement (pp. 48, 49). Certainly it would be a bad outlook



for the material betterment and moral elevation of mankind if all those interested in any philanthropic enterprise or moralizing endeavor had to be first agreed in theoretical principles. But this is only one side of the shield; for it is commonly understood and often proclaimed by the leaders of the ethical movement that the *raison d'être* of the ethical societies is not simply the fact that the churches alone do not and cannot do all that ought to be done toward elevating the masses, but also the fact that the old supernatural sanctions of morality have broken down, and it is therefore incumbent to seek a new basis for morality. As one would expect, the attitude of the ethical societies as a body toward the churches is not defined, but varies with the opinions of the individual leaders and members. With some it is an attitude of frank hostility, while others, like J. R. Seeley, feel that there should be 'a friendly and hearty alliance' (p. 29). Which view is taken doubtless depends chiefly upon whether the theoretical or the practical aspect of their work is emphasized. With the ethical movement as an organized endeavor to promote the most important of all practical aims, and with its relation as such to the Church we are not now concerned. As preacher and exemplar of righteousness there should surely be room for both church and ethical society. *Moral predigen* is as important as, according to Schopenhauer, it is easy; but *Moral begründen ist schwer*. Still, the difficulty is one which the ethical societies should not shirk. If their *raison d'être* is the avowed failure of the churches, not only in the sphere of practice but even more in that of theory, if the old theological basis of morality has been condemned and may be expected soon to crumble into dust, must not a new foundation be laid for the ethical sanctuary? This is Dr. Carus's contention in the book already mentioned.

'The Ethical Problem' now before us in the form of a second edition is a reprint of Dr. Carus's three lectures on 'Ethics as a Science,' together with criticisms thereon, and the author's answers to these criticisms. To secure to the modern world the boon of moral unity despite intellectual diversity is a noble ambition, and to this end, to build upon the content of the 'common conscience' ('Ethics and Religion,' p. 43) in preaching righteousness of life, is to stand upon a sound enough practical platform, since it is quite true that men are better agreed as to the fact of moral obligation and as to what is right than they are to *why* they should do the right. But the fact that there is such wide diversity of opinion in regard to this latter question suggests that it is not as meaningless as Mr. Salter thinks it, nor as absurd as Dugald Stewart long ago asserted.

The discussion between Mr. Salter and Dr. Carus, which centers about this point, forms the best part of a book which contains some very inferior essays. Mr. Salter contends for the authority of conscience, the categorical character of obligation; but he has no 'basis' for this except the intuition itself of duty. By 'basis of ethics' Mr. Salter apparently means the 'motive' to do the right; and there can be, he very truly says, no other properly moral motive than reverence for the right and the desire to do it. Dr. Carus objects that this leaves the moral law 'in the air.' But the fact is that the statement of this question, Why should I do right?, which is surely a natural and rational question, seems to put us in an unpleasant dilemma. For we can either give no answer to the question, the consciousness of obligation is a part of our nature (whether it be 'original' or 'derived') carrying its own credentials, and beyond that we cannot go in search of any more ultimate moral motive; or, if we attempt to give any further reason for right conduct, this can only appear in the form of some appeal to self-interest, and this would be a non-moral motive which is at variance with the very idea of morality and would seem to undermine it altogether. Nevertheless the fact remains that there is no other alternative than this: either virtue for virtue's sake, doing the right out of reverence for the right; or virtue for the sake of something else, some reward, say, *e. g.*, happiness. Either view may receive a theological setting. But in the latter case the theological setting does not help us; it simply substitutes supernatural sanctions for natural. Nor do we mend matters by making universal happiness the end. Utilitarianism gives us a criterion of morality, and it points to a desirable, though not the highest end. But when we ask, Why, should I further this end of universal happiness? the answer is either, Because in so doing I best contribute to my own happiness, or, Because I ought, I feel it to be right to do so. But then, why should I do right? The former answer, which is that of early utilitarianism, is manifestly a falling back into egoistic Hedonism. The later and nobler form of utilitarianism says in substance: The highest rational end for every man is his own happiness; but what is good for me is good for others also; reason dictates, therefore, that what I would choose for myself I should promote for others likewise. To promote universal happiness is right because it is rational. One can seek no farther 'reason' for right conduct than its reasonableness. The motive of right conduct is its self-evidencing rationality. To ask why I should do what is rational is absurd. If then we ask, Why should I do right, meaning why promote the greatest happiness on the whole at

personal sacrifice, no final *rationale* can be given. We have here a 'dualism of the practical reason.' It is rational both to seek and to sacrifice my own happiness. Why then should I sacrifice it? Simply because I feel this to be right. I must do the right out of reverence for the right. We are thus thrown back on the moral motive as the ultimate *ποῦ στῶ*. Theological Hedonism, moreover, in either the individualistic or the universalistic form, seems to leave us worse off than we were before; since, though it provides powerful motives in the form of supernatural sanctions, it furnishes no criterion of morality. For if it be said: Be virtuous, do your duty here, that you may be happy hereafter, we have then to ask, What is my duty? And unless it be held that we know intuitively what duty demands, we must define duty or moral conduct with reference to some end which it is incumbent upon us to follow now as the condition of future happiness. But granting that we have established the mundane end which furnishes the criterion of morality, the content of duty, fulfillment of which brings happiness, we are just where non-theological hedonism left us. If I do the right now that I may be happy hereafter, this is to act from a prudential motive. If I do the right in order to increase the present or posthumous felicity of others, this is because I feel it to be right to promote their happiness. Duty for duty's sake is the ultimate moral motive. The only alternative to this is that the motive to right conduct consists in the sanctions which attach to it. Dr. Carus denies this dilemma, but he does not refute it, and his own answer is an appeal to the sanction of natural law. Sanctions, indeed, natural or supernatural, universally attach to moral conduct (if nature is a moral order this must be so), but they do not necessarily constitute its motive, and the higher the type of morality the less stress it lays upon sanctions of any sort. But if it is a low view to base morality upon the supernatural sanction of a good and just Moral Being, surely it must be a still more unworthy view to base it upon the natural sanctions of a universe which, as Dr. Carus holds, is neither good nor just, but entirely non-moral. Prudence may dictate obedience to natural law, but such action is only expedient, not moral. To obey natural laws because we thus escape painful consequences, is Hedonism, to which Dr. Carus objects. The theistic morality to which he likewise objects may also, it is true, enforce obedience to the moral law by the consequences attending our attitude toward it. But the law of obedience for theological moralists is not always, nor even generally, founded in the *will* of God, which decrees rewards and punishments, but in the Divine *Nature*. Many



who may be called theological moralists make no appeal to sanctions, and certainly do not hold that God's will is the source of right. Professor Thilly, therefore, speaks inaccurately when he applies the name *theological school* exclusively to those who hold that certain things are right or wrong simply 'because God has arbitrarily decreed them to be so' (p. 117).<sup>1</sup> Von Gizycki knew better, and though he heartily repudiated theological ethics, we commend his remarks on this subject ('Ethics and Religion,' pp. 162-164) to Dr. Carus, who regards the familiar distinction urged by Mr. Salter between 'basing the right on the will of God' and regarding 'God's will as identical with what is right' as irrelevant ('The Ethical Problem,' pp. 143, 144).

But by 'basis of ethics' Dr. Carus apparently means something more than the motive or the sanction of right conduct; though just what he does mean is not quite clear. We take it, however, that he means that the moral law must have something more than a merely subjective existence; it must be the subjective representation of an objective reality. We are to look for this objective reality in nature, and Dr. Carus scorns the idea that the source or basis of the moral law may be found in any supernatural or transcendental region. "An ethical man is he whose aspiration is to live in perfect harmony with the moral law" (p. 37); but "the moral law is simply a formulation of the lessons taught us by experience" (p. 98); "in case you want fire, produce it by friction"; "if you wish to live, obey reason." "All we can say about the ethical ought is to state the facts as they are: the man who does not care about being a useful member of society, or who does not care about his physical, mental and moral health, who does not care for going to the wall, and whose actions are expressions of this indifference, he will do harm to his fellow beings, and he will be doomed to perdition. \* \* \* These are the facts, and the moral ought is a statement of such and kindred facts for pastoral purposes, or as a help for self-education." There is no obligation beyond these facts (p. 148). "Morality is to be based upon the authority of natural laws" (p. 113). "The ought is a comprehension of the must"; it is conformity to the *is to be* (p. 154). "Morality is nothing but the intentional conformity to nature and to the order of nature." But since "nature is non-moral" (p. 312), it is difficult to see how we are here to discover the wished-for basis. Dr. Carus indeed is difficult to make out. Now he speaks of nature as non-moral (p. 312), and again of

<sup>1</sup> One other point of detail may be noted. Mr. Thilly's classification of ethical theories and writers is in the main good, but it is surely a mistake to define 'energism' in such a way as to make it necessary to place Hobbes among antihedonists (cf. pp. 127, 190).



'the moral law in the world' and 'the moral order of the world' (pp. 131, 132). Nor can he mean to distinguish between 'nature' and 'the moral order of the world' and to make the latter transcend the former; for he criticises Mr. Salter on the ground that there is a 'dualism' lurking in his ethics, "as if the moral order were something radically different from the order of this world" (p. 126).

But if nature is so moral that it furnishes the standard of morality, why are we told that the only ideal worthy of man's attention is "that which aims at creating a better state of things," and that "morality is based upon, it is creating a better state of things by conforming to the order of this very same world in which we live"? (pp. 176, 306). How can we 'create a better state of things' by conforming to the present state of things?

We are forced to the conclusion that Dr. Carus does not hold to a moral order of the world which furnishes the ethical standard. In logical accord with his monistic philosophy, he uses the words God and nature indifferently. Man can be said to be moral only if he conforms to the will of God; but God is non-moral. He is only 'an inflexible law, immutable, irrefragible, eternal' (pp. 192, 312). In what sense can we say that morality is based upon the existence of such an impersonal non-moral law?

We repeat that Dr. Carus is not easy to follow. Morality is conformity to nature, yet "that which we should do must be regarded as the highest we can think of" (p. 145). Does this mean that we *ought* to realize our moral ideal? If so, then we have that absolute obligation which Dr. Carus repudiates, and an end to realize which assuredly external or physical nature does not reveal; while if this end be sought not in the cosmic process without man, but in human nature, then we have a subjective basis of morality and not that objective basis which Dr. Carus seeks. Or does the sentence last quoted mean that 'the highest we can think of' is conformity to nature's must; do this or take the consequences? In this case, we not only have no moral obligation; but furthermore we may well ask whether the imitation of a non-moral nature, or conformity to its laws, is 'the highest we can think of.' An ethical ought founded upon facts or laws which confessedly have no moral quality is assuredly meaningless and without basis.

Again: "An ethical teacher ought to appeal to the highest motives man is capable of" (p. 61). Is the highest motive, then, that man should seek to escape 'the doom of perdition' which waits upon the breaking of nature's laws? Apparently not; for Dr. Carus at times

risers above his prudential morality, as when he says: "An ethical action is performed from a broader motive than self-interest, from the desire to be somehow of service to the development of humanity." But in what does this differ from Mr. Salter's 'moral motive' for which no farther reason can be given? As Dr. Carus here unconsciously falls back upon Mr. Salter's moral motive, so also by a virtuous inconsistency he seems to transcend the ethics based upon the facts of nature. We may then agree with Mr. Salter in saying that the moral motive is the only proper motive for morality, and at the same time go with Dr. Carus in search of a basis of ethics, an objective grounding of the moral ideal. It is here that theology, or metaphysics, legitimately comes in. Dr. Carus refuses to identify the useful and the good, he desires 'the health and nobility of our soul-life,' his ethics looks toward the perfection of character. But where do we get our idea, our criterion of this ethical end? Surely not from nature as a whole, but from nature in what all are agreed in calling its highest aspect, from man, from within ourselves. Aside from the fact that evolutionary moralists are by no means agreed as to the end to which nature points, it may be confidently asserted that the ideal for man's life cannot be derived from anything lower in the scale of being than himself. The fundamental postulate of evolution is that what is last in order of time is first in importance, is that for which what went before existed. Evolution is an unfolding of the less into the more perfect form. If then man cannot derive his ideal from nature nor seek his ethical standard in the cosmic process, but must find it in himself, he cannot find a basis of ethics in the laws of external nature. The ideal, Dr. Carus says, 'is rooted and must be rooted in the real,' and by 'the real' he means the 'nature' of science. But the ideal cannot be rooted in infra-human nature, since man as a part of nature is the highest part. The ideal must be rooted in something which corresponds with its own character or quality. If there is no 'real' to which the ideal corresponds, then the latter is a subjective phantasm without objective basis, a 'mirage' as Dr. Carus calls it. But if it is not a 'mirage,' then the ideal is the shadow of the real. The latter is greater, higher, more perfect than the ideal. † Our ideals change and grow in fullness of form and content. With them grow our conceptions of the real. As we endeavor to realize our progressive ideals, we approach more and more to the similitude of the real. The form of the moral law, therefore, is obedience to the ideal within us; its justification and basis are in the fact that in obeying it we are not only realizing the end of our own nature, but are at the same time realizing

a *whole* by identifying our life with that in which we live and move and have our being. This whole, however, must be not less but greater and more perfect than the self we seek to realize. It must transcend external nature, it must transcend human nature; they are both manifestations of its inner reality, but together do not exhaust its content. The individual finds his fullness in the universal, the Many find their life in the One of whose fullness they have all received. To separate the Many from the One, the Infinite, would be to limit, and thus to deny the infinity of the One. While, on the other hand, completely to identify them, to make the content of the One consist of the sum of the Many, and completely to submerge finite beings in the Infinite, would be to deny that element of spontaneous activity which constitutes moral freedom and is of the essence of morality. The relation of the One to the Many carries us to the heights (or the depths) of metaphysical thought; but we hold that we may recognize, and for an idealistic obligatory ethic we must recognize, a pluralism of personality and at the same time a unity of spiritual nature between the One and the Many.

Dr. Carus would doubtless stigmatize so much of transcendentalism as this as supernaturalism or dualism. But there is, apparently at least, a moral dualism between nature and man which is not of our making. As regards ethics, there seems indeed to be but three possible positions—either (1) Nature, meaning by nature what Huxley calls the cosmic process, may be taken to furnish the standard or ideal of morality—which we cannot admit; or (2) There exists between nature, in this restricted sense, and man, an ethical dualism, so that man does not find the moral ideal in the *actual* course of nature—in which case the ideal is a purely subjective affair; or (3) Man is superior to nature, its course is not his moral law, the latter is self-enunciated and therefore subjective; but if his higher nature which rises superior to cosmical nature allies him to the real spiritual nature of the universe, of which both cosmical nature and his own human nature are less and more perfect expressions, then we seem to have transcended the subjective ethical standpoint and found the objective basis of ethics of which Dr. Carus is in search.

We have avoided the use of theological language; but the necessity which the 'religion of science' seems to be under to employ the formulas of religious thought, may not improperly be regarded as a notable tribute to the rational hold which religion has upon the human mind, and should make us hesitate the less to employ language which is after all best suited to convey our ideas clearly and concisely. In



seeking for an objective basis of ethics, then, let us say: Be a person, seek to realize the perfect fullness of your moral personality; do this from no unworthy motives, but from sheer love of goodness and right. You will find the objective basis of this, however, in the perfect moral nature of God, the All-Real. God did not create Goodness or Righteousness by an act of will, but he realizes them in His nature. They exist subjectively in us as ideals, and obligation consists in the duty of seeking to realize them. The moral law, therefore, is summed up in the injunction: Be ye therefore perfect, as God is perfect. The basis of ethics does not consist in the naturalistic or the supernaturalistic sanctions of the moral law, but in the objective existence of goodness and righteousness in the Divine Nature, conformity to which is the absolute obligation and the supreme end of human life. Morality thus issues in religion—but not the religion of science. The ‘religious motive’ of morality, however, does not consist in the fear, but in the love of God, *i. e.*, love of the good and the right. God is the personification of these; it is for this reason that we love and seek to imitate Him. But why personify these concepts? it may be asked. Because goodness, moral perfection, are attributes of personality, of character; they imply a thinker in whose consciousness they exist and whose moral character consists in their realization. Morality can not be predicated of the unconscious. “Neither moral relations nor the moral law can swing *in vacuo*. Their only habitat can be a mind which feels them.” In calling God a person we do not mean to limit His nature to our conception of finite personality. He may, nay must, be above, but cannot be beneath our conception of personality. By calling Him ‘personal,’ however, we mean in this connection to attribute to Him those attributes of thought and feeling which are the necessary implicates of moral being. If moral relations and the moral law exist only *in us*, they are subjective merely; but if they exist in the Divine Nature, they have objective and eternal reality. If such a divine thinker exist, ‘then actualized already in his thought must be that ethical philosophy which we seek as the pattern which our own must evermore approach’ (William James, in ‘The Will to Believe’). Here is our ‘basis of ethics.’

Objection may be made that the existence of such a Supreme Moral Being can not be proven, and that it is a mistake to base morality upon such a peradventure; or it may be contended that morality needs no such foundation; but nevertheless such an hypothesis seems necessary to satisfy Dr. Carus’s demand for an objective basis of ethics. Finally, it is often urged that the geocentric concep-



tion of the universe having become obsolete, we can no longer cling to the idea of man's supreme place in the cosmos. But to base ethics on the moral nature of man only, is this not as anthropocentric a view as that of any schoolman? And is not the true universal point of view to make man's ideal also the universally real?<sup>1</sup>

Mr. Kedney's theory of ethics differs radically from that of Dr. Carus. He agrees with the latter in finding both utilitarianism and intuitionism defective. They agree farther in that they both hold ethics to be impossible unless based upon a 'world-conception.' But here their agreement ceases; for if "the one question whose true answer makes a moral philosophy possible is, What is the *idea* of the universe?" ('Problems in Ethics,' p. 1) their ethics will differ as their respective 'ideas of the universe,' and of course the former must be true or false according as their answers to this question are true or false. Mr. Kedney is not only sure of the truth of his own view, but apparently takes a very optimistic attitude toward the possibility of general agreement. "If human thinking," he says, "is to be guided and corrected, and its results harmonized, the first and indispensable thing to be done is to make men, or those men who guide the others, agree as to their philosophy of the universe." One may be pardoned for thinking it a bad outlook if such guidance and correction are to begin only then, when it would surely no longer be necessary. Mr. Kedney is presumably a theologian, and he takes as his ethical '*primum*' or starting-point, belief in a personal Author of the universe, whose existence is taken to have been established in a former work entitled 'Christian Doctrine Harmonized.' Thus the present work in reality constitutes only a portion of a larger system of thought, and moreover is only a partial execution of the author's original intention to make this treatise on moral philosophy 'his main literary work' (Preface, p. iv). The result is naturally a somewhat fragmentary product, and this may in a measure excuse the fragmentary character of the slight criticism we intend to offer.

<sup>1</sup>If we hold that the possibility of knowledge is conditioned upon the existence of an eternal self-consciousness, we are led by another path—the epistemological—to the same goal. One who lays stress, as Dr. Carus does, upon the necessity of a philosophy of the universe as a basis for ethics, could not object to this cumulative method of proof. Mr. Salter holds that "in truth there is nothing on which to base morality" ('Ethical Religion,' p. 37). Dr. Carus's basis is inadequate. The view briefly sketched above seems to me to indicate the direction of the only lines along which the 'ethical problem' can be solved. Mr. Salter quotes with approval the saying of Channing that "to love God is to love morality in its most perfect form" (*Ibid.*, p. 22). May it not with equal truth be said that to love the moral ideal, as above interpreted, is to love God?

Mr. Kedney both omits and includes topics not ordinarily omitted or treated in ethical works. Why, *e. g.*, should he devote a chapter to the 'question of the existence of evil spirits'? This is a very proper question for the Christian theologian to deal with, since Scripture has something to say on the subject. But how does it concern the philosophical moralist? Since if evil spirits do not exist they may be left out of account; and if they do exist and exert an 'alien influence,' their *modus operandi* is so obscure that the less we reckon with it the better; while if they *control* the will of human beings this would seem to impair moral responsibility. We agree with Mr. Kedney that it is probably best for us that we are left (where his chapter leaves us) in ignorance as to their existence and influence.

Mr. Kedney adheres to the triplicate Kantian postulates of morality. God is treated of in the former work already mentioned. "The argument for human freedom" (Ch. X.) we do not profess to understand. The author's position is indeterministic. In support of the ethical implications of immortality much may be said and the arguments for it may be stated with some force in various ways. Thus it may be argued that the human consciousness demands the final identification of virtue and happiness, and since this is manifestly impossible in the present life, we necessarily look for it in a life beyond the grave. Or it may be urged that the moral life is the progressive endeavor to realize our moral ideal of perfection of character, and that since this realization is manifestly impossible in the present life, our efforts would be in vain, a following of a will-o'-the-wisp, an unsatisfactory and incomplete episode, a tragedy of ambition doomed to sudden disappointment, if there were not an eternal opportunity given to realize an infinite demand.

Nevertheless, it is surely too much to say that "there is no morality unless there be immortality," that "morality, otherwise, is mere expediency" (p. 104). "If this world's experiences and possibilities only are to be considered, morality is a mere question of expediency, which each individual must determine for himself. There is no higher authority than the individual will, no truer idea than the idiosyncratic subjective one; therefore no right but might—successful will" (p. 105). But surely the 'higher authority' of the Divine will, in which the author profoundly believes, would still exist, the true idea of moral perfection would be objectively realized in it, the moral law would still have significance, and obedience to it would bring its own reward; it would still be the law of life and the condition of human welfare; it would still make the universe a moral order and its laws moral laws, even if their sanctions were confined to this life.

We may hold to the Kantian idea that the moral life would be robbed of its supreme significance unless it were of infinite duration as well as worth, and yet not stake morality upon the doctrine of immortality. It is absurd to say that the universe without immortality 'is a soulless machine' (p. 119). Faith in God and a future life are coördinate aspects of the Christian conception of the world, and they are commonly associated in thought. Together they furnish the best, if not the only, rationale of an optimistic philosophy. But they do not necessarily stand or fall together. One may surely believe in God and in his wise providential ordering of the universe as a whole, including the race of man, and in the reign of moral law, while still expressing doubt about personal immortality. The eternal life being life in harmony with the nature of the Eternal, may begin here and now, and though it cannot reach its full fruition in our short finite existence, and for this we are led to look beyond, it would still not be true that 'without immortality might would be right and expediency morality.' Morality might be God's will for mortal as for immortal beings. It is at least thinkable that in three score years and ten man might so realize the blessedness of a life in harmony with the Eternal Righteous One as to make the moral life worth living. It may of course be objected that in order to justify the ways of God to man individually, in order to conceive of Him as a just and loving Father who cares for each one of us, we must postulate a sphere where the wrongs of this life may be righted and the crooked made straight. And this is true. Only thus can the demands of the religious consciousness be completely satisfied, and the providential order of the world be made to cover individual cases; but it does not follow that personal immortality is necessary to the existence of moral law or that without it the world is a soulless machine.

It is also true as a matter of fact that most of those who refuse to put faith in the doctrine of immortality do so on the basis of a materialistic philosophy. Nevertheless, the answer to the metaphysical question as to whether the universe is 'a soulless machine' surely sustains no logically necessary relation to the ethical demand for immortality. It is perfectly possible to believe in a God without believing in immortality—that is, to find the 'proofs' of God's existence stronger than those for immortality. Though here again, as a matter of fact, the moral consciousness which demands a future life also furnishes one of the strongest arguments for the existence of God. Mr. Kedney, however, is disposed to overwork a willing beast. He doubts "the alleged indifference with which it is said that some men of philosophic temper-



ament have met death"—a doubt which we do not share, but need not dwell upon, since the fact, whatever it be, would tend neither to prove nor to disprove anything of importance. Mr. Kedney continues: "The human soul clings to life as no animal does, which shows its instinct that life has meaning," *i. e.*, according to the context, that man must be immortal. The statement in regard to animals may be doubted. Even a fish may be said to 'cling' desperately to life when it struggles to regain its natural habitat; and cases of animal-suicide, if proven at all, are at least rare. We know too little about the feelings of animals to speak about them with any certainty. But if the facts be as Mr. Kedney asserts, may we not with equal justice argue precisely otherwise? We instinctively cling to what we fear to lose; but if we are to pass out of this life into a richer, fuller existence, why is it not our instinct that 'to depart is far better'? Mr. Kedney says the fact that man "cannot imagine his extinction as a self-consciousness may be taken as supplementary evidence that such extinction is impossible" (p. 116). If Mr. Kedney cannot 'imagine' this 'extinction,' it seems a curious limit to the imagination of one who lays so much stress upon the necessity of "taking the ideal out of the cold region of pure thought and bringing it within the warm one of imagination" (p. 27).

Again Mr. Kedney says: "The neglect to take into account the fact of continued conscious human existence after death as furnishing a set of relations affecting human conduct before death, is the fatal flaw of utilitarianism as a reasoned system." Mr. Kedney forgets that utilitarians like Mill and Sidgwick show that theological sanctions may be added by those who choose, in support of a morality which makes universal happiness the highest good; and in criticising Kant on this point he comes dangerously near the position of Paley.

Mr. Kedney's desire to avoid accepted philosophical phraseology (p. 23) is unobjectionable, though it may be asked wherein consists the advantage of avoiding terminology which would abbreviate expression without sacrificing lucidity. But if accepted terms are to be used at all, they should be used with proper discrimination. Thus Mr. Kedney lumps all utilitarians together and criticises them *en masse* (p. 43). He holds out as utilitarian doctrine the thesis that 'might determines right.' But what is right is duty, and just over the page our author teaches that for utilitarians the only *duty* is to promote the aggregate of happiness. There are, of course, utilitarians, like Sidgwick for example, who neither ignore the idea of duty, nor resolve it into might. On the other hand, Mr. Kedney himself appar-



ently falls into this identification of might with right. The origin of a nationality, he says, may not have been necessary or morally right; "but if the new state can maintain itself in its autonomy, it must be thought to be comprised in the providential plan, and therefore to have the divine sanction." That is, whatever is, is right, or might makes right. And if a state can *not* maintain itself, then presumably it has not the divine sanction. This reminds one of much of the pulpit and political sophistry at present current.

From what has been said, it may be surmised that Mr. Kedney's edifice is somewhat loosely joined together. With a single final glance we leave it. The author says: "Many animals are superstitious" (p. 198). "Superstition is simply the irrational." "Superstition implies a false philosophy" (p. 199). From this it would follow that many animals are irrational and have a false philosophy. The book is written in fine spirit, however, and in its definition of the good as "a community of loving souls," recognizes both the social and the ideal elements which are characteristic of the tendency of recent ethical thought.

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*Essai sur l'Imagination Créatrice.* TH. RIBOT. Paris, Felix Alcan. 1900. Pp. vii + 304. Fr. 5.

This monograph, the latest work from Professor Ribot's hands, is of considerable importance, as the first attempt in the direction of a classification of the results of the investigation of the inventive imagination which has been going on for some time, especially among the French psychologists. The author recognizes fully the difficulties of this field, so long neglected, its obdurate attitude toward experimentation, the multitude of problems that it gives rise to without affording the means to their solution, and especially the chance it offers for overhasty generalizations and hypotheses. The work is therefore largely a description and classification of the processes of the creative imagination and the modifications which these general processes undergo according to the ideal content with which they are concerned. Professor Ribot modestly calls it an essay, not a complete monograph.

It would give an incomplete view of the book, however, to call it merely descriptive. Where psychological and physiological hypotheses force themselves upon the attention—though with habitual reserve decision is suspended—the conditions of their solution are fairly and critically presented. Besides this a number of empirical gener-

alizations have been developed, which if not permanent contributions to the subject, ought, at least, to be decidedly valuable hints for further investigation. The entire presentation is carried out with that system and detail of classification which is characteristic of the author's method, and which is especially valuable in the present chaotic conditions of this sphere of investigation.

The essay is divided into three parts, respectively, analytical, genetic and concrete descriptive, in their method of treatment. The analytical undertakes to determine the nature and relative importance of the intellectual, the emotional and the unconscious factors in imagination, to distinguish the passive from the creative imagination, and to investigate the principles of unity and the organic conditions involved. The genetic treatment studies the imagination in animals, in the child, and in the myth formations of the race, and formulates the general laws of the growth of the imagination in the race and the individual. The third, or concrete portion, in some respects the most original and valuable contribution to the subject, is a study of the different types of imagination as differentiated by social conditions, namely, the æsthetic, the mystical, the scientific, practical and mechanical, the commercial and utopian.

Of course, in its great outlines this essay is but a chapter in Professor Ribot's psychology. The basis of the creative imagination he finds in the motor processes of the psycho-physical organism. All invention arises out of a need to objectify or externalize images in consciousness, the pathological forms of this dynamogenesis being found in hallucinations and the bodily effects produced in extreme cases of hypnotic suggestion, especially in auto-suggestion, *i. e.*, the phenomena of the *stigmata*.

All invention, in whatever sphere, is a function of two elementary factors, namely, certain fundamental needs of the organism, desires and tendencies, and a rapid and spontaneous association of ideas. All qualitative differences in imagination and the degree of imaginative possession, if the expression may be used, are functions of the relations of these two factors. The difference between passive and creative imagination is then one of degree, the process of invention 'being in the intellectual order equivalent to volition in the order of impulsive movements.' In the mere dreamer and vaguely imaginative man we have the *aboulia* of the creative imagination. This volitional theory, already developed by Paulhan in his article, 'L'Invention,' with much documentary evidence, Ribot has demonstrated for every sphere of in-

<sup>1</sup> *Revue Philosophique*, March, 1898. Reviewed by the present writer in this REVIEW, May, 1899.

vention, showing clearly that even the scientific, commercial and mechanical creations have their origin in the volitional and affective sides of consciousness.

In connection with this motor theory is developed an interesting and very probable doctrine of the creative imagination in animals, namely, that invention with them consists in new combinations of movements. An analysis of Groos' catalogue of animal plays shows them to consist largely of an elaboration of old movements into new complexes. Animals have memories and spontaneous imagination, but, although the results of observation are very uncertain, creative imagination in the sphere of images is generally denied them. Color is lent to this notion by our general knowledge of the animal mind and by the anatomical studies of Flechsig and Wernicke, which point out the undeveloped state of the coördinations in the sensorial regions of the lower mammals. Moreover, mental disease in young children expresses itself largely in a lack of motor coördination. In this exclusively muscular imagination of animals and young children Ribot sees one of the chief supports of his general theory.

The motor and affective elements in invention, besides being initiative, further tend to dominate the associations which make up the process. In the analytical study great importance is given to the rôle of emotional association, and the degree in which associations are emotionally determined becomes the principle of division for the classification of the creative imagination into its two great forms, the 'plastique' and the 'diffuente,' a distinction which becomes fundamental in the æsthetic sphere. The different types of concrete imagination are determined then, not alone by their content, but by the degree in which associations are emotionally determined and, consequently, by the coefficient of belief attached to them.

There are two favorite positions of Ribot, still experimentally undetermined and much disputed, which are fundamental in the entire work, namely, the doctrines of affective memory and of mediate association. Affective memory and its corollary, emotional abstracts, become the explaining terms for a large class of imaginative processes, which are termed 'diffuente.' And indeed, if we look upon this conception as a hypothesis, the value of which consists in its ability to explain imaginative processes, otherwise scarcely to be understood, we may well give it conditional credence, for there is a large group of æsthetic phenomena which seem to admit of no other explanation. Ribot can still admit the experimental uncertainty of the problem and set this explanatory power of the hypothesis over against the largely *à priori* considerations advanced by Titchener and others in disproof



of affective memory. In regard to mediate association, the case is much the same. In so far as the associative side of the creative imagination is concerned, many facts of the concrete imaginative life seem to require it. To mediate associations and Ziehen's doctrine of association by constellation, the spontaneous associations are reduced, whether the unconscious element in the invention be understood physiologically or psychologically.

In strict dependence upon the foregoing theory of affective memory and emotional abstracts, the principle of unity in invention is to be found in the 'emotion fixe.' It is the center of attraction about which the associations group themselves and by which they are determined. Not that an absolute distinction is to be made between the 'idée fixe' and the 'emotion fixe,' but in imagination ideal control is predominately emotional. We find, then, in the realm of the imagination stages of the attention parallel to those in the sphere of fixed ideas, stages passing from the (a) *unstable* and unorganized imagination, little differing from the passive, where the combinations are merely possible, to the (b) *fixed*, where there is still an oscillation between the ideal and the real, and, finally, to the (c) *objectified* imagination, where credence is total and where there is no longer return to the real. At this stage the pathological is reached. True to his monistic principles throughout, here, where physiology refuses to be explanatory, the author admits an inexplicable unconscious factor and contents himself with mere description. While excluding all the teleology that often creeps into this field, whether from idealistic or biological sources, he cannot, however, avoid coquetting with the idea. The constant use of such terms as 'immanent logic,' 'logic of the emotions' are blemishes in an otherwise thoroughly consistent treatment. Perhaps they are ghosts that will not down!

Although no sharp line of division can be drawn between the 'idée fixe' and the 'emotion fixe,' there are however two relatively distinct modes of imagination, according as the emotional or intellectual associations prevail. The distinction between the 'plastique' and 'diffuente' imagination is found throughout all types of inventions and is especially important in art creations. By no means identical with the idealistic-realistic classification, being rather a cross division, it is one which has in it the possibility of displacing this worn-out distinction with one more scientific and developed from the more complex experience of the present. Ribot, who is, it would seem, in the æsthetic portion of his treatise considerably influenced by the studies of Guyau, likewise finds the differentia of artistic method in the subjective atti-



tude of the inventor toward his ideal content. The *plastic* imagination is essentially externalizing. Whether the subject matter be sentiments or images, an imagination of this type seeks the clearest and most distinct forms, for its externalization is visual or tactual. In the sphere of literary form its ideal is clear and vivid imagery. If its materials are those of the painter or sculptor, clearness of outline and detail is fundamental. The *diffluent* imagination is, on the other hand, to use Ribot's constant expression, governed by the 'logique emotionnelle,' its reality is internal, its associations less rigid and stable, its contours undecided. Here the emotional abstracts or moods have full sway as unifying principles. That such a distinction is fruitful the reader of Ribot is convinced. On the whole, too, the author's classification of the various tendencies in art, which puts Victor Hugo and the Parnassians among the former, the impressionists, the symbolists and the pre-Raphaelites among the latter, will be found to be just. He has shown, too, that, whether idealistic or realistic, the subject matter is entirely independent of the mode of imaginative treatment. Victor Hugo and Poe are of the plastic, while some of the realists are of the *diffluent* type. The conception ought, however, to be carried out more fully by determining the laws according to which the details of material treatment produce these effects in appreciation and by showing the difference of the two types in the various arts. This was brought to my attention by a comparison of the drawings of Victor Hugo, recently reproduced in *Harper's Magazine*, with Ribot's analysis of his written style. While Hugo's literary imagination seems to be decidedly plastic, yet some of his sketches have, if I understand the distinction, decidedly the opposite effect.

Moreover, it would seem that, if this distinction is to be one applicable to all the arts, Ribot has conceived it somewhat too narrowly in defining the plastic imagination as determined by a dominance of visual and tactual ideas. This idea, joined to the fact, for which the author finds abundant documentary evidence, that, at least among those who appreciate music most, there are few visual associations, leads him to classify music as wholly of the emotional or *diffuente* type. While this is true of much of the so-called intellectual music, much of Bach, Glück and some of even Beethoven and Mozart, are decidedly plastic in effect; but in the case of music the muscular sensations in the experience of effort and rhythm are so definitely and objectively conceived as to be almost spatial in effect. The phrase "architecture is frozen music" is, psychologically at least, not wholly meaningless.

It is to be regretted that this description of the two types is not

supplemented by an attempt at a temperamental explanation of the distinction—a not impossible task and, certainly, one for which Ribot is preëminently adapted. Wundt's theory of the temperaments might have proved as fruitful here as in religious psychology. In chapter IV. there is a suggestion that the two modes of procedure in creative imagination, intuitive and combinative, are respectively due to motor and sensory types, but it is left uncertain whether these two modes of procedure are related to the distinction between 'plastique and diffluente.'

Notwithstanding these undeveloped portions of the theory, a study of the detail of the discussion is sufficient to impress upon the reader the great value of the distinction for æsthetics, an impression that has been further deepened in the present writer's mind by the fact that a comparison of this distinction with Ruskin's division of imagination into penetrative and contemplative has developed many points of likeness between the two.

Ribot has also found this distinction fruitful in the classification of myths. The Hindus with their unstable Augenblicksgötter and their emotionally determined symbolism, and the Celts with their dreamy changing myths, are 'diffluente' in type, while the clearly conceived historical and dramatic myths of the Greeks and Teutons are 'plastique.' All mysticism is of the former kind.

The limits of this review will not allow a discussion of the laws and stages of the development of the imagination in its ontogenetic and phylogenetic growth. Important as are the studies of the different concrete forms of imagination, so penetrative in their analysis, always developing the salient points of distinction, and enriched as they are by illuminating detail from ethnological and sociological sources, it must suffice merely to characterize them as valuable for psychology and sociology alike. In M. Ribot has been found the systematic psychologist needed to supplement the brilliant work of Guyau and Tarde, and likewise to set the problems for more specialized work in this neglected field of psychology.

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*From India to the Planet Mars, a Study of a Case of Somnambulism with Glossolalia.* TH. FLOURNOY. Translated by DANIEL B. VERMILYE. New York, Harper & Brothers. 1900. Pp. xix + 449.

The translation of this work (the original of which has been noticed in these pages, July, 1900, p. 406) has been well done and

offers, except for its abbreviations for which the translator is not responsible, the English reader a good opportunity to study a most important case of secondary personality simulating spiritism. Apart from this defect, which amounts almost to a mutilation for the scientific man, the work is one of the best contributions that I know to the study of a very obscure set of phenomena until the patience is shown to unravel its tangled threads. Professor Flournoy deserves and will receive unstinted praise for the way in which he has done his work, inasmuch as it penetrates into every remote corner of his case for facts to reduce it to a normal explanation. But I shall leave the merits of the book to the reader who may care to examine it.

The chief criticism to be made on the book is its ready acceptance of such doctrines as telepathy, telekinesis and clairvoyance. The author's scepticism in these questions does not serve him so well as in the case of alleged spirits. He accepts these questionable theories without adequate evidence, as it appears to the present writer. At least that evidence is not apparent in Professor Flournoy's work. Had he been as exacting of their claims to recognition as he was in the case of spiritism, he would not have betrayed any apparent credulity. I see no reason as yet to admit telekinesis and clairvoyance, and telepathy is only a name for a coincidence that requires a cause and is not an explanation.

To my mind, however, there is one important result from the book which I have not seen remarked. This is its indirect influence on such cases as that of Stainton Moses, whose experiences have puzzled almost a generation of inquirers. We have in this man an instance of perfect probity, so far as unanimous human testimony can indicate this, and yet a lot of incredible phenomena reported which have tempted Mr. Lang and others to call it a 'moral miracle' rather than accept the facts at their face value. These people have preferred to believe Stainton Moses a fraud rather than seek an explanation consistent with his honesty. Now Professor Flournoy's case offers a perfectly natural explanation without impeaching the facts or normal honesty of any one. Mlle. Smith, Professor Flournoy's somnambulist, simply passed into a secondary state, did certain things, and emerged from this state into normal consciousness with complete amnesia of the secondary condition. I have long held this a possible explanation of the Moses case, but have not found until now any authentic case sufficiently like it to enforce the conjecture. It is thus interesting to find an intricate but natural explanation to such baffling phenomena as these often are, and we owe it largely to the patient disregard of



dignity on Professor Flournoy's part that we have so valuable a case for appeal.

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*Introduction à la Vie de l'Esprit.* LÉON BRUNSCHVICG. Paris, Alcan. 1900. Pp. 175.

This is an introduction to philosophy conceived as reflection upon the mental and spiritual life. It shows the simplicity, directness and clarity of statement that come from full mastery of the subject matter, and it is written in a delightful style. It treats briefly the mental processes under the heads of representation, action and feeling, and devotes a chapter each to the scientific, the æsthetic, the moral and the religious life. Science, art, morals and religion are all treated from the standpoint of their function in developing and enlarging the individual and social life rather than as ends in themselves, and the result is to make evident what ought to be but unfortunately is not always manifest—that philosophy is the most concrete and vital of all departments of human thought. It is admirably adapted for either the general reader or the college student who needs to be introduced to philosophy. It will tempt him farther.

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*Essai sur la Soif, ses Causes et son Mécanisme.* ANDRÉ MAYER. Paris, Alcan. 1901. Pp. 170.

The aim of this monograph is to determine the organic conditions which give rise to the sensation of thirst rather than to investigate, on purely psychological grounds, the sensation itself. The method was suggested by the results of comparatively recent experiments upon osmosis in animal tissues. Briefly stated, the conclusion is that thirst is due to increased osmotic pressure of the blood.

After sketching earlier hypotheses in regard to the causes of thirst, the author describes at some length the rôle played by osmosis in the organism, and shows how changes in osmotic pressure may be measured. In his own experiments he used the cryoscopic method. He found that the blood of animals (rabbit, dog) which had been subjected to thirst-producing conditions had a lower freezing point than the normal and consequently a higher osmotic pressure. Conversely, when the abnormal pressure is relieved thirst disappears. If, on the



other hand, we except thirst of nervous origin, all those pathological conditions which produce thirst increase osmotic pressure. There is, thus, a constant relation: the heightened pressure is the cause of thirst.

The mechanism which, under normal conditions, controls osmotic pressure, is vascular. The injection of hypotonic solutions has little or no effect upon the circulation, while hypertonic solutions cause dilatation both local and general. Similar reactions take place in the tongue and intestines, but in the kidneys dilatation follows upon the injection of either the hypotonic or the hypertonic solutions. These variations in turn restore the normal pressure; the control is automatic. As the changes are instantaneous, they are due, presumably, not to the direct action of the blood upon the centers, but to excitation passing over afferent nerves. This view is confirmed by experiment; injection of the carotid with hypertonic solutions, after curarization, produces vaso-constriction, an effect just the opposite of that which is obtained when the centers are normal. The center in question, as is shown by elimination, must be the bulb. This means not that there is a special thirst-center, but that the osmotic pressure of the blood which causes thirst is controlled by the bulb. Whether there is also a cerebral center the author does not decide, but his experiments persuade him that if there be such a center it is probably one of association and not one of projection.

The mechanism here described is the same in man as in the lower animals, since the physiological accompaniments of thirst (observed by the author in three subjects and reported by other writers) present variations analogous to those which are produced by experiment. On the subjective side, thirst, when it is pathological, contains these elements: a general uneasiness and depression, followed by agitation and anxiety, along with the localized sensation, which is impulsive in character and which leads up eventually to paroxysm. Under normal conditions we perceive only the sensation and the impulse to drink. These psychical processes correspond to the organic changes which have been described and which originate in the excitation of the central nervous system by blood under abnormal pressure. Consciousness, however, reacts in its own way, magnifying and emphasizing the needs which the organic mechanism reports, until the sensation of thirst amounts to an auto-suggestion.

Dr. Mayer has considered a difficult problem from many points of view and has brought to convergence many lines of research. For the theory which he advances the question of changes in osmotic

pressure within the organism is pivotal. A more thorough discussion of the methods by which these changes are measured would have been both appropriate and useful.

The presentation of the subject is clear and orderly. To say that too much care was taken with the contents of the book would be unjust; and yet one might possibly explain in this way the carelessness which appears on nearly every page, in misprints of all sorts, in lack of references or inaccuracy when they are given, and in the blurring of curves which are supposed to help out the text. The page of 'errata' at the end of the volume gives samples only.

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## HUMOR.

*Le rire, essai sur la signification du comique.* H. BERGSON. Paris, Alcan. 1900.

The material of this book appeared originally as three articles in the *Revue de Paris*, but it well deserves reproduction in this more permanent form. For in attempting a theory of the comic based largely upon social relations, the author has certainly contributed to the better understanding of certain species of the comic, even if his theory be not accepted as wholly adequate.

The main positive thesis is that the comic results from the social life, and always shows a certain lack of adaptation to society on the part of the individual. Every one who goes his own way automatically without troubling himself to come into touch with others is comic. The prig, the pedant, the man behind the times, the odd or peculiar man is comic because out of touch with society; he is stiff and mechanical, instead of responsive and living. Laughter may be regarded therefore as one of the powerful factors in socialization, and this would afford a biological justification for its development, although I do not notice that the author calls attention to this point.

So far the comic of character in which the author is convincing. But how explain the comic of form, of movement, of situation and of words? These are held to rest in last analysis upon an element which appears in the analysis already noted, viz., the element of automatism or stiffness. All distraction or absent-mindedness is comic. A systematic distraction such as that of Don Quixote reaches the climax of effect. In applying the principle of automatism to comic situations the author analyzes a large number of stage situations, and shows much ingenuity

in detecting a mechanical element interjected where one would naturally look for life. The jack-in-the-box motif, the puppet motif, and situations where by inversions, repetitions and entanglements the actors seem to be manipulated at will, are all based on this one theory. The book concludes with a discussion as to the place of the comic among the arts.

*Das Komische.* JOHANNES ZIEGLER. Leipzig, Avenarius. 1900.  
Pp. 39.

Taking as starting-point the various theories which have made contrast in some form the essential feature of the comic, Ziegler attempts to define more specifically the exact nature of that contrast which is comic. He holds that there is always a conflict or collision between some mechanical process or occurrence on the one hand and a human purpose or end on the other hand. This mechanical process has in itself no end, but, owing to its apparent thwarting or perversion of some human aim, we are prompted to view it as if it intended its opposition. There is, hence, a double contrast: (1) between the purposeless and the (seeming) crossing of some purpose; (2) between the crossing or perverting of a purpose and the assertion of a purpose. The first furnishes the factor of surprise and contrariety, the second the comic in the proper sense.

Like the preceding theory, this may be applied to many comic incidents, but it seems inapplicable in others.

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## VISION.

*Eine Anpassung.* DR. REDDINGIUS. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. XXII., pp. 96-100.

The adaptation here referred to is one already noted by Helmholtz that if objects be viewed through a triangular prism, and an attempt be made to touch them, the finger at first goes astray; but the movements rapidly become adjusted to the amount of the refraction, and the objects are thereafter exactly touched. On removing the prism a similar, though reverse, adaptation is necessary before the reactions of the hand again become entirely suited to normal vision. Dr. Reddingius, oculist at the Hague, here reports a number of interesting modifications and supplements of this experiment, to show that Helmholtz was mistaken in ascribing the adaptation to a change in the



judgment of visual locality. Helmholtz assumed that since, as he showed, the muscular localization was not altered, the visual localization was, by exclusion proved to be the seat of the adjustment. Reddingius, however, now produces quite as strong positive evidence that this other alternative—change in the visual judgment—is also out of the question, and that the true explanation must take a still different form.

The most telling of the author's experiments against the assumption of a change in visual localization is this: If the prism be so placed that objects seem too far to the right, and one of the hands be practiced for some minutes in pointing to them, until all error has disappeared, and this hand be then kept out of action and the other hand be similarly practiced with the prism turned around so that objects now seem too far to the left; and again in turn the feet be practiced so that they become adjusted, respectively, to an upward and downward refraction, and then the prism be removed; each member now retains his own peculiar adaptation, and the four different directions of error are found lying side by side. Since it is manifestly absurd to suppose that the direction of vision could be falsified in four directions at once, the visual-judgment hypothesis is thus excluded. Similarly the supposition that the muscular direction has been altered is excluded (the author believes) by the fact that the hand accustomed to the prism makes no error in touching the nose when the eyes are closed, and yet on opening the eyes shows the usual errors in movement.

The author would explain this whole group of results as due to an abnormal motor effect of the impulse which arises from the visual impressions—an abnormality which, he finds, can be detected as long as thirty-six hours after an adaptive exercise lasting but ten minutes, provided the adapted member remain unused in the meantime.

Reddingius's explanation seems much juster than Helmholtz's, so far as the facts of this particular experiment are concerned. The exercise with the prism brings about no change of conscious localization of objects. It is like learning to direct one's movements in a mirror, or to control an object under a microscope. In the presence of the unusual sight impressions we learn to make abnormal innervations in order to bring our hand to a given place in the visual field.

In this respect the character of the adaptation is quite different from that which comes about when the disturbing visual conditions are submitted to, not for minutes, but for hours or even days. In the latter case there is a change not merely of movements, but of the conscious localization of things. Perhaps the alteration of the motor im-



pulse, upon which the author lays stress, is an important factor in this more peculiarly psychic adaptation also. It is possibly one of the important conditions leading up to the readjustment of tactual and visual local signs, by which a new tactual locality comes to feel as if it actually were the true counterpart of a visual place to which it does not belong. But the change of the motor impulse is not this psychic adaptation itself; as is proved by the altogether different amount of time and practice which each requires for its production.

In contrasting his own results with Helmholtz's, the author makes a curious slip. He quotes Helmholtz's account that when one hand has become accustomed to react suitably with the prism before the eyes, and then *the eyes be closed*, any object which we have localized with that hand can now be unerringly found with the other—the unaccommodated—hand, showing that the process of accommodation had not given a false color to the muscular feelings in the adapted hand. Reddingius then goes on to say that when he himself used a prism whose refractive angle was  $36^\circ$  (as against  $16^\circ$  or  $18^\circ$  in those of Helmholtz) he got quite the opposite result: the unaccommodated hand also went astray. Yet this 'opposite result,' one finds, was obtained *while one eye was open and looking through the prism!* The fact is that, with either weaker or stronger prisms (the reviewer finds), one can get Helmholtz's result or its real opposite, according as the unpracticed hand, on closing the eyes, is guided by a visual or by a muscular memory of the locality sought. Guided by the memory of the impression obtained the moment before through the prism, the hand goes astray; guided, however, by the dying feeling in the accommodated hand when last it touched the object, the other hand finds the place exactly. This, however, is quite in keeping with Reddingius' conclusion that the abnormality in the movement only occurs in connection with optical presentations. Movements whose starting-point and goal are figured entirely in muscular terms are not affected by the 'adaptation.'

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UNIVERSITY OF CALIFORNIA.

## EDUCATIONAL PSYCHOLOGY.

*Kritische Untersuchungen über Denken, Sprechen, und Sprachunterricht.* A. MESSER. Schiller and Ziehen's *Abhandlungen*, III. Bd., 6 Heft. 1900.

Dr. Messer's monograph is a critical discussion of the value of classical studies. It takes its departure from the antagonistic results

arrived at by two men who have recently written upon the importance of linguistic study.<sup>1</sup>

Of these, the former holds that no better means than language study can be found for the material enrichment as well as the formal discipline of the mind; the latter considers it unadapted to accomplish either end. Both appreciations—the low as well as the high—are connected with special theories of the psychological significance of verbal signs on the part of these authors.

According to Keller, words do not signify things themselves, but our ideas of things, of relations, of movements, and our concepts. They stand for a peculiarly subjective range of experiences, which cannot be apprehended by observations of the objects with which they are secondarily connected, but must be directly communicated through these symbols themselves. Were words but the signs of things the acquisition of a foreign tongue would have little bearing upon the mental development of the learner. Being nothing of the kind, but rather symbols of a most strongly individualized conceptional way of regarding these things, the significant word becomes a new material element in the learner's thought content. To acquire an unfamiliar speech is not merely to learn a novel set of signs for the same old world; it is to win a fresh spiritual possession and vastly to enrich the material of our mental life.

In reply to this Messer very briefly and conclusively points out that the foreign word, heard for the first time, is a bare sound or visual impression, and that it takes on intelligible content only when there has been connected with it one or more words of one's mother-tongue or the perception of the object which it—directly or indirectly—indicates. It takes on meaning only through its translation into terms of an intuition of our own culture-world, in which process no material enrichment is gained. Such a widening of our spiritual horizon can take place first when a study of the literature of a foreign people has been added to that of its speech, and, indeed, even this becomes effective only when richly supported by information concerning the actual conditions of their material and social life.

For Ohlert also the term fails to represent the thing, but for a different reason than that of Keller. The word, he says, is thoroughly and always one-sided. It indicates not the sum of characteristics which actually constitutes the object, but only that one of these which attracted the attention of the name. The word *Boûs*, for example, seizes

<sup>1</sup> 'Denken und Sprechen und Sprachunterricht.' Julius Keller, Lörrach, 1899. 'Das Studium der Sprachen und die geistige Bildung.' Arnold Ohlert, Berlin, 1899.

upon only one of the many aspects which the ox presents, and characterizes it as the bellowing beast. Or again, letting slip all thought of the wolf's size, color, form, gait, and a thousand other things, we apperceive him only as the *Render*, not even creating a new verbal element, but only applying a special already existing root-wood.

But all this, it is objected by our author, applies only to the original naming of the beasts, and in no way holds of the speech of any cultured people. The specialized apperception which gave birth to the verbal form is but one of a vast range of possible aspects and relations which the thing may possess, all of which are implicit in the term. For the word does indicate the object and all its characteristics, however the ideational content which it arouses may vary in passing from the child to the adult, or from one special practical relation to another. And just because the term signifies for our consciousness not a single characteristic, but the object at large—or rather a whole class of objects—the utterance of a word, apart from the connections of rational speech, may arouse a whole series of images in consciousness, which renders it impossible to determine in advance what particular course the reproduction will follow in any given case.

Nor is Ohlert's second point better taken, namely, that we customarily employ verbal symbols without having in mind the characteristics which they indicate. The term 'unconscious' here is unhappily chosen; it can signify only 'beyond the focus' and not 'below the threshold' of consciousness. And even so, it can mean only that in hearing and reading intelligent language there is not aroused in us a specific awareness of the many concrete aspects and relations under which the thing may be conceived, but only a faint fringe of feeling concerning the existence of other meanings and applications which we could realize if we wished. Yet here we must remember that in all connected speech the particular content is really quite definitely determined, and a departure from it—the appearance of ambiguities or arbitrary associations—is the exception, not the rule. The truth lies between these two extremes. The learning of a foreign language does not enrich the mind with a new range of material knowledge, but it is not on that account the less valuable as a part of school instruction. Ohlert's assertion that capacity for right judgment and training in logical thought is to be gained only by the process of thoroughly acquainting ourselves with the concepts and objects about which such judgments are made simply cannot be maintained.

The acquisition of a rich material knowledge is undoubtedly an important part of education. But no less significant is that whole



group of studies which direct the pupil's attention to the activity of thought itself, and call upon him to analyze the nature of the concepts which he constantly employs, to discriminate and compare their applications, and to practice the *process* of thought so that, in the words of Paulsen, he may be able clearly to separate and apprehend the problems of thought relations. And for this logical training the study of foreign languages, in virtue of its unremitting demand for comparison with the verbal concepts of its mother-tongue, offers a rich and appropriate material.

R. MACDOUGALL.

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## PSYCHOPHYSICAL.

*Quelques contributions à la psychologie du sommeil chez les sains de esprit et chez les aliénés.* By ALEXANDRE PILCZ. Annales Médico-Psychologiques, Vol. LVII., No. I., Jan.-Feb., 1899. Pp. 66-75.

Dr. Pilcz, assistant in the first psychiatric clinic of Vienna, gives in this interesting and clearly written paper the results of experiment on himself and of inquiry among competent patients. The monotonous life of the hospital made it possible for him to retire to sleep on the moment, as he says, and he arose regularly at seven in the morning. Before going to bed, during a period of several weeks, he took doses of bromide, of paraldehyde, tea, alcohol, etc., or else subjected himself to severe physical or mental labor. On awakening he wrote out his dream-experiences and likewise the content of his consciousness whenever awakening during the night, which was a frequent occurrence.

He states that he was entirely unable to verify the results reported by Nelson (Am. Jour. Psy., Vol. I.) as to an alleged periodicity or regularity of the intensity of his dreams, although he devised a scale by which the subjective intensity could be in each case accurately recorded.

During the nights when he was under the influence to a greater or less extent of excitants, such as tea, coffee, alcohol, exciting events or of intellectual or emotional experiences such as the opera, his dreams were short and confused, and they related to *recent* and relatively unimportant events. When, on the other hand, he had taken paralde-



hyde or bromide, or had subjected himself to serious but not exaggerated physical or mental exertion, when, in brief, he was fatigued or under the influence of a somnifacient, his dreams like his sleep were heavy and deep, and related to events in his experience of relatively great importance to him which had occurred at *a much earlier date*, perhaps years before, and with which his waking mind was more often occupied. The first hours of sleep at night gave him dreams of events of years before, perhaps, while the morning hours were productive of dreams relating to recent events. In cases where he was over-excited on retiring the fatigue of hours of restlessness caused a reversal of this relation.

He could discover no correlation between the depth of sleep (with its consequent characteristic dream material) and the intensity of its dreams.

He confirms the observation of several other experimenters: that generally every impression, however important or however often repeated, remains in mind until it has had a chance to be reviewed in a dream.

From extensive inquiry among competent insane patients he found general confirmation of these results, paranoiacs in particular being useful in this respect. He noticed, as have others, that the delusions of the waking hours seldom or never came to harass the sleep of the monomaniac.

He suggests by way of cytologic explanation of these facts that the association cells and fibers of the cortex which are the most fatigued recover in sleep their activities last, the elements active least during the day being at night well supplied with energy. The phenomena observed in simple acquired dementia, as in senile dementia, strongly corroborate this view of cortical cellular fatigue as the occasion of sleep, the earlier experiences being best retained when the cells representing in some way the recent events of life are worn out.

*Sur la Methode pour etudier les sentiments simples.* By Dr. F. KIESOW. Archives Italiennes de Biologie, Tome XXXII., Fasc. I., pp. 159-164. 25 Novembre, 1899.

The author's researches have been mostly on taste, but he suggests that in any field the graphic method can be advantageously used to indicate the relation which the variation in the sensation as such bears to the variation in the accompanying feelings with changes of stimulation. By feeling Dr. Kiesow, as most writers now, indicates the

relative tone of pleasure and of pain, it extending in two opposed nodes from an indifference zero.

In the author's experience the majority of subjects can discriminate these two factors of an empirical complex, although both are abstractions purely. The sensation-curves are constructed by using the stimulus values as abscissas, and sensational intensities as ordinates. The feeling-curves use likewise the stimuli as abscissas, the curve thence extending on either side with the varying tone.

A large set of such curves made from averages of many persons and sensations would have considerable descriptive interest and value, being suggestive at least.

*Unilateral Facial Paralysis for Emotional but Not for Voluntary Movements.* R. T. WILLIAMSON, M.D. London, British Medical Journal. March 10, 1900. Pp. 576.

Nothnagel (*Zeitschr. f. klin. Med.*, Bd. XVI., Hft. 5 and 6) has already shown that lesions of the optic thalamus give rise to paralysis of emotional movements of a part, without interfering with the voluntary control of that part. This, of course, is strongly in evidence of the James-Lange theory of the emotions, indicating as it does that a large and important division of the brain is devoted, in part at least, to the innervation of emotional bodily reactions to certain stimuli. The case of Dr. Williamson's, here reported, is a striking illustration of this neurological relation.

The patient in this case was a woman forty-one years old. Consequent to great mental excitement her left leg became totally paralyzed and her left arm partly so; the tongue, however, could be protruded straight. There was no loss of consciousness. There was hemianæsthesia to tactile and to pain stimuli on the entire left side, as well as loss of sensibility to heat and cold. There was no hemianopsia. All the voluntary movements of her face, such as showing the teeth, etc., were very nearly alike on both sides, the left exhibiting a very slight deficiency as compared with the right. But when, by the proper stimuli, she was induced to smile, the left side was a complete blank, while the right side showed the ordinary typical expression of joy.

Dr. Williamson judges the lesion in this case to involve directly or indirectly the posterior part of the posterior portion of the internal capsule, and in all probability the optic thalamus. This presumption previous typical cases have made probable.

G. V. N. DEARBORN.

TUFTS COLLEGE.

## NEW BOOKS.

- Die Quantität in psychischen Vorgängen.* TH. LIPPS. Sitzgsber. d. Bayer. Akad. d. Wiss. (Phil.-Philol. u. histor. Cl.). 1899. 3 Heft. (Repr.).
- Kant and Spencer.* P. CARUS. Chicago, Open Court Publ. Co. 1899. Pp. 105.
- La justice sociale.* F. DUGAST. Paris, Girard & Brière. 1900. Pp. 72.
- Le problème des sexes.* J. LOURET. Paris, Giard & Brière. 1900. Pp. 302.
- What Is Thought?* J. H. STIRLING. Edinburgh, Clark. 1900. Pp. viii + 423.
- Le problème de la mémoire.* P. SOLIER. Paris, Alcan. 1900. Pp. 220.
- Form and Formal Thought.* P. CARUS. Chicago, Open Court Publ. Co. 1889. Pp. 35. (Excerpt from 'Fundamental Problems.')
- Notes on the individual Psychophysiology of the Crayfish.* G. V. N. DEARBORN. Repr. fr. Amer. J. of Physiol. 1900. IX. April.
- Introduction to Ethics.* F. THILLY. New York, Scribner. 1900. Pp. xi + 346.
- Das Blut in Glauben und Aberglauben der Menschheit.* H. L. STRACK. 15te-17te Auf. Münschen, Beck. 1900. Pp. xii + 208.
- The Dawn of Reason, or Mental Traits in the Lower Animals.* JAMES WEIR. New York and London, Macmillans. 1899. Pp. xiii + 234. \$1.25.
- L'Avenir de la Philosophie.* HENRI BERR. Paris, Hachette. 1899. Pp. x + 512.
- Interpretations of Poetry and Religion.* G. SANTAYANA. New York, Scribners. 1900. Pp. x + 290.
- L'Origine de la Pensée et de la Parole.* M. MONCALM. Paris, Alcan. 1900. Pp. 316. Fr. 5.
- Le Rire.* H. BERGSON. Paris, Alcan. 1900. Pp. vii + 204. Fr. 2.50.

- The Gospel According to Darwin.* W. HUTCHINSON. Chicago, Open Court Co. 1900. Pp. xii + 241.
- A Brief Introduction to Modern Philosophy.* A. K. ROGERS. New York and London, Macmillans. 1899. Pp. 360.
- Die Gemüthsbewegungen und ihrer Beherrschung.* C. M. GIESSELER, Leipzig, Barth. 1900. Pp. 68. M. 1.20.
- Beiträge zur Philosophie des Gefühls.* F. RITTER v. FELDEGG. Leipzig, Barth. 1900. Pp. 122. M. 2.50.
- Phänomenologie des Wollens.* A. PFÄNDER. Leipzig, Barth. 1900. Pp. 132. M. 4.50.
- Ueber Psychologie der individuellen Differenzen.* L. W. STERN. Leipzig, Barth. 1900. Schr. d. Gess. f. psych. Forschung. Heft. XII. (III. Sammlung). Pp. 146. M. 4.50.
- Das Grundgesetz alles neuro-psychischen Lebens.* J. PIKLER. Leipzig, Barth. 1900. Pp. xvi + 254. M. 8.
- La tristesse et la joie.* G. DUMAS. Paris, Alcan. 1900. Pp. 426. Fr. 7.50.
- Methods of Knowledge.* W. SMITH. New York and London, Macmillans. 1899. Pp. xxii + 340.
- Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie.* H. SCHILLER und TH. ZIEHEN. Berlin, Reuther & Reichard. The following:
- O. ALTENBURG.—*Die Kunst des psychologischen Beobachtens.* II. 3. 1898. Pp. 76.
- H. SCHILLER.—*Studien und Versuche über die Erlernung der Orthographie.* II. 4. 1898. Pp. 63.
- A. CRAMER.—*Ueber die ausserhalb der Schule liegenden Ursachen der Nervosität.* II. 5. 1899. Pp. 28.
- A. HUTHER.—*Die psychologische Grundlage des Unterrichts.* II. 6. 1899. Pp. 83.
- A. OHLERT.—*Das Studium der Sprachen und die geistige Bildung.* II. 7. 1899. Pp. 50.
- A. MESSER.—*Die Wirksamkeit der Apperception in den persönlichen Beziehungen des Schullebens.* II. 8. 1899. Pp. 69.
- H. SCHILLER.—*Die Schulartzfrage.* III. 1. 1899. Pp. 56.
- W. S. MONROE.—*Die Entwicklung des socialen Bewusstseins der Kinder.* III. 2. 1899. Pp. 88.
- F. SCHMIDT.—*Ueber den Reiz des Unterrichtens.* III. 3. 1900. Pp. 36.



*Essai sur l'imagination créatrice.* TH. RIBOT. Paris, Alcan. 1900. Pp. vii + 304. Fr. 5.

*Zur psychologischen Analyse der Welt.* K. B.-R. AARS. Leipzig, Barth. 1900. Pp. 295. Mk. 6.

*Transactions of the Canadian Institute.* Semi-centennial Memorial Volume. Toronto, Murray. 1899.

*Ueber d. Möglichkeit u. d. Aufgaben einer Social Psychologie.* F. EULENBURG. Report from Schmoller's Jahrbuch XXIV., 1. Leipzig, Dunker u. Humblot. 1900. Pp. 201-237.

*Essai sur la Soif.* A. MAYER. Paris, Alcan. 1901 (for 1900). Pp. 170. Fr. 3.

*Statistical Methods with Special Reference to Biological Variations.* C. B. DAVENPORT. New York, Wiley. 1899. Pp. vii + 148.

An extremely timely and useful handbook of the statistical method. It is to be commended to students of psychology as well as to workers in biology. It is an introduction to a department of work which is probably the most important advance in biology since Darwin.

J. M. B.

*Studies from the Yale Psychological Laboratory.* E. W. SCRIPTURE. Vol. VIII. (1899). 1900. Pp. 108. \$1.

*Grundzüge der Psychologie.* H. MÜNSTERBERG. Bd. I., Allgemeiner Teil: die Principien der Psychologie. Leipzig, Barth. 1900. Mk. 12.

*Aspects of Mental Economy.* M. V. O'SHEA. Bull. Univ. of Wisconsin, No. 36. Madison, Wis. 1900. Pp. 36-198. 75 cents.

*Les Facteurs de l'Évolution des Peuples.* A. MATTEUZZI. Trad. de Ital. Paris, Alcan. 1900. Pp. 411.

*Life and Letters of Thomas Henry Huxley.* LEONARD HUXLEY. Two vols. New York, Appletons. 1900. Pp. xii + 539, and 541. \$10.

These handsome volumes are of extraordinary interest to all who cherish the life and freedom of science. Huxley is allowed to do his own speaking and he speaks in his letters without reserve. Whatever one may say as to his place in the making of science—and that is possibly overrated—no one will deny that the world of emancipated thought and speech owes him a debt of lasting gratitude. J. M. B.

*The Biography of a Baby.* M. W. SHINN. Boston, Houghton, Mifflin & Co. Pp. 247. \$1.50.

A popular going-over of the life history of the baby already reported upon in more detail and fuller discussion in Miss Shinn's *Notes on the Development of a Child* (Pts. I.-IV.). Miss Shinn's work has had deserved recognition.

J. M. B.

*Psychology, Empirical and Rational.* M. MAHER. Fourth Edition, rewritten and enlarged. Longmans, Green & Co. 1900. Pp. xvi + 602.

*Fact and Fable in Psychology.* T. JASTROW. Boston, Houghton, Mifflin & Co. Pp. xvii + 375. \$2.00.

*The Origins of Art.* YRJÖ HIRN. London and New York. 1900. Pp. xi + 341. 10s; \$3.25.

*Animal Behaviour.* C. LLOYD MORGAN. London and New York, Ed. Arnold. 1900. Pp. viii + 344. 10s 6d.

*An Essay on Personality as a Philosophical Principle.* W. RICHMOND. London and New York, Ed. Arnold. 1900. Pp. xix + 219. 10s 6d.

*The Census of Cuba, 1899.* J. P. SANGER, Insp. Gen. Washington, Government Printing Office. 1900. Pp. 786.  
Contains social and educational statistics of interest.

*Report of the Commissioner of Education, 1898-9.* Volume I. 1900. Washington, Government Printing Office. Pp. xcii + 1248.  
Devoted largely to valuable reports on education in foreign countries.

*An Enquiry Concerning Human Understanding.* DAVID HUME. Chicago, Open Court Co. 1900. Brochure. Pp. xxiv + 180. 25 cents.

This is the second volume of the series of philosophical classics which the Open Court Co. are issuing in cheap form. It is an unannotated reprint of the edition of 1777, together with Hume's autobiography and the letter of Adam Smith to William Strahan. The *Enquiry Concerning the Principles of Morals* is to follow in the same series, which will undoubtedly be of great service.

*Les Approximations de la Vérité.* H. BLONDEL. Paris, Alcan. 1900. Pp. xii + 238. Fr. 2.50.

*David Hume, Moraliste et Sociologue.* G. LECHARTIER. Paris, Alcan. 1900. Pp. 275. Fr. 5.

*La Philosophie de H. Taine.* G. BARZELLOTTI. Trad. de l. Ilat. Paris, Alcan. 1900. Pp. xxvii + 448. Fr. 7.50.

- Schopenhauer's Philosophie in seinen Briefen.* R. SCHLÜTER. Leipzig, Barth. 1900. Pp. 125. Mk. 3.
- Variétés philosophiques.* J.-P. DURAND (DE GROS). 2 ed. revue et augmentée. Paris, Alcan. 1900. Pp. xxxii + 333. Fr. 5.
- The Soul of a Christian.* F. GRANGER. London, Methuen. 1900. Pp. 303. 6s.
- L'Année philosophique.* F. PILLON. 9<sup>me</sup> Année, 1898. Paris, Alcan. 1899. Pp. 316.
- Contains articles on *Le Principe de Relativité* by Renouvier, and *l'Esthétique criticiste*, by Dauriac, and the usual critical review of French publications of the year 1898.
- Experimental Study of Children.* A. MACDONALD. From the Report of the Commissioner on Education (1897-8, Chaps. XVI. and XXV.). Washington, Government Printing Office. 1899.
- Contains 'Anthropometrical and Psychological Measurements of Washington School Children,' and a Bibliography of 'Child-Study.'
- Ueber das Verhältniß der ebenmerklichen zu den übermerklichen Unterschieden bei Licht- und Schallintensitäten.* W. AMENT. Inaug. Diss., Würzburg. Leipzig, Engelmann. 1900. Pp. 68.

## NOTES.

PROFESSOR GUIDO VILLA has been called from Pavia to the Chair in Philosophy in the Lycée Tasso at Rome.

DR. T. P. BAILEY, of the University of California, has been appointed Lecturer in Philosophy and Assistant Professor in Pedagogy in the Extension Division of the University of Chicago.

THE chair of Philosophy and Pedagogy at Butler College, made vacant by the resignation of Professor E. S. Ames, has been filled for this year by the appointment of A. J. Rogers (Ph.D., Chicago), who comes from Alfred University.

H. HEATH BAWDEN (Ph.D., Chicago) has been appointed to a teaching fellowship in Philosophy in the State University of Iowa.

THE position of Dean of Women and Instructor in Pedagogy in Southwest Kansas College, Winthrop, Kansas, has been filled by the appointment of Miss Pearl Hunter, Fellow in Pedagogy at the University of Chicago.

A. H. PIERCE (Ph.D., Harvard), of Amherst College, has been appointed Associate Professor of Philosophy in Smith College; and R. B. Perry (Ph.D., Harvard), of Williams College, Instructor in Ethics in the same institution.

WE regret to record the death of Professor F. Max Müller, at Oxford, on October 28th.

PROFESSOR TH. ZIEHEN, of Jena, has accepted a call to the chair of Psychiatry in the University of Utrecht.

DR. L. W. STERN and Dr. G. Simmel have been promoted to Assistant Professorships in the Universities of Breslau and Berlin respectively.

C. K. SMITH, B.A., has been appointed Demonstrator in Experimental Psychology in Princeton University.

WE are informed that Professor Münsterberg has no intention of preparing or authorizing an English edition of his *Grundzüge der Psychologie*. This information answers a question which his readers in England and America are naturally asking.

PROFESSOR G. H. HOWISON'S work *The Limits of Evolution* (named from the opening essay) is in press for early publication by the Macmillans.

WE learn that Professor William James, whose health is improving, is intending to spend the winter in Rome, and that he is making good progress in the preparation of the first series of his Gifford lectures.

PROFESSOR J. MARK BALDWIN has withdrawn his name from the Committee of Patrons of the International Psychical Institute of Paris.

As appears on the title-page and on the second cover-page, Professor H. C. Warren, who has been in charge of the PSYCHOLOGICAL INDEX from the first, is hereafter to be associated with the REVIEW also as associate editor and business manager. As editor he will have especial charge of the 'literature' department so far as it relates to the reporting of the contents of journals and other periodical publications. He wishes to interest in this department as large a corps of writers as possible.

WE may also call attention to the fact that G. E. Stechert from now on takes over the English and continental agencies of the REVIEW, and will be able to fill orders promptly.



# THE PSYCHOLOGICAL REVIEW.

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## THE PSYCHOLOGY OF PROFANITY.<sup>1</sup>

BY PROFESSOR G. T. W. PATRICK.

*University of Iowa.*

In this paper I shall deal with only so much of the psychology of profanity as may be involved in the attempt to answer two questions: Why do men swear? When they swear, why do they use the words which they do? It is my opinion, however, that when the whole subject of profanity is finally worked out, it will be found to throw considerable light upon two unsolved but much discussed problems—one an old one, the origin of language, the other a new one, the relation between emotion and its expression.

We distinguish two kinds of swearing, asseverative and ejaculatory. The former will include, first, legal swearing, and secondly, popular asseverations taking the form of legal oaths. Of these, legal swearing is not, of course, included in profanity. Nor should we include popular asseverations apart from legal proceedings, provided only they are used with sufficient solemnity, as when a person accused of a serious offense calls the gods to witness his innocence. On the other hand, the light and flippant use of the name of a deity in asseverations, as in the *μὰ Δία* or *νῆ τὸν Δία* of the Greeks, or the *mehercle* or *edepol* of the Romans, or the name of God following the particle *by* (*bei*) in English or German, would be considered as profane swearing, although its moral quality will depend upon the accepted code of the age or country in which it is used.

<sup>1</sup>A paper read at the meeting of the Western Philosophical Association at Lincoln, Neb., January 1, 1901.

The psychology of these asseverative phrases is comparatively simple and need detain us but a moment. Truth-telling is a modern virtue and represents a highly developed civilization. The mendacity of former days and of primitive peoples is well known. Under such circumstances, truth and honesty, whether real or pretended, protect and assert themselves by the strongest appeals to the most sacred objects. Hence, invocations to the deities, to the holy grave, to the saints, to the sword or javelin, to the head of the emperor, to the sun or the moon. Later these phrases, which at first are solemn protestations of honesty or truth, become merely conventional expressions having an adverbial force and differing only in degree from words like 'truly,' 'verily' or 'indeed.'

It is, however, with the second kind of swearing, the ejaculatory, that this paper is chiefly concerned. From this point of view, we may then define profanity as the ejaculatory or exclamatory use of a word or phrase, usually the name of the deity or connected in some way with religion or other sacred things, having no logical connection with the subject in hand, and indicative of strong feeling, such as anger or disapproval. This definition we may accept with sufficient latitude to include the severer forms of profanity, such as cursing, vituperation and blasphemy, and the milder and more common forms, such as the mere interjectional use of words and phrases that have lost their once sacred character.

Since any theory or explanation of profanity must, of course, rest upon the facts to be explained, a brief summary of the more obvious facts will be the best introduction to our study.

The words and phrases used in profane swearing we may roughly divide into seven classes.

1. Names of deities, angels and devils. Such as Indra, Zeus, Jupiter, God, Lord, Christ, Jesus, the Devil, Beelzebub, etc. In this class should be included the numerous corrupted or euphemistic forms of the above names, such as gad, egad, gol, gosh, deuce (*Deus*), *potz*, law (Lord), etc.

2. Names connected with the sacred matters of religion, such as *sakrament*, *kreuz*, the holy mass, zounds (God's wounds), etc.

3. Names of saints, holy persons or biblical characters, such as holy Mary, holy Moses, holy Peter, Jehosaphat, etc.

4. Names of sacred places, such as Jerusalem, the holy grave, the land of Goshen, etc.

5. Words relating to the future life, such as himmel, heavens, hell, bless, damn (with its numerous corrupted or euphemistic forms, like darn, dern, dang, demn).

6. Vulgar words. Words and phrases unusual or forbidden by polite usage.

7. Expletives, including words or phrases having unusual force for various reasons, such as mercy, goodness, gracious, for pity's sake, confound it, hang it, *tausend*, *million*, etc. Many of these will be found to be fossil remains of religious terms or of ejaculatory prayers, such for instance as mercy and gracious.<sup>1</sup>

I shall return later to the question of the common quality possessed by these words adapting them to the use of profanity.

Continuing our inductive study, we should next notice the history of profanity. Here our knowledge is scanty and fragmentary.<sup>2</sup> The history of profanity, so far as it is written, is bound up with the history of religion, profane swearing having prevailed at those times and among those people where great sacredness has been attached to the names of the gods or to matters of religion. This rule does not, however, apply to ribaldry and vulgarity, which under many circumstances are included in profanity and constitute a very objectionable form, but rather to profanity in its ordinary sense, such as cursing, blasphemy and execration. The psychological grounds for this relation between profanity and religion will be apparent as we proceed. Hence it was that among the Hebrews the vice was so common and so offensive that its prohibition found a prominent place in the decalogue, while in the Levitical law it was punishable by death. For a like reason, among the less serious Greeks the vice was uncommon, being practically lim-

<sup>1</sup> For full lists of common swear-words and their supposed origins, see Small, 'Methods of Manifesting the Instinct of Certainty,' *Ped. Sem.*, V., 313.

<sup>2</sup> Sherman, in his quaint book entitled 'A Cursory History of Swearing,' throws interesting side-lights on the subject. I am indebted to him for some of the facts under this head.

ited to a few asseverative phrases, and although the abuse of these was ridiculed at Athens and forbidden in Crete where Rhadamanthus made a law that the people should not swear by the gods, but by the dog and the goose, and the plane tree, it was never taken very seriously. The Athenian boys, for instance, were allowed to swear by Hercules, but only in the open air. In Rome, custom allowed the men to swear by Hercules and the women by Castor.

In modern times it is again in serious and religious England and America that the vice has most prevailed. In England in the thirteenth and fourteenth centuries, after the monkish teaching had implanted a vivid consciousness of the suprasanctity of the body of Christ and of every scene connected with His sufferings, there burst upon the country a wave of imprecation in which profane use was made of the body and members and wounds of Christ and of many things connected with His death.<sup>1</sup> Fossil remains of these oaths have come down to us in such expressions as zounds, 'sdeath, bodikins, ods bodikins, etc. The significance of this historical circumstance will be seen when we discover that the psychological value of an oath depends upon the force of the 'shock' which it is capable of giving.

After the Reformation swearing in England took on a different coloring. It was sonorous in sound and was assumed to be manly. It smacked of the field, the army and the court. Elizabeth herself is said to have been proficient in the lordly art. Hotspur demands of Lady Percy, "Swear me, Kate, like a lady as thou art, a good mouth-filling oath." In the army the favorite English oath was so very common that in France and Holland the name 'Goddam' became a mere nickname for an Englishman. In the seventeenth century an attempt was made to suppress profanity by parliamentary enactments, with every kind of penalty from a fine of twelpence an oath in England to punishment by death in Scotland. At different epochs in

<sup>1</sup> We should not, however, overlook the fact that owing to the deeply religious feeling of the times, this impious language would cause great offense and distress to the more refined minds, which would find expression in the literature of the day, and coming down to us give us an exaggerated picture of the English profanity of those centuries compared with that of other times and places.



subsequent English history there have been epidemics of profanity, as in the reigns of Charles II., Anne, and George II. Until recent times it has usually been considered manly or lordly to swear, giving the swearer a certain kind of distinction. This, indeed, is noticed now among boys and some classes of men. But in general in this century profanity has become unfashionable. The stamp of vulgarity and social disapproval have proved far more effective agencies in suppressing the vice than any legislation. But the habit still widely prevails throughout the world, especially among soldiers and sailors, in the laboring classes, among the uneducated and among criminals.

We may next notice some psychological facts about swearing. Under what circumstances do men swear, and what are the subjective effects of the oath? In general, profanity is the accompaniment of anger or of emotions of the anger type. People swear when they are provoked, or annoyed, or surprised by a hurt or injury. They swear in personal encounters or altercations when actual bodily injury is not attempted, the most dangerous men not being the hardest swearers. They swear at horses as an incentive to greater exertion and at all domestic animals when irritated by them. Finally, they use oaths in any discourse where ejaculations, interjections and superlatives are demanded or where the poverty of language makes it incommensurate to the occasion. In general, we may say that the occasion of profanity is a situation in which there is a high degree of emotion, usually of the aggressive type, accompanied by a certain feeling of helplessness. In cases of great fear, where action is impossible, as in impending shipwreck, men pray; in great anger, they swear.

As regards the subjective effects of profanity, they are characteristic and peculiar. The most striking effect is that of a pleasant feeling of relief from a painful stress. It seems to be the appropriate expression for certain mental states and is accompanied by that satisfaction which attends all emotional expression. To take a simple illustration: Even men who do not swear can by a sort of inherited instinct appreciate the teleological relation existing between the behavior of a refractory collar-

button during the hurried moments of dressing for some evening function and the half-smothered ejaculation of the monosyllable 'damn.' The word seems to have been made for the occasion. The feeling of annoyance in this case is slight and the instance trifling, but in more serious affairs under the influence of great anger the mental stress demands an outlet which the oath seems to afford in a striking manner. One is reminded of the Aristotelian doctrine of *κάθαρσις*, and tempted to think that its application is more fitting here than in respect to the action of the drama. A forcible illustration of the instinctive desire to relieve the overburdened soul by the use of swear-words came to my notice lately. A friend of mine, a clergyman, has a boy of six years, a sturdy and combative child, but of good habits and careful training. One day, having suffered some serious childish trouble with his playmates, he came in and said, "Mamma, I feel just like saying 'God damn'; I would like to say, 'Jesus Christ,' but I think that would be wrong." This pacifying and, so to speak, purifying effect of profanity is one of the phenomena which any theory of swearing must take into account. It is observed also in other forms of emotional expression, as for instance in the 'good cry,' whose purifying effect in relieving the tension of grief or anger is well known. H. Campbell, writing on the physiology of the emotions, says, "The shouting and gesticulation which accompany an outburst of passion act physiologically by relieving nerve tension; and, indeed, as Hughlings Jackson has suggested, swearing may not be without its physiological justification. Passionate outbursts are generally succeeded by periods of good behavior and, it may be, improved health."<sup>1</sup>

Certain facts also in the field of abnormal psychology must be taken account of in any theory of profanity. Mental pathology confirms the evidence of philology that profanity is one of the oldest forms of spoken language. In progressive aphasia, profanity is often the last form of speech to be lost and aphasic patients who can swear oftentimes cannot repeat the profane words from hearing. The oaths slip out quite reflexly. Reformed swearers revert unconsciously to their profanity in moments of great excitement. In automatic writing, in trance

<sup>1</sup> 'The Physiology of the Emotions,' *Nature*, Vol. XVI., 306.

utterances, in the language of instinctive criminals and in subconscious and reversionary psychoses in general, profanity, usually of the milder sort, has a conspicuous place. These phenomena afford at least some ground for the belief that profanity is an ancient and deep-seated form of expression standing in close organic connection with gesture language.

Keeping in mind, then, the principal facts to be explained, let us proceed to consider the theory of profanity. Previous to the fruitful discussion which followed upon the James-Lange theory of emotion and in general acceptance of Darwin's theory of expression, the explanation of profanity, had it been attempted, would doubtless have proceeded along the following lines: Profanity is an expression of emotion, particularly of the emotion of anger. Anger, like other emotions, has as its physiological accompaniment an inner turmoil, an increased metabolism in the nervous centers, an increased excitement and stress, seeking an outlet in motor channels. Profanity is one of the many forms of the outburst of this inner excitement. Why the surplus released nervous energy escapes through this particular channel is to be explained partly by the law of serviceable habit and partly by the constitution of the body. The natural and primitive form of expression of anger is combat, involving a supreme effort of the whole muscular system and high-pressure activity of the heart and lungs. The inhibition of these earlier forms of reaction makes other outlets necessary. The organs of speech serve well as such drainage channels. Animals in anger may fight, but if actual fighting is impracticable, they may snarl or growl or bellow or scream or roar. Men in anger may perhaps be obliged to repress every overt act and every expression of their emotion except facial movements or some form of vocalization. Profanity is therefore a safety-valve; it represents partial inhibition; if the man did not swear, he would do something worse. It may be likened to the engine blowing off steam. Why the vocalization takes the form of the profane oath may also be explained upon the same principles. Oaths are more forceful and give greater vent to the inner turmoil than less sacred words. In the same way we understand why the voice in profanity is usually loud and high. The sub-



jective pacifying effect of the oath, the feeling of relief, is also readily explained from this point of view. It is an actual physiological relief of a central stress. It is a kind of purgation.

This explanation is open to criticism at nearly every point. As popularly understood, it regards the emotion as a kind of psychic force which is aroused by the perception of an object and seeks an outlet. If interpreted physiologically, it presupposes a central excitement or diffusive wave of energy which is itself unexplained. It involves, too, the doubtful theories of accidental discharge and drainage channels. It assumes finally that the emotion precedes the expression and is not mediated by it.

Other recent theories of emotion, such as the James theory<sup>1</sup> or the Sutherland theory,<sup>2</sup> afford us scarcely more help in explaining profanity and its relation to the emotion of anger. The former, which, as originally stated, finds the emotion to be the sensational outcome of the outer expression, or, as later explained, of certain idiopathic changes or 'visceral stirrings,' does not of course make any attempt to explain the expression itself, and is in any case in serious conflict with one of the most important phenomena of profanity, viz., its alleviating or purifying influence. The latter, which locates the emotions in the sympathetic nervous system and finds their physical basis in changes in the vascular tone of the body, encounters like difficulties in the present application and some others peculiar to the popular view first mentioned. The theory of emotion and its relation to expression offered by Professor Dewey<sup>3</sup> is free, as far as the case under discussion is concerned, from the difficulties of the other views. This theory, which the author calls a modification of that of Professor James, discourages the belief in 'accidental discharges' and 'drainage channels'; it holds that all emotional attitudes are either purposeful and adjusted movements, or else disturbances or alienations of adjusted movements, and that the distinctive psychical *quale* of emotion is always the result of

<sup>1</sup> 'Principles of Psychology,' Chap. XXV. PSYCHOLOGICAL REVIEW, I., p. 516.

<sup>2</sup> 'The Origin and Growth of the Moral Instinct,' by Alexander Sutherland, London, 1898. Chaps. XXII., XXIII.

<sup>3</sup> PSYCHOLOGICAL REVIEW, I., 553, II., 13.



obstructed activities, that is, of the inhibition, tension or effort, involved in the readjustment of former habits of action to present necessities. So far as the following discussion involves any theory of emotion and emotional expression, it will be based upon the theory last named.

It is evident, then, that we can no longer say that profanity is an outburst of emotion and use the emotion of anger to explain it, nor can we affirm that the vocal organs are simply easy drainage channels for excessive nervous discharge. The vocal ejaculation must be more directly connected with its object, *i. e.*, with the perception of whatever evokes it. Profanity, therefore, can be explained only by the genetic method. It must be shown to be a useful form of reaction, at least in the beginning.

If we consider very simple forms of animal life, we may say that the activities of the individual are of three kinds, those connected with the procuring of food, with protection from enemies and with reproduction. So long as these activities are normal there would be no psychical accompaniment which could be called emotion; at most, it would be pleasure or pain. Again, all these activities involve a sensory mechanism and a gradually perfected coördination between the sensory and motor mechanism, and this coördination we may call habit. The failure of this coördination, conscious or unconscious efforts at readjustment, may have for its psychical equivalent something that we may call primitive emotion. It is more nearly related to pain than to pleasure. Let us now confine our study to the second of the above mentioned forms of activity, that connected with protection from enemies. These activities will take two forms, which we may call combat and escape, the latter including flight and concealment. Failure to coördinate the usual sense impressions and muscular reactions leading to flight or concealment will be accompanied by the primitive emotion of fear. A like failure to coördinate the usual sensory and motor elements connected with combat will be accompanied by the emotion of anger. Let us again confine ourselves to the reactions connected with combat. These reactions will need constant readjustment to adapt them to changing environment connected with and leading to changes in bodily structure.

Suppose an animal to be attacked by an enemy of superior strength under circumstances where escape would be impracticable. Any modification of the usual reactions of combat of such a character as to induce in the opponent reactions of flight will be of distinct advantage to the combatant, and therefore used and preserved. Darwin's illustrations will at once occur to us, such as the display of teeth, or reactions which are designed to increase the apparent size of the combatant, such as the erection of the hair or feathers, or the arching of the back. Under this head we may include all kinds of *noises* which an animal may make in order to 'strike terror to the heart' of the opponent, such as the growl, the snarl, the roar, the bellow and the hiss, all of which are, like the curse or oath of anger in human beings, harmless in themselves, but useful as indirect means of defense, since they induce in the opponent the reactions of flight instead of combat. It is conceivable that the faculty of phonation arose originally in this way, as a modification of the organism useful in defense against a more powerful foe. In that case the earliest form of speech would be the ejaculation of anger. It is probable, however, that a more careful inquiry into the origin of phonation will lead us to a somewhat different conclusion.

Fortunately the present discussion does not involve us in the ancient and wordy controversy on the origin of language. The valiant defenders respectively of the bow-wow theory, the pooh-pooh theory, the ding-dong theory, the yo-heave-ho theory, the music theory (which we might christen the tra-la theory), all agree in this—that vocalization of some sort preceded articulate language by vast periods of time. Take for instance the bow-wow or onomatopoeic theory. If a dog was named bow-wow by a hypothetical *homo alalus* because of his bark, it is assumed that the dog himself already possessed a faculty of vocal expression. One wonders then to what extent this faculty of vocalization had developed previous to the time when it became 'speech.' If the dog or other vertebrate could express his anger by a growl, we can hardly doubt that 'speechless man' was very far from speechless at least as regards his emotions, or that he possessed a considerable emotional vocabulary. The tedious discussion

about the origin of language has been very much a waste of words for the reason that it has been conducted usually from the standpoint of philology rather than from that of genetic psychology. In reality it has been a discussion about the origin of conceptual thought and has had to do with a relatively recent period in human development. If we go back of this period, we see at once that primitive man must have possessed an extensive and useful vocabulary.

It would be possible to make a trial list of the forms of vocalization which would be useful to a species of animals, assuming only that the sense of hearing was possessed by its own and other species. There would be for instance the cry of pain, the scream of fear, the shout of joy, the growl of anger, the 'song' of love, and finally the articulate word as expression of thought. None of these are to be considered as mere expressions of emotion, not even the shout of joy. They are merely useful activities, all of them probably being forms of communication. The cry of pain for instance brings food and aid to the young. The 'song' of love, including all forms of vocalization that are pleasing to the ear, is useful in alluring the desired mate. The scream of fear is a warning of danger, while the growl or snarl or roar of anger is useful in putting to flight an opponent. Now the human analogue of the growl or roar of anger is the profane oath, and carrying out the list of analogues we shall have as the various species of human vocalization weeping, screaming, laughing, swearing, singing and talking. The accuracy or completeness of such a classification is unimportant for our purpose. It may be left to anthropologists. But it suggests the wide extent and primitive character of vocalization as contrasted with mere articulate speech. Furthermore, when we reflect that these various forms of vocalization are not mere expressions of feeling, but life-serving forms of communication, we see that they may properly be included in the term language, and the problem of the origin of language takes on a different form and a much simpler one. Moreover, much of the wealth of this primitive, so-called emotional, language has come over into articulate language in the form of the most various modulation, intonation and accent, so that the *meaning* of an



articulate phrase or sentence may be tenfold more than the mere conceptual equivalents of the words.

The saner treatment of this problem from the standpoint of psychology is illustrated in Wundt's new work on *Völkerpsychologie*, the first volume of which, on language, has now appeared. According to Wundt, the order of language development is as follows: (1) expressive movements, (2) gestures, (3) natural sounds, (4) primary interjections, (5) secondary interjections, (6) vocatives, (7) imperatives, (8) onomatopoetic words.<sup>1</sup> The above considerations are of interest to us mainly in revealing the primitive character of the interjection and the ejaculatory expression of anger. They suggest an explanation of the facts cited above concerning the instinctive and reversionary peculiarities of profanity.

We now approach our last problem, and we find this easily solved from the standpoint which we have gained. What is the explanation of the peculiar words used in profanity? These we have seen to be in our era the names of deity, of holy things and places, religious terms of many kinds, and finally vulgar words. Recalling our classification of these terms, do they possess any quality in common which makes them serviceable as expressions of anger, *i. e.*, as means of offense? Yes, they possess that which all weapons possess, the power of producing a shock in the one against whom they are directed, that is, they are all 'shocking.' According to the law of selection which we are applying, the vocal accompaniments of anger will always be those sounds or words which are most terrifying. Before the advent of conceptual language we may expect phonation which is merely loud or which suggests natural enemies or destructive agencies, such as the roar of the storm, the crash of the lightning or the growl of the thunder. When articulate language appears, we shall have the *names* of these destructive agencies, together with the vocal stress and intonation of the original expressions. Hence 'thunder and

<sup>1</sup> It should not be inferred that Wundt adopts the old interjectional or pooh-pooh theory of the origin of language, nor that that theory receives any especial support from this article. The question has become much broader. For Wundt's discussion of the general problem of the origin of language, and his 'developmental theory,' see *Völkerpsychologie*, Vol. I., Part II., Chapter IX.



lightning,' 'donnerwetter,' 'Gottes donner und blitz,' or 'potz tausend,' or 'tausend donnerwetter,' where the oath is made as awful as possible by the appeal to mighty numbers. If there is anything upon which the imagination has been accustomed to dwell with peculiar dread, or fear, or awe, its serviceableness in producing a shock is still greater. Hence the particular effectiveness of oaths relating to future punishment, such, for instance, as the English word 'damn,' or the expression 'hell and damnation.' Owing to physical disability, or to social or legal restraints, the angry man may not be able to inflict actual bodily harm upon his adversary, but he can with impunity and much satisfaction condemn him to eternal punishment and in doing so make his voice as awful as his vocal capacity will permit, and fortify his curses by invoking the terrible name of God or making rash use of phrases which are holy or sacred or usually forbidden. In some of the German accumulative oaths we have a mere promiscuous piling up of many of these 'strong' words, as for instance in this: *Alle Weltkreuzmohrentausendhimmels- und granatensakrament*. If this is 'wielded' with sufficient force, one can imagine the enemy to fall before it as before a double-edged sword.

We are thus able to understand why the forms of profanity vary with the age and people. When long monastic teaching has given an unspeakable sanctity to the cross and body of Christ, or to the holy sacrament, these words become the material for oaths. Even at the present time the word Jesus used profanely gives us a greater shock than the word God. A German peasant who left the train for a moment at a small station on his journey returned just in time to see the doors of the cars shut by the guards. As soon as he realized that he was left, he stopped with a look of helplessness, and then with great emphasis he pronounced the one word '*Sakrament*.' At other, time and among other peoples greater sacredness may attach to wholly different things, as, for instance, to the sword or javelin, or to the sun, or moon, or to the chief, king or prophet, and then these names furnish the profane vocabulary. The sacredness attaching to asseverative phrases and to legal oaths makes these again serviceable for profanity, and we hear them on every side

of us. Finally, there are certain words that are very vulgar, that polite usage forbids. These again are in a way sacred. Their open use indicates great daring or recklessness, or that the user is so beside himself with passion that he must be in a very dangerous mood. Hence these words produce the desired shock and become serviceable for profanity.

Summarizing the results of this study, profanity is only to be understood by the genetic method, the point of departure being the growl of anger in the lower animal, which is not an expression of emotion, but a serviceable form of reaction in cases of combat. It belongs therefore to a primitive form of vocalization, and hence is ancient and deep-seated, being one of several forms of speech preceding articulate language by an indefinite period of time. By a process of selection it chooses at all times those forms of phonation or those articulate words, which are best adapted to terrify or shock the opponent. The words actually used in profanity are found to have this common quality. Although originally useful in combat, the occasion of profanity at the present time may be any analogous situation in which our well-being is threatened, as in helpless distress or disappointment. There is always, however, some object, though it may even be one's self, against which the oath is directed.

Profanity is a primitive and instinctive form of reaction to a situation which threatens in some way the well-being of the individual, standing next to that of actual combat. Like all instinctive reaction it does not generate emotion but allays it. The emotion arises where the reaction is delayed or inhibited. We are thus able to account for the *katharsis* phenomena of profanity. It seems to serve as a vent for emotion and to relieve it. It really acts as a vent only in this sense that it brings to an end the intolerable period of inner conflict, of attempted inhibition, of repression and readjustment, and allows the 'habitual attitude' to assert itself. The relief is only that of any completed activity. The '*Sakrament*' which the peasant uttered completes in a certain sorry fashion the activity which should have been completed by his entering the carriage. The emotion, his disappointment and chagrin, are the accompani-

ments of other inner idiopathic changes, the result of inhibitory effort, and are decreased, not increased, by the oath.

If then the oath is a form of instinctive reaction, and even a purifying agent, why is it considered to have an immoral quality? For two reasons: first, because advancing civilization bids us evermore inhibit and repress, and secondly, because of the unfortunate but inevitable connection between profanity and the sacred names of religion.

## ART, INDUSTRY AND SCIENCE.

### A SUGGESTION TOWARD A PSYCHOLOGICAL DEFINITION OF ART.

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The following is a suggestion toward a psychological definition of the field of art. By *psychological* I mean that the definition is derived from immediate observation of the art-consciousness as it appears in contemporary life rather than from the history of art; as such it can offer only a partial view of the subject. And I call it a definition of the *field* of art to indicate that it deals only with the boundaries which separate art from industry and science—not with the basis of art-values. The distinction that I have in mind is not that of the beautiful from the ugly, but of the beautiful (or ugly) on one side from the good (or bad) and the true (or false) on the other—in other words, the category of beauty from that of use, goodness, necessity and practical serviceability, all of which are represented by the term *industry*, and also from that of truth and reality, indicated by the term *science*.

There are two possible attitudes to be taken toward beauty, according as it is conceived to be related or unrelated to truth and usefulness. The tendency of both esthetic theory and popular thought has, I believe, been mainly in the direction of the latter alternative,—in the direction, namely, of defining beauty as a unique quality, and art-values as irrelevant to the standards by which we measure practical usefulness and scientific fact. The impulse toward artistic creation is regarded as a derivative from the ‘play-impulses’ and as a part only of our holiday-life; and beauty is conceived as in some sense an illusion. As opposed to this view, the theory that I shall suggest conceives art, on the one hand, and industry and science, on



the other, to be successive phases in the evolutionary process ; art and industry are conceived as successive phases in the development of impulse ; art and science as successive phases in the development of cognition. According to this view beauty is related both to truth and practical serviceability.

## I

We shall first examine the relation of art and industry. My argument here will begin with showing that there is no objectively distinct class of esthetic impulses or of objects of beauty, such as we assume in defining beauty as a unique quality, but that the line separating the beautiful from the useful is a function, in the mathematical sense, that is, varies with the stage of culture reached in the agent for whom the distinction is made. It will be convenient to open our inquiry with an examination of the basis upon which the objective distinction is usually made. It is claimed that, while men vary in their range of esthetic susceptibility, they have a common measure of practical necessity. This common measure rests upon an identity, or an approximate identity, of physiological structure ; inasmuch as men are physiologically alike they have like necessities of life. The conditions of the preservation of life constitute, then, an objective mark of distinction between practical necessity and esthetic taste.

But what these objective necessities may be no one seems to know. I do not deny that objective signs of disease have been established by medical science, though again, for the purpose of diagnosis, an account of the subjective feelings is usually not less important ; but though the presence of certain symptoms be a sure sign of disease, their absence is by no means a sure sign of health. And the conditions of health seem incapable of objective definition. Whether to drink tea and coffee, or milk ; to use alcohol or tobacco ; to dine at midday or at evening, to rise early or to yield to an inclination to sleep late, to eat before going to bed, to take cold baths, to lead a regular life or a life of change—upon all of these points it seems impossible to give any general directions. A physician when consulted in regard to a particular case would probably decide with reference to the

peculiar constitution of his patient. This means that the identity of human physiological structure is in a measure superficial. What is one man's meat is another's poison, or what is meat for one man is for another merely, so to speak, a condiment.

If we attempt to define the necessities of life so as to include all that may under any circumstances prove to be necessary, we shall cancel the distinction that gives the term its meaning; for there is no object ever desired by a human being which may not be for some one a necessity. It is customary to assume that the only really vital activities are those of nutrition and reproduction, nutrition for the maintenance of the individual and reproduction for the continuance of the race. But is race-survival all that makes reproduction a necessity? Surely it is not unrelated, or even distantly related, to individual health. And the same holds for reproduction in its broader sense as it stands for home and family; granting that many persons lead apparently normal and successful lives without marriage, it is none the less true that for most persons marriage is a condition of practical efficiency and, ultimately, of health and life. In view of the tragic sacrifices which men frequently make for the love of women, and women for the love of men—sacrifices not merely of wealth, but of honor, ambition, and even of life itself—it would be surely paradoxical to describe the romantic passion as, for the individual, a purely esthetic impulse. But the same may be said for mere companionship; it is for most persons a condition of relaxation after the strain of work and, as such, hardly less necessary to health than sleep or exercise. And that which, in the narrower sense, we call culture may prove equally a necessity, though it is true that most men exist without it; for, to a person of keen sensibility, an atmosphere of culture is a normal stimulant, like a cup of coffee at breakfast—and not a stimulant only, but a food, since a barren and uninteresting environment means eventually an impairment of health and a consequent shortening of life. But the same holds to an extent of any impulse whatever, however absurd and capricious. A child when denied a favorite toy may cry himself ill and the illness may conceivably become serious. And apparently there

is no object so worthless that the failure to obtain it may not be for some one a cause of suicide.

On the other hand, if we limit the necessities of life to those that are strictly universal, we shall cut out nearly all the activities found in civilized life. Strictly necessary food, for example, would lack most of what necessary food means for western civilization. For most Americans it means meat more than once a day, for the German peasant rye-bread and potatoes, for some Asiatic peoples an unvarying diet of rice and millet. And no one has yet discovered the minimum upon which life could be supported. Similarly, the clothing which seems to us so necessary to life and health could hardly be accounted a strict necessity, since the Fuegian, who lives in a fairly cold climate, is able to support life without it.

If we take a middle course and attempt to define a normal or natural standard of necessities as distinct from an artificial standard, we reach the result that I have already indicated, namely, that the conception of vital necessities is purely relative. For the necessities of a Fuegian are not in any sense a measure of our own necessities. It is conceivable, indeed, that we might gradually accustom ourselves to an absence of clothing—but not, I believe, while maintaining our cultural activities on their present level. For a combination of the complexity and fineness of organization which distinguishes the civilized man with the hardihood that makes nudity possible for the primitive man appears to be psychologically impossible. Clothing protects the civilized man not merely from cold, but from heat and from unpleasant contact with all kinds of material objects; and the delicacy of organization which demands this protection is also a condition of superiority in sensory discrimination and intellectual power. So, too, we might accustom ourselves to the Asiatic diet, but not without at the same time changing our whole manner of life. The diet which suffices for the placid and indolent nature of most Asiatics would hardly support the varied and intense activities of our western, and especially our American civilization. Similar relations may be observed between the several occupations of our civilized life; salt pork, which for a farmer or sailor may



prove wholesome and nourishing, is clearly an impossible diet for a teacher or student. Our standard of necessities—and hence our distinction of necessities and ornaments—depends, therefore, wholly upon the nature of the activities to be supported. The conditions for the maintenance of life are as various as the kinds of life to be maintained.

We have now to express this result in terms of the process of development. The view that I venture to suggest is the following: the esthetic or practical character of a want, the beautiful or useful character of an object, the artistic or industrial character of a form of activity, depends upon the extent to which it constitutes a fundamental feature in one's organized system of habits. As an illustration of such a system we have the co-ordination of activities in the human body. To apply the conception to the system of impulses, let us suppose for the moment that the impulses toward, say, food, sleep, work, and physical exercise constitute within themselves a closed system of such a character that the gratification of each depends upon the gratification of all the others. And this picture is in a measure true to fact, inasmuch as the capacity for work and exercise depends upon food and sleep, sleep upon food and exercise, and food, in turn, upon work. But the human organization is not a closed system of this simple kind. Outside of the group of impulses relatively fundamental to any kind of activity there is a large number only incompletely related to the organic system. In addition to work we have other relatively serious interests, such as art and literature; in addition to pure exercise there are athletic sports, summer outings and journeys abroad; and besides food, in the strict sense, there are feasts, where every one wears his best and where most of the preparations appeal distinctly to esthetic taste. Now the difference between those two kinds of gratifications depends upon the extent to which they condition the group of activities as a whole. Ordinarily one may live without salads, and wines, and candles, and flowers, but to miss one's dinner altogether may mean a headache and complete temporary prostration. There are thus some impulses that form an integral part of the organic system, while others are related to it in a merely occasional and indefinite way. But the



distinction is of degree only. All that we can say is that there are some impulses *more* nearly related to the life-process than others. Nothing that appears in human life is completely unrelated to the organic processes. Food is in general more immediately necessary than table-companions; but a continued practice of eating alone is likely to impair one's digestion. And one cannot continuously abstain from the pleasures of social life without reducing one's efficiency and ultimately one's health. Insanity may be due as well to a deficiency of social stimulus as to an excess of social excitement. Everything that in any way interests us affects the organic processes in some way and has therefore a relation to the life-process itself. We have, therefore, instead of an absolute distinction of esthetic and practical qualities, a graded continuum, with the distinctively practical at one end in the needs more nearly organized into a system, and the purely esthetic at the other in those least related to the organic system, while between the two are those whose esthetic or practical character is a matter of doubt.

But the full significance of the distinction appears only when we regard human life as a process of growth, and the human being, not as the stable product of a completed process of evolution, but as an evolutionary process himself. The transformation of esthetic tastes into relatively organic needs appears then as one of the features of this process. Typical cases of such transformation are to be found in the evolution of social custom. For example, twenty years ago in this country a dress-coat was hardly so constant and necessary a feature, even of fashionable life, as it is now. For the mechanic or day-laborer it would still be regarded as an adornment and possibly an undesirable one. But for those who would move in polite society it is surely a condition of social intercourse; and so far as social intercourse of a specific kind is a necessity of life—and I have already pointed out how far that may be the case—one must wear a dress-coat if one would live a normal life, and possibly if one would live at all. In the same manner, convenient bathing and other sanitary appliances have come to be regarded as necessities among cultivated persons; they are still regarded as luxuries in most parts of Europe; and in many parts of our

own country the demand for modern sanitary equipment would still be resented as fastidious. Closely related to this development, the demand for personal cleanliness has become throughout society at large gradually more exacting. And perhaps nothing illustrates so well the transformation of esthetic into practical. The evolution of any particular requirement of cleanliness—a daily bath, for example—passes clearly through the stages, first, of an evidence of refined taste, then of a requirement of good breeding, and finally of a condition of health. Again, fine china and table linen, changes of plates for different kinds of food, are primarily esthetic requirements; they are not supposed to be necessary for nutrition. But they tend clearly to become so, since it seems to be the experience of most persons that a continued abstinence from the niceties of a well-ordered table tends to destroy one's appetite for the food itself. The same process is observable in fields more distinctly recognized as esthetic. The cultivation of a good quality in the speaking voice would have for most persons a purely esthetic value, but for a cultivated ear a harsh voice is a source of marked discomfort, comparable only with that produced by a cold room or a straight-backed chair. A good voice is at present merely one of the finer marks of a cultivated person, but it tends, in the development of social intercourse, to take its place among the elements of good breeding and the necessities of social life. A want of harmony in combinations of color is also, for many persons, productive of a discomfort amounting nearly to physical pain. The tendency of all such needs to become general and imperative is illustrated by the demands, now become loud, for the control of city-noises and the removal of advertising bill-boards. A few years ago the latter demand, at least, would have been condemned as fastidious. Thus it happens that needs that were at first purely esthetic, and as Kant puts it, disinterested, become interested and organic and hardly less necessary to the life-process than food or sleep.

The process of evolution means, from the standpoint of psychology, a constant elaboration of wants. The difference between the savage and the civilized lies in the number and complexity of the conditions that must be fulfilled in order to render

life efficient, enduring, or even possible. Human life is being gradually improved in the same manner as we improve a machine, say, a steam-engine or a typewriter. Such a machine never fully meets all the demands that are made of it. Each newly felt need leads to a study of the whole system and a possible re-organization in order to meet it. The satisfaction of the present want, it is felt, will make the system ideally complete. But this result is never reached. As each new demand is satisfied—for example, the attainment of an academic degree, or the ownership of a home—at least one more requirement is discovered which must be satisfied to make life truly complete. The same is true in the history of a race or a nation; the attainment of independence and the abolition of slavery in our own country have not at all resulted in the extinction of social problems. Now the transformation of esthetic into practical is one side of this elaboration of wants. As a new impulse makes itself felt, and a new object is desired, it has first the appearance of the ideal and the beautiful; then, as the impulse becomes more imperative, the object loses the appearance of beauty and ideality, and becomes finally a necessary element in the life-process itself.

## II.

A similar result is reached in the analysis of the relation of art and science. For psychology this is a relation between the appreciation of beauty and the cognition of truth and reality.

No sharp line is to be drawn between appreciation and cognition. The appreciation of the beauty of a picture is, indeed, far removed from the cognition of relations within a geometrical figure, but between the two there are numerous intermediary terms. The acquisition of a foreign language, for example, is not quite a process of cognition in the sense that this is true of geometry or physics, yet it is not wholly unrelated to cognition; on the other hand it is not at first sight an apprehension of esthetic relations, yet its success demands a subtle linguistic sense, the value of which any teacher will recognize. Again, every physician knows that a successful diagnosis is as often the product of shrewdness and skill—again of a sense—as of ana-



lytic reasoning. And the same holds true of the solution of the mathematical problem. Any one of normal capacities can test the reasoning given in support of a geometrical proposition, but to solve problems is often a work of art.

The line separating appreciation from cognition varies from one individual to another. To the critic, much that, to the layman, produces the effect of beauty becomes mere technique. The critical study of works of art may, and indeed does, bring one within range of elements of beauty inaccessible to the uncritical observer, but it also dispels the illusion that has hitherto rendered many of the elements beautiful. Indeed, it is a commonplace that those who experience the greatest emotion in the presence of a work of art are not those who understand it best. The relative impassiveness of the critic is due to the fact that he 'understands,' that is, has worked out in detail, the means by which many of the effects have been produced, with the result that, though he judge them correct, they fail to produce in him the specifically esthetic effect. Instead of *feeling* them as elements of beauty he *sees* them clearly and analytically as elements in a sequence of cause and effect. If he could carry the process of analysis so far that there would be no effect the ground of which he did not fully understand there would also be, for him, no beauty. All things would then be judged from a purely rational standpoint, as elements in a causal series.

This brings me to a statement of the relation between appreciation and cognition: whether an object be apprehended as a work of art or as a fact of science depends wholly upon the extent to which it is apprehended in analytic detail, that is to say, upon the extent to which all the features of an object—using this word to cover all possible relations contained in the object—can be explicitly stated. The difference between esthetic taste and reason or cognition is thus one of degree only. Let us look at this relation more in detail.

I assume that the apprehension of an object is a mental process with successive stages. The exact repetition of a mental state is something that never occurs; a chair or a house is never for our consciousness twice the same. Each appearance in consciousness is an appearance of something new, of some



relation contained in the object which was not previously apprehended. Each appearance is thus a more advanced stage in a continuously progressive movement which begins with a relatively vague and advances toward a relatively clear conception of the object.

Now, in the course of this process, the sense of beauty appears with the dawning conception of a form or meaning. A sense of order and arrangement is a condition not merely of the appreciation of beauty but of any apprehension of an object as such. A formless grouping of details, like the rocks on a New England farm, is not merely not beautiful; it is not even capable of detailed description. However, the relation of form to beauty is the only one that concerns us here. Form and significance are the first condition of beauty. A chance grouping of rocks may indeed be appreciated as beautiful, but not while they remain a merely formless aggregation. Nature of herself offers nothing that is beautiful. The beauty of a landscape is the creation of the artist or beholder, who, by selecting his point of view and limiting his field of vision, derives from a relatively formless nature a picture which, for his purpose, is internally consistent and harmonious. In the same manner the appreciation as well as the creation of a work of art begins with the first vague apprehension of form. The recognition of beauty thus arises at a particular stage in the process of apprehension. It is not the first stage. The sense of beauty is not the spontaneous and immediate reaction to the presence of a particular object; on the contrary—and this is particularly true of the appreciation of the greater works of art—it is the result of study, and even of effort. A child's, or any untrained ear, is incapable of appreciating a Beethoven symphony or a Wagner opera—because, as we commonly say, he does not 'understand' music. But what do we mean by 'understanding' music? Simply, I believe, the recognition, in an apparently formless medley of tones, of order and harmony. The Wagner opera offers an excellent illustration. At first hearing, the *Nibelungen* trilogy appears to be a merely fortuitous and sometimes discordant aggregation of musical notes. At this stage of apprehension it has no esthetic quality whatever. Then, as we hear it oftener, there comes to us

a vague consciousness, which never becomes quite capable of clear expression, of a deeply mysterious meaning underlying the apparently formless mass of detail; and this consciousness constitutes the sense of beauty. The works of Tschaikowsky and Richard Strauss are also a good illustration. I believe that the average of cultivated musical taste (not the critics') is just beginning to appreciate Tschaikowsky and still questions the value of Strauss. In the former case they have just begun to have the certainty of meaning; in the latter they are not yet sure that any exists. The point may be illustrated in literature by the attitude of the public toward Browning, Whitman, and George Meredith. For the public at large the works of these authors lie just on the boundary-line between the most deeply significant and the utterly meaningless.

Now when we have arrived at a sense of form, the next condition is that the form remain elusive. Appreciation of beauty begins with a sense of form, but it ends, that is, ceases to be esthetic appreciation, with a complete comprehension of the form. The appearance of the object, *i. e.*, of a complex of relations, as a candidate for esthetic judgment involves the presentation of a problem, namely, the reduction of apparently formless being to form and unity; a belief in the solubility of the problem marks the beginning of esthetic appreciation; its continuance as such demands thereafter that the solution remain incomplete. In this manner, I believe, we are to explain the enduring charm of the great work of art. "A thing of beauty is a joy forever" because it offers the perpetual stimulus of an unsolved problem—of values not yet completely realized. To most lovers of music a Beethoven symphony is always fresh and stimulating—because each new hearing is not a mere repetition of the last, but a more advanced phase in a continuously progressive mental process. This process is a constant effort to express a unity of purpose, of whose existence we feel assured, in something analogous to a diagram or a mathematical formula, to comprehend a complexity of relations in one act of thought. But the effort is never quite successful. Each new hearing results in a more detailed grasp of relations but also reveals new elements or relations not previously appre-

hended. Compared with the Beethoven symphony the popular song or novel, which is the vogue for one year and forgotten the next, is simple and readily comprehended. I do not mean that its structure is easily reduced to a scientific formula, though this seems in many cases a near possibility, but merely that it comes quickly within the grasp of the imagination. The first chapter of a novel tells the whole story; the first measure of a song starts a train of tone-imagery which anticipates all the following measures. The difference between the enduring and the ephemeral in music is not due to the merely mechanical effect of monotony. It is true that the more classical works, being usually more difficult, are less often heard. But monotony is itself due to exhaustion of meaning; it signifies that the imagination has become accommodated to every detail so that no more work is left for it to do. The beautiful is thus, within the field of meaning, the relatively complex and incomprehensible. Compare, for example, Beethoven with Haydn. By the side of some later composers Beethoven seems simple, but his work has none of the transparent and mechanical symmetry of some of Haydn's. Compare, again, an old German hymn with a modern production of Moody and Sankey. Both seem at first sight equally simple, but imitation seems by no means equally possible. The 'gospel hymn' has the appearance of being the result of a mathematical system of permutations and combinations of notes. For the cultivated taste it is no longer a thing of beauty, nor yet perhaps positively ugly. It is merely a mechanical fact, outside of the range of esthetic judgment. If anything, it is merely flat—an unsuccessful attempt at the production of esthetic effect. The approach to mathematical simplicity and symmetry marks, then, the end of the stage of appreciation and the disappearance of the sense of beauty. When apprehension reaches the point of detailed comprehension the object is no longer a thing of beauty but merely a fact of science.

The line that separates appreciation from cognition also distinguishes the artist from the workman. Art, in order to remain art, must always retain the element of the mysterious. The world is at our disposal because, and so far as, we can take it



to pieces and put it together again. A machinist, by a careful study and measurement of the parts of a machine, can usually make another like it; the work requires nothing but patience and industry. If a student of music could formulate the structure, say, of a Beethoven symphony with the same articulate detail and the same grasp of principles with which we apprehend the parts of a machine, there would be no reason why we should not all with sufficient patience compose symphonies equal to Beethoven's. Or it might be merely a matter of hard work to write as well as Thackeray. But it is needless to say that, in this case, Beethoven and Thackeray would no longer be artists. The essential differentia of the artist is the mystery of unexplained personal power, to which we give the name of 'genius.' A form of activity that is inimitable, because it cannot be formulated into rules, is an art, whether it be painting, musical composition, cabinet-making, or, for that matter, cooking.

Appreciation is thus a vague apprehension of reality. But has the relatively vague apprehension of reality in all cases the character of appreciation of esthetic quality? I believe that a careful study of the cognitive processes will show this to be the case. In all cognition there is a margin within which relations are felt rather than known; the criterion of truth in such cases is not logical but esthetic; it is ultimately a question of fitness or propriety. If we ask a biologist why he takes one side or the other in the controversy about heredity, or if we ask a psychologist why he is an associationist rather than an 'apperceptionist,' I doubt if we get a completely explicitly answer; for such convictions do not rest ultimately upon special grounds; the sum of the considerations upon which they rest involves a mass of detail not yet fully grasped in one consistent view. A candid person would probably admit that a part, at least, of his ground for holding any of the larger theories of science is simply that they appeal to him as reasonable—which means that they appeal to one of his intellectual tastes and temperament. No form of procedure is so distinctly artistic as that of the typical German *Gelehrter* in the act of formulating a theory *nach der richtigen wissenschaftlichen Methode*. Like the modeller in clay, he adds a bit here and takes away a



bit there, and the final outcome is determined, not merely by the facts at his disposal, but by his conception of the beauty and symmetry which a true theory ought to have. No one who has attempted the construction of scientific theory can fail to recognize the esthetic element involved in it. What is it that distinguishes the greater from the lesser lights in science? Not merely a greater industry in the accumulation of facts; but rather the ability to make use of a chaotic mass of fact for the construction of an ideal system. In this construction the scientist, like the artist, is guided, among other considerations, by those of ideal fitness. Nowhere is the mental process more nearly akin to the esthetic than in philosophy. The natural scientist may legitimately consider certain fundamental principles as, for his purpose, fixed, but the attitude of the philosopher is critical throughout. His facts and his theories, his logic, metaphysics, psychology, and ethics, stand in relations of mutual dependence; there is no independent ground anywhere. The final question is then a question of taste. All that a philosopher can do is to work out his theory of the world in, as far as possible, detailed completeness and then ask whether it has the symmetry and consistency which a true theory of the world ought to have.

I have dwelt upon the relative vagueness of appreciation as compared with cognition. This is, however, merely the negative view, to which there is a corresponding positive view. From the general point of view here outlined it follows that appreciation, though a more vague, is also a more subtle apprehension of relations than that found in the cognitive process. Remembering, then, that the apprehension of relations involves more or less directly the estimation of values, we may say that beauty is simply a more exactly proportioned and refined form of good than the good that we can understand and estimate in detail; ugliness is similarly a more subtle form of evil. Beauty in the human face, for example, is a finer suggestion of spiritual quality than can be reduced to scientific ethical principle; beauty in the human form is a more complete realization of the conditions of health and serviceable activity than can be outlined in physiology. Creative art is a more subtle response to human needs than that given in the organized industrial activities.

But the full significance of this conception of art appears only in connection with the conception of a system of wants suggested some pages back. The esthetic and practical were there defined as phases in the organization of human needs into a system. Appreciation and cognition are, similarly, successive phases in the organization of a scientific system of reality. Science offers us a system of reality by no means completely articulated. From this point of view we must discriminate in favor of the natural sciences, and, within the natural sciences, in favor of physics as opposed, say, to biology. But the ultimate aim of science is the incorporation of all its material into one system of reality. Now when we observe the expressions of common sense regarding the esthetic we find, I think, that the apprehension of an object remains esthetic *only so far as it finds no place for the object in the scientific system of reality*. Our repugnance to the odor of decaying organic matter is esthetic until we become aware of its relations to disease. Our liking for the odor of flowers would be no longer esthetic if it appeared to us clearly related to health, that is, if we understood those relations in complete detail. Our pleasure in the arrangement of light and shade in a room is esthetic so far as it is not felt to be related to the comfort of the eyes; recognized as a fulfilment of physiological conditions it becomes a purely scientific and practical matter of fact. If we could relate the pleasure in harmony and rhythm, term for term, to the functioning of organic processes—that is, if the habit of perceiving such relations were so fully developed as to render perception immediate and spontaneous—harmony and rhythm would no longer be esthetic qualities. And the same would be true of a pleasing literary style if it could be in all cases immediately analyzed into conditions of clearness of statement.

In the foregoing I have tacitly assumed that the distinction between art and industry is parallel to that between art and science. This implies that the development of impulses into practical needs is coördinate with the development of knowledge. Inasmuch as this assumption seems to be the presupposition of most recent psychology, I shall not attempt a special justification here. Whether knowing is, as Professor James holds, always

for sake of doing, or whether doing is the mechanical result of knowing, it seems clear that the two must develop together, since it is only our knowledge of conditions that sets the limits to our range of activity. It remains then merely to take a final glance at the two processes—the organization of needs and the organization of reality—considered as parallel and coördinate. From this point of view the process of evolution is a process of gaining control of previously undeveloped resources. These resources may be regarded as already contained in the nature of the evolving being or as conditions existing in his environment to be controlled through knowledge; for our present purpose it makes no difference which view we take. The development of human nature is analogous to the growth of a country like our own, which is new, yet not uncivilized, some parts of which are highly developed, while others are nearly quite unknown. We are constantly bringing waste land under cultivation, developing its minerals and timber; and for this purpose we are constantly exploring and surveying. Every new discovery, like that of oil or natural gas, works a change in the old civilization, reorganizing its industries and modifying the daily manner of life. The same is true of mental development. Every newly acquired taste means a reorganization of the old habits of life to prepare for its possible accommodation. And every newly acquired impression means ultimately a reorganization of our categories of truth and reality, and of the fundamental conceptions of science, to provide for its possible inclusion. The relation between art on the one hand, and science and industry on the other, is a feature of this process. So far as the element in question is new, unexplored and uncontrolled, it is an object of esthetic appreciation and the demand for it is an esthetic want; in proportion as it comes under our control it becomes an organic need, an object of industrial activity and a fact of science.

NOTE.—I believe that this conception of beauty will cover and bring into mutual relation nearly all the terms in which beauty has at various times been defined—provided the terms in question be recognized as purely relative. Thus, according

to my definition, beauty is relatively *subjective* in the sense of holding a relatively distant relation to our most scientific and objective conception of reality as conceived (*e. g.*) by physics. It is for the same reason relatively *illusory*; its values are least capable of scientific demonstration. And the illusion tends to be an *illusion of personal activity* in the sense that personal activity is that which is significant (*i. e.*, it is not pure chance) yet relatively unintelligible in terms of cause and effect. It is also relatively *disinterested* and *free from desire* merely as not yet fully incorporated into the system of practical wants. And for this reason it belongs, relatively, to our *holiday-life* and is the object of an impulse which is relatively a *play-impulse*. Artistic activity is also relatively *free*, that is, unfettered by logical and scientific rules of method—because its relation to practical life is not yet obvious. But the artist is not absolutely free from a consideration of the probabilities. The rule of license permits him, not to sin against the probabilities, but merely to keep the question of probability out of sight.



## THE ANGLE VELOCITY OF EYE MOVEMENTS.

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Until recently the question of the angle velocity of the eye, as the point of regard moves over the field of vision, was a very minor problem of merely physiological interest. So far as the authors can discover, only two experimental answers were published previous to the year 1898. These differed widely in their numerical values and were regarded at best as rather curious but insignificant fragments of knowledge. The problem first assumed psychological importance when it was pointed out by Erdmann and Dodge that the movements of the eye in reading and writing are rhythmically broken by periods of rest which constitute the moments of significant stimulation.

Out of this advance in the physiology of vision has developed a constantly growing group of psychological questions which only a quantitative knowledge of the eye movements can answer. For any adequate analysis of the complex processes involved in reading it became necessary not only to know the number of the alternating periods of movement and rest, but their time values as well. The explanation of the general failure to apprehend the periods of eye movement either as breaks in visual perception or as a fusion of the field of vision demands an accurate knowledge of their duration. Moreover, the problems of the visual perception of motion, binocular coördination, and fatigue of the eye muscles, all presuppose a knowledge of the eye movements which at present is conspicuously lacking.

In view of all this the present paper would present first of all an exact method of recording the movements of the eyes, and secondly, a group of what we may justly claim to be the first

accurate measurements of the angle velocity of the eye movements under normal conditions.

#### METHOD AND APPARATUS.

When the problem of the velocity of the eye movements first assumed psychological importance two methods had already been proposed for its measurement. Of these the method of Volkmann<sup>1</sup> was manifestly inaccurate. He reckoned the time of one movement of the eyes by dividing the total time during which the eyes moved rapidly back and forth between two fixation points, by the number of movements. This would obviously include not only the time of movement, but the interval of rest as well.

The other method was devised by Lamansky<sup>1</sup> under the direction of Helmholtz. While it gave results only in large units of measurement, it was unquestionably the better of the methods available, and as modified by Dodge gave more nearly accurate results than had hitherto been obtained. The method in brief was as follows: As the eye moved through a given arc a pencil of light was flashed into it at regular intervals. The number of flashes seen during the movement would be in direct ratio to the time of movement. Besides the error involved in the large unit of measurement, the method showed several other sources of error. Since the flashes appeared elongated in direct proportion to the velocity of the eye, flashes which fused in consequence of the slower movement at the beginning or end of an excursion would give no clue to the number of their constituent elements. Moreover, the experimentation was necessarily limited to monocular vision in a darkened room after adaptation and consequent rest, while it was apparently possible to materially reduce the velocity of movement under slightly modified conditions of the attention. All these facts rendered it doubtful if the results could be applied to the eye movements of normal vision, without correction. Our results show that the defects of the Lamansky method were not over-

<sup>1</sup> Wagner's 'Handwörterbuch,' III., 1, 1846, pp. 275 ff.

<sup>2</sup> 'Ueber die Winkelgeschwindigkeit der Blickbewegung,' *Pflüger's Archiv*, 1869, II., pp. 418 ff.

estimated, while they justify in the main the corrected values used by Erdmann and Dodge<sup>1</sup> in their analysis of the reading processes.

A material advance was made in the technique of the problem when Huey<sup>2</sup> attached a delicate recording apparatus to a Delabarre eye cup and obtained kymographic records demonstrating the number and general character of the eye movements with great distinctness. The measurements of the eye movements during reading, which were obtained from these records, were more satisfactory than any measurements by the earlier methods. However, as was pointed out in the notice of Huey's work in the REVIEW, they could not be regarded as final. In moving the long pointer at short leverage the eye muscles do no inconsiderable amount of extra work. That this should materially delay the eye and fatigue it was *a priori* very probable. A further source of error in the record was the overshoot occurring in the record at the end of every long movement. The method left it quite undetermined what part of the overshoot, if any, belonged to the movement and what part was conditioned by the momentum of the pointer.

The experimental requirements of a satisfactory apparatus for recording the eye movements may be summed up as follows: (1) It must be capable of operating under normal conditions of binocular vision. (2) It must be capable of registering both eyes simultaneously. (3) The unit of measurement must be 10 or less. (4) The registering medium may have neither momentum nor inertia, while the eye must perform no extra work during registration and be subjected to no unusual conditions. (5) The apparatus should be such as can be used to record the movements of a large number of eyes, without serious inconvenience either during or after the experiments.

None of the methods already used satisfy all these requirements or even the larger part of them. It is indeed doubtful if any attachment to the eye could be devised which would be satisfactory. But the qualities which are demanded of the re-

<sup>1</sup> 'Psychologische Untersuchungen über das Lesen,' pp. 66-67 and 357-360.

<sup>2</sup> 'On the Psychology and Physiology of Reading,' *American Journal of Psychology*, Vol. XI., 1900.



cording medium are possessed in an eminent degree by light rays, and a method of registration satisfying all the above mentioned requisites is possible with comparatively simple photographic apparatus.

The general plan of such apparatus is as follows: A sensitive film is moved evenly, in a vertical plane, immediately behind a narrow horizontal slit in the plate holder of a photographic camera. The subject's eye is brought into such a position before the camera that a horizontal plane bisecting the eye through the middle of the pupil bisects the lens of the eye and passes through the horizontal slit. If the eye were held immovable while the sensitive film is exposed behind the slit, the negative would present a series of parallel lines corresponding in cross section at every point to the light and dark parts of an imaginary line drawn horizontally across the eye, bisecting the pupil. A horizontal movement of the eye, while the sensitive film is moving behind the slit, would be marked in the negative by oblique lines. These oblique lines are the records of the eye movements and their time values may be read off from a time record marked on the sensitive film during its movement.

Photography of the eye movements in this simple way is, however, open to some serious objections. An illumination of the eye, capable of affecting the rapidly moving film, through the small slit, must be rather brilliant, and (even though the subjects experienced no discomfort), it was not *a priori* certain that the conditions of vision might not be disturbed sufficiently to affect the eye movements. Still more serious is the character of the records. The lines of demarcation between the pigmented and unpigmented portions of the eye are usually not sharply defined; and although the photographs appeared fairly clear and distinct to the naked eye, it was found impossible to read them with any accuracy when enlarged by the telescope of the cathetometer.

Satisfactory results were first obtained when, after considerable experimentation, we hit upon the plan of utilizing the eccentric surface of the cornea as a reflector. Instead of photographing the eye directly, we photographed the movement of a bright vertical line as it was reflected from the surface of

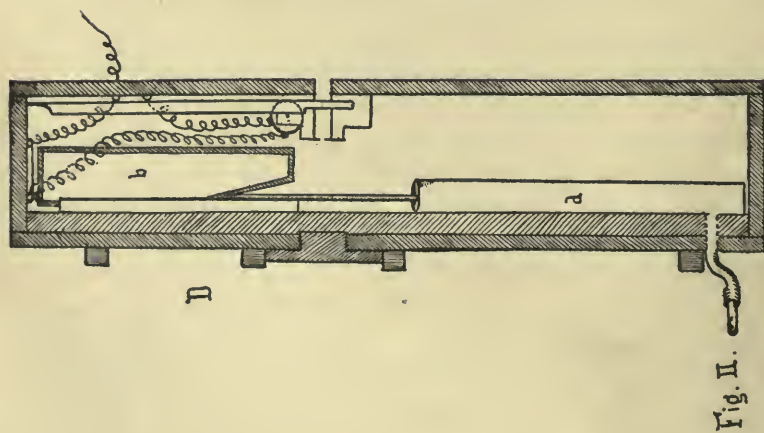
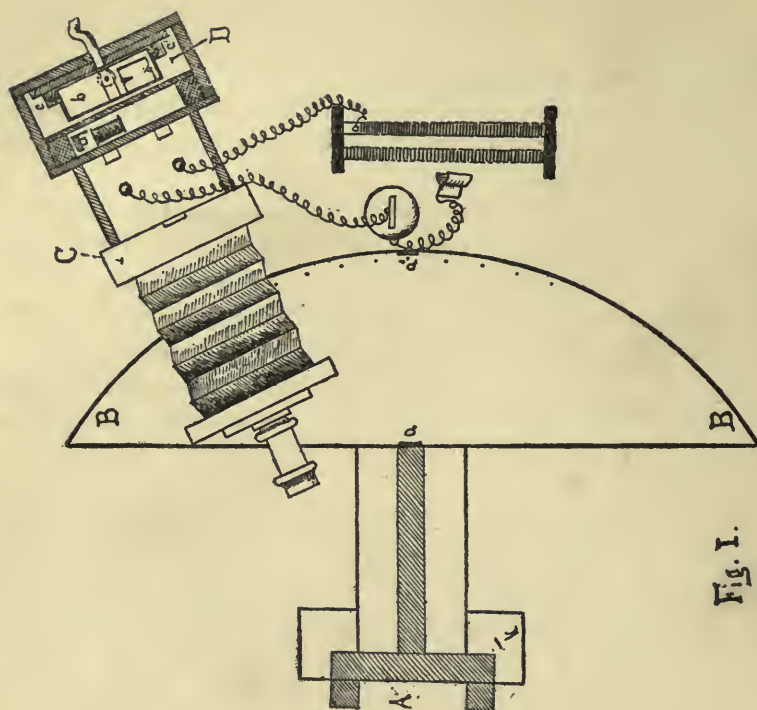


the cornea. Such lines give clean-cut records permitting considerable magnification in reading, while the amount of light needed is comparatively small.

The description of the essential parts of the apparatus used for all the measurements given in this article follows the cuts, Figs. 1 and 2 (Plate I.).

The head rest (*A*) is fitted with an attachment for holding a narrow strip of white cardboard (*k*) which is illuminated by direct sunlight from behind the subject. It is the image of this piece of cardboard reflected from the cornea which makes the records. The ordinary Helmholtz head holder with its mouth-piece of sealing wax was discarded in favor of a simple forehead and chin rest; while a peep sight (*aa'*) enabled the subject to find the right position for his head, irrespective of individual peculiarities.

A horizontal perimeter (*BB*) carries the adjustable fixation points and a holder for printed matter. The perimeter was a horizontal wooden table fastened securely to the head rest, and strong enough to support the camera, which could thus be readily adjusted to the most favorable point of view, without refocusing. The fixation points were bits of white paper attached to knitting needles which could be set vertically into holes in the perimeter table. The knitting needles amply justified their value as fixation objects. In preliminary experiments black cross lines on a white surface were used as fixation objects, but it was found that practically every movement above  $15^{\circ}$  was a double movement. The first served to bring the eye within  $3^{\circ}$  or  $4^{\circ}$  of the cross lines; while the second, occurring after a well-defined pause of about .2", rectified the faulty fixation. This was clearly inadmissible, since it never permitted us to measure the angular movement desired; and, moreover, burdened the results with a large and varying source of error. It is worth noting in passing that the subject was rarely aware of a broken movement; while the length of time intervening stamps the correction as a time reaction, thus effectually demonstrating that, under ordinary conditions of eye movement the stimulation during the movement does not serve the eye as a guide. Whenever a second or corrective movement was found, the



first movement was regularly too short. The long bright needles, affecting so large a number of retinal elements with a clear-cut stimulus, served so effectually as a stimulus to movement that up to  $40^\circ$  only half a dozen in all were double movements. Of course such movements do not appear in the tabulated results.

The camera (*C*) was an ordinary  $5 \times 7$  bellows camera fitted with a fair lens of  $1\frac{3}{8}$ -in. aperture. An especially constructed lens would be a great improvement in the apparatus, and might possibly permit direct photography of the eye.

The plate-holder (*D*) is the vital part of the apparatus, since it must provide, not only for the regular motion of the sensitive film across the slit, but also for the registration of the time record. Perhaps the best means of moving the film would be to stretch a collodion film over a kymograph cylinder enclosed within a light-tight box. This would permit indefinitely long records and insure an even motion. Lack of the necessary apparatus compelled us to use ordinary plates. The plate is held in a small wooden frame (*b*) guided by two brass rails (*cc'*). Since there must be absolutely no lateral motion of the plate, all play is taken up on one side by a spring pressing the guide on that side against its track. The rapidity of the fall is governed by the escape of air from a cylinder into which the falling plate presses a closely fitting piston. The cylinder (*d*) and piston are an ordinary bicycle pump. A second pump on the outside of the plate-holder (not shown in the drawing) serves to force the plate up to the top of the plate-holder, where it is held by a valve. (In the side elevation of the plate-holder, Fig. 2, the plate is shown at the top of the plate-holder.) The release of the air is provided for by inserting between the valve and the inner air pump, a set of two tightly fitting brass tubes, such as are used for fish pole joints. A set of holes was drilled in the inner tube, and the fall is occasioned and controlled by uncovering one or more of these holes.

It may not be improper to forestall the inevitable criticism of the exceedingly simple and homely means utilized in construction of the apparatus by mentioning the fact that we were compelled to design our apparatus not only to secure the required accuracy of results, but also so that we could construct it ourselves.



The first movement of the plate on the release of the air is subject to considerable variation, and the first  $\frac{3}{4}$  inch of the 5 x 8 plates used is regularly disregarded, but the remainder of the fall is quite regular if the parts are kept well lubricated. Very slight inequalities in the fall are detected with great delicacy by unevenness in the density of the shadows.

The time records are produced by the oscillations of a pendulum within the plate-holder and just in front of the horizontal slit. The light at one end of the slit is intercepted at each oscillation of the pendulum, making a continuous time curve the entire length of the plate. The pendulum is set in motion at the beginning of each exposure by an automatic break in an electric circuit, including a small electric magnet which holds the pendulum motionless as long as the plate remains at the top of the plate-holder.

Fig. 3 is a reproduction of a part of one measurement of the pendulum time record, actual size. The fine curve at the left is the record of the vibrations of a König tuning fork of 100 vibrations per second. The large curve at the right is the pendulum time record. The pendulum was adjusted as closely as possible to .2". Our measurements show its interval to be  $203\sigma$  without any variation which we had the means of detecting.

Fig. 4 is a reproduction of a part of a typical record of reading. While the lines are too delicate to be reproduced satisfactorily, the cuts (Plate II.) indicate their general character.

The reading of the records is a tedious process. It was originally planned to measure the negatives directly, but after considerable experimentation it was found impossible to make the delicate lines on the negatives as distinct as the corresponding lines of a good print.<sup>1</sup> All the results published in this paper are, therefore, obtained from prints which were dried and pressed with great care. They were measured with a cathetometer reading with a vernier to .02 mm. Since .02 mm. had a value of from  $.4\sigma$  to  $.8\sigma$  according to the velocity of the sensitive plate, the unit of measurement is well within the required  $1\sigma$ .

<sup>1</sup> Since this paper has been made ready for the press a method of preparing the negatives has been devised which permits direct reading as at first planned. This will be used in all future experiments.







FIG. 3.



FIG. 4.

Dodge and Cline on the angle velocity of Eye Movements.

The largest error in the readings is due, not to the unit of measurement, but to the character of the eye movement itself. The transition from rest to movement and from movement to rest presents no sharp point on the curve at which the cross line of the reading telescope may be placed. This not only increases the error in the readings, but also increases their difficulty. The problem of placing the central hair line at the beginning and end of the oblique line of movement finally reduced itself to the problem of placing the central hair at that point where the straight lines of rest first showed a deviation from the vertical. The point at which the central hair finally rested for each reading was reached by a series of oscillations in which the amplitude was gradually reduced to zero. The average variation between two consecutive readings each, for 36 consecutive records, was .045 mm., corresponding in the records used to an average difference between the readings of  $1.5\sigma$ . Assuming that each reading has equal probability of being the true one, and using the formula  $E = 6745 \sqrt{\frac{S}{n-1}}$ , in which  $S$  is the sum of the squares of the differences, we get a probable error of a little less than  $.5\sigma$  for each reading. In a series of 8 to 10 readings the total error becomes a negligible quantity when the required accuracy is only  $1\sigma$ .

## RESULTS.

The results of our measurements are presented in the following tables:

TABLE I.

VELOCITY OF THE EYE IN READING.										
Movements toward the right.						Movements toward the left.				
	No.	M.	M. V.	Max.	Min.	No.	M.	M. V.	Max.	Min.
<i>A</i>	12	23.7	3.9	36.5	17.5	5	45.6	3.7	50.1	40.9
<i>B</i>	18	21.9	3.9	29.2	15.0	5	40.1	2.9	43.8	35.0
<i>C</i>	10	23.2	6.1	32.8	14.2	4	37.1	1.9	39.0	33.4
General Av.	22.9					40.9				

Table I. shows the measurements of the movements of the right eye while reading four or five lines of popular scientific

matter not familiar to any of the subjects. The three subjects, A, B, and C, were Mr. Cline, Mrs. Dodge, and Professor Dodge respectively. The measurements were made from two records for each subject. The way the records happened to commence and end prevents the relation between the number of movements to the right and those to the left from representing the actual relation in reading. There was an average of a little less than five reading pauses per line, making an average of a little less than four movements to the right for every movement to the left. The designation of the columns is self-explanatory. No. shows the number of movements measured; M., the average; M.V., the mean variation; Max., the maximum value; Min., the minimum value. The *general average* is reckoned from the average for each individual. It may be objected that the number of measurements is too small. The authors discussed this objection; but, in view of the accuracy of the method and the small mean variation, it seemed that the number was sufficient unless we should regroup the results according to the arcs of movement, and this scarcely lay within the limits of the present study.

Estimating the distance from the surface of the cornea to the axis of rotation of the eye as 1 cm., the distance from the center of the printed page to the axis of rotation of the eye measured was 31 cm. The arc represented by each full line of print was  $16^{\circ}$ . The arcs of the movements to the left varied from  $12^{\circ}$  to  $14^{\circ}$ . The arcs of the movements to the right varied from  $2^{\circ}$  to  $7^{\circ}$ . The large variation in the arcs of the short forward movements is clearly responsible for the larger comparative variation in the results, as represented in the mean variation and in the maximum and minimum values.

A comparison of our results with the results of Huey shows that the delay occasioned by the extra work of moving the recording pointer amounted to almost as much as the normal duration of the movement for the small angles, and had about the same numerical value for the longer movements to the left. Of course it is possible that individual peculiarities would modify this somewhat. The small differences in the angles measured is also a source of error; but the difference between our results is apparently a very close approximation to the error of the



Huey apparatus, as far as his measurements of the angle velocity of the eyes are concerned.

READING MOVEMENTS.—SUMMARY.					
To the right.			To the left.		
Huey average	3°36'	43.9σ	Average	12°12'	57.9σ
Dodge Cline	2°-7°	22.9σ		12°-14°	40.7σ
Difference		21σ	Difference		17.2σ

In this connection it may be noted that the estimate of .02'' each for the reading movements, made on the basis of the Dodge-Lamansky measurements by Erdmann and Dodge in their analysis of the reading processes, is surprisingly close to the true value.

Table II. gives the speed of the eye movements for the same three subjects for the angles 5°, 10°, 15°, 20°, 30° and 40°.

TABLE II.

	A			B			C			General average.
	M.	M.V.	No.	M.	M.V.	No.	M.	M.V.	No.	
5° 5-	34.5	1.5	8	29.4	2.9	8	22.4	3.3	10	28.8
10° 5-5	41.8	1.4	9	40.9	3.8	8	33.7	2.1	5	38.8
15° 10-5	46.7	4.5	8	47.9	2.6	10	49.9	3.1	10	48.2
20° 10-10	54.5	8.0	8	51.3	3.5	10	58.6	4.1	10	54.8
30° 15-15	84.3	8.9	7	74.3	9.3	10	82.5	3.8	10	80.4
40° 20-20	100.4	4.5	7	93.4	7.3	10	106.0	8.0	8	99.9

In Tables I. and II. we have followed the time-honored custom of giving the mean variation of each series. This includes all errors of reading and apparatus; but as these errors can account for only about .75 σ, the remainder must be accounted for from a physiological basis. Two of the factors entering into this physiological variation are clearly indicated in the records. (1) It is partially due to fatigue. The last four movements in a series of ten uniformly average longer than the first four. In view of the enormous number of eye movements during the day's work of a student this evidence of fatigue within a group of ten is surprising and demands further investigation. (2) It will be noticed that the M.V. is considerably larger for larger angles, although the proportion between M. and M.V. holds fairly constant. In

all cases where the M.V. is larger than  $4.5\sigma$  there is an unequivocal difference between movements of the eye to the right and to the left which is evident even in the small number of records at hand. The data are doubtless too few to permit a quantitative generalization, but we may be permitted to give an illustration. If one follows the records of *B*, it will be seen that while up to  $20^\circ$  the M.V. does not exceed  $3.8\sigma$ , the M.V. for  $30^\circ$  is  $9.3\sigma$ . Reckoning the mean value of each direction by itself we find the following:

	Total	30°	M.	M.V.	
	movement to right		74.3	9.3	
	movement to left		83.6	2.1	
			<u>65.0</u>	<u>4.1</u>	Difference 18.6
Similarly the total for 40° is			93.4	7.3	
	movement to right		99.6	5.3	
	movement to left		<u>87.1</u>	<u>4.2</u>	Difference 12.5

The same fact is well marked for *A* as low as  $20^\circ$ .

	Total	20°	M.	M.V.	
	movement to right		54.5	8	
	movement to left		62.5	2.9	
			<u>46.5</u>	<u>2.1</u>	Difference 16.

In discussing the eye's inability to see its own movements in a mirror, Dodge<sup>1</sup> called attention to the fact that this did not hold true absolutely except for monocular observation, and he suggested as the probable reason why in binocular observation we occasionally perceive a slight quiver, apparently the beginning or end of a movement, the hypothesis that the eyes do not move absolutely together. This hypothesis seems to be substantiated by the difference just remarked between the duration of eye movements in different directions. A few binocular records of eye movements which we already have show us that the hypothesis is a fact. Apparently the two eyes neither start their movements nor end them at the same instant. This observation suggests that valuable practical as well as theoretical results may be expected from a quantitative study of binocular coördination; it also throws an interesting side light on the facts of the lack of perception during eye movement.

A comparison of Tables I. and II. shows that the *G. A.* for reading movements to the right falls below the *G. A.* for eye

<sup>1</sup> PSYCHOLOGICAL REVIEW, Vol. VII., p. 456.

movements of  $5^{\circ}$ ; while the *G. A.* for reading movements to the left falls between the *G. A.*'s for eye movements of  $10^{\circ}$  and  $15^{\circ}$ . This concurrence between the two sets of measurements, not only in their general averages but also in corresponding movements of each individual, amply justifies the conclusion that the character of the eye movements in reading is not materially different, qualitatively or quantitatively, from the eye movements which are made in response to peripheral stimuli as the eye looks back and forth between two fixation points.

Comparison of our results with Dodge's results by the Lamansky method is impossible for angles larger than  $15^{\circ}$ , since the Lamansky method can measure only the internal movements for larger angles.

C.		
	Lamansky method. <sup>1</sup>	Photographic method.
$5^{\circ}$	15 $\sigma$	22.4 $\sigma$
$10^{\circ}$	16-20 $\sigma$	33.7 $\sigma$
$15^{\circ}$	30 $\sigma$	49.9 $\sigma$

How much of this difference between the results is to be regarded as the result of the previously mentioned sources of errors in the Lamansky method and how much represents the influence of the peculiar conditions it imposes we have no data for determining.

All the measurements in Tables I. and II. were made from records of the right eye under conditions as nearly constant as possible. Each mean value in Table II. is from consecutive records from a single plate; such records only were disregarded as represented, either broken movements, or irregularities in the movement of the plate.

<sup>1</sup> 'Psychologische Untersuchungen über das Lesen,' p. 358.

PROCEEDINGS OF THE NINTH ANNUAL MEET-  
ING OF THE AMERICAN PSYCHOLOGICAL  
ASSOCIATION, JOHNS HOPKINS UNI-  
VERSITY, BALTIMORE, MD.,  
DECEMBER, 1900.

REPORT OF THE SECRETARY.

The ninth annual meeting of the American Psychological Association was held at Johns Hopkins University, Baltimore, Md., on Thursday and Friday, December 27th and 28th, 1900, in affiliation with the American Society of Naturalists. The President of the Association, Professor Joseph Jastrow, presided at the meeting, at the various sessions of which there were thirty members in attendance. Many of the members were present at the reception to the visiting societies on the evening of the 27th, as well as at the annual dinner on the 28th. The program of the philosophical section of the Association had been planned to extend through the morning of Saturday the 29th, but it was voted to hold an extra session on the afternoon of the 28th, instead of adjourning for the discussion before the Naturalists, which action allowed the Association to adjourn on Friday afternoon.

The regular business meeting was held on the 27th, at which the following was transacted. Election of officers for 1901: *President*, Professor Josiah Royce, Harvard University; *Members of the Council, to serve for three years*, Professor J. Mark Baldwin, Princeton University, and Professor John Dewey, University of Chicago. The following new members were elected: Professor M. M. Curtis, Western Reserve University; Mr. W. H. Davis, Columbia University; Professor Erwin W. Runkle, Pennsylvania State College; Professor George M. Stratton, University of California.

The Council reported an invitation from President Harper



to the Association to hold the next annual meeting at the University of Chicago, and after discussion it was voted unanimously to accept the invitation, power being given to the Council to make other provision for the meeting should circumstances render it desirable.

With regard to a resolution presented to the Council, recommending the establishment of a psychological laboratory in Washington under federal control, the Council reported that it deemed it inadvisable to take action in the matter at the present time.

The Council offered the following resolution which was adopted: *Resolved*, That the Committees of Arrangements of Foreign Congresses of Psychology be requested to confer with the American Psychological Association with regard to the American representation at such Congresses and the participation of American members in their proceedings.

Professor Baldwin reported briefly for the Committee on Terminology and Professor Cattell for the Committee on Physical and Mental Tests.

Dr. Leuba made application to the Association for financial support in publishing his catalogue of psychological literature. After an active discussion, upon motion of Professor Sanford, it was voted that a committee of five, of whom Dr. Leuba should be one, be appointed by the chair to take the whole matter under consideration and report to the Association at its next annual meeting. This committee was appointed as follows: Professors Baldwin, Warren, Newbold, Judd and Leuba.

## REPORT OF THE TREASURER FOR 1900.

Dr.

To balance at last meeting.....	\$1,041 38
To dues of members .....	282 00
	<hr/> \$1,323 38

Cr.

By expenditures for

Grant to Professors Bryan and Lindley ...	\$ 50 00
Printing .....	13 95
Postage and telegrams .....	17 30

Stationery.....	1	40	
Clerical assistance .....	16	20	
Expenses of meeting.....	2	00	\$100 85
Balance .....			\$1,222 53
Interest on deposits (approximate) .....			86 34
Total ..			\$1,308 87

Audited by the Council and found correct.

LIVINGSTON FARRAND,  
*Secretary and Treasurer.*

#### ABSTRACTS OF PAPERS.

Address of the President: *Currents and Undercurrents in Psychology*. By JOSEPH JASTROW.

The survey of the currents and undercurrents of present-day psychology which is to be here undertaken makes no pretense to comprehensiveness or system, but is to proceed frankly under the guidance of personal interest. The conception serving as the background of the survey is that of psychology as a science of mental functions; and the functional aspect of mental phenomena is itself one of the influential streams along which the psychological knowledge comes freighted. This is largely the result of the evolutionary conception, which in psychology, as in all other sciences that deal with the phenomena of life, has wrought such vast and deep-reaching changes. It dominates the general aspects and equally the specific details of problems and of their solutions; it produces attitudes towards problems and directs attention to questions which otherwise would be passed by unobserved; its complete utilization in the field of psychology is far from realized. A further characteristic of recent psychological currents, and one flowing from the recognition of the functional aspects of psychology, is the three-fold mode of approach to many of its problems which has been productive of interesting and valuable advance—the developmental or genetic, the normal, and the abnormal or the decadent. It is the mutual corroboration by or the application to the other methods, of the results of each of these three modes of approach, that widens the methodological equipment of the psychologist, and makes possible an insight into the real relations of processes

and phenomena otherwise quite unobtainable. The specific efficiency of each aspect of study is emphasized and developed and likewise checked by its combination with the others.

The study of normal functional efficiency is apt to fail of its proper appreciation by reason of its remoteness from noteworthy analytical results and its close connection with the tedious methods of anthropometry. But the proper pursuit of the problems centering about the nature of normal mentality requires a painstaking analysis; for upon the correctness of such analysis depends the value and appropriateness of the resulting tests; while the conception of psychology that includes the application of its principles to the determination of normal mental endowment seems at once more rational and more fertile than a conception which on theoretical grounds ignores these aspects of mental life.

The study of the abnormal suggests not alone the utilization of Nature's experiments for the insight thereby obtainable into obscure relations, but it likewise is suggestive of a noisy undercurrent in contemporary activity, which under the misleading name of 'psychical research' has fostered a popular interest in certain phenomena,—an interest that does not contribute to their psychological interpretation so much as to the mystification and distortion of true psychological methods, problems and interests. In this interest there is greater danger to the appreciation of psychology on the part of the public at large than to the development of psychology by its professional representatives. None the less, this extraneous menace is worthy of the psychologist's earnest attention.

Current psychological literature testifies to the widespread interest in the study of functional complexes. Of these, reading and writing furnish the best instances. The minute analysis of the contributory processes involved in these activities, their relations to the resulting efficiency, and the many ramifications into which these considerations naturally lead, have been ingeniously investigated, and have been inspired in part by the possibilities of contributing to a knowledge of these important practical highways of intellectual intercourse. These fundamental activities of civilized life have indeed assumed an overwhelming impor-



tance, and any insight into their nature seems destined to bring with it some suggestions for their improvement, and thus to be freighted with significant practical consequences.

These and other practical applications of psychology have brought to the foreground the general question of the practical bearings of this science,—a theme strenuously contended and variously presented from different points of view, favorable and unfavorable. Out of this discussion has come a recognition of the misleading kinds of application of psychology which seem to pertain in certain educational circles. There has also resulted a weakened sense of the significance of the messages which psychology bears for the direction of the intellectual life. A more mature contemplation seems destined to strengthen the cautious and critical application of psychological principles to general and in certain cases to specific educational and cognate problems, avoiding equally crass rule-of-thumb dogmatism and a transcendent repugnance of theory for homely practice. In these various ways have the currents of contemporary psychology come to the surface and influenced the direction of thought and activity. Out of similar and yet more complex tendencies will the psychology of the coming century develop and take shape.

(The address was published in full in *THE PSYCHOLOGICAL REVIEW* for January, 1901.)

*Habit Formation and Memory in Invertebrates and Lower Vertebrates.* By ROBERT M. YERKES.

This was a preliminary report of some experimental studies in animal psychology now being conducted in the Harvard Laboratory. The chief purposes of the work are: (1) to determine to what extent, with what rapidity and precisely how animals learn; (2) to test the permanency of any associations formed, and (3) to make as many supplementary observations on the general habits and reactions of the animals used as possible. The labyrinth method, in various forms, has been used for all the work.

Observations have thus far been made on the crayfish, green crab, fiddler crab, newt, frog and turtle. Crayfish in a simple labyrinth, involving choice of direction only once, improve



rapidly from 50 per cent. correct in the first ten trials to 90 per cent. correct in the sixth ten. There is evidence of the habit (*i. e.*, memory) after two weeks' rest. Newts, judging from these studies, learn very slowly and there is excessive variability among individuals. Frogs alone, of the animals named, have shown the formation of a habit as the result of a single experience. Turtles are very apt in the formation of labyrinth habits and retain them for weeks. They give a curve of learning very similar to those of the cat and dog. In all the experiments record is kept of the time taken to escape from the labyrinth and of the course followed.

*Frontal Lobes and Association.* By SHEPHERD IVORY FRANZ.

Although it is generally admitted that the function of the frontal lobes of the brain is the production of certain association processes, the evidence for this view is very meagre and what evidence we possess is not conclusive. Embryological research has shown that the frontal lobes are two of the six areas in which the association fibers arise, but the results of physiological experiments and of pathological and clinical examinations have not made clear what associations take place.

The experiments reported were undertaken to determine whether or not the frontal lobes in animals are concerned in the production of certain sensory-motor associations. Cats were given the opportunity of learning simple habits, after the learning of which the frontal area anterior to the crucial sulcus was excised. After the operation the habits previously learned were lost. A series of control experiments shows that this result cannot be accounted for on the supposition of surgical shock. The conclusion was drawn that the frontal lobes are *normally* concerned in the formation of these habits.

The animals that had thus acquired and lost certain associations were found to have the ability for relearning these habits and for acquiring new ones. This ability (relearning) may be due to the use of other association areas (either the parietal or the occipito-temporal lobes), but the evidence is not yet complete.

A continuation of the work especially concerning the func-

tions of the parietal and the occipito-temporal regions is in progress and similar experiments upon the brain of the monkey are planned.

*The Influence of Special Training on General Ability.* By R. S. WOODWORTH and E. L. THORNDIKE.

The following conclusions seemed to the authors justifiable :

1. It is misleading to speak of sense-discrimination, attention, memory, observation, accuracy, quickness, etc., etc., because multitudinous separate functions are referred to by any one of these words, functions which may have little or nothing in common.

2. Even such phrases as 'attention to words' or 'discrimination of lengths' or 'quickness of visual perception' mean mythological not real entities. For instance, attention to the meaning of words, attention to their spelling, and attention to their length, are functionally independent of each other to a large extent.

3. The mind is, on the contrary, on its dynamic side a machine for making particular reactions to particular situations. It works in great detail, adapting itself to the special data of which it has had experience. Change in the time or precision or quality of any of these particular reactions need not and often does not influence appreciably other reactions, similar enough to be called by the same name. Change in any one seldom brings about an equal change in any other reaction, no matter how similar, for the working of every mental function is employed. The amount of influence of changes in any one mental function upon others is much less than has been supposed.

4. The examination of all the cases of such influence and of its absence makes it seem probable that change in one function affects others only where and in so far as identical elements are present in both. By identical elements are meant concrete elements, such as sensation, images, movements, etc., the actual content of which is identical.

The methods used in the study were: (1) tests of certain functions before and after a period of training of some one function; (2) an investigation of the extent to which, in judgments

of a series of magnitudes, each judgment was uninfluenced by improvement in the others, and (3) an investigation of the extent to which closely allied functions were correlated.

*Psychological Tests of Abnormal and Exceptional Individuals.*

By J. McKEEN CATTELL.

Attention was called to the desirability of extending physical and mental tests to those suffering from disabilities and disease on the one hand and to those showing exceptional aptitudes on the other. A description was given of cases of nervous disease tested by the speaker, and it was shown how these differed from normal individuals and how far the nature and progress of the disease could be deduced from such tests. Thus in one case—a paranoiac with subsequent mania and threatened with dementia—the higher senses were normal, but the skin senses and movement were defective. The pressure causing pain was 10.5 kg. as compared with 5.5 kg. in normal subjects. The quasi-automatic processes in adding, writing, sorting cards, etc., were quick and accurate. Organic memory was good, but logical memory was almost gone and associations were highly incoherent. Imagery, suggestibility and suspicion (when instruments were used) were absent at first, but were found when the general condition had improved somewhat. The tests seemed to indicate changes in condition, giving an objective record. Turning to an exceptional case, similar tests made on the chess-player Mr. Pillsbury were described. His head was unusually long (211.5 mm.) the index being 70 (average 78.8). His organic memory was good; his logical memory poor. In accuracy of perception and quickness of movement he was somewhat below the average. Thus, his reaction time was  $170\sigma$  (normal for college students  $159\sigma$ ), marking 100 A's in 500 letters, 132 sec. (normal 100), making 100 movements 39 sec. (normal 34). He was shown a chess board with six men in normal positions for 0.1 sec., but did not seem to perceive the complex better than others. He played simultaneously sixteen games of chess in the laboratory without sight of the boards, winning twelve and drawing two. He did not visualize the board as a rule, but said that he could do so with special effort



and did so occasionally when in a difficult position. He remembered the positions in relation to the previous plays, all of which he could repeat for each game. Motor innervations of the speech organs were used, but his memory must be described not as sensory but as dependent on logical relations. The speaker also noted the correlation of the tests made on the freshmen and seniors of Columbia College, mentioned tests made on the cleverest and dullest children in a primary and in a high school and described a photographic method of measuring the features.

*Volition and Experiment.* By E. F. BUCHNER. (Read by title.)

This paper continued the special treatment of the active consciousness presented in a paper entitled 'Volition as a Scientific Datum,'<sup>1</sup> read at the last annual meeting of the Association, in which an analysis of mental processes was undertaken in justification of volition as a characteristic element. The present paper attempted further justification in this direction by developing the relations between volition as a fact and experiment as a method, incidentally reviewing some experimental results. At the same time it endeavored to justify psychology as a science by maintaining that the results of certain introspective analyses are 'quite in accordance with the possible development of experimental methods.'

There appears a direct antithesis between volition and experimentation, inasmuch as the former receives chief recognition in a direct analysis of mental processes, and least recognition through instrumental means, the latter being a highly refined mode of physiological approach to those processes. The experimenter is not responsible for 'foisting' volitional facts upon psychology; and he cannot distinguish in fact between mere stimulus control and volitional control. The unpracticed ideal in every laboratory is a method which shall catch a subject freed from all self-contributory mental data. Volition may become an object of experimentation; but every psychological experiment is radically dependent upon volition as a factor from the very beginning. A study of laboratory direc-

<sup>1</sup> PSYCHOLOGICAL REVIEW, Sept., 1900, p. 494.



tions reveals the presence of volition at all important stages. All experimentation demands preliminary analysis of mental action. Mechanical adjustment, actual testing, and counting, are simply expressions which develop at large the qualities which inhere in the volitional *motif* of every experiment. Volitional changes are only indirectly subject to experimental measurement. Several phases of volitional connections were reviewed in the light of these criteria. We may continue to praise experimentation for its contributions to the science, so long as it does not attempt to give, or to take away, the volitional features of mental action. The limitation of experiment appears when the valid claims of mental data are subverted by the syllogisms of the laboratory.

*A Note on Binocular Rivalry.* By EDWARD A. PACE.

The purpose of the experiments reported in this paper was to determine whether the fluctuation of retinal fields is influenced by such mental factors as expectation and recognition. It was found that when the fields (colored squares or figures) are presented in succession, the new field dominates in consciousness. The mere fact that one field is familiar and the other strange does not affect the result. Efforts of the will to retain a field when a new stimulus is applied to the other retina, are not at first successful. By repetition, however, control is gradually acquired, so that, in proportion as the novelty of the intruding field wears off, inhibition becomes easier. Eye movements were in all cases perceptible, and they seemed to play a more important part than retinal fatigue. There is evidence to show that volition produces different results according as it is directed by expectation or is deprived of such assistance. Granting that the will effort is accomplished by eye movements and that these largely determine the outcome of the rivalry, we may still hold that the psychical processes in question exert some influence. If the path be blocked along which a visual idea, strongly motor in character, would discharge, an incoming stimulus which corresponds to that idea and would, in its efferent course, follow the same path, must have its effect reduced. The will acts indirectly upon the mental content, but its action

is more effectual when it is prepared, by expectation, to control the motor discharge.

*The Analysis of Writing Movements.* By CHARLES H. JUDD.

The writing movement is a complex act consisting of elements contributed by the fingers, hand and arm. In the earliest stages of development these components are separate and distinct, and may easily be observed as uncoördinated factors of the writing process. In developed life, on the other hand, the elements of the writing movement are so strongly fused into a unitary, continuous form of movement that some method of analysis must be employed. Such a method consists in attaching a tracer to the hand in such a way that it will not be affected by the fingers, but will record any movement of the hand as a whole. If one writes with such a tracer attached to the hand, the written letters will contain the finger components as well as hand and arm movements, while the tracer record will not contain the finger components. Apparatus and records were exhibited to illustrate the method.

It is found by this method that the hand and arm do not participate in the finer formative parts of the writing movement, but merely carry the fingers forward, thus contributing the grosser elements, especially those in a forward direction. Marked differences appear in the modes of coördination employed by different individuals. The same individual when placed under different conditions also shows noticeable differences in the form of movement. Thus, a change in the direction of the attention, or a change in the character of the writing material, will show noticeable effects.

*An Ergograph, with a Discussion of Ergographic Experimentation.* By JOHN A. BERGSTRÖM.

In the instrument exhibited, the load or resistance was furnished by springs, and throughout, the extent of movement might be either uniform, as in the experiments of Mosso, or gradually increasing, as in those of Cattell, and Binet and Vaschide. By the use of these two modes of resistance in conjunction, it is possible to ascertain the total force of the contraction of the

muscle and to use as a load with the uniform resistance method some definite fraction of the total force. To secure the uniform resistance a compensating attachment is used.

The graphic record is obtained in ink on a small continuous paper kymograph mounted on the machine and kept at a uniform speed by a rapidly vibrating watch movement. A summation recorder shows in millimeters the total length of the graphic record. The amount of work done with the uniform resistance method is ascertained by multiplying together the readings of the scale and the summation recorder.

While the different modes of resistance used afford each its own group of important problems, reasons were given for regarding the uniform resistance curve as the one more generally useful.

With this ergograph, records may be obtained from the flexor of the third phalanx, the flexors of the last two phalanges, and the extensors of the last two phalanges of any finger of either hand, also from the abductor indicis of either hand. In the adjustment of the hand in an experiment, the center of rotation of the joint to be used and of the resistance are made coincident. To avoid the disturbing effects of movements of the whole body, or arm, the ergograph is suspended by a spring or by a cord with a counterpoise.

*Susceptibility to Disease and Physical Development in College Women.*<sup>1</sup> By ARTHUR MACDONALD.

Those with no diseases are less in weight, but greater in height and lung capacity than those having had one or more diseases.

Those with constitutional diseases are less in height than those having had no disease. Those having had typhoid fever are superior in lung capacity and strength but inferior in weight to those having diseases in general; the typhoid cases are also inferior in weight, height and strength to those with specific infectious diseases, indicating that delicate and slender persons are more subject to typhoid fever.

<sup>1</sup> The data (1,486 persons) were kindly furnished the writer by the professor of physical culture and the resident physician in one of our women's colleges.



Cases of infectious diseases are superior in weight, lung capacity, height and strength to those having had diseases in general; but those with hereditary diseases are inferior in weight, lung capacity, height and strength to those with infectious diseases.

Scarlet fever cases are inferior in height to cases of infectious diseases in general.

Those with diseases of the digestive system are less in weight and lung capacity, but greater in height than those with diseases in general.

Cases of insufficient respiration have less weight, but greater *lung capacity* and height than those with diseases in general.

Those with heart murmurs show greater weight, lung capacity, height and strength, than cases of disease in general. Those with habitual headache are inferior in weight, height, lung capacity and strength to those with diseases in general.

*Demonstration of Apparatus.* By E. C. SANFORD.

This was an informal report upon two pieces of apparatus recently brought into use in the laboratory of Clark University: one an instrument for communicating electrical signals (*e. g.*, the interruptions of an electrical tuning fork) to a pneumatic system, so as to allow recording by means of a tambour; the other, an application of Lord Rayleigh's plan for securing uniform rotation to the regulation of chronographs and other psychological instruments requiring exactness in this particular.

An instrument for the same purpose as the first mentioned has already been used by Professor Marey, but the form described is different and is believed to be in some respects an improvement. It consists essentially of a thick tambour enclosing an electromagnet, the drum-head of the tambour carrying the armature of the magnet and being adjustable in distance from the magnet within. A somewhat similar apparatus has, the writer is informed, been employed by Rousselot.

The second instrument originated with the physicists, and has been perfected by them, though not generally known as yet in the psychological laboratory. It consists essentially of a cylindrical cage of soft iron bars rotating before the poles of an



electromagnet in circuit with a tuning-fork interrupter. When the apparatus functions properly, one bar of the cage, and only one, slips by the magnet at each interruption. The proper functioning of the instrument is tested by means of a stroboscopic disk attached to the same shaft as the cage.

*Researches in Experimental Phonetics.* By E. W. SCRIPTURE.

A report was made of the progress since the last meeting of the Association. A greatly improved machine has been developed for tracing off the speech records of gramophone plates, the vibrations of some of the vowels being enlarged to amplitudes of nearly a centimeter. Among the results were measurements of various vowels and certain conclusions in regard to English verse. The full accounts appear in the current number of *Studies from the Yale Psychological Laboratory*.

*The Kantian Doctrine of Space.* By GEORGE STUART FULLERTON.

This is the first of a series of papers which the author is about to publish on the doctrine of Space and Time. It examines the teachings of the Critique of Pure Reason, and concludes that Kant's three main positions are supported by an equivocal use of the word 'intuition.' These positions are: (1) Space is a *necessary* 'form' of thought, and, hence, we cannot conceive the possibility of the non-existence of space, although we can easily conceive of the non-existence of objects in space; (2) we can represent to ourselves but one space, of which all spaces are parts; from which it follows that space cannot be conceived as limited; (3) all space is composed of spaces, that is, space is infinitely divisible, and that which fills space, the 'thing' given in sense-intuition, must be infinitely divisible too. The writer maintains that Kant's reasonings are found to be unsound just as soon as one has seen clearly that what is 'given in intuition' may be taken in two senses, *i. e.*, as what is *actually present in consciousness*, and what is *known independently of proof*. The argument can scarcely be given in brief outline, and is merely indicated in the above.

*Nietzsche.* By GRACE NEAL DOLSON.

Nietzsche's significance for philosophy lies in his ethics, which is both critical and constructive. The former is concerned chiefly with an historical account of ethical concepts, while the latter treats of the moral idea, which may be called fullness of life, the complete expression of the will for power. The type of conduct arbitrarily set up as an ideal is an extreme individualism. The individual is not only the judge of moral distinctions, but is himself the end. The existence of the social feelings is admitted, but they are branded as immoral. The fact that living examples of Nietzsche's ideal are more common in the past than in the present has led men to compare him to Rousseau, just as his emphasis upon strife has gained his theories the epithet of Darwinism. Both comparisons can be defended, but are superficial. The one name that can be given to Nietzsche's system without qualification is egoism. Its peculiarity consists in the fact that instead of seeking to show that the egoistic impulses dominate all conduct, Nietzsche makes them into an ethical end. The egoism is, however, strongly anti-hedonistic. Self-repression is to be done away with for the sake of power, not of pleasure. The value of Nietzsche's theories lies in his emphasis upon the self-assertion of the individual and in the special form of his egoism. He treats both egoism and altruism ethically, not psychologically, admitting that both exist, but taking egoism as the moral ideal.

*Professor Ladd's Theory of Reality.* By WILLIAM CALDWELL.

This paper attempted a positive criticism of Professor Ladd's book on the Theory of Reality, and tried to show that in the doctrine of activity developed in the central portions of the book may be found a means of rationally overcoming that dualism between conception and the trans-subjective with which Professor Ladd starts and which persists to the end of his volume, and which is apparent even in his contention that reality must be construed after the analogy of the self. The doctrine that the reality of the self, no less than the reality of things, is to be explained as an activity determined by ideas, and also the exposition of the oneness of the theoretical and practical ac-

tivity of the self, completely do away with the idea that there is a fundamental dualism between the ideas of the self and things. Altogether Professor Ladd in his doctrine of activity has builded better than he knew; his exposition of this doctrine has some novel and ingenious features of its own, although it is essentially the line that renders possible the argumentation of other recent writers upon the theory of reality, such as Ward, Royce, and Ormond.

*The Doctrine of the Twofold Truth.* By F. C. FRENCH.

The central problem of mediæval thought was the rationalization of dogma. The solution reached was a negative one. Instead of establishing the unity of philosophy and theology, the schoolmen finally reached the conclusion that each is an order of truth by itself, distinct and separate from the other. The steps by which this doctrine was evolved were as follows: First, faith and reason are completely identified (Erigena). Then they are recognized as two but easily reconcilable by a little logic (Anselm). Then one after another of the doctrines of faith are declared indemonstrable (Albertus, Thomas, Scotus) and finally all (Occam).

Philosophy and theology are regarded as both valid but irreconcilable systems, and we have the twofold truth.

This doctrine was common in the Renaissance period, and in the 17th century Bacon, Hobbes and Descartes held substantially the same position with respect to religion. Locke avoided the contradiction of a twofold truth by distinguishing what is above reason from what is contrary to reason; Kant by the separation of phenomena and things in themselves; others (the Ritschlian theologians, certain pro-religious agnostics, *e. g.*, James in his 'Will to Believe') by distinguishing the theoretical and the practical, and regarding religion as entirely practical. The twofold truth however is still with us. This is very clearly the attitude of Professor Münsterberg in his 'Psychology, and Life.' The natural sciences, physics and psychology, give us a world of atoms material and mental. The historical and normative sciences give us a system of will acts. Psychology teaches that we have no will at all, immediate self-



knowledge that we are nothing but will. Scientific truth is, to be sure, only a part of the truth of life. But Professor Münsterberg makes the truth of science and the truth of life both complete wholes without any essential relation to one another.

*A Study of Pluralism.* By A. H. LLOYD. (Read by title.)

[This paper will be published in full in the *Philosophical Review*.]

*Some Unscientific Reflections about Science.* By A. H. LLOYD.

Science has certain limitations, to which it often confesses, so that there must be some justification in being unscientific. Limitations, moreover, are as much a source of danger as of opportunity; for example, the objectivism, the specialism and the positivism of science.

Objectivism tends to bury science in technique, where technique includes everything that makes scientific work mechanical, external criteria of originality, the preëmption of fields of study, conceit about linguistic proficiency or technical phraseology, satisfaction over 'research,' 'production,' and the like, as well as the more familiar matters of method, apparatus and material. Technique makes science possible, but also a 'pure,' 'objective,' 'inductive' science is not true science. True science is invention, the invention of a tool, as something visible or as a method or as a point of view, with use of which human life is made more vital, more effective. Certainly evolution must see science in this light, and so must human history; and society, sure in time to resent the aristocratic temper of technical science, is bound to enforce the translation, the application of science.

Specialism, secondly, incident to objectivism, tends toward blindness and so induces dreaming. Reality is never special, and thought—or mind—conserves an indivisible reality for the specialist by leading him into marvelous logical constructions which he knows as his 'working hypotheses,' but which are hypotheses that work because—albeit darkly and indirectly—they compensate for the neglected points of view. Simply they are as if doors in the panelling by which the other sciences are secretly admitted to a room that seems closed to all comers. Thus in the course of history the sciences have often shown



inner affinities, particularly the different sciences within any general group, but even sciences so far apart as the physical and the psychical have been entertaining each other. (See two former papers before the Association, 'Epistemology and Physical Science—A Fatal Parallelism' (*Phil. Rev.*, July, 1898) and 'Physical Psychology' (*Psych. Rev.*, March, 1900.)) Community of method, too, as in the case of the historical method, and the striking analogies so easily traced among the sciences of any one time, while not exactly doors in the panelling, have the effect of relating the different sciences by making the panelled partitions all but transparent. Specialism, then, cannot but end in the unification of science. Unification, however, means application also. A world of distinct details is not a world for action, but, the many becoming one, will and action are inevitable.

And, lastly, positivism, springing as it does from the scientist's aloofness, which distorts and discolors what is known, making space and time empty and divisible and causation formal and decomposing an indivisible reality into unreal elements, threatens science with morbid intellectualism and what some would call the 'real life' with dead monotony and routine. The agnosticism, however, which a positivistic science involves, can not mean that there is a distant region of faith and will, or of the 'real life'; that there is literally an unknowable, but this instead. Knowledge as mere knowledge, science solely for science's sake, objective, special science, is indeed blind to reality, but knowledge identified with will, applied science, is even reality itself. In short, the Unknowable is really a negative that bears witness, not to another sphere, which some mind different from ours might consciously apprehend and in which we can only blindly will, but to another relation than that of mere knowledge which we *as knowing creatures* have to reality. To know *is* also to will. "The end of man is action, not thought, though it were the noblest."

*The Problem of a 'Logic of the Emotions.'* By WILBUR M. URBAN.

An important *desideratum* in the mental sciences is a clearer conception of the relation of emotional values to each other.

Throughout the ethical and æsthetic theory of the present time is the assumption of the existence of generalized emotional states (emotional abstracts) under which particular emotions and feelings may be subsumed. A semi-logical conception of subsumption in the sphere of the emotions has taken the place of the intellectualistic theories of the past. (Meinong, Ehrenfels, Guyau, Hirn.) Moreover, the prevailing conceptions of transmission and expression of emotional states implies a relative abstraction from an independence of intellectual content. (Tarde, Hirn.)

Ribot, the only psychologist who has developed a conception of a '*logique émotionnelle*,' has found in the phenomena of emotional recognition and revival, particularly marked in artistic creation and in pathologically emotional types, grounds for a doctrine of *émotional abstracts* (sentiments and moods) which act as '*émotions fixes*,' subsuming particular feelings under them.

Two hypotheses are made necessary by such a theory, (1) That there are qualitative elements in feelings upon which abstraction can act, (2) that there exists an emotional memory. Facts and reasons speak in favor of both hypotheses. On the assumption that there are elementary feelings of value, other than organic sensations and intensities of pleasure and pain, between which, in abstraction, equivalences of value are established, thus developing generic phases of emotionalism, it would follow that the extension of these spheres over varying particulars would increase with the relative insignificance of the pleasure-pain elements and the elimination of the particularizing organic or motor elements. An analysis of typical generic phases of emotionalism (play psychosis, sense of obligation and reverence), too involved to be reproduced here, shows this to be the case.

The hypothesis of '*Affective Memory*,' does not, as Ribot has clearly shown, involve the absolute revivability of feeling tone, without ideal content, but rather the identification of a present emotional content with a revived emotional state. The advocates of emotional memory fail to see that this is all that is implied in the empirical methods of experimentation upon sen-

sational and ideal memory. An experimental examination of Ribot's two generalizations in regard to emotional revivability (the details of which it is hoped may be published later), brought out facts which seem to indicate that the process of abstraction (as here understood) is the very condition of revivability. In both artistic expression and appreciation, the abstract mood and sentiment are the first to be recognized as true, or identical with former experiences. Ribot's generalization that revivability increases with the complexity and abstraction of the emotional state seems to be true. It is only with effort that particular emotions are revived. On the other hand his partial generalization that the presence of motor elements tends to increase revivability did not maintain itself, nor should we expect this result if the characteristic of emotional abstraction consists in the gradual elimination of the particularizing motor elements. The explanation of the origin of emotional abstracts seems to be in the mythogenic tendency which creates symbols, with which generic emotional states associate simultaneously.

*Self-Consciousness and its Physical Correlate.* By HENRY RUTGERS MARSHALL.

If each mental state in a given individual corresponds with a differentiation of process in that individual's nervous system, then 'self-consciousness' must have coincident with it some special form of neural activity.

The neural process in man is the activity of an enormously complex neural system which itself is made up of minor neural systems: consciousness then, under this hypothesis must be looked upon as a vast psychic system made up of minor psychic systems.

In the complex neural system as a whole, any increment of activity in any minor system will stand in contrast with the mass of activity of the complex system as a whole. The most ordinary presentations to the Self correspond with such special increments of neural activity: hence we are led to ask whether the Self may not be that part of consciousness which corresponds with the mass of psychic activity in the complex neural system as a whole.



Under such an hypothesis the state of 'self-consciousness' would be explicable as the correlate of a special form of neural activity where (1) a whole minor system of the great complex system is aroused to a higher grade of activity than that of the complex system of systems as a whole; and where (2) within this minor system a special part is raised to a still higher grade of activity than appears in the minor system itself as a whole.

Thus, under this hypothesis the higher activity of this minor system as a whole, but excepting the still higher activity of its special part, has as its correlate what we call the empirical ego: and the still higher activity of the special part of this minor system appears as a presentation to this empirical ego: the empirical ego and the presentation to the empirical ego together being a presentation to the Self: which Self corresponds with the activity of the mass of elements of the complex system of systems as a whole.

*Reduction to Absurdity of the Ordinary Treatment of the Syllogism.* By CHRISTINE LADD FRANKLIN.

The traditional treatment of the syllogism errs both by redundancy and by insufficiency—that is to say, the validity of the syllogism can be tested by a far more simple method of procedure, and, moreover, the ordinary method fails of application to a vast number of pairs of propositions which are nevertheless the premises of a valid syllogism. In the first and second moods of the first figure, the syllogism is in what may be called its primitive form,—it is doubtless the only form in which it is used by children and savages; but there is another form, in which negative modes of expression are given free play, which is far superior to it in facility of manipulation. This superiority has its source in the fact that of the eight propositions (not four, to which the ordinary logic has reduced the forms of speech) necessary to a complete description of the universe:

<i>a.</i> all <i>a</i> is <i>b</i> ,		<i>ā.</i> not all <i>a</i> is <i>b</i> ,
<i>n̄.</i> none but <i>a</i> is <i>b</i> ,		<i>u.</i> some besides <i>a</i> is <i>b</i> ,
<i>ī.</i> no <i>a</i> is <i>b</i> ,		<i>i.</i> some <i>a</i> is <i>b</i> ,
<i>o.</i> all but <i>a</i> is <i>b</i> .		<i>ō.</i> not all but <i>a</i> is <i>b</i> ,

it is the last four that possess the great advantage of being symmetrical (that is, of having subject and predicate amenable to



the same rules of manipulation), and of these it is 'no  $a$  is  $b$ ' and 'some  $a$  is  $b$ ' that possess the other advantage of naturalness. These should therefore be regarded as the Canonical Form of the proposition; and correspondingly, the ideal form of the syllogism is that in which it appears as a statement of the impossibility of concurrence of the premises and *the denial of the conclusion* of the ordinary syllogism. In the canonical form of the syllogism therefore,

no  $a$  is  $b$   
no  $c$  is non- $b$   
and some  $a$  is  $c$

are inconsistent (or cannot all three be true together). This may be called the Inconsistency, or the Incompatibility or perhaps the Antilogism. But in this all three propositions play an exactly similar rôle—there is no distinction between premise and conclusion, and it is therefore the one single form to which *every* syllogism may be at once reduced, provided we (1) express every universal proposition in the negative form, no  $p$  is  $q$ , (2) every particular proposition in the affirmative form, some  $r$  is  $s$ , and if we also (3) deny the conclusion when thus reduced to the form of an Inconsistency, the rule for validity is this: of the three propositions, two are universal and one is particular; each two propositions have one and only one term in common; the term common to unlike propositions appears with like signs and the term common to like propositions appears with unlike signs. (Thus, in the above typical form,  $b$  and non- $b$  are common to the two universal propositions, but  $a$  or  $c$ , of like quality, are common, respectively, to the particular and either universal.)

Any given statement of fact may be expressed in terms of any one of the four different copulas given above, or again, with the aid of the special terms *the non-existent* and *the existent* (0 and  $\infty$  in Symbolic Logic), it may be expressed in four different ways with one and the same copula: thus,

all $a$ is $b$ ,	nothing is $a$ and $\bar{b}$ ,
all $\bar{b}$ is $\bar{a}$ ,	everything is $\bar{a}$ or $b$ ,

are four different forms of one and the same statement of fact (expressed in the four possible combinations of two terms and their

negatives); but in the two symmetrical copulas (no  $a$  is non- $b$ , all but non- $a$  is  $b$ ) the four forms all become practically identical. There are therefore ten ( $4 + 4 + 1 + 1$ ) essentially different ways of saying one and the same thing. As each proposition of the Inconsistency can be expressed in any of these different ways, and again as each Inconsistency can appear in the form of the universal or of the particular syllogism, the total number of possible syllogisms (when full latitude is given to mode of expression) is two thousand ( $10 \times 10 \times 10 \times 2$ ). An example of one of these outlying forms is this: none are athletic and unhealthy, none are healthy and unhappy, hence all are either happy or non-athletic. Any one of these two thousand forms can be at once tested as regards its validity by the above Rule for the Inconsistency.

*An Examination of Professor Sidgwick's Proof of Utilitarianism.* By ERNEST ALBEE.

The argument as a whole naturally falls into two main divisions: (1) the determination of certain supposed infinitive principles, and (2) the determination of the nature of the Good, which latter is held to be necessary, in order to give to these purely formal principles of conduct the concrete character which they require. The author's three so-called intuitions, corresponding to Justice, Prudence, and Benevolence, are by no means on the same plane, the first being merely the postulate of objectivity, or impartiality, implied in all ethical speculation as such. It is thus on a plane with the most fundamental methodological postulates of the various sciences and disciplines. As regards the other two principles, only that of Rational Prudence is really treated as an Arborate intuition, that of Rational Benevolence being arrived at indirectly. The first step is the disguised translation of the original principle of Justice into terms of the Good; the second step is a deduction from this principle in its modified form. Moreover, these three principles do not all imply the Good, as the author holds. The very abstract principle of Justice, as here formulated, does not necessarily imply the Good at all, while the other two principles, even as here formulated, are not necessarily to

be regarded as distributive rather than as regulative principles. That will depend upon the nature of the Good, still undetermined. Finally, even assuming these principles to be distributive, the author's hasty determination of the nature of the Good hardly pretends to be a philosophical treatment of this all-important problem. When he practically rests his case upon the argument that pleasure is the only practicable criterion of the value of feeling as feeling, he unconsciously begs the question, which is, and must remain, whether or not the value of conscious life is to be expressed solely in terms of feeling.

*A Peripatetic Formula for the Moral Ideal.* By W. R. NEWBOLD.

This paper discussed the interpretation and probable origin of the formula "For the good man the things which are absolutely good are good" ( $\tau\tilde{\omega}\ \acute{\alpha}\gamma\alpha\theta\tilde{\omega}\ \tau\acute{\alpha}\ \acute{\alpha}\pi\lambda\tilde{\omega}\varsigma\ \acute{\alpha}\gamma\alpha\theta\acute{\alpha}\ \acute{\alpha}\gamma\alpha\theta\acute{\alpha}$ ) which occurs in various parts of the works attributed to Aristotle. The conception 'good' contains two leading elements *pleasure*, *rightness* or *reasonableness*. 'Absolutely' ( $\acute{\alpha}\pi\lambda\tilde{\omega}\varsigma$ ) properly emphasizes the essential elements of the concept, as distinguished from its non-essential connotations. 'Absolutely good' therefore ought to mean 'Something absolutely pleasurable and absolutely right,' and in part it frequently has this meaning, as for example when used of the good of the gods. But when 'absolutely good' is contrasted with some modified notion of good, one or the other of these elements may be obscured. The phrase  $\acute{\alpha}\gamma\alpha\theta\acute{\alpha}\nu\ \acute{\epsilon}\chi\acute{\alpha}\sigma\tau\omega$  most commonly denotes what is good for the individual in the ethical sense, whether pleasurable or not, but occasionally it means what is (*i. e.*, seems) good *to* the individual, whether right or not. When  $\acute{\alpha}\pi\lambda\tilde{\omega}\varsigma\ \acute{\alpha}\gamma\alpha\theta\acute{\alpha}\nu$  is contrasted with  $\acute{\alpha}\gamma\alpha\theta\acute{\alpha}\nu\ \acute{\epsilon}\chi\acute{\alpha}\sigma\tau\omega$  in *either* of the senses,  $\acute{\alpha}\pi\lambda\tilde{\omega}\varsigma\ \acute{\alpha}\gamma\alpha\theta\acute{\alpha}\nu$  assumes the other sense. Illustrations of all these usages were given. In either case the contrast lies in the first instance between Pleasure and Duty, and the formula  $\tau\tilde{\omega}\ \acute{\alpha}\gamma\alpha\theta\tilde{\omega}\ \tau\acute{\alpha}\ \acute{\alpha}\pi\lambda\tilde{\omega}\varsigma\ \acute{\alpha}\gamma\alpha\theta\acute{\alpha}\ \acute{\alpha}\gamma\alpha\theta\acute{\alpha}$  usually means that in the good man pleasure and duty coincide; *i. e.*, his desires are all in accord with Reason. The precise meaning of the formula varies however somewhat with



the context. The writer had met with the formula only twice outside the Eudemian Ethics, Magna Moralia and those books of the Nicomachean Ethics which are commonly regarded as Eudemian. Of these two occurrences, one was in a passage in the Metaphysic which is regarded by Bonitz and Christ as displaced and is probably a gloss. The other was in the Politics in a passage which not only interrupts the tenor of the context but refers back to the 'Ethical writings.' Unless the formula be found in the Nicomachean Ethics, therefore, the reference must be to the Eudemian and is probably a gloss. The writer was inclined to believe that the formula was of post-Aristotelian origin, but reserved judgment pending a more careful search of the Aristotelian writings for further occurrences.

*Active and Passive Reason in the Writings of Aristotle.* By  
W. A. HAMMOND. (Read by title.)

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## PSYCHOLOGICAL LITERATURE.

*Fact and Fable in Psychology.* By JOSEPH JASTROW. Professor of Psychology in the University of Wisconsin. Boston and New York, Houghton, Mifflin and Co. 1900. Pp. xi + 375.

The life of a science has many points of comparison with the life of an individual. There is the time of appearance, and there is the time of growth and formation. 'Well begun is half done,' may be a very serviceable ideal for all sorts of training; but the work-a-day world of investigation and of living does not conform readily to the saying. After formation—and even within that period itself—comes the problem of possibilities. How wide may the science become, and how much will the individual accomplish—continue to be parallel phases. Formation entails dangers, which are met within periodical accentuations, and herein the character of a science begins to part ways with the character of the individual.

The historical truth of science as a method, and of the physical sciences as aggregates of particular results, is doubly applicable to psychological science. With an origin in the mystical and legendary interests of men, this science slowly formed itself by strenuous reflections. Ages and efforts succeeded ages and efforts, until the science became more or less able to stand upon its own feet, and walk as a true companion hand in hand with other and older sciences. The original interests in the unusual and extraordinary, however, kept themselves vigorously alive, adjusted their results and the acceptability thereof to the case of men's minds, and finally fostered a propaganda greater than the academic doctrine of the soul could ever countenance. As soon as psychology came to its majority, so to speak, it was called upon to incorporate into its body all the various psychical fancies grown upon the soil of those original interests of the race. This call continues loudly to-day, and is echoed by the votaries of every uncanny altar. The book under consideration finds a most fitting place for itself in the necessities of a defense for psychological science, warding off, as it attempts to do, the multiple attacks made by those fancies upon its domain. The present state of human opinion and belief about matters of an unusual psychical sort is such that there is great need for a vigorous presentation and criticism, and these are happily

to be found in the present volume. It is a book for the hour as well as for the future. The demand for plain rebuff, and for fresh psychological surveys is as serious within the precincts of the science as out in the great surrounding wilderness of laymen's opinions and half-matured hopes. It is a designed attack upon opinionated psychology, and succeeds in bringing freedom to the science by carrying the warfare into the haunts of the psychological guerrillas who infest the border.

We are, however, in danger of either mis- or over-representing the volume. Its title is guileless. The psychological 'fables' do not prove to be identical with those taught to college juniors or seniors, and the 'facts' soon begin to be the potencies of logical criteria. The book, however, makes a distinct contribution to psychology, inasmuch as it goes over the genesis of certain notions and ideas about things and selves, especially the latter; or, perhaps, better said, it has for its theme the genesis of false ideas, and the processes which have been, or may be, operative in developing those errors within the realm of psychology. It thus works out a varied answer to the question: What is variable psychology? Being an astute 'defender of the faith,' it also stands as a contribution to the history and logic of science, dealing with the range of error within a field of this sort of learning, and thus linking it to the whole life of science itself.

The volume is a collection of essays which have appeared from time to time in scientific and other periodicals from 1888 to 1900. The commonality they possess, giving them place under one title, is doubtless due to an after-reflection and late revision. The fate of the article-writer is not altogether an agreeable one, and the reprint is often made to be a tale of woe. The real power of the true psychologist and the true scientist is seen, however, in the high degree of perfect elaboration of one idea, through the mazes of creeds and problems, which revision has succeeded in accomplishing. The eleven essays have grown into one thought, and one can believe again that the scientific spirit is ever truly at one with itself in whatever form it may appear. While discussing a number of special and somewhat varied problems, most of which are fascinating because of the absorbing power of the unusual, they have the singleness of purpose well expressed in the preface thus: "When the conception of the nature of our mental endowment and the interest in the understanding thereof are derived from the unusual, the abnormal and the obscure, instead of from normal, law-abiding observations systematized and illuminated by long and successful research, there is danger that the interest will

become unwholesome and the conception misleading" (p. v.). We are promised a justification of the accepted principles of psychology.

The scope of the discussions and the type of psychological misconceptions may be most clearly seen and readily inferred from the topics of the essays, which are arranged in the following order:—"The Modern Occult, The Problem of Psychical Research, The Logic of Mental Telegraphy, The Psychology of Deception, The Psychology of Spiritualism, Hypnotism and its Antecedents, The Natural History of Analogy, The Mind's Eye, Mental Prepossession and Inertia, A Study of Involuntary Movements, and The Dreams of the Blind." The first appeared as an article latest, and the last appeared earliest, the others appearing at irregular intervals. One may well feel convinced that there has been a necessity for some true psychological gardener to come along to do the clearing work represented in this volume, by seeing how much *Unkraut* has been threatening the real growth and expansion of acceptable scientific plants, as appears in a hasty glance at the specific subjects and forms of abnormal interests, treated more or less fully from cover to cover, which are herewith enumerated:—Theosophy, spiritualism, astrology, palmistry, solar biology, descriptive mentality, Christian science, divine healing, apparitions, presentiments, haunted houses, multiple personality, automatism, ghosts and hobgoblins, rappings, hallucinations, telepathy, ignorance, illusion, delusion, deception, mesmerism, electro-biology, mental vision, prejudice, power of ideas, error, dreams, visions, subconscious activities, coincidences, suggestions, conjuring tricks, mental contagion, animal magnetism, analogy, metaphor, superstition, folk-medicine, muscle-reading, involuntary movements, belief, blindness, interest, attention, the supernatural, logic, education. Such a quantitative enumeration leaves one considerably in doubt as to the results of the author's labors. In such topics, we all readily agree, whether from the interest of the specialist or of the layman, in asking for definite qualitative expressions.

It is interesting to note how the articles have grown into one thought through revision and recasting. The modesty of the author allows him to make extensive positive claims, by way of marked changes, for only the second and the third essays in the above enumeration. When a comparison of the essays is made with the original articles, excepting two which have not come under our notice, it is found that the specific results of the varied interest and study of years readily yield to a fusion under the dominance of one conception. The author's belief is, that it is the explanation of normal experience alone



which will equip psychology with the principles necessary to undertake a proper study of the abnormal and the extraordinary. Essay II bears no resemblance to its original, the recasting having made it new and far more critical than was the original writing. Essay III has been increased by an addition almost equal to its former length. Essay IV is increased by irregular expansion. Essay V has grown by addition and recasting to more than double its original form. Essay VI has been equally well favored by increase in its historical and critical material. Essay VII has received a small amount of expansion. Essay VIII is a reprint. Essay X is a rearrangement with a slight addition, while the last essay presents new material to the extent of one-fourth of its size. The revision has brought an increase in the amount of historical details, a more elaborate expounding of the varied doctrines reviewed, and a very great increase in the conspicuity of the criticism and logical guidance needed in the ascertainment of all truth. The author has been very successful in avoiding the repetitions which would seem so inevitable in the preparation of extended discussions under such a program. The most notable recurrence in the discussion is found in the subject of spiritualism, which is handled in two or three distinct places. Under logical guidance, the article-writer has ended his work of entertainment, and by successful dove-tailing he has placed us under the obligation of thoughtful learning. The new material incorporated amounts to more than one-fourth, which, with the expansions and recasting here and there, swells the fresh treatment of the special problems to fully one-third of the work as it stands at present. The longest essay is the sixth, and the shortest is the ninth. So much for the volume and its antecedents.

It must be confessed that it is not an easy matter to tell briefly just what one finds upon reading the volume. Where so much is brought forward, and from so many different points of view, one is in danger of following by-paths, and losing sight of the great highways and tendencies which may lie before him. In his brave attempt to rid the field of pseudo-science, Professor Jastrow rightly apprehends that the psychologist is confronted with a double problem. He has the tasks of really explaining the alleged mental performances which are cultivated under mythical and legendary interests, and also of accounting in a truly psychological manner for the beliefs and theories which have overgrown the layman's mind in regard to these matters. The physicist, for example, has but a single problem, namely, to put forward a defensible explanation for the alleged objective phenomena, without troubling himself about the weird conceptions which may



lurk in the piratical 'isms' of abnormal beliefs. Our author thus unearths permanent and rather deep-seated tendencies in the human mind, while discussing the ever-changing forms in which these tendencies express themselves. This is especially true in essays I and V. The readiness to accept error, and to make foundations out of excerpted facts of experience, is traced to limited observation and to inadequate logic, as well as to that emotional demand which determines to fortify itself by maintaining that 'the key to their riddles of existence is to be sought in the personal significance of events.' This conception has its fullest application in the first seven essays. The last four tend, however, to offer further corroboration of the general argument, that it is the mind, rather than objective events, which demands that certain theories shall be accepted. The most profound account given for the persistency of the incredible and the erroneous is traced, perhaps, in the analysis which finds all in the will attitude of the mind, as expressed in the 'all-powerful magicians—an expected result and the willingness to credit a marvel' (p. 127).

It is hardly possible within the scope of a critical review to go in detail over the specific results reached in each of the essays. In the articles the author addressed himself mainly to the layman. In the book he has written equally for the psychologist and for the layman. Thus it should go into the hands of both. It is not to be understood that the volume is denunciatory from beginning to end, nor that it is credulous to a like extent. It is critical, sifting the theories and the facts, now accepting some suggestions in popular views regarding the abnormal in our experiences and now rejecting such theories. This procedure does not advance under any rule of thumb, and herein lies the difficulty of the author's task, and the scope of the great service he has performed for psychology and psychologists. As it is said recurrently, 'the detection of error in another's work does not protect against a similar error in one's own.'

The particular achievements of these studies can be best set forth, perhaps, in a few passages selected here and there. "Rationality is doubtless a characteristic tendency of humanity, but logicity is an acquired possession, and one by no means firmly established in the race at large" (p. 45). "The safest and most efficient antidote to the spread of the pernicious tendencies inherent in the occult lies in the cultivation of a wholesome and whole-souled interest in the genuine and profitable problems of nature and of life, and in the cultivation with it of a steadfast adherence to common sense, that results in a right perspective of the significance and value of things" (p. 46). "We are

all rational only in spots; and many a 'psychical researcher' pursues some of his investigations under the guidance of a scientifically psychological interest, while in other directions the occult interest takes the helm" (p. 59). "There is no obligation upon any science to reconstruct its basal principles whenever it is suggested [by pursuers of the abnormal] that these are incorrect or inadequate" (p. 73). "The optimistic psychologist anticipates the day when he will no longer be regarded, either in high life or in low life, as a collector of ghost stories or an investigator of mediums" (p. 77). The third essay is wanting in that decisive conclusion which negates many of the superstitions of current popular thought. The author seems half disposed to accept some hypothesis of telepathy, but refuses to do so on general scientific grounds. "While I incline to the belief that the hypothesis of telepathy is, as usually advanced and in essence, an illegitimate one, I still regard it as possible that in the future some modification of this hypothesis may be framed, which will bring it within the scope of a liberal conception of the scientific" (p. 101). "With the spread of an education that fosters independence and self-reliance, with the growth of the capacity to profit by the experience of others," etc. \* \* \* the author sees a time when "the soil upon which superstitions, psychic delusions, mental epidemics, or senseless fads are likely to flourish will gradually be rendered unfit" (p. 136).

The essays are replete with caution, patience, facts, and a high and continuous regard for the integrity of psychological science and the application of its truest virtues to the problems of education and a proper and healthy mental training. In working along the lines of interest 'in a special field,' the author has succeeded in bringing forward something for us all. It remains to state that he has written in a charming and non-technical style, which will appeal to the untrained layman, as well as to the psychologist who may turn to the book with confidence in getting ripe suggestions for additional lines of special studies. An analytical table of contents and an index render the volume very serviceable.

Special attention must be called to two points. The explanations given of Fig. 13, page 321, showing the automatograph when 'thinking of a locality,' leave the record standing in direct and complete contradiction with the results of other records and with the chief contention of the investigation, namely, that involuntary movements tend to take the direction of the attention. If the explanations are correct, then the contradiction, which has not been noted in the text, must stand, tending to discredit the whole investigation's results. If the

first arrow were reversed, so as to point to the left, instead of to the right, then the record would agree with the other records mentioned. Upon turning to the original article, it is found that this figure, which is there used, has the arrow pointing in the right direction according to the above supposition.

In dwelling upon the significance of the period of 'blinding,' on page 345, the author states that 'this is assigned at the earliest age at which we have a remembrance of ourselves,' and then cites 5.2 years as the average age of the earliest self-recollection of one hundred blind persons. According to data gathered on this point from university students during a number of years, I have found that the average age of the earliest self-recollection of such ordinary and normal persons varies from 3.3 years for females to 3.6 years for males. The point is not altogether serious, and doubtless is due to the use of the personal pronoun in the text.

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*Psychology: Empirical and Rational.* By MICHAEL MAHER.  
Fourth edition. London, New York and Bombay, Longmans,  
Green & Co. 1900. Pp. xvi + 602 + xii.

The 'Stonyhurst Philosophical Series' comprises six volumes, written by teachers in the Jesuit College at Stonyhurst, England, and a supplementary volume on political economy written by an examiner in the Royal University of Ireland. The 'Psychology,' which is now in the fourth edition, appeared first in 1890.

The general character of this work is clearly indicated in the preface. "My aim here," the author says, "as in the previous editions, has not been to construct a new original system of my own, but to resuscitate and make better known to English readers a psychology that has already survived four and twenty centuries, that has had more influence on human thought and human language than all other psychologies together, and that still commands a far larger number of adherents than any rival doctrine." The psychology referred to is that of Aristotle and the Scholastics. This, however, is to be recast and adapted to actual conditions. The author desires 'not merely to expound, but to expand this old system; not merely to defend its assured truths, but to test its principles, to develop them, to apply them to the solution of modern problems, and to reinterpret its generalizations in the light of the most modern researches.'

Quite consistently, psychology is defined as 'that branch of phil-



osophy which studies the human mind and soul.' Hence the principal division into 'Empirical Psychology' (Book I.), and 'Rational Psychology' (Book II.); the one dealing with mental phenomena and the other with those ultimate problems which are now generally handed over to the philosophy of mind. There is also a long chapter on 'perception of the material world,' which discusses various theories belonging to epistemology, on the ground that it is impossible to separate the problems of the genesis and the truth of knowledge.

The mental faculties are classified generically as cognitive and appetitive. Running through both classes there is another division, according to which the powers of the mind are sensory or rational. Feeling is merely an aspect of the cognitive and appetitive energies, their variable phase or tone. Emotion is treated at some length in a chapter which, curiously enough, comes under the heading 'rational life.' Some of the well-known classifications are discussed; but the more recent attempts to define the scope and map out the work of psychology are passed over.

Experimental methods are taken into account, though the author is inclined to a cautious view of their pretensions. Of psycho-physics he says: "it is only a small part, and that the lowest and most unimportant part of mental life, that can be at all approached by the instruments of this science. Emotions, volitions, and all intellectual processes are obviously beyond the reach of any form of quantitative measurement. Even, then, if psycho-physics had attained the utmost hopes of its supporters, and if—what appears equally unlikely—these supporters became agreed as to their results, our knowledge of mental life would not really be thereby much advanced" (p. 57). However, he avails himself freely of the results which have been obtained by the psycho-physicists.

The fundamental thesis in this work is that of moderate spiritualism. The soul is not an entity apart nor merely a *res cogitans* in the Cartesian sense. It permeates the organism and is the principal of all activities bodily and mental. On the other hand, its spiritual nature is manifested in the processes of intellect and will.

The development of this thesis and its corollaries shows that the author is familiar both with the Scholastic theories and with those that are now current. His criticisms, which form the best feature of the book, are trenchant but objective. References to the literature of the subject are given in abundance, and each chapter is supplied with a list of 'readings' which suggests the more important works.

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*The Soul of a Christian: A Study in the Religious Experiences.*

By FRANK GRANGER. London, Methuen & Co. 1900. Pp. ix + 303.

I think Mr. Granger is justified in saying that his inquiry 'will be of interest to the psychologist, the philosopher of religion, and to the theologian,' and though 'The Soul of a Christian' perhaps falls more properly within the sphere of the religious philosopher than of the psychologist, it calls for brief notice in this REVIEW.

The author's thesis is that such phenomena as the religious emotions, inspiration, poetic invention and genius, which 'are not very well understood as yet even by professed students of the mind,' may be seen in clearer light by approaching them from a new standpoint. More specifically, "It is the purpose of this essay to describe the Christian life, as far as possible, in the terms, and with the methods, of psychology" (p. 1). The whole method of psychology, however, has to be modified (24). "Psychology can mark off and classify the products of consciousness; but it is almost incompetent to seize the processes" (25). "Just as the methods of psychology have been extended in one direction, namely, that of psychophysics, so it would seem that they may be capable of extension in other directions, and in particular in the direction suggested by the mystical temper" (23). This is the keynote of the book. Ordinary psychological methods are inadequate for dealing with the religious experience. Some psychologists seek to explain the deepest religious experiences as pathological in character. Mr. Granger seeks to account for them by Emerson's hypothesis of the 'oversoul.' The individual comes to his full right in the oversoul (71), while the oversoul is a unity which does not find its center in any individual (51). The oversoul is beyond personality, the one process of which personality is like an aspect repeated at different centers (49). Two aspects of the oversoul 'seem to be manifested in the religious experience. On the one hand, such souls find not only their ground, but their unity and communion within the oversoul, and on the other there seems to be a special disclosure of the oversoul in inspiration' (67).

This is the basis of the author's 'romantic' psychology. The central hypothesis of the oversoul is introduced to explain personality (47). In the light of this fruitful principle Mr. Granger discusses The Depths of the Soul, The Soul's Awakening, Ecstasy, The Dark Night of the Soul, Visions and Voices, Human and Divine Love, Symbol and Ritual, Prophecy and Inspiration, Illumination and Progress, Direction, Confession and Casuistry, and Mystical Theology—these being the titles of ten of the twelve chapters which compose the book, the

other two being On Method and The Oversoul. The principle which is introduced to explain personality of course serves also to explain all the 'experiences' of the religious life. The idea of the average must be distinguished from the idea of the normal. The mystic's experience is not to be adjudged abnormal by the extent to which it diverges from common experience. Suppose that after all the experience of Blake is the more true one. Apart from the universe to which it belongs the religious experience is in itself meaningless; and the 'universe' here spoken of is of course that denoted by the term 'oversoul.' "In coming to God the soul also finds itself" (Chaps. I. and II.).

This brief objective statement perhaps sufficiently indicates the general tenor of Mr. Granger's inquiry. The student will find in it a pleasant relaxation from the tedium of Mr. Starbuck's painstaking statistical study of the 'Growth of the Religious Consciousness'; and while their methods are radically different, Mr. Granger's metempirical inquiry is quite as illuminating as Mr. Starbuck's 'Empirical Study.' (For notice of the latter, see Vol. VII., No. 5, of this REVIEW.) One comment must suffice. I do not understand why the author speaks of Emerson's doctrine of the oversoul having been passed over with contemptuous silence—unless he means that it has been neglected by the psychologist; in which case it is fair to suggest that possibly this is because the theory in question falls rather within the sphere of the metaphysician than of the psychologist. I had supposed that *processes* of consciousness are just what psychology is concerned with; if *it* cannot 'seize' them, it is open to ask, What can? It is quite possible that psychology cannot give a final explanation of all the processes of consciousness, and for this some such hypothesis as that of Mr. Granger may be necessary. He appears to me, however, to jump from history over psychology into metaphysics. That is, given the experiences of the religious consciousness as we find them in the autobiographical writings of the 'saints,' we are introduced not to a psychological treatment of them, but to a popular metaphysical explanation of their origin and meaning. If, however, it be true that beyond the small aspect of experience with which psychology can deal there is a religion where the truly characteristic part of the individual life is to be sought (49), one may be less surprised at the author's failure to confine himself to the psychological standpoint, than at his purpose of attempting to describe the Christian life 'in the terms and with the methods of psychology.'

As to the neglect of Emerson's doctrine, although the word oversoul is not often used, the ideas which it represents are common

enough; they are not peculiar to Emerson, but came to him through a long line of mystics and transcendental philosophers. Nor are they now neglected; for most, if not all, theologians account for the 'soul's awakening' or the mystical experiences of religion in a way not unlike Emerson's, namely by the communion of the individual soul with 'the one spirit.' On the intellectual side, theists, in accounting for the origin of the idea of God, quite as frequently lay emphasis upon the Divine initiation *in* the individual soul, as upon the latter's seeking after God if haply it may find Him. And Calvinism (which Mr. Granger is prone to cavil at, without, I think, properly understanding it), has at least this in common with mysticism, that it attributes the quickening of the religious life directly to the influence of the indwelling spirit.

The author's specific views, as well as his fundamental principles, suggest the influence of Emerson and the Ritschlian theology, and his book has much of the stimulating suggestiveness as well as of the mystical vagueness of these types of religious thought.

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*Pragmatism.* W. CALDWELL. *Mind*, October, 1900.

Professor Caldwell's paper is a review, in amplification and criticism, of Professor James' address before the University of California on 'Philosophical Conceptions and Practical Results.' The doctrine there set forth was that of pragmatism or practicalism—the doctrine, namely, that our conceptions of reality are the outcome of practical needs. Professor Caldwell's treatment of the subject, which is very suggestive, is too detailed for adequate reproduction. In his criticism of Professor James he seems to take the latter too seriously, overlooking the limitations of a spoken address before a partly popular audience. But his main point of criticism is well taken, however we may share the point of view from which it is made. Professor James, in allowing us to choose for a future conception of reality that which satisfies practical necessity, seems to overlook the fact that we must have some basis in the world as it is now for knowing which of a set of alternatives will give the desired consequences. Professor Caldwell's criticism is that, to secure a statement of the possible alternatives, it would be necessary for Professor James to resort to the *a priori* criticism of the categories of which he has so sorry an opinion in Kant; the outcome would still, however, in Professor Caldwell's view, be determined by teleological considerations. But (I suggest the question)



is not Professor James' omission simpler and more fundamental? Has he not merely overlooked the limiting conditions of activity, to explain which constitutes the chief difficulty of any teleological system of metaphysics?

In Professor Caldwell's very complete list of the representatives of pragmatism it is strange that he should have overlooked Professor John Dewey. Surely no one has stood more squarely for the view or done more to develop in detail its various possibilities.

WARNER FITE.

*The Apperception of the Spoken Sentence: A Study in the Psychology of Language.* By WILLIAM C. BAGLEY. Am. J. of Psychology, XII., 1. Pp. 80-130.

The article gives experimental answers to three questions: 1. "What is the effect of context upon the perception of auditory symbols?" 2. "How are the objective elements related to one another?" 3. "What are the conscious processes involved in the apperception of the spoken sentence?" In all the experiments the stimulus was a phonographic reproduction of a mutilated word, either isolated or in context. The mutilation of a word consisted in the elision of one consonant.

The first question was the primary problem of the investigation. It was found that the chances for the correct interpretation of a mutilated word are increased 82 per cent. when two related words are given just before the test. They are still further increased when the mutilated word occurs in a sentence; greater at the middle than at the beginning, and greatest at the end of a sentence.

In answer to the second question it was found that the elision of mutes works the greatest injury to the correct interpretation of a mutilated word, while the elision of semi-vowels works the least injury. The percentage of wrong interpretations is 56 per cent. in the former case and only 28 per cent. in the latter.

The answer to the third question is obtained from the introspective statistics of four trained psychologists. It was found that the presentations occurring upon hearing sentences, each containing one mutilated word, were chiefly visual and verbal ideas. The most complete form of the visual idea is an 'ideal reproduction of a typical environment as represented by the context.' More usually the visual idea reproduced only a part of such environment. The verbal ideas, when they occupy the focus of consciousness, are usually antithetical or explanatory clauses. In only a small percentage of the instances were



the ideas even approximately the same for the different observers. They were often inconsistent with the significance of the context, sometimes directly contradictory. The understanding of the sentence, notwithstanding the incongruity of the ideas, leads the author to the theoretical conclusion that the uniformity of meaning under disparate and even incongruous focal presentations is secured by the compensatory mass of marginal experiences which constitute the true meaning consciousness.

Although the monograph represents a large amount of the most conscientious work, the questions it proposes are much broader than the answers it gives. It is a valuable beginning, but it seems to the reviewer that a precondition of further investigations along the same lines must be an adequate psychophysical analysis of the auditory word and phrase. To make the visual word the basis for the mutilation of the auditory word can scarcely be considered a final or a very satisfactory method; for instance, the *m* elided in rhy(m)ing is a very different acoustic fact from the *m* elided in no(m)ination, while the effects of the elision on the resulting complexes in the two cases are still more emphatically different. Even the use of the phonograph, with its distortions and interpolations, is admissible only with cautious discrimination. The attempt to determine the relative importance of the various groups of consonants by omission is peculiarly suspicious. The results certainly give plausibility to the conjecture that one reason for the more marked effect of eliding the mutes is that they are utterly dissimilar to the accompanying noises of the phonograph, while the semi-vowels, which, perhaps approach most nearly the noises of the apparatus, are wrongly substituted for the elided sounds more often than any other letters.

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## ETHICAL.

*Morale et psychologie.* By E. DE ROBERTY. *Revue Philosophique*, October, 1900. Pp. 329-345.

This is one of a course of university lectures on the constitution of ethics, delivered last year both in Brussels and in Paris, and much of the obscurity of the published article is no doubt due to its being a fragment torn from its setting.

The aim of the author is to establish the thesis, maintained before in his *Ethique* and elsewhere, that sociology is identical with ethics and quite as distinct from psychology as it is from biology. The author's account of the genesis of association will suggest the basis on which he rests this classification.

The phenomenon which sociology is to investigate M. de Roberty calls indifferently altruism and sociability (*socialité*). What these terms stand for it is not easy to say. The author is willing that 'sociability' should be called an instinct, in fact the social instinct, though he considers the term vague. Sociability appears to be a psychic fact, but it is present not alone in all animals, but also in plants, though to be sure in most of the former and all of the latter it is latent, or in a 'heavy slumber.' At all events, this quality being so generally latent, the author asks what it is that in exceptional cases arouses it to activity, and answers that it is psychic differentiation. There are specialization and coördination of psychic elements ('primordial representations, emotions, desires, or needs') in all genuine associations, and this is possible only on the basis of psychic differentiation. Accordingly, where this differentiation exists, latent sociability is stimulated into activity, and association and 'collective psychism,' some sort of psychic interplay among the members of a group, are the results. Moreover, the grade of the psychic communion is determined by the brain power (*cérébralité*) of the individuals concerned, and, as the author repeatedly insists, where the collective psychism is allied to a high order of brain power, as in man, the two are transmuted into what he calls ideological capacities, which are essentially instable, and which substitute purposeful action (*finalisme conscient*) for mere causality or *vis a tergo*.

Now this ideological activity is regarded by M. de Roberty as the essential and exclusive psychological fact, and that is the reason why he refuses to identify psychology and sociology. This 'mental phenomenon,' which is the object of psychology, is anterior and exterior to sociability or altruism, which is the object of sociology; indeed sociability, or more properly its product, collective psychism, in reaction with brain power, also called psychophysical aptitude, is what gives birth to the mental phenomenon, and the two sciences therefore differ in subject-matter. In a sense this is a question of terminology, but comparative psychologists are not likely to admit that psychophysical aptitudes, still less that 'sociability,' fall without their province. But even where psychologists dissent from M. de Roberty's reasoning, they probably will assent to his conclusion, that psychology and sociology are distinct sciences.

On the other hand, convenience in specialization, if nothing else, seems to forbid that sociology and ethics should be identified. M. de Roberty is, of course, right in saying that the investigations of conduct, of the realization of ideas in action, of character and of moral judg-

ments, belong alike to sociology and to ethics. But partial identity of subject-matter does not identify two sciences. If it did, psychology and sociology would be merged, for all these phenomena are proper objects of psychological investigation. Indeed, the issuance of ideas into action falls little short of being M. de Roberty's essential and exclusive psychological fact. Again, the author is doubtless right in maintaining that accepted moral rules have value only if they 'express the essential laws that govern the social activities of man,' but when these latter are in turn declared to constitute the proper object of sociology, the statement is justified only if the word 'essential' shifts its meaning. It is tolerably clear that when sociology finally comes to know its own mind it will settle down as a descriptive science, which is interested in moral phenomena, but lays its emphasis elsewhere; while ethics, greatly assisted by sociology, among other sciences, will continue to be essentially concerned in discovering the types of conduct men had best unite in encouraging. One is tempted to wonder whether the author's vague and eccentric use of 'altruism' is not responsible for his identification of the two sciences. It is dangerous to use so pregnant a word loosely.

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*On the Conception of 'ΕΝΕΡΓΕΙΑ' ΑΚΙΝΗΣΙΑΣ.* F. C. S. SCHILLER.  
Mind, October, 1900.

*The Normal Self: a Suggested Formula for Evolutionary Ethics.*  
R. R. MARETT. Mind, October, 1900.

The aim of Mr. Schiller's paper is to rescue from an unmerited obscurity the Aristotelian idea of being. According to the author, Aristotle's aim was to construct an idea of being which might be conceived, in contrast to Plato's *οὐσία*, as actually functioning, and yet, in opposition to the apparent tendency of Heraclitus, as having a permanent reality. The resulting conception was that of *ἐνέργεια ἀκίνησις*, which may be translated as 'activity in equilibrium.' According to this conception, the perfect or divine life is an unceasing and unchanging activity, an eternal consciousness of supreme happiness. But eternity is not, like the illusory 'eternity' of Green, a mere abstraction from the factor of time; on the contrary, time is merely the measure of the impermanence of the imperfect, and it is the perfecting of the time-process—not the abstraction from it—which carries us out of time into eternity. The Aristotelian conception is thus a scientific formulation of the theological conceptions of Heaven and Eternity. Mr. Schiller applies the conception to motion, life and consciousness.



It is admitted that motion tends ultimately to equilibrium; but equilibrium is not the equivalent of rest or negation, but 'a perfecting of motion until it has everywhere become perfectly regular, steady, smooth and frictionless.' Similarly, life tends not, as popularly conceived, toward death or cessation of activity, but toward an ultimately perfect adjustment of activities. And fluctuation is not a condition of consciousness as such, but of unsatisfied consciousness. A conception of being as activity in equilibrium would, the author thinks, stimulate us to develop our powers to the utmost and at the same time rescue us from neurotic restlessness.

'Ethics,' says Mr. Marett, 'is, or ought to be, a concrete science in the fullest sense'; but, as a matter of fact, its formulations are not, like those of other sciences, really applicable to concrete reality. The cause of this deficiency is that most of what passes for ethical science is really metaphysics. The norm or 'true self' there formulated abstracts from all actual conditions and becomes a conception of what one might be under ideal social conditions, with no immediate reference to what one can be under actual conditions. To remedy this difficulty the author proposes the 'normal self' as a methodological conception.

Science has two methods, definition and discovery. Definition applies to a completed and static view of phenomena; it is also, to some extent, an essential of discovery, in that it supplies a trial hypothesis and afterwards reduces the facts discovered to workable consistency so as to serve as stepping-stones to fresh discovery. As an illustration of a conception which is definitory yet capable of modification, and thus dynamic, we have the biological conception of 'types.' Now the proposed normal self is analogous to the 'type.' A type expresses not the ultimate form which a species may in the future reach, but a progressively selected character actually transmitted by inheritance. In contrast to the biological conceptions those of ethics must of necessity be teleological, but the normal self would express, not the ultimate ideal significance of self, but the mixture of relatively adjusted characters which the good man as good, and also as mortal, displays in contrast to all other characters not so good. For example, assuming that other-regarding impulses were originally self-regarding, and that the ultimate destiny of present self-regarding impulses is to be other-regarding, it is none the less true that normal moral action, according, in the author's opinion, to the unanimous verdict of the ages, is a mutual adjustment of both tendencies. The determination of this norm would be the task of ethics as a concrete science.



These two interesting papers appear to me, so far as they cover the same ground, to have identical import. Both Mr. Schiller and Mr. Marett conceive of moral action as that which is normal, regulated, and thus relatively (in Mr. Schiller's view, absolutely) spontaneous, unconstrained and painless. This kind of activity implies an equilibrium between activity and limiting conditions—a 'moving equilibrium,' to be maintained constantly between the development of ideals and that of actual and immediate possibilities. Activity thus regulated would, according to one author, be productive of ceaseless happiness, and, according to the other, would be most effective in concrete results. Now I believe we may grant the value of such a conception of moral action, especially for the age in which we live, and I prefer not to discuss its inner consistency. What I do wish to question is Mr. Marett's conclusion that this value may be realized through the rejection of metaphysics from ethics and the establishment of the latter as a concrete science upon the basis of a 'normal self.' His 'normal self' is itself a distinctly metaphysical conception, and as such is in direct contrast to the biological 'types' to which he has assumed it to be analogous. An animal type is a relatively concrete conception precisely because the characteristics of the object defined are relatively limited, a subhuman species having few instincts and a restricted environment. But the instincts of the human type are limited by nothing less than the possibilities of reason and consciousness itself. And who, at the end of this nineteenth century, will venture to define even the immediate limits of human capacities? The human environment is also nothing less than the world as a whole. To define the human type and its actual possibilities is thus immediately to overstep the boundaries of concrete science into the field of metaphysics. If, then, the value of the 'normal self' is to be realized, it will be, as I believe, not through the rejection of metaphysics, but, as other ethical values are realized, through a relatively unanalytic and æsthetic sense of the values proposed in metaphysical speculation.

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#### EDUCATIONAL.

*Die Zahl im grundlegenden Rechnenunterricht.* GEORG SCHNEIDER. Schiller and Ziehen's *Abhandlungen*, III. Bd., 7. Heft. 1900.

Of the two parts of this essay, the first is an inquiry into the ori-

gin and development of our notion of number. It attempts to single out the conditions essential to the existence of the idea of number, and to determine the factors upon which depend the successful use and extension of the number concept. This investigation is a preliminary laying down of principles for the discussion, which occupies the second part. The latter concerns a problem of practical method, namely, the best way of representing number in elementary school instruction.

First, of the development of number concepts. The gist is: without things, no numbers; for with them the idea of number arises and disappears. In the primitive mind number is always concrete. The child learns the difference between one and many and the discrimination of simple number groups by handling his toys and the objects of the nursery. Number is first of things, uniform or manifold, seen, heard or touched; its use is to reckon these in multitude, extent, volume or worth. Number in the abstract is a late and highly developed concept, and even among those who possess it the vastly preponderant use of numbers is in measuring, comparing and describing concrete quantities. To the qualities of things and the nature of perception, therefore, we must look for the conditions which underlie the development of the notion of numbers. Sight, hearing and touch have each a share, but of unequal significance. The sense of touch plays the chief rôle. It was the first to appear in the development of organic species; through it we apprehend objectivity and the complexity of space relations; upon it we depend for perceptions of movement and change of mass, and about it cluster all operations of measuring and estimating.

In the conditions of sensory apprehension thus lie the factors which contribute to the development of the number sense. These are, roughly, four: (1) *Contrast*, without which no discrimination is possible, and in accordance with the strength of which varies the stimulus to attention; (2) *Likeness*, without which individuals cannot be apprehended as a group. The less striking the characteristic in virtue of which the things are combined, the weaker will be the sense of their unity or sum; the more salient this characteristic, the more clearly and readily their number relations can be conceived; (3) *Discriminability*, without which no plurality can exist. The ease of discrimination is in proportion to the intensity and abruptness of the interruptions between the individual objects; (4) *Unity*. Spatial arrangements are more readily apprehended numerically than temporal series; and of the former, figured groups are more easily conceived than rows.

For the representation of simple number relations in primary grades it follows that certain conditions must be observed. Manifoldness of characteristics in the material used must be avoided; variety of objects is disturbing and confuses the number sense. Hence, the dot—globe or half globe—affords the best unit for such an apparatus. [These conclusions are supported by the results of direct experimentation by the author. The dot (•) surpasses the stroke (I) in the relation of 179 : 307 errors in a given series; in other words, as a unit it is twice as fit for such use.] The color should be the same in all objects employed, and the conditions of contrast are best fulfilled by white and black arrangements. The size of the objects cannot be fixed; it is a function of their distance. The units must be distinctly visible, and the scheme should be susceptible of apprehension as a total at a single glance. As to distances between the objects, a space equal to the diameter of the units has proved most satisfactory. Since we are accustomed to number things presented in temporal series, as well as those forming simultaneous spatial arrangements, all such apparatus must be capable of rapid adjustment to either method.

These conditions the writer has tried to fulfil in his invention of a device called in the text Schneider's *Rechen-Federkästchen*. The author closes his account by urging home this most important point, that, in all early instruction in the use of numbers, the *operation* involved, as well as its units and results, should be represented.

It would be interesting to have such a discussion as Herr Schneider's carried to the point of considering the conditions under which an end is to be put, in the development of the individual, to the process which is here elaborated. For get rid of it we must; the very freedom and security with which we apply number concepts in all complex reckoning depend upon our release from the encumbrance of such pictorial representation, and the storing of the mind with directly associated abstract symbols, such as  $9 \times 12 = 108$ . Such a relation cannot be directly intuited, and the use of visual units would confuse instead of enlightening the sense. The question may very well be raised whether this dependence upon concrete material may not, in cases, hamper the capacity for handling more complex number-groups.

*Der Aufsatz in der Muttersprache.* By H. SCHILLER. Schiller and Ziehen's *Abhandlungen*, IV. Bd., 1. Heft. 1900.

The question of method in language teaching has long been under discussion. In our own schools to-day practice shifts from side to side. The mastery shown by the average college student is so far



from perfect as to cast serious doubt upon the methods now employed in the lower schools. One cries out for careful spelling as the indispensable preliminary to reading and writing; his neighbor conceives the process of syllabizing to be the only rational and economical procedure. One advocates the presentation of material visually, a second aurally, a third urges the addition to both of speech-motor reproduction. This instructor would dictate, that copy, the other trace the script already written upon board or slate. The one thing, Professor Schiller observes in beginning his essay, conclusively shown by these discrepant results—such, for example, as those of Lay, Fuchs, Hagen-Müller and Lobsien—is that no satisfactory psychophysiological method of learning to spell and write correctly has yet been discovered.

The task which the language teacher is called upon to accomplish is really a herculean one. He must teach the pupil to speak, to read, to write correctly. He must put into his possession as rich a vocabulary of mother-speech as possible; he must drill him in its grammar and construction; he must acquaint him with pieces of its best and most idiomatic literature. And, further, the pupil must be enabled to give adequate literary expression to that varied mental content which he has gained from the total group of studies which he pursues and from the flowing experience of life beyond the limits of school instruction. Moreover, this literary form of speech and writing must usually supplant an already existing dialect with which the child has entered school.

How is this flexibility and precision in the use of his mother-tongue to be attained by the pupil? By much reading, is commonly answered; supply the child with good literature and insensibly his own expression will take on precision, choiceness and variety. On the contrary, says our author, such a method cannot yield good results, for it is psychologically false. Where interest lies not in the literary form itself the reader, whether child or adult, habitually ignores the expression in his desire to apprehend the thought. Attention cannot be given to both content and form in the same reading. Training in the correct and pleasing use of speech requires that attention should be turned deliberately to construction and literary form. Mastery is to be won by *concentrated*, not extensive reading. Choose but few pieces for study and let the child read and re-read these, analyzing and studying them in detail until the meaning of every incident and word and the way of their putting together is mastered.

Training in one's mother-tongue seeks indeed to increase the gen-



eral store of ideas, but more especially to give flexibility and precision in the use of speech as an instrument for the description of experience. It accomplishes this end by providing variety in the form of expression, and developing a sense for the finer discriminations in thought relations.

Children describe and relate at an early age. What is lacking when they enter upon the school period is not so much mental images as means of expression. They commonly possess but one way of naming an experience, and can rarely give synonyms for terms. For many of their perceptions they have indeed no terms at all. The work of the teacher is to make of this halting speech an adequate expression of their growing experience.

In the process of communicating the content of consciousness oral expression regularly precedes the written. It should be a constant preliminary to it in the formal instruction of the school. The material to be worked up in written exercises should first be gone over carefully and repeatedly by reading, spelling, analysis and committing to memory, until not only the story as a whole is apprehended, but each division and connection of the piece, every incident and term, is intimately understood by the pupil.

To this end every unfamiliar term which the piece presents should be so explained that the pupil shall have a clear mental image corresponding to it. He should then be practiced in naming it by a variety of expressions. The formal phraseology of the book must not be fixed in mind; the pupil must break up this association and learn to revert to the image itself, with which now a plurality of terms has been connected. The invention of synonyms increases the child's command of the material already possessed, and makes the single mental image a starting-point of many associations. Only when the piece has thus been worked over and mastered orally is the pupil ready for his written exercises, a 'productive-reproductive' process, in which the pupil retells the story, shaping it in his own way and giving individualistic expression to it.

Not quantity, but quality of work, is the writer's repeated injunction. Much 'reading is loose reading. Study few pieces, and attend always to the form. Above all remember that oral exercises are the indispensable preliminary to successful written work. Professor Schiller illustrates his argument by specimens of analysis and paraphrase in detail.

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## EXPERIMENTAL.

*Ueber das Verhältniß der ebenmerklichen zu den übermerklichen Unterschieden bei Licht- und Schallintensitäten.* WILHELM AMENT. Philosophische Studien, Bd. XVI., 2 Heft. Pp. 135-196.

The investigation which Herr Ament reports was conducted in the Würzburg laboratory during the years 1897-99. Its point of departure is the distinction between two possible interpretations of just perceptible differences. Are these to be regarded as equally perceptible? Are they to be regarded as perceptibly equal? The answer to the second question only is given, and, within the limits of this investigation, it is negative: discrepancies in the results obtained by two methods are to be explained on the assumption that the difference-threshold becomes larger as the intensity of the stimuli increases.

The work of earlier investigators in this field, especially of Merkel and Angell, is critically reviewed in the first part of the article; the second part gives an account of the experiments upon which the author bases his conclusion. Light and sound stimuli were employed. The former consisted of colorless strips of paper giving 49 shades of gray, which were selected by the observer and arranged beneath a plate of glass upon white and black backgrounds. For the sound stimuli Kämpfe's apparatus was used.

The relation of the brightness thresholds was determined directly, *i. e.*, by comparing the just perceptible differences found in a given series with the quite perceptible differences of the same series. The corresponding relations in the auditory series were determined indirectly: the results obtained by the method of minimal variation were compared with those obtained by the method of mean variation. They were also determined directly for the purpose of comparison with the optical series.

The outcome of this research is that the estimated mean between two limiting stimuli varies from the geometric mean in the direction of the stronger stimulus; and that the variation is more considerable according as the proportions and the absolute intensities of the stimuli are greater. In other words, discrimination of stimuli and comparison of the intervals discriminated do not agree. Since the discrepancy is conditioned both by the size of the intervals and by their position in the scale, it cannot be gotten rid of by simply reducing the differences. Nor should it be attributed to such factors as contrast and

order of presentation, the influence of which is doubtful or at best insufficient to afford an explanation. The alternative is the author's conclusion, viz: that the assumption of the perceptible equality of all difference-thresholds shall be abandoned, together with Fechner's supposition that the threshold is the unit for measuring sensation.

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*Discrimination of Clangs for Different Intervals of Time.* F. ANGELL. American Journal of Psychology, Vol. XII., No. 1, 1900. Pp. 58-79.

The object of this investigation is to find out how the comparison of two reed tones differing slightly in pitch is influenced by the length of the time interval and by different distractions during this time interval. The experiments were undertaken with a special view of testing the memory-image theory of comparison. The differences of pitch were 4 and 8 vibrations for a norm of about 600 vibrations. The time interval varied from 1 to 60 seconds. Seven kinds of distraction were used: 1. Continuous addition of written figures. 2. Counting metronome beats—3 per second. 3. Counting metronome beats—1½ per second. 4. Reading letters of printed words backwards. 5. Reading interesting literature aloud (intoning). 6. Listening to interesting reading. 7. Discriminating between pairs of clangs differing by 8, 4 or 0 vibrations.

The results of the experiments are very interesting. Distraction often failed to distract in the sense of diminishing the number of correct judgments. If distraction had any effect at all on accuracy of judgment, for differences of 8 and 4 vibrations between norm and variable, it increased the accuracy about  $\frac{2}{3}$  as often as it decreased it; in a few instances it had no effect. Very different are the results for  $\Delta = 0$ . Here we find only one instance where the distraction was accompanied by an increase in accuracy of judgment, while in 26 instances the accuracy was decreased. The objectively like cases seem to differ indeed from the unlike stimuli in their relation to the processes of judgment, and this makes still more questionable the propriety of mixing the results of like and unlike stimuli together into general averages. Distraction affected differences of 8 and 4 vibrations about equally, a result that is not easily intelligible on the basis of a memory-image theory of comparison. The question as to the agreement between the *subjective* impression of the depth, or absorbing power, of a given distraction and the decrease in the percentage of right judgments is to be



answered negatively. The results of the experiments with distraction reflect very fairly the different opinions of different psychologists in regard to the effects of attention during the time interval. Wolfe inclines to the belief that the accuracy of comparison depends in great measure on the exertion of attention during the interval. Lehmann, on the other hand, found that his reagents could discriminate better between gray disks if they did not concentrate their attention on a memory-image in the meantime. Radoslawow gives for his results the explanation that by repeated reproductions of the norm the memory for it becomes 'blunted.' The common psychological explanation is so 'plastic,' as the author remarks, that it suits even diametrically opposed facts. The main conclusion to be drawn from the distraction experiments is that judgments of tone discriminations can take place without conscious comparison between the present sensation and a memory-image of a past sensation.

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*Studies from the Yale Psychological Laboratory: 1. Researches in Experimental Phonetics.* By E. W. SCRIPTURE. Vol. VII., 1899.

This article, though it contains few results which can be called quite new, is excellent in many respects. It contains a bibliography and a discussion of the various theories usually comprehended under the term 'phonetics,' so that it may be recommended as an introduction to phonetics. The experiments which the author made himself are valuable, since they confirm some theoretical conclusions of previous investigators with such certainty that no further doubt is left, and they demonstrate that some problems, unsolved as yet, no longer offer any technical difficulty, so that their final solution is but a question of time. The author has been particularly fortunate in selecting for a recording instrument the gramophone, which doubtless possesses, for investigations of this kind, great advantages over every other recording instrument, including the Edison phonograph.

There can no longer be any doubt that a *vowel* is made up of a 'cord tone' and one or more 'resonance tones' produced in the cavity of the mouth by the air puffs sent out from the larynx. It was for a long time supposed to be impossible to produce a resonance tone by means of the air puffs of a sounding reed. Hensen, who tried to prove experimentally that this was impossible, called those of his experiments in which a resonance tone was heard 'unsuccessful,' as



Scripture mentions, whereas he should have called just these experiments the successful ones. Scripture demonstrates, as previously shown by Willis and Hermann, that a resonance tone of a cavity may indeed be produced by the air-puffs of a reed instrument. Those opposed to this theory (Hensen, Helmholtz) assume that only an *overtone* of the cord tone could be *reinforced* by the resonance of the mouth cavity. Actually, however, the pitch of the resonance tone of the mouth is quite independent of the frequency of the air-puffs by which it is produced, except so far as the resonance tone of the mouth cannot be lower than the cord tone. A consequence of this independence is that in analyzing graphic records of speech sounds one must not make use of the Fourier analysis. This analysis would be permitted only if the 'resonance tones' were reinforced overtones of the cord tone. A vowel, therefore, is made up of a cord tone with its overtones and a certain number of independent resonance tones of the cavity of the mouth.

The author's comparison of the records of the diphthong *ai* in various words and in the same word when spoken with different emphasis and emotion is extremely interesting. The analyses show that the *ai* is not simply the succession of the two vowels *a* and *i*, but an organic union into a new sound *ai*. Thus, there is no necessary pause or sudden change of intensity, or change in pitch, or even change in character. The later sound shows its influence in the earlier one, and the earlier one keeps its influence far into the later one. This is what would be expected on psychological grounds. The author is certainly right in his assumption that the difference of character of a vowel, so noticeable in language, must depend upon certain differences of structure, though he has not yet succeeded in finding a definite law.

Of other results we will mention here only the most important: The tone of the *h* (*e. g.*, the first sound in 'who') is a resonance tone arising from the passage of the air through the mouth; it is not a cord tone. The first sound of 'who' is *not* a voiceless form of *u*.

Previous investigators have had in mind almost exclusively the vowels sung on musical notes. The voice tones of *spoken* vowels are seldom of constant pitch. Some are nearly constant in pitch, some fluctuate, some rise and fall, in various simple or complicated ways. When words are sung they lose most of their character; speech is capable of expressing by its modulations the various emotions and conditions of the individual, whereas the singer has few resources at his command.

English verse makes use of all three elements, length, pitch and

intensity, by means of which strength may be produced. The strength of a syllable may be kept the same by increasing one of the factors as another one decreases.

2. *Observations on Rhythmic Action.* By E. W. SCRIPTURE.

The author points out that regulated rhythmic action differs from free rhythmic action mainly in a *judgment* on the part of the subject concerning the coincidence of his movements with the sound heard or light seen. All physiological theories of regulated rhythmic action must be discarded. The 'difficulty' of free rhythmic action of various frequency the author expresses by a simple formula.

MAX MEYER.

UNIVERSITY OF MISSOURI.

## VISION.

*Beiträge zur Analyse der Gesichtswahrnehmungen.* By F. SCHUMANN. Zweite Abtheilung. Zur Schätzung räumlicher Grössen. Zeitschr. f. Psych., XXIV., 1 u. 2. Pp. 1-33.

In this second paper of the series Professor Schumann gives an analysis of the factors involved in the judgments of comparison, as they affect space perception. Briefly put, his problem is to seek out the conditions that are involved in the statement that one line is greater, less or equal to another. It is possible that an explanation may be in terms of elements that are to be revealed by an immediate examination of consciousness; that these mediating elements are conscious, but are only to be demonstrated indirectly, or that they are unconscious. The success of Schumann himself in the time-sense, of Müller and Miss Martin with little weights, and of Stumpf in his analysis of the factors involved in the recognition of the musical intervals give hope that a similar analysis of the space perception will also reveal mediate elements that will serve to explain the judgment.

The first fact revealed by the analysis is that other factors than the mere forms compared must be considered. For instance, a square upon its base seems smaller than the same square with one diagonal vertical because in the latter case we assume that the area is determined by the diagonals, instead of by the sides. Also the lines that bound areas tend to be added to the areas in the comparison.

The second principle to be considered is that the area or line attended to is increased in size. If, for example, you attend to the space between series of squares, the size of the space is increased as compared with the squares. Upon this principle is explained the over-es-

timation of vertical lines. Vertical lines tend to attract the attention, and therefore seem longer than the horizontal lines in a square. In a square made up of parallel vertical lines without the horizontals, on the contrary, the whole series cannot be grasped by the attention at once, but must be apprehended by successive acts; the attention travels over them in the horizontal direction, and therefore that distance predominates in the attention and consequently seems greater than the vertical. The Müller-Lyer illusion is to be explained from the fact that the areas are compared, and not the linear distances between the intersections of the lines.

In general, the whole series of distance illusions is to be explained in terms of the attention, instead of in terms of eye-movements. Professor Schumann's reasons for rejecting the old and substituting the new are not made very clear. The eye-movement theory is said to be *sehr hypothetisch* and *sehr unwahrscheinlich*, but how is not stated. On the other hand, he does not show any reason why the vertical should catch the attention, or why the thing attended to should seem larger, and he does not bring forward sufficient instances to warrant the statement as a bare fact.

However, the paper is to be welcomed as the best attempt to correct the over-valuation of the eye-movement sensations in the explanation of illusions that has as yet been made.

W. B. PILLSBURY.

### PSYCHOPHYSICAL.

*Studies in the Psychology of Alcohol.* By GEORGE E. PARTRIDGE. Amer. Jour. of Psych., Vol. XI. Pp. 318-376.

This paper is an account of a valuable investigation on 'the nature and origin of the intoxication impulse, considered in its relations to the mental development of the individual and of the race.' Mr. Partridge's effort to gather together the significant facts touching this singular impulse, as they appear in the race and in the individual, and to interpret them from the point of view of a theory of development, is deserving of serious attention.

According to our author, the intoxication impulse is neither a 'secondary instinct' having an accidental origin and involving heredity, nor the effect of an alcoholized protoplasm, a pathological perversion of physiological cell action (a physical craving); no more is it to be looked upon as an acquired 'taste'; but it is 'one form of expression of a general instinctive tendency which has developed in the race as an



aid to mental growth.' It is in this respect analogous to the play-instinct. This is the conclusion reached at the end of a paper of some sixty pages, divided into eight sections, which we proceed to name and summarize.

*Intoxication Among Primitive and Civilized Peoples and Among Animals.*—An examination of the anthropological literature leads to the conclusion that "Intoxication seems to have originated in connection with the religious and social consciousness; the use of intoxicating beverages came probably at a later stage. Taste was not much concerned at first."

*Comparative Effects of 'Intoxicants.'*—A report on an investigation upon the 'Effect of small doses of alcohol upon the ability to do muscular and mental work,' added by Mr. Partridge at the end of his paper (pp. 368-376) may be mentioned here. After a summary of the work done by others, the author passes to his own experiments. He used the ergograph, working twenty-five days, one hour in the morning and one hour in the afternoon, some days without, some days with alcohol. Another subject was tested under similar circumstances during thirteen days. The results agree in the main with those of the other investigators: 60 gr. of  $33\frac{1}{3}\%$  alcohol taken just before work decreased decidedly the working power (muscular) of H., while in the case of P. even 90 gr. failed to change the total amount of work done, but there is an increase during the first half hour compensated by a decrease during the second half hour. During a second hour of work the amount of work done was decreased. He tested further the effect of alcohol upon the rapidity of adding, reading (aloud at a maximum speed columns of figures) and writing (the digits at a maximum speed). Ninety grains of  $33\frac{1}{3}\%$  alcohol taken five minutes before work produced no material change in the amount of adding performed during two hours. For reading and writing—both essentially motor processes in this case—there is, as for the ergographic work, at first a quickening and then a decrease in the rapidity, both very small. For instance, the average number of figures written during the first hour is 221.6 without, and 224.3 with alcohol, and during the second hour respectively 228.9 and 227.4.

*A Study of Cases of Inebriety*, in which is given the alcoholic history of 65 dipsomaniacs or drunkards studied—most of them in criminal institutions—chiefly to determine the nature of the craving for intoxicants. The testimony of all the men who were questioned goes to show that the craving for alcohol is a rather unimportant part of the intoxication impulse. In 58 out of 65 cases there is no evi-



dence of a conscious craving for alcohol. The statement that after liquor has been for any reason cut off for a period of 10 to 20 days (10 days is usually enough) the craving ceases, must be regarded as expressing the true mental attitude toward drink of a class of people commonly considered incurable drunkards. The usual reply after they have been confined from 1 to 3 weeks is: "I never think of it, it never bothers me at all." It is equally true that the craving is generally intense for the first few days after cessation. But although they soon cease to desire alcohol, they continue to wish for tobacco, a fact for which the author finds a satisfactory explanation—provided his general theory is accepted—in the new and different circumstances in which the confined drunkard is placed with respect to alcohol and tobacco.

These and other facts, particularly the cures, of which he reports two, 'give evidence of the mental nature of the craving for drink,' *i. e.*, the craving is not specifically for alcohol; other drugs and other kinds of means could give it satisfaction.

*Effect of Intoxication upon the Literature and Language* containing a list of several hundred expressions used to denote the state of intoxication—a marvelously suggestive list! The glorification of pleasure and of abandon and the desire to escape from pain pervade the bacchic literature.

*Analogues of the Intoxication Impulse.*—Facts of animal, child, and adult human life show that there is "a normal craving for certain states of consciousness, the common element of which is intensity. These facts lead to the conclusion that intoxication is one form of expression of a more general impulse which is deep-seated in consciousness and very far-reaching in its effects." The habit of periodic intensification of consciousness by various means has been preserved and has become an instinct because it has favored mental development.

*Résumé and Criticism of Theories of the Intoxication Impulse. Summary of Facts and Outline of a Theory of the Intoxication Impulse.*—We have said in the beginning whatever the little space permitted on this final section of Mr. Partridge's work.

JAMES H. LEUBA.

BRYN MAWR COLLEGE.

*Sur la structure du cylindre axe des nerfs à myéline.* By M. G. WEISS. Comptes Rendus Soc. de Biol., Vol. LII., No. 13. April 6, 1900. Pp. 315-317.

In this brief communication Professor Weiss reiterates his previously expressed opinion that the axis-cylinders which have a medullary covering are made of a *homogeneous* mass in which the chromophil fibrils form a *network*. This is in direct contradiction to the belief of most observers of the finer neural structures, their common notion being that the axis-cylinder is made up of fibrils each separate, in its continuity, from its neighbors. In support of his contention, however, Professor Weiss offered to his colleagues preparations which he considered confirmatory of his opinion. And so still do 'doctors disagree.'

*The Minute Structure of the Medullary Sheath of Nerve-fibres.* By WILLIAM H. WYNNE, B.Sc., London. Journal of Anatomy and Physiology, Vol. XXXIV., Pt. III. April, 1900. Pp. 381-397.

The observations here described, made possible by the employment of a particular staining method in this particular place, add further structural detail to a part of the nerves about which there has been prolonged discussion. The author names more than thirty neurologists who have published opinions of this matter and gives the summary of the conclusion of each. There is general agreement only that the medullary sheath consists of two portions: a fatty substance or myelin, semi-fluid in consistence, and a firmer supporting framework or stroma. Mr. Wynne considers it certain that the axis-cylinder has a covering which is neurokeratinic in nature—a tough, strongly osmotic, insulating membrane such as Martinotti is certain surrounds the cortical nerve-cell bodies (see this REVIEW, May, 1900, p. 320).

Mr. Wynne's work was done mostly on the sciatic of the dog and of the cat, but in part on other nerves also and on the posterior right ganglia and on the spinal cord itself; in some instances human nerves were employed. The tissue was killed and hardened in five per cent. formalin and cut with the freezing microtome. The improvement in clearness obtained he considers due to the employment of a modified Weigert-Pal method of staining, a process peculiarly adapted to this particular work. For the details of the precise method employed the reader is referred to the original report.

His 'conclusions' give best the substance of the paper: "1. The medullary sheath examined by the Weigert-Pal method consists ap-

parently of two parts, \* \* \* myelin and a supporting framework. 2. The latter, as seen in sections prepared by the Weigert-Pal method, probably consists of two thin protoplasmic sheaths, one beneath the primitive sheath [that 'of Schwann'], the other surrounding the axis cylinder; and of a chain of hollow cones with their bases in the peripheral protoplasmic sheath and their apices in the central sheath. 3. Each cone is composed of six segments, placed at regular distances apart, and converging from the primitive sheath to the axis cylinder. 4. These cones are apparently protoplasmic in nature. 5. The neurokeratinic network, as usually described, is probably artificially produced from the protoplasmic layer beneath the primitive sheath. This network may not be composed of neurokeratin, but of protoplasm; its resistance to digestive fluids being due to the contained myelin. 6. The incisures of Schmidt or Lantermann's slits may be ruptures in the medullary substance between the cones."

These cones, then, in shape are very much like the bell of a cornet, having even the concavity on the outer side. They are placed at intervals about their own length. All those in one internode, the portion between two nodes of Ranvier, point in the same direction, while in alternate internodes they point in opposite directions. When the microscope-sections are made transversely across the nerve and only moderately mordanted, the appearances are, he thinks, still more interesting, for then the cones in question are shown to be made up of six (or rarely seven or eight) segments arranged symmetrically around the axis cylinder; the portions of the cone connecting these segments appear to be less strongly protoplasmic in composition, staining less easily. Analysis of nerve shows it to contain much proteid; as this is neither in the myelin nor in the neurokeratinic covering, it must be in the cones and in the axis cylinder, which last is relatively small in bulk. This report, says Mr. Wynne, is only preliminary to later more complete revelations. Four photomicrographs (on two plates) and four drawings, illustrate the interesting structures here described.

*On a Modification of the Helmholtz Theory of Hearing.* ALBERT A. GRAY, M.D., F.R.S.E. *Journal of Anatomy and Physiology*, Vol. XXXIV., Pt. III. April, 1900. Pp. 325-350.

This paper, read before the Royal Society at Dover, in September of last year, suggests what its author names 'the theory of maximum amplitudes' as an improvement on the classic doctrine of the action of the membrana basilaris of the cochlea of the ear. He was struck with the fact that "the ligamentum spirale increases in size and be-



comes distinctly fibrous as it passes from the apex of the cochlea to the base," and that the transition, though gradual, is very pronounced, the ligament becoming also more protoplasmic as it becomes more fibrous. [Todd and Bowman long since called it a muscle.] The fact that sound is undoubtedly analyzed in the cochlea was explained by Helmholtz, as is well known, by the supposition that each transverse fiber of the many thousands composing the basilar membrane is tuned to a particular pitch and to no other, and that when the stimulus of one fiber is transmitted to the fluid in the labyrinth this string alone is set in vibration, thence communicating its movement to the hair-cells, to the nerve-fibers under them, and so to the cerebral auditory center. Aside from the improbability that one fiber of the membrane could vibrate separately, Dr. Gray's objections to this long-accepted theory are chiefly two: one that it does not account for the empirical existence of *noise*, and the other that, as Lord Kelvin has demonstrated, the ear is, under certain circumstances, able to appreciate difference of *phase*, namely in case of imperfect harmonies other than unison. The former of these circumstances Helmholtz explained in a way now known to be inadmissible; the second he wrongly denied altogether, saying that in the supposed cases the upper partials of two notes beat together.

The author points out very interestingly that the ear properly is only an organ of touch, although one, of course, greatly more sensitive than that of the dermal end-organs. A mistuned unison giving slow beats is homologous to the case of two stimulated points of the skin so close together that they are felt as one; while the fact that movement is perceived along the skin between two points so close together that when stimulated simultaneously it is not felt, is similar to the circumstance that we can easily discriminate between two tones when sounded in succession when we could not if they were given together. The chief difference between the two sorts of touch-organs is said to lie in the fact that in the stimulation of the hair-cells of the ear there can be no surface stimulated, but only a point or usually a line.

The chief contention of Dr. Gray is that the ligamentum spirale of the cochlea draws on or at least firmly restrains the membrana basilaris at the place stimulated by the incoming waves of lymph, thus by its vibration projecting the hair-cells situated on it against the membrane of Corti or membrana tectoria; the pressure against one line of hair-cells is then restrained by the non-vibrating membrane last mentioned, and the hair-cells are pressed backward against the nerve fibers

entering at their base, and thus the auditory nerve is stimulated in proportion to the amplitude of the vibrations of the basilar membrane. But just as in stimulating one 'point' in the skin only one sensation is clearly felt, although several end-organs are doubtless stimulated, so in the ear consciousness is aware of only one tone or pitch, that namely of the line of cells which form the apex of the periodically-raised A-shaped portion of the basilar membrane. Hence the name given, 'the theory of maximum amplitudes.' By this theory a noise is merely a complex sound which we cannot analyze fully or at all; if it can be analyzed, a degree of musical experience is obtained. Our power of perceiving difference of phase under the circumstances above noted is explained by the supposition that in imperfect harmony displacement of the basilar membrane comes at intervals sufficiently long to allow of perception by the mind, while in the case of unison (when phase is not distinguished) this interval is too short.

This suggestion of what is the obviously more probable mode of action of the basilar membrane, as well as the close comparison of the ear and the dermal organs of touch, seems to be a distinct addition to neurology as well as to physiology.

*The Total Number of Functional Cells in the Cerebral Cortex of Man, and the Percentage of the Total Volume of the Cortex Composed of Nerve Cell Bodies, Calculated from Karl Hammarberg's Data; Together with a Comparison of the Number of Giant Cells with the Number of Pyramidal Fibers.* By HELEN BRADFORD THOMPSON. *Journal of Comparative Neurology*, Vol. IX., No. 2. 1899. Pp. 113-140.

This enumerative research, performed in the Neurological Laboratory of the University of Chicago, gives as its chief result a number of the nerve-cells *eight times as large as the largest previous estimate gave*, in mathematical terms 9,200 millions. Following the method and data of Hammarberg, but making corrections and supplying important omissions, this result was obtained with very great certainty as regards its relative accuracy. (He divided the cortex into sixteen regions and into six cellular layers, using for counting purposes unit cubes 0.1 mm. square containing in different regions varying numbers of cells, and then computed in columns extending through the cortex according to the data which the microscope has furnished.) The second part of the research estimates that the functional nerve-cells occupy only 1.37 per cent. of the total space filled by the cortex.

The third and last portion of the report concludes that as the num-

ber of the cortical 'giant cells' is only about 1,500 in excess of the pyramidal fibers which supply the limbs and trunk, in number about 158,222, the head and neck must be supplied from cells other than the giant cells, most probably by the large pyramidal cells which are found in the fifth layer of the lower part of the motor region, the centers for the head and neck.

G. V. N. DEARBORN.

TUFTS MEDICAL SCHOOL.

## NEW BOOKS.

*Ethics, Descriptive and Explanatory.* S. E. MEZES. New York and London, Macmillans. 1901. Pp. xxi + 435. \$2.60.

*Völkerpsychologie.* W. WUNDT. I Bd., Die Sprache. 2<sup>te</sup> Theil. Leipzig, Engelmann; New York, Stechert. 1900. Pp. x + 644. M. 15.

*The Riddle of the Universe.* E. HAECKEL. Trans. by J. McCABE. New York and London, Harpers. 1901. Pp. xii + 391. \$1.50.

*The Child: A Study in the Evolution of Man.* A. F. CHAMBERLAIN. London, Scott; New York, Scribners. 1900. Pp. xii + 498. \$1.50.

*James Martineau: a Biography and Study.* By A. W. JACKSON. Boston, Little, Brown & Co. 1900. Pp. xii + 459.

A well-written and interesting account of the life and thought of Martineau. Dr. Jackson's sympathy with his subject makes his account of Martineau's views attractive, and he writes with full information.

J. M. B.

*The Story of the Nineteenth Century Science.* By H. S. WILLIAMS. New York and London, Harpers. 1900. Pp. viii + 475.

Whatever experts may say of the other chapters of this book, that on psychology is lamentably meagre and poor. Certain work in brain physiology and mental pathology passes for the whole of 'experimental psychology.' The book, however, is instructive; and is illustrated with a galaxy of good portraits of men of science (not always, perhaps, well chosen).

J. M. B.



*Bibliography of Child Study for the year 1899.* By L. N. WILSON.  
Rep. from the Pedagog. Seminary, Clark Univ. Press. 1901.

In this we have the continuation of the excellent bibliography by the same hand already spoken of in these pages. It comprises 441 titles and a subject index. It is to be hoped that Mr. Wilson will continue to issue these lists.  
J. M. B.

*Constitution de l'Ethique.* By E. DE ROBERTY. Paris, Alcan, 1900. Pp. 223. Fr. 2.50.

*Justification and Reconciliation.* By A. RITSCHL. Trans. by H. R. MACKINTOSH and A. B. MACAULAY. Edinburgh, Clark. (Imported by Scribners, New York.) 1900. Pp. xii + 673. \$4.50.

*Addresses and Proceedings of the National (U. S.) Educational Association, Charleston Meeting, 1900.* Pub. by the Assoc., 1900. Pp. vi + 809.

*Dictionnaire de Physiologie.* By CH. RICHEL. Tome V., Fasc. 1, Dig-Ele. Paris, Alcan. 1900.

Each fascicle of this fine work contains one or more articles of great importance to psychologists. In earlier parts we have come upon the articles *Attention* (Janet), *Audition* (Gellé), etc. In this part there is an extended and complete treatment of Dioptrics (*Dioptrique oculaire*) extending over 71 pages (unsigned), an article on *Effort* by Wertheimer, and one on Pain (*Douleur*) by Richet. The extended bibliographies constitute an important feature of the 'Dictionnaire.' In the case of the biographical sketches (*e. g.*, in this volume, of Donders) we find in each case exhaustive lists of publications of all sorts by the subject, arranged by years, of dissertations printed under his supervision, and of biographical notices by others.  
J. M. B.

*Sanity of Mind.* By D. F. LINCOLN. New York and London. 1900. Pp. ii + 177.

A brief popular treatment of the causes, symptoms, and preventions of insane tendencies, with especial reference to education and mental hygiene.  
J. M. B.

*Le Mystère de Platon Aglaophamos.* By LOUIS PRAT. Paris, Alcan. 1901. Pp. xxii + 215. Fr. 4.

*Les Dilemmes de la Métaphysique pure.* By CH. RENOUVIER. Paris, Alcan. 1901. Pp. 282. Fr. 5.

*L'Éducation par l'Instruction et les Théories pédagogiques de Herbart.* By M. MAUXION. Paris, Alcan. 1901. Pp. 188. Fr. 2.50.

*Dix Années de Philosophie.* By L. ARRÉAT. Pp. 179. Fr. 2.50.  
A collection of book reviews—most of them superficial. (J. M. B.)

*Vorlesungen über Psychopathologie in ihrer Bedeutung für die normale Psychologie.* By G. STÖRRING. Leipzig, Engelmann. 1900. Pp. viii + 468. M. 6.

A course of university lectures (with bibliography). It would be serviceable in an English version. J. M. B.

*Psychologie des Willens, zur Grundlegung der Ethik.* By H. SCHWARZ. Leipzig, Engelmann. 1900. Pp. vii + 391.

*Psychologie de l'Invention.* By F. PAULHAN. Paris, Alcan. 1901. Pp. 185. Fr. \$2.50.

*Essai sur l'Esthétique de Lotze.* By A. MATAGRIN. Paris, Alcan. 1901. Pp. 163. Fr. 2.

*Education and the Philosophical Ideal.* By H. W. DRESSER. New York and London, Putnams. 1900. Pp. v + 254. \$1.25.

*The Haunting of B— House.* Ed. by A. GOODRICH-FREER (Miss X.) and JOHN MARQUESS OF BUTE. London, Pearson. 1900. 2/.

*L'Année Psychologique.* By A. BINET. Sixième année (1899). Paris, Schleicher Frères. 1900. Pp. 774. Fr. 15.

*Audition colorée observée chez des écoliers.* By A. LEMAITRE. Paris, Alcan; Geneva, Eggimann. 1901. Pp. 169.

*Stachyologie; weitere vermischte Aufsätze.* By P. J. MÖBIUS. Leipzig, Barth. 1901. Pp. xi + 219.

A 'mixed lot' of essays ranging from metaphysics to degeneration, and all showing the touch of an expert psychiatrist.

*Experimentelle Beiträge zur Lehre vom Gedächtniss.* By G. E. MÜLLER and A. PILZECKER. Leipzig, Barth. 1900. Pp. xi + 300. M. 8.

## NOTES.

WE have received the first issue of a new French journal, the *Revue de Philosophie*, a bi-monthly 'directed' by É. Peillaube, and published by Carré et Naud, 3 rue Racine (Paris) at 15 fr. (out of France) or 3 fr. per number. It is published under Catholic auspices. Its program is interesting and promising.

PROF. WM. JAMES writes from Rome under date of January 13: 'The health is better, and I expect to get on my walking legs again.'

WE are glad to learn that the life of the late Professor Henry Sidgwick is being written by Mrs. Sidgwick, his wife, Principal of Newnham College, in conjunction with his brother, Mr. Arthur Sidgwick, Reader in Greek in the University of Oxford.

THE *Cambridge University Reporter* of December 7 contains full reports of the addresses made, resolutions passed, etc., at a meeting held at Cambridge, on November 26, to consider the question of a suitable memorial to Professor Sidgwick. An influential committee was formed to solicit subscriptions. Several proposals were made as to the form of the memorial itself, among them that of a Lectureship in Moral Science and that of a Studentship in Philosophy, open to women. It would seem especially appropriate, considering the wide use of Professor Sidgwick's books in the American universities, and the great indebtedness which so many of our instructors and students owe to his teachings, that the American contributions to this fund should be large. The undersigned has been asked to act for the Executive Committee in America, and he will be glad to receive and forward any sums sent to him (at Princeton, N. J.).

J. MARK BALDWIN.

PROF. E. H. GRIFFIN, of Johns Hopkins University, has completed his course of Stone Lectures on 'Aspects of Contemporary Philosophy' before the Princeton Theological Seminary. They are soon to appear in book form.

WE regret to announce the death of Mr. F. W. H. Myers, of Cambridge, England, which occurred on January 17th. His new work, entitled 'Human Personality and its Survival of Bodily Death,' is soon to appear. Mr. Myers is principally known to psychologists from his work in 'Phantasms of the Living,' his connection with the British Society for Psychical Research, and his articles in the 'Proceedings' of that society; but he was an eminent literary critic as well.

M. DURAND (de Gros), whose theories in biology and philosophy have recently been revived in France, died on November 17th. His latest publication, 'Variétés Philosophiques,' came to us some weeks since.

IT is proposed to start a new 'Journal of Biological Statistics' to be called the *Biometrika*, and a circular inviting advance subscriptions has been issued by Prof. Karl Pearson (University College, London) and Prof. W. F. R. Weldon (Merton Lea, Oxford), to either of whom correspondence may be directed. The price announced (per year of four numbers) is 30 shillings—\$7.50—which seems dear; but it may be lowered, one should imagine, after one or two years. J. M. B.



It is with great regret that we announce the sudden death on February 19th of one of our younger collaborators, Professor Francis Kennedy, of the University of Colorado. Professor Kennedy graduated from Princeton University and took the Ph.D degree at Leipzig. He served as assistant in psychology at Princeton for one year, went to the University of Colorado in 1897, and was promoted to the full professorship in philosophy there the following year.

We congratulate our esteemed contemporary and older brother, *Mind*, upon its start under a new organization—the ‘Mind Association’—of which it prints details in the last issue (January, 1901). Long may it continue its career of ‘high thinking,’ even though—judging from its balance sheet—it continue to be a career of ‘plain living’! We advise every one—who has already subscribed for *THE PSYCHOLOGICAL REVIEW*!—to join the Mind Association. J. M. B.

ALL editorial (as well as business) communications should be addressed, from April 1 to September 15, 1901, to *THE PSYCHOLOGICAL REVIEW*, care Prof. H. C. Warren, Princeton, N. J.

# THE PSYCHOLOGICAL REVIEW.

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FROM THE PSYCHOLOGICAL LABORATORY OF  
THE UNIVERSITY OF CHICAGO.

## THE MONAURAL LOCALIZATION OF SOUND.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The present paper reports a series of observations upon the capacities of auditory localization in a person entirely deaf in one ear. In several of the experiments we have made parallel observations upon a person of normal hearing, and the cases are presented comparatively, in order to bring out as clearly as possible the peculiarities of monaural localization.

### I.

We may conveniently preface the report of our experiments with a brief survey of the current theories of binaural localization.

Two general types of theory have been advanced to account for the ability to localize sounds. One of these, with which the names of Preyer and Münsterberg are associated, refers auditory localization to the action of the semicircular canals.<sup>1</sup> This view has as yet met with no general acceptance. The other type of theory, which is widely maintained by psychologists and physiologists, is formulated with admirable precision in the following words of the British scientist, S. P. Thompson:<sup>2</sup>

<sup>1</sup>Preyer, *Pflüger's Archiv*, Bd. 40, S. 586. Münsterberg, *Beiträge z. Psychologie*, Heft II., S. 182. These writers differ distinctly in their conceptions of the precise *modus operandi* of the canals.

<sup>2</sup>S. P. Thompson, *Philosophical Magazine*, January-June, 1882, p. 415. This article contains an excellent critical discussion of the theories of Luca, Steinhauser, Graham Bell, Mayer, Mach, Lord Rayleigh and others. Among the best of recent investigations are those of Pierce, *PSYCHOLOGICAL REVIEW*, Vol. I., p. 461, and Matsumoto, *Yale Studies*, Vol. V., p. 1.

"Judgments as to the direction of sounds are based, in general, upon the sensations of different intensity in the two ears; but the perceived difference of intensity upon which a judgment is based, is not usually the difference in intensity of the lowest or fundamental tone of the compound sound (or 'clang'), but \* \* \* the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result in the quality of the sound."

This statement obviously makes differences in the intensity and quality of the sound sensations perceived by the two ears the fundamental factors in our localizations. Many of the adherents of this general view emphasize the importance of the intensity element, as if that were really the basal fact, and mention difference in quality as though it were somewhat incidental and distinctly of secondary importance. Thompson's formulation puts the matter in an undoubtedly more accurate form. The significant objective difference in sound waves reaching the two ears with unequal intensity is not so much in the amplitude of the fundamental vibration, which gives the sound its pitch, as in the amplitude of the upper partials, which contribute very largely to the timbre or quality of an auditory complex.<sup>1</sup> Generally speaking, then, a difference in the intensity of the sound stimulations reaching the two ears results in the consciousness of a difference in the auditory quality of the two sensations, rather than in a mere perception of inequality in the intensity of the two stimulations. Of course this cannot be the case with pure tones. But it is to be remembered that pure tones are notoriously difficult to localize with any approach to accuracy. Our auditory orientation corresponds, therefore, to the intensive and qualitative difference in the sound sensations of the two ears, and of these the qualitative differences are seemingly much the more conspicuous in consciousness.

<sup>1</sup>Thompson (*loc. cit.*) inclines to accept Lord Rayleigh's view that this modification of the partials is due chiefly to the effects upon the sound waves of the bones of the skull, as against Mach's contention that the pinnae are mainly responsible for the phenomena. Our observations, as will be seen, tend on the whole to confirm Lord Rayleigh's theory on this point. The relative importance of the two factors varies with different objective positions of sounds, and both of them are at times clearly operative.



Unless modified by explanatory corollaries, a formulation phrased in this manner is unquestionably open to the criticism urged by Stumpf<sup>1</sup> and others who follow him. It is somewhat more precise than the earlier statements of v. Kries<sup>2</sup> and the similar formula of Matsumoto,<sup>3</sup> quoted approvingly by Ladd.<sup>4</sup> But at best it leaves much to be desired on the score of accuracy of expression. When dealing with intensity differences, the explanation which the statement advances requires a perception of the intensity of the sound in one ear as greater or less than the intensity of the sound in the other ear. Or, when expressed in terms of quality, it requires a perception of the quality of the sound in one ear different from the quality of the sound heard by the other ear. Neither statement corresponds exactly to the facts. We are conscious of only one sensation, only one intensity and only one quality. This single sensation, with its intensity-quality attributes, is referred to some objective point in space. We may at times connect it with the predominant or exclusive action of one ear. But in no case is there any real comparison or analysis of the two intensities or qualities proceeding from the two ears.

Undoubtedly on some occasions the physical and physiological conditions resulting in the stimulation of the two ears with unequal intensities give rise to psychical processes involving localization of the sound. And undoubtedly, also, the physical and physiological conditions which are most often encountered are such as accompany differences in the quality of the sounds falling upon the two ears; with which, in turn, localizations may be connected. But this is a very different proposition from the one above quoted, with its implication of a conscious comparison of the intensities and qualities of sounds heard by the two ears.<sup>5</sup>

<sup>1</sup> Stumpf, *Tonpsychologie*, Vol. II., p. 50, ff.

<sup>2</sup> v. Kries, *Arch. f. Anat. und Physiol.* (Physiol. Abth.), 1877, p. 329. This author has, in a later publication (*Zeitsch. f. Psychol. und Physiol.*, Vol. I., p. 235) criticising Preyer and Münsterberg, made a much broader statement, including more explicit reference to the effects of quality upon auditory localization.

<sup>3</sup> *Loc. cit.*

<sup>4</sup> Ladd, *Outlines of Descriptive Psychology*, p. 191.

<sup>5</sup> We omit discussion of sounds heard as double, either in binaural or monaural hearing. They do not involve the point we are here discussing. Cf. Stumpf, *loc. cit.*

We learn to localize sounds on the basis of differences among them, which are partly differences of intensity and partly differences of quality, involving sometimes variations in apparent pitch. These differences in intensity and quality have their physical and physiological basis largely in the differences between the stimulations of the two ears. But these physical and physiological differences are not reported in consciousness in the form of sensations connected with the separate ears. The theories which refer auditory localization immediately to these factors are, therefore, primarily engaged with the physical and physiological pre-conditions of the process.<sup>1</sup> Intensity and quality are unquestionably involved in conscious processes of auditory localization, to say nothing of motor and visual elements.<sup>2</sup> But these formulations with which we have been dealing are in point of fact only secondarily concerned with the strictly psychical phases of the phenomena. We shall take cognizance, in connection with our own work, of both kinds of facts.<sup>3</sup>

## II.

We are now ready to begin the account of our experiments, and we shall anticipate at the outset our most important conclusion, in the belief that we may thus materially clarify our subsequent descriptions and discussions.

In the case of monaural hearing there can obviously be no question of the operation of the intensity factor in the form considered in the previous section. Provided the position of the head is fixed, changes in apparent intensity are, if unaccompanied by changes in quality, wholly ambiguous in monaural

<sup>1</sup> This procedure is much after the manner of the current color theories.

<sup>2</sup> Münsterberg's account (*loc. cit.*) of the part played in auditory localization by reflex and semi-reflex movements corresponds closely to the accepted view of the more careful psychological writers. This statement in no wise implies assent in such quarters to his theory of the dependence of these movements upon the action of the semicircular canals.

<sup>3</sup> It is no part of the present undertaking to go into the merits of the nativistic-empiristic space controversy, and our investigations touch only indirectly, if at all, the problem of the intrinsic voluminousness and extensity of auditory sensations. Our immediate interest is in the fundamental physical and psychical conditions upon which monaural localization depends, and such localization is clearly in a space of the conventional visual-tactual-kinæsthetic kind and not in one of a purely auditory character.

hearing. They may mean change of distance, change of direction, change of actual objective intensity with or without change of either distance or direction, or finally any combination of these factors, and the subject has absolutely no reliable means of determining which of these alternatives is really involved in any specific case.

The qualitative differences already described in the quotation from Thompson are differences connected with variations in the stimulations of two ears. We find that qualitative differences in sounds coming from different directions and falling upon one ear are quite adequate to serve for localizations of considerable accuracy. Monaural hearing involves apparently a system of local signs based on qualitative differences, which are rapidly acquired for any given series of sounds, when not possessed at the time the sounds are first heard. This development is quite independent of any knowledge imparted through channels other than the auditory sensation itself, and independent of any assistance from the turning of the head, which might confirm, by changing the perceived intensity, the original judgment based on quality.

The subject of the experiments is a man of thirty, who lost the use of his left ear, when four years of age, through ulceration of the middle ear consequent upon an attack of scarlet fever. The tympanic membrane was practically destroyed and a scar has replaced it. How far the bones of the middle ear are affected it is not possible to state. The malleus appears, however, to be in place. The internal ear was probably attacked by the disease, of which all traces save deafness disappeared at the age of fifteen. In any case the deafness on the affected side seems to be complete. To assure ourselves of this as far as possible, the subject was examined by an experienced aurist. The Weber test applied with the C fork resulted in strong lateralizing in the intact ear. For both high and low tones complete deafness was demonstrated. The  $C_4$  fork was heard when the intact ear was tightly closed. But it was heard with equal distinctness when the defective ear was also closed, showing that this ear had not participated in the transmission. The test for the duration of bone conduction could not be made,



because of the entire normality of one ear. The evidence points almost conclusively to lesion in the labyrinth.

The subject reports that, since the onset of the disease, he has never had the slightest reason to suppose he hears at all with this ear. The whole region of the external ear is, as compared with the intact side, rather numb in feeling. The normal ear is somewhat unusually acute. The intensive threshold is lower than that of the average person, and the sensibility for tone runs from 16 vibrations per second up to 35,000 per second. The musical capacity is good, with a limen for pitch discrimination rather better than the average. In no respect, however, is the hearing very remarkable.

The subject employed for comparison is entirely normal in every particular and requires, therefore, no further description.

*Apparatus.*—The apparatus used in these experiments was substantially the same as that used by Pierce and Matsumoto.<sup>1</sup> Our purpose was to give the subject sounds of the same quality and intensity from different directions. Equal intensity was secured by use of the same source of sound—in our experiments a telephone—placed always at the same distance from the head. To secure a uniform distance Pierce and Matsumoto used a spherical cage supported by standards, and so placed that the head of the subject, which was supported by a head-rest clamped to the back of his chair, should be in the center of the sphere. We found it more convenient to suspend our cage (diameter 4 ft.) from the gas-fixture by means of a wire. Using the wire as an axis of rotation, the experimenter could then stand always at the same place and, after attaching the telephone at the proper height, noiselessly turn the cage the required distance. Thus no suggestion of the location of the sound was given by the movements of the experimenter. In order to secure a constant position for the axis of rotation two uprights were fastened to the floor, and the equatorial circle of the sphere rested against them when the telephone-click was given. It should be added that we were thus able to reduce the framework of the sphere to two circles, the equatorial circle

<sup>1</sup> *Op. cit.*

and one vertical circle, the latter interrupted below to make room for the body of the subject.

In previous experiments upon auditory localization the subject was seated with the center of his head (the intersection of the visual axis with the line joining the two ears and named the auditory axis) in the center of the sphere. But since our subject was monaural it was at first thought necessary, in order that all stimuli might reach him with the same intensity, to center his intact ear (the right ear) rather than the head. This position was afterwards abandoned, for reasons to be mentioned later. It was also found convenient to nail a strip of wood to the floor, parallel with the front legs of the chair, and another strip in the same direction across the under side of the chair with the ends projecting. By placing his feet upon the first strip and his hands upon the projecting ends of the second, the subject could keep himself informed of the position of his body, in reference to which his localization was made. Without these aids his sense of bodily position seemed sometimes uncertain and seemed also to have a disturbing effect upon his localization of sounds.

We began by giving the sound twice for each test, but after the first series it was repeated often six to ten times, until the subject was satisfied with his judgment. The effect of this was simply to secure a normal condition of attention. The subject was then required to name the point from which the sound came, knowing of course the number and location of the points from which it might be given. The sound was given by the experimenter, who closed a circuit by means of a noiseless mercury-contact key. In some of the later experiments, however, it was found convenient to place a contact-key in the hands of the subject, allowing him to give the sound after being informed that the telephone was in position. This method would be recommended for future experiments as securing the best conditions of preparation. To avoid any suggestion of regularity in the order of the points given, the order for each day was determined in advance by shuffling cards bearing the names of the points.

Except where otherwise stated, the electric current used was from three Gonda cells. The current was passed through the primary circuit of an induction coil, in whose secondary circuit

was placed a d'Arsonval galvanometer. The galvanometer enabled us to verify the constancy of the current and the consequent constancy in the intensity of the telephone sound.

The method of notation used by Matsumoto appeared to be the most convenient. In his system the points are designated by significant letters. But we have not included in our system all the points used by him; and since the number of points from which the sound could come has an important relation to the numerical value of the error, we shall not be able to give satisfactory comparisons between his results and our own.

Our system of points was contained in three horizontal circles: an equatorial circle, whose plane passes through the center of the head, a circle  $45^\circ$  above the equatorial circle, and a third  $45^\circ$  below. There were in all sixteen points.

The points on the equatorial circle are designated as follows: *f* (front), the intersection of the visual axis of the subject with the circle in front of the subject; *b* (back),  $180^\circ$  from *f*; *r* (right), the intersection of the auditory axis with the circle on the right; *l* (left),  $180^\circ$  from *r*; *fr*,  $45^\circ$  from *f* and *r*; *br*,  $45^\circ$  from *b* and *r*; *fl*,  $45^\circ$  from *f* and *l*; *bl*,  $45^\circ$  from *b* and *l*.

The points on the upper circle are *fo*, *bo*, *ro*, and *lo*, *o* (over) denoting a point on the upper circle; those on the lower circle are *fu*, *bu*, *ru*, and *lu*, *u* (under) denoting a point on the lower circle.

Fifteen series of experiments were performed.<sup>1</sup> *Series I.* was for the purpose of determining the general characteristics of monaural as compared with binaural localization. A set of 480 experiments was made upon the monaural subject, *A*, and for purposes of comparison under approximately identical conditions a set of 232 was made upon *B*, a binaural subject. The order of each day's experiments was 24 upon *A*, then 24 upon *B*, followed by 24 more upon *A*. The results obtained from *A* are given in Table I., those from *B* in Table II.

The following points of comparison should be noted:

1. In the binaural localizations there were only two instances in which sounds in the median plane and the right and left hemispheres were confused with each other. Matsu-

<sup>1</sup> The series are numbered in the order in which they were performed.



moto's subject made no errors of this kind. But 72 such errors are recorded for *A*, all of which, however, are confusions between the median plane and one of the two hemispheres; in no case are the hemispheres confused with each other.

2. The number of *A*'s correct judgments is 227 out of 480

TABLE I.

Position of sound.	Direction in which the sound is perceived to lie.																	No. given.
f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r			
f	17	6		2	1	1							3			30		
fo		3	5	4							1	3	10		4	30		
bo			20									2	5		3	30		
b			1	25									4			30		
bu				1	28				1							30		
fu				18	2	1	1						7		1	30		
fl			1	14	2		5	1	7							30		
lo			3	2			9	15		1						30		
bl							6	1	3	20						30		
lu							1	12	2	15						30		
l						1	2	2		25						30		
fr											10	13			7	30		
ro			2		1							14	6	1	6	30		
br				1								1	22		6	30		
ru					1								4	23	2	30		
r													8		22	30		
Total																	480	

NOTE.—Heavy faced figures indicate right cases. Figures outside the larger squares indicate confusions of middle and right and left.

judgments, or 47.3%. *B*'s correct judgments were 168 out of 232 judgments, or 72.4%.

It will be of some interest to compare with these figures the results obtained by Matsumoto, though, as we have already noted, the difference in the number of points from which choices may be made renders the two results not exactly comparable; for the

points common to his system and ours he obtained out of 800 trials 483 right cases, or 60.4%.

3. The distribution of right judgments for *A* is: median plane 94, left hemisphere 42, right hemisphere 91. Noting that the median plane contains 6 points as against 5 in each hemi-

TABLE II.

Position of sound.	f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	No. given.
f	12	10															22
fo	1	15															16
bo		7	6														13
b				16	1												17
bu				7	15												22
fu		1	7	5	9	3											25
fl							12	4									16
lo								7			1						8
bl								3	10		1						14
lu									1	9							10
l								1			15						16
fr												12	1			1	14
ro													10				10
br				1										11			12
ru					1										7		8
r															1	8	9
Total																	232

NOTE.—Heavy faced type shows right cases. Figures outside the larger squares indicate transpositions of middle and right and left.

sphere, the ratio of right judgments is as follows: middle 15.7, left 8.4, right 18.2; that is, about half as many right cases occur in the left hemisphere as in either the middle or right. It is to be remembered that the intact ear was on the right. The distribution of right cases for *B* was as follows: 58% of those

in the middle plane, 91 % of those on the left, and 83% of those on the right.

4. The points most often confused with each other were, for *A*, as follows: *l* and *bl* 22 (times), *lo* and *bl* 21, *b* and *fu* 18, *bl* and *lu* 15, *lu* and *l* 15, *fl* and *b* 14, *fr* and *ro* 13, *fo* and *br* 10. For *B* such points were: *f* and *fo* 11, *fu* and *bu* 9, *bu* and *b* 8, *fo* and *bo* 7. It should be noted that *B*'s chief confusions are between points in the median plane, while those of *A* are distributed generally. Since the number of experiments upon the two subjects was unequal, a numerical comparison is not intended; the figures are given merely to show the most important confusions of each subject in order of prominence.

5. These confusions may be further classified as follows: Transpositions forward, *A* 29 (times), *B* 9; transpositions backward, *A* 84, *B* 26. The tendency of both was to locate sounds backward rather than forward of their true position.

6. The following shows the number of errors, classified according to amount of error (disregarding direction). It should be noted that, in our method, the least ascertainable error was 45°. This undoubtedly over-emphasizes the actual error; but the only means of overcoming the difficulty promised more serious complications and was therefore disregarded.

	45°	90°	120°	135°	180°
<i>A</i>	164	26	21	40	2
<i>B</i>	35	17		5	7

The following shows the same, classified according to percentage of the whole amount of error:

	45°	90°	120°	135°	180°
<i>A</i>	64.8	10.3	8.3	15.8	.8
<i>B</i>	54.7	26.6		7.8	10.9

*Series II.* was performed in order to show the effect of practice. It seemed clear (the subject being in ignorance of the results obtained from himself) that *A* had improved rapidly in accuracy during the progress of Series I. A second series, of 271 experiments, was then given to ascertain the extent of this improvement. For convenience, five points (*bu*, *l*, *br*, *ru*, and *r*) which had already shown a large percentage of correct



cases were omitted in Series II. These points gave in Series I. 120 right and 30 wrong cases. Subtracting these amounts from the totals we have, for the remaining 11 points, 107 right and 223 wrong. The results for these points in Series II. were 139 right and 132 wrong, the number of experiments being about evenly distributed among the different points. A comparison of the two series shows, then, for *A*: in the first series 32.4% right, 67.6% wrong; in Series II., 51.3% right, 48.7% wrong. The distribution and general character of the errors was about the same as in Series I. No further experiments were made upon *B*.

*Series III. and IV.*—Having ascertained that *A*'s capacity for localization was in general not greatly inferior to that of binaural subjects, it became our object to determine, if possible, just what his data for localization were. It was suggested that he might be guided by cutaneous sensations from his head and neck. Accordingly, two series of experiments were performed in the endeavor to eliminate this factor. The head and neck were covered with a camera-cloth, in which a slit had been made to allow the right ear to protrude. In Series III. the camera-cloth was bound loosely to the head by a strap passed across the forehead and around the head. The results of this series appeared at first to give distinctly positive information. The number of right cases was only 109 out of 318 experiments, and not only were the hemispheres confused with the median plane, but left was a number of times transposed to right. The possibility remained, however, that the transpositions and other errors were due to the cutting off of partial tones, on which basis, perhaps, the localization was made, by the loose folds of the camera-cloth. Accordingly, another series (IV.) was tried, this time with the camera-cloth bound close to the head and face by means of straps passed around the head in three directions. The ratio of right cases (158 out of 310, or 51%) and the nature of the errors were neither of them materially different from those in series II. It seems probable, then, that cutaneous sensations constituted a small part, if any, in the data for localization, though it is necessarily uncertain how far the possible effects of the sound waves were checked by the camera-cloth.

It seems fairly certain, however, that the strong cutaneous stimulation afforded by the contact of the cloth with the skin must have overpowered any effects due to the sound waves alone.

In this connection we should mention a later series (IX.), which was performed with the same end in view. Both ears were fully closed with oily putty as far as the outer rim of the pinnæ. The battery power was increased from three to five cells. 57 positions were then given, about evenly distributed among *f*, *b*, *r*, *l*. The only noticeable difference from the normal was the almost universal transposition of *f* to *b*.<sup>1</sup> Otherwise the localizations did not seem to be seriously affected. The net result of these three series was thus merely negative. It is possible, however, that interesting results in this direction might still be obtained from a subject absolutely deaf in both ears.

*Series V.*—A reference to Series I. and II. shows that sounds in the median plane were transposed frequently to the right but practically never to the left. The subject, *A*, who remained in ignorance of the results, also remarked that sounds which were (correctly) localized as *f* and *fl* appeared to come from a point somewhat to the right of the point which, from his knowledge of the system of points, he judged to be their true position. These facts seemed to indicate that he localized a sound with reference to his head rather than his ear. Accordingly, a series was tried in which the center of the head was located at the center of the sphere. The results are given in Table III.

Compared with Series I. and II., the following facts should be noted :

1. There were only 12 cases of confusion between the median plane and the hemispheres, as compared with 72 in Series I. and 33 in Series II. Comparing the number of experiments with Series I., there should be 48 such cases; with Series II., there should be 39. We have to do here undoubtedly in some measure with continued practice effects. It is, however, extremely improbable that practice is wholly accountable for the result.

<sup>1</sup> Münsterberg (*op. cit.*) using a partial filling of wax for the external meatus and conch, decided that the pinnæ were chiefly important in the localization of sounds from in front. Compare also note above on page 226.

It seems more reasonable to suppose that the conditions of this series were intrinsically more favorable for monaural localization.

2. The number of correct judgments is 240 out of 322, or 75%, as compared with 47.3% in Series I., 51.3% in Series II., 60.4%

TABLE III.

Position of sound.	Direction in which the sound is perceived to lie.																No. given.
	f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	
f	20																20
fo		16		7										1			24
bo			18													2	20
b				19										1			20
bu					19	1											20
fu	2			4		10								3			19
fl						1	16		3								20
lo		1						19									20
bl						1	17		0		2						20
lu									4	11	5						20
l							8	2	2		7						19
fr												19	1				20
ro													17		3		20
br	2													9		9	20
ru															20		20
r																20	20
Total																	322

NOTE.—Heavy faced type indicates right cases. Figures outside the larger squares indicate transpositions of the middle and right and left.

for Matsumoto's subject, and 72.4% for *B*. Taken as it stands the percentage is thus larger than any other recorded.

3. The distribution of right cases is: median plane, 102; right hemisphere, 85; left hemisphere, 53. Noting again that the number of points in the middle was 6, as compared to 5 in each hemisphere, the ratio is: median 17, right 17, left 11.



The greater proportion of error is naturally, as at the beginning, on the side of the deaf ear.

4. The main confusions are between  $f$  and  $bl$ , 20;  $f$  and  $l$ , 8;  $br$  and  $r$ , 9;  $fo$  and  $b$ , 7.

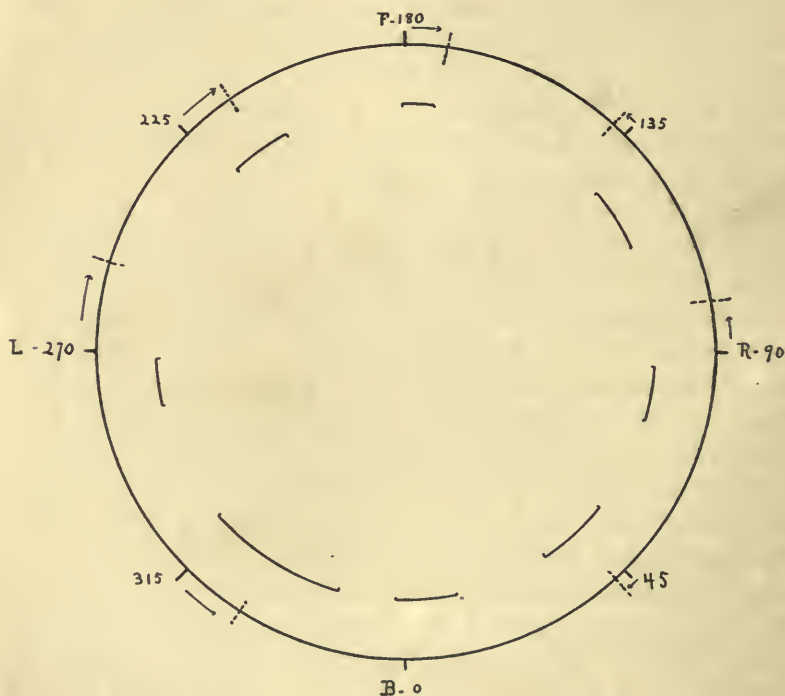
5. The most uncertain of all positions is  $bl$ , for which there are no right judgments. For some reason  $l$ , which was easily localized in Series I. and II., now partakes of the general uncertainty of all localizations in the left hemisphere. In this connection it should be noticed that  $lu$ , which has hitherto been very uncertain, now shows a fair percentage of right cases, the reason for which seemed to be that, in the first position of the body,  $lu$  was damped by the proximity of the subject's sleeve. This fact seems to have had a marked effect upon the apparent position of the sound.  $fu$ , which was close to the knees, was not at any time clearly identified, and was the only point seriously uncertain for  $B$ . These considerations point to the quality of the sound as a factor in localization.

*Series VI. and VII.* were merely confirmatory. Series VI. was for the purpose of eliminating as possible factors in localization the modifications of the sound due to the walls of the room and objects in the room. The subject had previously faced north; he now faced successively south, west and east, and in each position was given forty sounds, distributed equally among the four sound-positions most accurately localized ( $f$ ,  $r$ ,  $b$ ,  $bu$ ) and the four least accurately localized ( $fu$ ,  $l$ ,  $bl$ ,  $br$ ). A similar procedure was followed in Series VII., with the exception that here the variable factor was the intensity of the sound. The current had been furnished hitherto by three cells. In Series VII. the strength of the current was varied—two, three and five cells being used. Forty sounds were given with each arrangement of the cells—not, however, in separate groups, but in irregular order in one group, the subject not being informed of the strength of the current. No departure from the normal occurred in either series. This would again seem to indicate that the quality of the sounds, rather than the intensity of the fundamental tone in the sounds, was the important factor in the localization.

In *Series VIII.* we attempted to ascertain the location of the

eight points in the equatorial circle in the subjective auditory field of the subject *A*. Starting from a point on the equatorial circle about  $45^\circ$  from the point whose subjective location was required, the telephone was moved slowly toward the latter point, until the subject declared that it had been reached. The distance between the subjective and objective location of the point was then measured. Five measurements were made from each side of the point, and the mean of the averages of the two sets of five was chosen as the subjective location of the point. The locations were found to be as follows:  $f$ ,  $8.2^\circ$  toward  $r$ ;

FIG. 1.



The dotted lines intersecting the outer circle indicate the places at which the subject localized the cardinal points on the equatorial circle. (For explanation, see Series VIII.)

The arcs of the incomplete inner circle indicate roughly the limits of variation in the subjective field of the sounds localized at the several cardinal points opposite the arcs. Thus, a sound localized at  $90^\circ$  was almost invariably felt as coming from a point somewhat back of  $90^\circ$ , although the amount of this subjective displacement varied, as the line shows.

*fr*,  $2.6^{\circ}$  toward *f*; *r*,  $9.4^{\circ}$  toward *f*; *br*,  $2.2^{\circ}$  toward *b*; *b* at *b*; *bl*,  $12.5^{\circ}$  toward *b*; *l*,  $16^{\circ}$  toward *f*; *fl*,  $10.1^{\circ}$  toward *f*. The mean variations for the sets of five were, in degrees, as follows: at *f* 2.2; at *fr* 2.5; at *r* 2.7; at *br* 2.7; at *b* 3.4; at *bl* 4.8; at *l* 2.6; at *fl* 1.7.

The accompanying diagram illustrates the facts brought out in this series, together with certain other peculiarities of the subjective field of auditory localization.

*Series IX.* has already been adequately described.

The remaining six series were for the purpose of testing definitely the hypothesis that *A*'s localization of sounds rested upon the modification, through the shape of the ear, head or body, of particular partial tones for particular directions. The hypothesis will be stated more fully later. Assuming it to be true, we should expect the accuracy of localization to vary with the complexity of the sound, with the pitch, and possibly with the distance. Accordingly, the remainder of the experiments was devoted to testing these points.

In *Series X.* we used the Galton whistle in place of the telephone. The tone employed was of approximately 10,700 vibrations per second. 92 positions were given, of which 29, or 31.5%, were localized correctly. From the median plane 3 were transposed to the left, 8 to the right; from the left 8 were transposed to the median plane, 3 to the right; from the right 3 were transposed to the median plane.

In *Series XI.* a tone of approximately 5,000 vibrations per second was used. 62 positions of the sound were given, of which 14, or 22.6%, were localized correctly. The transpositions were: from middle to left 6, to right 9; from left to middle 2; from right to middle 3.

In *Series XII.* the whistle tone was lowered to approximately 2,500 vibrations per second. 46 positions were given, of which 32, or 70%, were correctly localized; no new transpositions appeared.

The tone of the Galton whistle is not pure, but save in the case of the deeper tones which it is capable of giving, *e. g.*, that used in *Series XII.*, the partial tones are weak and very high. The complex nature of the tone is, therefore, not easily



detected. We should expect, then, that the localizations of its higher tones would be more uncertain than those of the telephone. A reference to Series X. and XI. shows this to be the fact. Comparison may be made with either Series I. or V.

We should expect Series X. to show a smaller percentage of correct localizations than Series XI., as qualitative differences ought to be more difficult of detection, because the partial tones should be higher and weaker. The percentage of correct localizations is, however, slightly in favor of Series X. This fact is more than offset by the nature of the confusions and transpositions which are encountered in Series X. The hemispheres are confused with each other, as well as with the median plane. This does not occur in Series XI. The total number of tests is too small to warrant great insistence upon this difference. But the marked difference between Series X. and XI. on the one hand and Series XII. on the other suggests strikingly the effect of using sounds whose partial tones are weak and very high, as compared with those possessing partials which are relatively strong and well within the limits of audition. Series XII. shows almost as good results as Series V.

In *Series XIII.* we tried to discover whether the peculiar quality of the sound supposed to be the basis of its localization was a function of its distance from the ear. Accordingly, the eight equatorial positions were given each four times at a point thirty inches beyond the surface of the sphere, the telephone being used as the source of sound. The results were the same, however, as the normal results given in Series V. We can hardly conclude from this that greater variations of distance would be equally ineffective.

In *Series XIV.* the sound was given with a tuning-fork of 1,024 vibrations. The fork was placed at the surface of the sphere and only the eight equatorial positions were given. 42 sounds were given, of which 6, or 14.3 %, were correctly localized; the latter were one each at *f*, *fr*, *l* and *fl*, and two at *r*. In 10 cases the subject was unable to localize the sound at all.

In *Series XV.* we substituted a fork of 512 vibrations. 24 sounds were given, of which none were correctly localized;

20 were incorrectly localized and 4 could not be localized. Similar results were obtained with a fork of 256 vibrations.

The tuning-fork tones of Series XIV. and XV. are theoretically pure tones. As a matter of fact it is difficult to avoid, in striking the forks, some slight production of partial tones. This is more noticeable with the 1,024 fork than with the 512 and 256. For the two lower forks localization is clearly out of the question. In the case of the 1,024 fork, also, it is essentially impossible. The number of localizations correctly made in the latter case is somewhat above the number to be expected by the laws of chance, but it is to be added that the subjective attitude even in these correct cases was one of extreme uncertainty, and the total number of tests made is too small to permit of attaching much importance to the operations of mere chance. With the tuning-forks, and in less degree with all of the sounds we used, the change in position is accompanied by slight apparent changes in pitch. In the case of the telephone and whistle tests this change is partially, at least, incidental to the modification of the partial tones. In the case of the tuning-forks the cause is probably of another character, but whatever its nature it is inadequate to afford reliable localizations under the conditions we have studied.

### III.

Taking account of all the facts brought out by the tests, we reach the following conclusions:

1. The differences in the localizing capacity for complex sounds in binaural and monaural hearing are, so far as concerns these subjects, interpretable as chiefly differences in the magnitude of the difference limen for locality, rather than as absolute differences in the kind of localizing process involved. In monaural hearing the localization of sounds is by no means a matter of chance. On the side of the intact ear the localizations are in no sense seriously defective, and the distinction between front and back may be made in a manner superior to that of binaural hearing. (Cf. Series I., II. and V.) In the region opposite the deaf ear the localizations are extremely uncertain, but

elsewhere the errors met with are simply slight exaggerations of those made in binaural hearing.

It is not intended to imply in this statement that monaural localization is generally as prompt as binaural, nor that it is as accurate in its capacities for dealing with new and strange sounds. (Compare Series I. with Series II. and V. showing practice effects.) But practically all complex sounds are localized with a reasonable degree of accuracy, subject to the exceptions mentioned in connection with Series X., XI., XII., even upon a first hearing, and a little experience with the sound, familiarizing the subject with its quality changes, leads to such accuracy in its localization as is exhibited in Series V.

2. The experiments objectively considered amply sustain the introspection of the subject in pointing to qualitative differences in the sounds coming from different directions, as the basis of the localizations. This was explicitly commented upon by the subject very early in the course of the experimentation.

Sounds which are complex in nature undoubtedly undergo modification through the damping and reinforcing of their partial tones by the *pinnæ*, the external meatus and the head, in a manner which must vary somewhat regularly with variations in the spatial position of the object from which the sound emanates. When the changes in these objective positions are small, the corresponding changes in the quality of the sounds are ordinarily minute. But there is undoubtedly a general parallelism between the two series of events, and there is no reason why these variations should not be employed for auditory localizations as genuinely in monaural hearing as in binaural hearing, albeit necessarily with less of accuracy in some portions of the field.

The one region throughout which in monaural hearing such differences in the quality of sounds would be absent, or seriously ambiguous, is that immediately opposite the defective ear. In this case the whole of the head is interposed between the source of the sound and the intact ear. Sounds are much weakened in absolute intensity by this circumstance, as anyone may speedily convince himself, and the damping of the partial tones, upon which depend the qualitative peculiarities of sounds,



is essentially similar for all points in the region.<sup>1</sup> The extreme uncertainty and the persistent confusions which we encounter in monaural localization throughout this region are then precisely what we should anticipate, provided the qualitative differences of sounds constitute the basis of such localization.

Certainly it is striking that those sounds are most accurately and most promptly localized, which contain a considerable number of partial tones well within the limits of audition. The more nearly the sounds approach pure tones, the more inaccurate the localization. This is true regardless of the absolute pitch. Genuinely pure tones are essentially unlocalizable in monaural hearing. Such localization of these tones as is possible in binaural hearing is, therefore, probably referable to the intensity factor.<sup>2</sup> It is to be remembered that the deeper tones which we employed are practically pure. Most of the higher tones possess a few overtones. The sounds most accurately localized were always those in which qualitative differences were noticed and in which modification of partial tones was physically possible under the conditions.

It is assumed, in this statement, that the effects of experience, involving especially motor and visual processes, enter indispensably into acts of auditory localization. With adults, these intensive-qualitative auditory differences, to which we have made constant reference, simply furnish the relatively fixed psychic contents, whose spatial relations can then be learned. The process of giving to these elements values in the general visual-tactual-kinæsthetic space world is definitely the business of experience. How far some of these motor processes may be connected in a genuinely reflex and hereditary way with auditory stimulations is not a matter which we can at present discuss.

<sup>1</sup> The failure of some persons deaf in one ear to localize with any considerable degree of accuracy may be due to any one of a number of reasons, no one of which would impeach the validity of our interpretation.

<sup>2</sup> This radical defect in the monaural localization of pure tones furnishes a serious difficulty to those theories which refer auditory localization to the reflex action of the semi-circular canals. One can conceive of reasons why, as in binaural hearing, such localization should be uncertain and relatively inaccurate. But it is not so easy to explain how in monaural localization the process should work fairly well for many sounds, only to break down hopelessly upon pure tones.

Our examination of monaural hearing suggests, however, the extent to which one set of auditory symbols, *i. e.*, qualitative peculiarities in sounds, may serve for localizations. It substantiates indirectly, therefore, those theories of binaural localization which emphasize quality changes as more important for consciousness than mere variations in intensity.

It may be suggested that the changes in the quality of sounds, which seem to furnish the basis of monaural localization, are in one particular seriously ambiguous. Changes in distance, as well as changes in direction, are frequent causes of an apparent change in auditory quality. This being true, it would seem to indicate that in monaural hearing these two changes would often be confused with each other.

Undoubtedly variations in the quality of sounds may be produced by changes of either kind. But it is by no means certain that the effects upon the quality of the two kinds of change are sufficiently similar to be often confusing. Certainly our tests in Series XIII., although by no means conclusive, suggest strongly the ability to distinguish such qualitative-intensive differences as are due to changes in direction, from such as are due to changes of distance.

3. The presence of eye-reflexes was often very marked, and the final localization was frequently made on the basis of a seeming correspondence between the eye-strains and the supposed direction of the sound. This statement leaves wholly untouched the physiological basis of the eye-movements. It may be added, however, that the subject is rather a vivid visualizer and almost always thinks of spatial relations in visual form.

4. There is no good evidence for supposing that cutaneous sensations played any part in the localizations.

THE INFLUENCE OF IMPROVEMENT IN ONE  
MENTAL FUNCTION UPON THE  
EFFICIENCY OF OTHER  
FUNCTIONS. (I.)

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This is the first of a number of articles reporting an inductive study of the facts suggested by the title. It will comprise a general statement of the results and of the methods of obtaining them, and a detailed account of one type of experiment.

The word function is used without any rigor to refer to the mental basis of such things as spelling, multiplication, delicacy in discrimination of size, force of movement, marking *a*'s on a printed page, observing the word *boy* in a printed page, quickness, morality, verbal memory, chess playing, reasoning, etc. Function is used for all sorts of qualities in all sorts of performances from the narrowest to the widest, *e. g.*, from attention to the word 'fire' pronounced in a certain tone, to attention to all sorts of things. By the word improvement we shall mean those changes in the workings of functions which psychologists would commonly call by that name. Its use will be clear in each case and the psychological problem will never be different even if the changes studied be not such as everyone would call improvements. For all purposes 'change' may be used instead of 'improvement' in the title. By efficiency we shall mean the status of a function which we use when comparing individuals or the same individual at different times, the status on which we would grade people in that function. By other function we mean any function differing in any respect whatever from the first. We shall at times use the word function-group to mean



those cases where most psychologists would say that the same operation occurred with different data. The function *attention*, for instance, is really a vast group of functions.

Our chief method was to test the efficiency of some function or functions, then to give training in some other function or functions until a certain amount of improvement was reached, and then to test the first function or set of functions. Provided no other factors were allowed to affect the tests, the difference between the test before and the test after training measures the influence of the improvement in the trained functions on the functions tested.

It is possible to test the general question in a much neater and more convenient way by using, instead of measures of a function before and after training with another, measures of the correlation between the two functions. If improvement in one function increases the efficiency of another and there has been improvement in one, the other should be correlated with it; the individuals who have high rank in the one should have a higher rank in the other than the general average. Such a result might also be brought about by a correlation of the inborn capacities for those functions. Finding correlation between two functions thus need not mean that improvement in one has brought increased efficiency in the other. But the absence of correlation does mean the opposite. In an unpublished paper Mr. Clark Wissler, of Columbia University, demonstrates the absence of any considerable correlation between the functions measured by the tests given to students there. Miss Naomi Norsworthy, of Teachers College, has shown (the data were presented in part at the Baltimore meeting; the research is not yet in print) that there is no correlation between accuracy in noticing misspelled words and accuracy in multiplication, nor between the speeds; that there is little or no correlation between accuracy and speed in marking on a printed page misspelled words, words containing *r* and *e*, the word *boy*, and in marking semi-circles on a page of different geometrical figures.

Perhaps the most striking method of showing the influence or lack of influence of one function on another is that of testing the same function-group, using cases where there are very

slightly different data. If, for instance, we test a person's ability to estimate a series of magnitudes differing each from the next very slightly, and find that he estimates one very much more accurately than its neighbors on either side, we can be sure that what he has acquired from his previous experience or from the experience of the test is not improvement in the function-group of estimating magnitudes but a lot of particular improvements in estimating particular magnitudes, improvements which may be to a large extent independent of each other.

The experiments, finally, were all on the influence of the training on efficiency, on ability as measured by a single test, not on the ability *to improve*. It might be that improvement in one function might fail to give in another improved ability, but succeed in giving ability to improve faster than would have occurred had the training been lacking.

The evidence given by our experiments makes the following conclusions seem probable :

It is misleading to speak of sense discrimination, attention, memory, observation, accuracy, quickness, etc., as multitudinous separate individual functions are referred to by any one of these words. These functions may have little in common. There is no reason to suppose that any general change occurs corresponding to the words 'improvement of the attention,' or 'of the power of observation,' or 'of accuracy.'

It is even misleading to speak of these functions as exercised within narrow fields as units. For example, 'attention to words' or 'accurate discrimination of lengths' or 'observation of animals' or 'quickness of visual perception' are mythological, not real entities. The words do not mean any existing fact with anything like the necessary precision for either theoretical or practical purposes, for, to take a sample case, attention to the meaning of words does not imply equal attention to their spelling, nor attention to their spelling equal attention to their length, nor attention to certain letters in them equal attention to other letters.

The mind is, on the contrary, on its dynamic side a machine for making particular reactions to particular situations. It works in great detail, adapting itself to the special data of which

it has had experience. The word *attention*, for example, can properly mean only the sum total of a lot of particular tendencies to attend to particular sorts of data, and ability to attend can properly mean only the sum total of all the particular abilities and inabilities, each of which may have an efficiency largely irrespective of the efficiencies of the rest.

Improvement in any single mental function need not improve the ability in functions commonly called by the same name. It may injure it.

Improvement in any single mental function rarely brings about equal improvement in any other function, no matter how similar, for the working of every mental function-group is conditioned by the nature of the data in each particular case.

The very slight amount of variation in the nature of the data necessary to affect the efficiency of a function-group makes it fair to infer that no change in the data, however slight, is without effect on the function. The loss in the efficiency of a function trained with certain data, as we pass to data more and more unlike the first, makes it fair to infer that there is always a point where the loss is complete, a point beyond which the influence of the training has not extended. The rapidity of this loss, that is, its amount in the case of data very similar to the data on which the function was trained, makes it fair to infer that this point is nearer than has been supposed.

The general consideration of the cases of retention or of loss of practice effect seems to make it likely that spread of practice occurs only where identical elements are concerned in the influencing and influenced function.

The particular samples of the influence of training in one function on the efficiency of other functions chosen for investigation were as follows:

1. The influence of certain special training in the estimation of magnitudes on the ability to estimate magnitudes of the same general sort, *i. e.*, lengths or areas or weights, differing in amount, in accessory qualities (such as shape, color, form) or in both. The general method was here to test the subject's accuracy of estimating certain magnitudes, *e. g.*, lengths of lines. He would, that is, guess the length of each. Then he would



practice estimating lengths within certain limits until he attained a high degree of proficiency. Then he would once more estimate the lengths of the preliminary test series. Similarly with weights, areas, etc. This is apparently the sort of thing that happens in the case of a tea-taster, tobacco-buyer, wheat-taster or carpenter, who attains high proficiency in judging magnitudes or, as we ambiguously say, in delicacy of discriminating certain sense data. It is thus like common cases of sense training in actual life.

2. The influence of training in observing words containing certain combinations of letters (*e. g.*, *s* and *e*) or some other characteristic on the general ability to observe words. The general method here was to test the subject's speed and accuracy in picking out and marking certain letters, words containing certain letters, words of a certain length, geometric figures, misspelled words, etc. He then practiced picking out and marking words of some one special sort until he attained a high degree of proficiency. He was then re-tested. The training here corresponds to a fair degree with the training one has in learning to spell, to notice forms and endings in studying foreign languages, or in fact in learning to attend to any small details.

3. The influence of special training in memorizing on the general ability to memorize. Careful tests of one individual and a group test of students confirmed Professor James' result (see *Principles of Psychology*, Vol. I., pp. 666-668). These tests will not be described in detail.

These samples were chosen because of their character as representative mental functions, because of their adaptability to quantitative interpretations and partly because of their convenience. Such work can be done at odd times without any bulky or delicate apparatus. This rendered it possible to secure subjects. In all the experiments to be described we tested the influence of improvement in a function on *other functions closely allied to it*. We did not in sense-training measure the influence of training one sense on others, nor in the case of training of the attention the influence of training in noticing words on, say, the ability to do mental arithmetic or to listen to a metaphysical discourse. For common observation seemed to give a negative

answer to this question, and some considerable preliminary experimentation by one of us supported such a negative. Mr. Wissler's and Miss Norsworthy's studies are apparently conclusive, and we therefore restricted ourselves to the more profitable inquiry.

#### A SAMPLE EXPERIMENT.

There was a series of about 125 pieces of paper cut in various shapes. (Area test series.) Of these 13 were rectangles of almost the same shape and of sizes from 20 to 90 sq. cm. (series 1), 27 others were triangles, circles, irregular figures, etc., within the same limits of size (series 2). A subject was given the whole series of areas and asked to write down the area in sq. cm. of each one. In front of him was a card on which three squares, 1, 25 and 100 sq. cm. in area, respectively, were drawn. He was allowed to look at them as much as he pleased but not to superpose the pieces of paper on them. No other means of telling the areas were present. After being thus tested the subject was given a series of paper rectangles,<sup>1</sup> from 10 to 100 sq. cm. in area and of the same shape as those of series 1. These were shuffled and the subject guessed the area of one, then looked to see what it really was and recorded his error. This was continued and the pieces of paper were kept shuffled so that he could judge their area only from their intrinsic qualities. After a certain amount of improvement had been made he was retested with the 'area test series' in the same manner as before.

<sup>1</sup>The judgments of area were made with the following apparatus: a series of parallelograms ranging from 10 to 140 and from 190 to 280 sq. cm., varying each from the next by 1 sq. cm. Their proportions were almost the same (no one of them could possibly be distinguished by its shape). For example, the dimensions of those from 137 to 145 sq. cm. were

15	× 9.133⅓
15	× 9.2
15.1	× 9.2 +
15.1	× 9.275
15.15	× 9.31
15.15	× 9.375
15.2	× 9.41
15.2	× 9.475
15.2	× 9.54

The function trained was that of estimating areas from 10 to 100 sq. cm. with the aid of the correction of wrong tendencies supplied by ascertaining the real area after each judgment. We will call this 'function *a*.' A certain improvement was noted. What changes in the efficiency of closely allied functions are brought about by this improvement? Does the improvement in this function cause equal improvement (1) in the function of estimating areas of similar size but different shape without the correction factor? or (2) in the function of estimating identical areas without the correction factor? (3) In any case how much improvement was there? (4) Is there as much improvement in the function of estimating dissimilar shapes as similar? The last is the most important question.

We get the answer to 1 and part of 3 by comparing in various ways the average errors of the test areas of dissimilar shape in the before and after tests. These are given in Table I. The average errors for the last trial of the areas in the training series similar in size to the test series are given in the same table.

TABLE I.

Subject.	Test series 2.		Training series. Av. error at end of training.
	Av. error before training.	Av. error after training.	
T	15.8	11.1	2.3
Be.	28.0	5.2	3.1
Br.	22.5	18.7	3.3
J. W.	12.7	21.0	1.5 approx.
W. (2)	17.0	20.0	4.0 "
E. B.	10.5	7.9	0.4

The function of estimating series 2 (same sizes, different shapes) failed evidently to reach an efficiency equal to that of the function trained. Did it improve *proportionately* as much?

This is a hard question to answer exactly, since the efficiency of 'function *a*' increases with great rapidity during the first score or so of trials, so that the average error of even the first twenty estimates made is below that of the first ten, and that again is below that of the first five. Its efficiency at the start depends thus on what you take to be the start. The fact is that the first estimate of the training series is not an exercise of 'function *a*' at all and that the *correction* influence increases



up to a certain point which we cannot exactly locate. The fairest method would seem to be to measure the improvement in 'function *a*' from this point and compare with that improvement the improvement in the other function or functions in question. This point is probably earlier in the series than would be supposed. If found, it would probably make the improvement in 'function *a*' greater than that given in our percentages.

The proportion of average error in the after test to that in the before test is greater in the case of the test series than in the case of the first and last estimations of the areas of the same size in the training series, save in the case of *Be*. The proportions are given in the following table :

TABLE II.

Proportion of error after to error before training.		
Subject.	Test series 1.	Training series.
T.	.70	.575
Be.	.19	.56
Br.	.83	.53
J. W.	1.75	.77 approx.
W (2)	1.18	.83 approx.
E. B.	.75	.13

Question 2 is answered by a comparison of the average errors, before and after the training, of Series I. (identical areas) given without the correction factor. The efficiency reached in estimating without the correction factor (see column 2 of Table III.) is evidently below that reached in 'function *a*.' The results there in the case of the same areas are given in column 3.

TABLE III.

Subject.	Av. error before training of series 1.	Av. error after training of series 1.	Av. error after training of same sizes in training series.	Av. error after training of series 2.	Proportion of error after to error before training.		
					Series 2.	Series 1.	Areas of training series identical with series 1.
T.	9.0	6.0	2.1	11.1	.70	.67	.31
Be.	21.9	6.4	1.8	5.2	.19	.29	.45
Br.	24.2	14.7	3.7	18.7	.83	.61	.37
J. W.	7.7	8.6	1.5 app.	21.0	1.75	1.11	.77 app.
W. (2)	11.6	3.3 app.	4.0 app.	20.0	1.18	.28 app.	.83 app.
E. B.	9.8	4.1	0.4	7.9	.75	.42	.08

The function of estimating an area while in the frame of mind due to being engaged in estimating a limited series of areas and seeing the extent of one's error each time, is evidently independent to a large extent of the function of judging them after the fashion of the tests.

If we ask whether the function of judging without correction improved proportionately as much as 'function *a*,' we have our previous difficulty about finding a starting point for *a*. Comparing as before the first 100 estimates with the last 100 we get the proportions in the case of the areas identical with those in the test. These are given in column 7. The proportions in the case of the test areas (series 1; same shape) are given in column 6. A comparison of columns 6 and 7 thus gives more or less of an answer to the question, and column 6 gives the answer to the further one: "How much improvement was there?"

We can answer question 4 definitely. Column 5 repeats the statement of the improvement in the case of the test areas of different shape, and by comparing column 6 with it we see that in every case save that of Be. there was more improvement when the areas were similar in shape to those of the training series. This was of course the most important fact to be gotten at.

To sum up the results of this experiment, it has been shown that the improvement in the estimation of rectangles of a certain shape is not equalled in the case of similar estimations of areas of different shapes. The function of estimating areas is really a function-group, varying according to the data (shape, size, etc.). It has also been shown that even after mental standards of certain limited areas have been acquired, the function of estimating with these standards constantly kept alive by noticing the real area after each judgment is a function largely independent of the function of estimating them with the standards fully acquired by one to two thousand trials, *but not constantly renewed by so noticing the real areas*. Just what happened in the training was the partial formation of a number of associations. These associations were between sense impressions of particular sorts in a particular environment coming to a person in a particular mental attitude or frame of mind, and a number of ideas or impulses.

What was there in this to influence other functions, other processes than these particular ones? There was first of all the acquisition of certain improvements in mental standards of areas. These are of some influence in judgments of different shapes. We think, "This triangle or circle or trapezoid is about as big as such and such a rectangle, and such a rectangle would be 49 sq. cm." The influence is here by means of an idea that may form an identical element in both functions. Again, we may form a particular habit of making a discount for a tendency to a constant error discovered in the training series. We may say, "I tend to judge with a minus error," and the habit of thinking of this may be beneficial in all cases. The habit of bearing this judgment in mind or of unconsciously making an addition to our first impulse is thus an identical element of both functions. This was the case with *Be*. That there was no influence due to a mysterious transfer of practice, to an unanalyzable property of mental functions, is evidenced by the total lack of improvement in the functions tested in the case of some individuals.

On pushing our conception of the separateness of different functions to its extreme, we were led to ask if the function of estimating one magnitude might not be independent even of the functions of estimating magnitudes differing only slightly from the first. It might be that even the judgment of areas of 40-50 sq. cm. was not a single function, but a group of similar functions, and that ability might be gained in estimating one of these areas without spreading to the others. The only limits that must necessarily be set to this subdivision would be those of the mere sensing of small differences.

If, on the contrary, judgments of nearly equal magnitudes are acts of a single function, ability gained in one should appear in the others also. The results of training should diffuse readily throughout the space covered by the function in question, and the accuracy found in judgments of different magnitudes within this space should be nearly constant. The differences found should simply be such as would be expected from chance.

The question can be put to test by comparing the actual difference between the average errors made, in judging each of



neighboring magnitudes, with the probable difference as computed from the probability curve. If the actual difference greatly exceeds the probable difference, it is probably significant of some real difference in the subject's ability to judge the two magnitudes. He has somehow mastered one better than the other. No matter how this has come about. If it is a fact, then clearly ability in the one has not been transferred to the other.

Our experiments afford us a large mass of material for testing this question. In the 'training series,' we have a considerable number (10 to 40) of judgments of each of a lot of magnitudes differing from each other by slight amounts. We have computed the accuracy of the judgment of each magnitude (as measured by the error of mean square), and then compared the accuracy for each with that for the adjacent magnitudes. We find many instances in which the difference between the errors for adjacent magnitudes is largely in excess of the probable difference. And the number of such instances greatly exceeds what can be expected from chance.<sup>1</sup>

These great differences between the errors of adjacent magnitudes are strikingly seen in the curves on page 259. The ordinates of these curves represent the mean square error of judgments of areas of 10 to 100 square centimeters, and for 3 individuals. The dots above and below each point of the curve give the 'limits of error' of that value, as determined by the formula,  $\frac{\mu}{\sqrt{2n}}$ , in which  $\mu$  is the error of mean square, and  $n$

the number of cases. These limits are such that the odds are about 2 to 1, more exactly 683 to 317, that the true value lies inside them. The dots thus furnish a measure of the reliability of the curve at every point.

<sup>1</sup> The smaller error at certain magnitudes is not the result of a preference of the subject to guess that number. Of course, if the subject were prone to guess '64 square centimeters' oftener than 63 or 65, he would be more apt to guess 64 right, and the error for 64 would be diminished. We therefore made a few tables of the frequency with which each number was guessed. But we found that the magnitudes that were best judged were not more often guessed than their neighbors.

These curves are all irregular, with sudden risings and fallings that greatly obscure their general course. Psychologists are familiar of old with irregularities of this kind, and are wont to regard them as effects of chance, and so to smooth out the curve. But as we find more irregularity than can reasonably be attributed to chance, we conclude that our curves at least should not be smoothed out, and that the sudden jumps, or some of them, signify real differences in the person's ability.<sup>1</sup>

If, for example, we examine Fig. 1, we notice a number of sudden jumps, or points at which the errors in judging adjacent magnitudes differed considerably from each other. The most significant of these jumps are at 10-11, 36-37, 41-42, 65-66, 66-67, 83-84, and 98-99 sq. cm. The question is whether such a jump as that at 41-42 indicates greater ability to judge 42 sq. cm., or whether the observed difference is simply due to chance and the relatively few cases (here 10 for each area). A vague appeal to chance should not be allowed, in view of the possibility of calculating the odds in favor of each side of the question. This can be done by a fairly simple method. We can consider two adjacent areas as practically equal, so far as concerns Weber's law or any similar law. The average errors found for the two would thus be practically two determinations of the same quantity, and should differ only as two determinations of the same quantity may probably differ.

We wish then to compare the actual difference between the errors for 41 and 42 sq. cm. with the probable difference. The error—we use throughout the 'error of mean square,' and the measure of reliability based on it—this error is here 6.2 and 3.1 sq. cm. respectively. The actual difference is 3.1 sq. cm. To find the probable difference, we first find the 'limits of error' or reliability of each determination, as described above, and then find the square root of the sums of the squares of these

<sup>1</sup> The fact that judgments of nearly equal magnitudes may show very unequal errors throws doubt on all curves drawn from the judgment of only a few 'normals.' If slightly different normals had been chosen, the errors might have been considerably different, and the course of the curve changed. If, for example, three normals be chosen from the 91 in our curves, and those three used as the basis of a curve, the curve will vary widely with the choice of the three normals.



FIG. 1.



FIG. 2.



FIG. 3.



'limits of error.' The 'limits' are here 1.0 and 0.7, and the probable difference 1.2 sq. cm. The actual difference is 2.6 times the probable. In this whole series we find 6 other instances in which the actual difference is over 2 times the probable. From the probability integral we find that, in the long run, 46 actual differences to the thousand would exceed twice the probable. The question is, therefore, what is the probability of finding as many as 7 such differences in a series of 90? This is a form of the familiar problem in probabilities: to find the chances that an event whose probability is  $p$  shall occur at least  $r$  times out of a possible  $n$ . The solution depends on an application of the binomial theorem, and may be evaluated by means of logarithms. In the present case, the value found is .1209 or about  $\frac{1}{8}$ .

Instead of vaguely saying that the large jumps seen in the curves may be due to chance, we are now able to state that the odds are 7 to 1 against this view, and 7 to 1 in favor of the view that the large jumps, or some of them, are significant of inequality in the person's power to estimate nearly equal areas. These odds are of course not very heavy from the standpoint of scientific criticism. But they are fortified by finding, as we do, the same general balance of probability in all of the series examined. In one other series, the number of large differences is small, and the probability is as large as .2938 that they are due to mere chance. In three other series, this probability is very small, measuring .0025, .0025, .0028, or about  $\frac{1}{400}$ . Finally, in the series corresponding to Fig. 3, there are a large number of actual differences which far exceed the probable. (The errors are small, and consequently the probable differences are small.) There are 31 that exceed twice the probable difference, and of these 9 exceed 3.5 times the probable difference. The probability of finding even these 9 is so small that six-place logarithms cannot determine it exactly, but it is less than .000001.

In four cases, then, out of six examined, it is altogether inadmissible to attribute the differences to chance, while in the other two the odds are against doing so. The probability that the differences in *all* the series are due to chance is of course

multiply small. The differences are therefore not chance, but significant; the ability to judge one magnitude is sometimes demonstrably better than the ability to judge the next magnitude; one function is better developed than its neighbor. The functions of judging nearly equal magnitudes are, sometimes at least, largely separate and independent. A high degree of ability in one sometimes coexists with a low degree of ability in the others.

## THE PROBLEM OF A 'LOGIC OF THE EMOTIONS' AND AFFECTIVE MEMORY.<sup>1</sup> I.

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After the philosophical and psychological turn to the conception of the primacy of volition in the mental life, the problem of a 'logique émotionnelle,' of a special immanental process by means of which affective values become generalized and objectified, could not long remain in abeyance. The intellectualistic point of view conceives the affective life, so to speak, as an epiphenomenon, a merely functional expression of the relation of ideas to ideas. All continuity of the mental life resolves itself into associational or logical relations of ideas. On the other hand, the biological point of view derives continuity from the motor relation of the organism to environment.

Now it is just this problem of continuity in the sphere of affective life that must come to the front as soon as the concrete mental sciences, ethics, æsthetics, etc., turn from intellectualistic to volitional and emotional points of view. There is no doubt that these sciences have been more and more setting themselves the task of analyzing and describing objectified and projected values, which spring from the volitional and affective life, and that the results of this study have presupposed mental processes which until recently have had very inadequate basis in traditional psychological theory. This continuity in the sphere of values, semi-logical in its nature, presupposes a generalizing and abstracting process upon affective values as such, giving rise to the so-called emotional abstracts, sentiments and moods, relatively independent of the particularizing ideal elements and capable of subsuming particular emotions under them.

<sup>1</sup> A paper read in part before the American Psychological Association, Dec., 1900.



It is often denied at the outset of a discussion of this nature that psychology can have anything to do with the description of meanings or values. These attach themselves to complex states of consciousness, and the analysis of these states into their elements fails to find anything in the elements explanatory of the values of the complexes. Values are to be appreciated, not described. A certain delightful mystical faith in values cannot be denied this point of view, and it has the advantage of being negative and of avoiding the errors of the positivistic school. A fundamental fallacy, however, it cannot escape, in that it overlooks the fact that in objective experience there exist worth projections whose meaning is so clearly determined as to be universally used as descriptive categories for mental states and to be themselves capable of description.<sup>1</sup> If this agnosticism means that the ultimate significance of values lies outside psychological method, this can lead to no dispute. Values are, however, once and for all in consciousness and, as such, have a right to an adequate psychology. It is curious how this agnosticism fails to see the serious implications of such reasoning for other spheres of psychology. Are not the objective conceptual values of cognition, spacial, temporal, the starting points of the psychology of space and time? Is not the problem just this—to understand the genesis of that which is now ordered and objectified meaning? It is more than probable that the temporary reaction against bringing the objective values of the æsthetic and ethical consciousness into the sphere of psychological analysis has been justified merely by the present state of psychology, but cannot be of fundamental and final significance.

The one assumption that underlies recent developments in ethics and æsthetics is this—that the evolution of sentiment, which all development of values involves, consists in a specialization of emotional dispositions, making them relatively inde-

<sup>1</sup> The antithesis between appreciation and description is one which cannot be pressed far without involving contradiction. There is a moment of value in all descriptions. Münsterburg, with all his emphasis on this distinction, in his '*Der Ursprung der Sittlichkeit*,' opposes the Wundtian ethics by insisting upon the sentiment of obligation as a psychological ultimate and the test of all moral consciousness!

pendent of the particular stimuli which call them out. These emotional dispositions having become fixed in consciousness by linguistic usage become relatively permanent values, capable of subsuming under them other dispositions and in turn being subsumed by judgment under higher values. A historical illustration of the actual specification of such an emotional value is the differentiation of the sentiment of sublimity in Neo-Platonic times. Doubtless those various emotions, later described as the mathematical and dynamical sublime, existed as separate emotions before the time of Longinus, as of course art itself proves; but this grouping under a new term, this specification within the general sentiment of the beautiful, is the result of a process of abstraction exercised upon the emotional reactions themselves by raising into clear consciousness a distinct qualitative element in emotion, a qualitative element the nature of which shall be considered later. The point of interest here is that a study of Longinus shows that he recognized clearly the varied ideal content that brings forth the reaction which we call the sentiment of sublimity. Such a specification of emotional values may be called a generalization of emotions. It results in a sentiment or emotional abstract capable of subsuming under it the particular emotions of which it is the abstract.

We have now to consider (1) the grounds for a theory of emotional abstracts, (2) the nature of the quality of an emotion that is brought to consciousness in the process of abstraction, and (3) the nature of subsumption of particular emotions under sentiments and moods.

But before entering upon this problem it is well to recognize the fact that there is already in existence a theory which, if adequate, would afford a much simpler explanation of the continuity of the affective life. Ideas, we are told, are generalized and abstracted, and as this process proceeds the intensity of the hedonic element and of the organic sensations decreases, but the feelings connected with general ideas are the same as those associated with the particulars. The assumption is that all affective states are reducible to varying intensities of pleasure and pain and varying qualities of organic sensations. Thus, for example, shame, an organic reaction, can be con-

ceived as successively associated with various ideas and even with general concepts. The difficulty with this association theory is its inability to account for certain characteristic phenomena in ethics and æsthetics. The ability of consciousness to pass judgment upon emotional tendencies or moods and sentiments independently of the ideal content which calls them forth makes them intrinsic values apart from the lapsed links, causal and associational, which have connected them with ends. The point to be observed here is that in the very dropping of these links a generalizing process has taken place, making the emotional disposition relatively independent of ideal content. In the second place, in the phenomena of transference and expansion of feeling, of sentiments and moods—or, on our theory, emotional abstracts—over cognate and related material, we find facts which point to a relative independence of the feeling or emotional abstract thus expanded. The expansion or extension of the feeling abstract may be discussed under two heads: (1) the æsthetic extension of a sentiment over particular feelings and emotions, and (2) the ethical phenomena of emotional sanction.

Both Meinong and Ehrenfels<sup>1</sup> have pointed out with considerable subtlety that the phenomena of transference of sentiment and its expansion cannot be explained by mere association of ideas. There is an element of judgment in the process which makes it of the nature of a subsumption. In recent æsthetic discussion, in place of the older conception of ideal unity, emotional unity has been substituted, in many cases without a clear understanding of what is thereby assumed.<sup>2</sup> Ribot has seen that such a conception implies the existence of emotional abstracts which subsume varied feelings and emotions under them. Now the emotional unity of a work of art consists in the fact that a given mood or sentiment can be maintained throughout, containing in it the varied particular emotional tendencies which are started by the particular content which is subsumed. The

<sup>1</sup> Meinong: 'Psycholog. ethische Untersuchungen zur Werth-theorie,' p. 61. Ehrenfels: *Vierteljahrschr. f. wiss. Philos.*, 1893, p. 216; also, 'System der Werththeorie,' Bd. I., 122, Bd. II., 154, 156.

<sup>2</sup> Compare the writer's criticism of Hirn's 'Origins of Art,' in this same number.



technical conditions of this expansion will be brought out in a second paper on the æsthetic attention. It is sufficient to point out here that the great work of art is that which contains the most varied emotional tendencies, all subsumed under the one emotional abstract.

A striking illustration of this is that exquisite mood poem of Tennyson's, 'The Lotos-Eaters.' All the technique of imagery and rhythm is expended not only to arouse the gray mood of forgetfulness and indifference, but to carry it on and on, intensified and fastened, until it becomes the mood of the very gods themselves through which they see the world. That world includes many things which do not fit this mood; yet, like gods,

"They smile in secret, looking over wasted lands,  
Blight and famine, plague and earthquake, roaring deeps and fiery sands,  
Clanging fights, and flaming towns, and sinking ships and praying hands."

But although these rapid pictures suggest incipient emotional responses of another sort than the dominant mood, they succeed each other so rapidly that it is

"Like a tale of little meaning, though the words are strong."

The general mood may become so strong that it will spread over all particular emotional tendencies, provided the technique of expression is such as not to allow these particular motor tendencies to get above a certain strength. In this case the technique consists in the rapid piling up of the pictures, thus preventing the particular emotional suggestions from getting in their full motor value. Experiments with people in the reading of this poem have led to the conclusion that if the mood of the poem is fully appreciated before this passage comes, it may be subsumed under the dominant mood. Otherwise the contrast is too great and the unity of the poem seems to be broken. So much for the passive appreciation of the relation of mood to particular emotions. On the side of active, creative expansion of a sentiment or mood over particulars, we have the interesting account by Poe of his construction of 'The Raven.' He tells us<sup>1</sup> that his starting point was the purpose to express the

<sup>1</sup> Poe: 'The Philosophy of Composition'; also Paulhan: 'L'Invention,' p. 81; also Ribot: 'L'Imagination Créatrice,' where Poe is classed as of the *diffuente* type of imagination, where the unity is the emotional abstract.

mood of melancholy. For this mood he found the characteristic refrain 'Nevermore.' His art then consisted in finding particular images, such as the raven, the locality of the poem, etc., which had emotional tones which could be subsumed under this mood. And then he did not scruple deliberately to tone down all ideas and images whose emotional intensity was too great for the dominant mood.

This expansive tendency of the generalized forms of feeling, mood and sentiment is shown also in the facts of ethical emotional sanction. The sentiments of shame, tragical elevation, and the moral moods and sentiments in general expand themselves over the particulars of affective experience, and it is this subsumption which sanctions the particular experience.<sup>1</sup> On its affective side ethical judgment consists in the subsumption of particular feelings under general forms of emotionalism which have become, through the usages of speech, well-defined objective values. Thus, Guyau<sup>2</sup> has formulated the end of the ethical life as a maximum extension of sentiment values consistent with intension of meaning. By this formula he is able to bring the ethical life into relation with the æsthetic and subsume them both under the common principle of maximum expansion and intensity of life. Out of this functional view of the moral end, which of course presupposes the doctrine of emotional abstracts and likewise a judgmental relation between these general sentiments and particular emotional tendencies, arises a distinction of importance in the present discussion, the distinction which in the logical sphere is made between the empirical and logical extension of a concept.

There is first of all, then, a purely empirical extension of an emotional abstract; that is, its 'Expansionstendenz' is limited by purely empirical conditions. The first of these is, as Ehrenfels has pointed out,<sup>3</sup> that a mood or sentiment tends to extend itself over all content of consciousness associated in time with the original stimulus of the mood. Upon this fact the first and most superficial unity of a work of art and of ethical judg-

<sup>1</sup> Ehrenfels : *System der Werththeorie*, Bd. II., 5, 154.

<sup>2</sup> Guyau : *Equisse d'une Morale*, etc., Livre II., Ch. I.

<sup>3</sup> *System der Werththeorie*, Bd. II., pp. 147, 148.

ments rests. To an uncultivated taste a strong sentiment, however crude, may envelope an entire poem, notwithstanding glaring emotional contradictions and contrasts which are patent to the more critical mind. Of the same nature are many of the illusions of ethical judgment. Beside this merely empirical expansion of feeling there is one which we may, by analogy, call the logical extension. The nature of this 'logical' extension we can discover only by making clear wherein the common element in both the particular and the abstract emotion exists. For such an extension of the emotional abstract over particulars must be of the nature of subsumption, and the relationship between the two must be judgmental in character. There must therefore be some 'equivalence of value' between two affective states for them to be related in this way. Now since all extension of an affective state which rests upon judgments of equivalences of value, as distinct from the mere empirical expansion, is conceived as æsthetic in its nature, whether it is found in the ethical or artistic experience, we shall call it 'æsthetic' instead of logical extension.

This tendency of simple feeling or emotion to pass over into sentiment and mood, or, on the other hand, for sentiment or mood to resolve itself into a particular emotion, is familiar to all students of the affective life, but it has assumed importance only where the question has been asked wherein lies emotional consistency of a work of art or affective continuity and consistency in the ethical life. To determine the criteria of the relation of these total and partial feelings then becomes important. Titchener, in his chapter on sentiment, tells us that "it is natural that sentiment, which is developed out of emotion, and characteristic of a higher stage of mental differentiation, should readily slip back into emotion." But his explanation of the phenomenon is inadequate, for the reason that just this development of sentiment out of emotion is left undescribed. We pass from the particular emotions of the poem or the novel to the æsthetic sentiments of harmony which are the affective attributes of judgment, but upon what this relationship between the emotion and the sentiment rests is not disclosed.

In this connection arises a question which becomes of still



more importance later—whether there is this direct dependence of affective state upon affective state which the doctrine of abstract and particular and of emotional unity and continuity presupposes? In recent psychological discussion the question has been asked: what contents of consciousness can become the ‘presuppositions’ of feeling? Presentations, sensation and idea, are of course conditions of feeling states. Wundt has made us familiar with the feelings brought out by concept and judgment, and Meinong with *Urtheilsgefühle*.

But may not feelings themselves be the ‘presuppositions’ of other affective states? This relation, of so much importance for an emotional theory of æsthetics and for a general theory of value, has been answered by a number of investigators in the affirmative. Meinong and Ehrenfels, Pillon and Witasek, all recognize its existence.<sup>1</sup> It seems probable that feeling may be the ‘presupposition’ of feeling in two ways: Memories of past affective states may call up new affective reactions, as when, in the case cited by Pillon, Saint Preux’s memories of the past are followed by new feelings of the present, of which they are the ground and presupposition. Or, in the second place, feeling may be the presupposition of feeling in the sense that in order that certain feelings may be experienced at all (this is particularly true in the case of represented feelings in art) there must be, as presupposition, a dominant mood, in harmony with it. Or perhaps it would be better to say that the affective state is part presupposition with the images of the emotion which shall be evoked. It is this second form of relation— affective states as the condition of affective states—that becomes so important in æsthetic effect. The emotional effect of those few lines in ‘The Lotos Eaters,’ descriptive of burning towns, sinking ships, etc., is entirely determined by the mood which precedes.

We may observe in concluding this point in the discussion, what is already evident to the reader, namely, that the whole question of presentation and representation to consciousness of the

<sup>1</sup> Ehrenfels, Bd. I., p. 207; Witasek, ‘Zur psychologischen Analyse der ästhetischen Einfühlung,’ *Zeitschrift für Psych. und Physiol.*, Bd. 25; Pillon and Mauxion, *Revue Philosophique*, February, 1901.

affective state as such, therefore of affective memory, is already anticipated. No one should hesitate to admit that mere pleasantness or unpleasantness as such cannot be the presupposition of mere pleasantness and unpleasantness, any more than can these hedonic elements, abstracted from the *fusions* that alone have meaning for consciousness, *i. e.*, emotions, sentiments and moods, be remembered. The question is simply this: have these fusions such unity as to make them capable of presentation and representation to consciousness? If so, they follow the laws of dependence of representatives upon each other.

Now if we examine the four orders of affective experience, simple feeling, emotion, sentiment and mood, we notice that, while for purposes of abstract analysis the simple feeling may be distinguished from the other forms, the ultimate of actual experience is the simple emotion. Although a sensation may conceivably be affectively toned without leading to any emotional reaction, in reality that affective tone has no meaning until it passes into some tendency to motor reaction. The principle that the ultimate of meaning is the motor idea, is strikingly brought out in its philosophical significance by Professor Royce in 'The World and the Individual.' It is equally true that no adequate theory of affective complexes or abstracts can be built up out of the simple elements of pleasure and pain and ideas. It is only when the selective processes of generalization take hold of ultimates that have conscious meaning, namely, motor tendencies in their relation to volition, that complexes of higher meaning may be developed. The ultimate of meaning, then, being the particular emotional tendency, the affective states of a higher order, in case they shall relate themselves to the particular emotion as general to particular by subsumption, must be conceived as developed from these by processes of generalization. The characteristic of mixed feelings, as Höffding calls the higher complexes, is that they make up a total feeling, a fusion, in which the parts are indistinguishable, and yet one out of which the particular is redeemable again. Whether we call this process, by which out of simple emotions, sentiments and moods are developed, *fusion* (Höffding, Elsenhaus) or *generalization* (Ribot), the conditions which make

possible the relations between the total and the partial feeling are the same.

First, then, let us consider what may be called the negative conditions of this relation. By sentiment is understood the total feeling connected (*a*) with any word which stands for a generalization of particular experiences; these are the concept feelings, such as connect themselves with words like *love, death, dreams*, etc.; or (*b*) with well-differentiated groups of sensations or ideas which remain relatively permanent, such as the sentiments connected with home, town, places and persons in general. By mood, on the other hand, is meant the total feeling arising (*a*) from the fusion of separate organic sensations into vital feeling, such as melancholy, depression, buoyancy, etc. Or (*b*), secondly, a generic phase of emotionalism made up of vital feelings together with generic emotional tendencies, such as the moods of reverence, playfulness, etc., according to what the nature of the vital feelings may be—for instance, a melancholy playfulness, a sombre or cheerful reverence. In all moods of this second order the element of judgment enters and the sentiment is modified by the mood. Finally, we call those affective states moods which gather about a group of sensational or ideal elements, but where the vital feeling is predominant, as the mood of a place or landscape. Upon examination of all these phases we find in their structure conditions which make possible, negatively at least, relations of subsumption. In the first place, in all these generic phases of emotionalism is noticeable a dampening of the intensity of the hedonic and sensational components. For it is only when a particular set of organic sensations or definite emotion becomes dominant that a mood can become intense. In addition to this, it seems probable that in the mood, at least, the number of the component or organic sensations increases the fusion and lowers the intensity of the component parts. In the concept feelings the hedonic element is, as has often been observed, of less intensity than that connected with the particular images which are the presuppositions of the particular feelings, and the organic sensations qualitatively less definite. The technique of 'The Lotos Eaters' illustrates the bearing of these facts upon the question of subsumption. Such imagery



as is employed in this poem is always in vague, general terms, so that the affective resonance may be dampened and not overcome with particular emotions, the languor of the general mood developed by the rhythm and the careful use of vowel sounds. "Here are cool mosses deep"—"the ivies creep"—"the long leaved flowers weep."

The same criteria of dampened intensity and qualitative indefiniteness are found to apply in greater degree to the other forms of sentiment and mood. The sentiments formed about places, or the ethical and religious sentiments, are fusions of many particular emotional reactions (wherein this likeness consists we shall see later), which cause the particulars of ideas and organic sensations to become indefinite and the hedonic intensities of the particular emotions to sink to a common level.<sup>1</sup>

Upon what positive element, then, does the selective attention act in the generalization of generic phases of emotionalism from particular emotional reactions? The most common notion is that the ideal element alone distinguishes the sentiment from simple feeling, while the mood is different from the emotion merely in the fact that it consists of a greater summation of organic sensations and in the length of time it lasts (Titchener). These distinctions do not, however, do justice to the objective nature of these projections of affective values. The lapsed links, ideas, hedonic intensities and organic sensations which the psychologist in his reconstruction of these emotional states supplies, *are* lapsed links—have escaped attention—are no longer constituent elements of the projected value, the objectivity and independence of which make its extension over particular feelings possible. There is a constant element in these emotional abstracts which is to be found in none of the elements into which psychology analyzes the emotional complex. The contention of

<sup>1</sup> These same criteria of (a) weakened intensity and (b) qualitative indefiniteness have been developed by Elsenhaus in an article: 'Ueber Verallgemeinerung der Gefühle,' *Zeitschrift für Psych. und Phys.*, Bd. 24, Hefte 3 and 4. He also brings out the point that the representation of a general feeling by a particular differs from the representation of a concept by a particular in the fact that as soon as the general passes over into the particular or partial feeling, the latter tends to expand over the entire affective life, qualitatively determining it.

the agnostic, at this point, to which we referred at the beginning, is in so far right, that the universals of affective meaning can never be explained by analysis into the abstract elements, sensation and simple feeling. The retrospective psychologist centers his artificial attention upon the elements into which the psychosis has been analyzed; but in pursuance of the 'acquisition of meaning' the selective attention must have been directed upon just those values of the psychosis which the abstraction of the psychologist ignores. We repeat the question, then. Upon what positive element in the particular emotional reactions does the selective attention fasten in its generalizing process? What positive element remains constant in sentiments and moods, making it possible to subsume under them particular emotions in such a way as to give æsthetic truth?

The hypothesis of qualitative elements in feeling, different from those of pleasure and pain, is one that is constantly coming forward in those quarters where the starting point for a psychology of the affective life is the value-resultants as they are found in the projections of the ethical and æsthetic judgments. Thus, in addition to the Austrian school of psychology, Krüger, in a monograph entitled '*Der Begriff des absolut Werthvollen als Grundbegriff der Moralphilosophie*,' insists upon the existence of feelings of value as such, different from pleasure and pain, and seeks to point out that feelings are valued not only according to their intensity and duration, but also for their breadth and depth in the personality. The criticism which such conceptions call forth, certainly presented in one of its most searching forms in Külpe's '*Introduction to Psychology*,' owes its force largely to the notion that the starting point for constructions of a higher order in the affective life is a simple feeling. If every feeling attached to a sensation or idea has its peculiar affective quality we are no doubt burdened with a superfluity of qualitative richness, but, just as in the intellectual life generalizations and abstractions start with a percept or idea and not with a sensation to develop the relationships which give meaning to thought, so whatever meaning may develop in the affective life through generalization must start with the ultimate of meaning, the emotion.

Now any emotion that I may call up, waiving for the time being the question whether an emotion can be recalled (the problem of emotional memory), is a generic image. When I try to realize an emotion, say fear, I find a succession of motor tendencies and images. I start back, shrink into myself, close my eyes, in short, my representation of fear is a series of images, of motor tendencies that I have experienced at different times. It is generic. If I analyze further and try to find what images acted as stimuli for these different motor tendencies, I find them gradually trooping into consciousness and associating themselves with these various motor tendencies. In this case, with the starting back comes the image of the locomotive which I once but barely escaped. The shrinking into myself is vividly connected with a particular experience of supposed social disgrace, the closing of the eyes with a threatening face. Now the point here is that these generic motor tendencies appear first, and they are then associated with visual images but are themselves revivable independently of the visual images. These motor tendencies, then, are all parts of the generic emotion fear; but this is not the extent of the analysis. What makes these motor tendencies parts of one generic emotion is the dynamic element they have in common. However these reactions may differ in the quality of their organic sensations, there is a dynamic resonance which may well be called the dynamic constant that makes them generic. By the *dynamic constant* of a generic phase of emotionalism is meant nothing more than a relatively permanent system of intensities and of temporal and rhythmic relationships among the organic sensations of an emotional reaction. If one examines the peculiar qualitative distinctness of an emotional state, it will be found invariably capable of expression only in terms of resonance of the whole organism. To describe these qualities of the emotional state we must invariably have recourse to some figure from the outer world or some system of time or intensity values which shall stand as symbols for the inner system. Thus Mauxion, in the article already referred to, has pointed out that the increase of brightness intensities in a sunrise and the corresponding decrease in a sunset correspond to definite affective resonances.



Now it is not at all inconceivable that when we speak of an angry puff of an engine, or of a long drawn sigh, we are putting into figurative expression the real characteristics of inner emotional reactions; and it is also conceivable that, if the unitary quality of an affective state consists in a system of time and intensity of relations of the component organic sensations, the characteristic of the resonance will remain constant no matter how the absolute intensity and the qualitative definiteness of the mass of sensations may vary. Just as in the fusion of tones into chords the unitariness of the fusion is affected only by the relative intensities of the components and not by the absolute, it is quite conceivable that the unitary nature of a generic emotional state consists not in the quality of the organic sensations but in a system of relative intensities and time values.

The advantages of such a conception of the constant element in generic affective states are obvious. It enables us to see, not only how the expression of emotion in objective forms is possible, but also how this very expression of relationship, especially in rhythm and light and shade values, has the effect of bringing to consciousness the most intimate quality of affective states, segregating them by processes of abstraction from the qualitative ideas of the stimuli and the organic sensations. It enables us to understand how words, whose emotional values are ultimately in the emotional tendencies involved in their pronunciation, may express abstract emotional states even when the processes of generalization have lowered the intensities of the organic sensations and made their qualities indefinite. Finally, we would then be able to understand how representation of an emotion itself is possible, for this dynamic constant character of an organic resonance could be represented without the hedonic being prominent and even through varying organic sensation content.

In such a notion that the essence of an emotional state is a *dynamic constant* there is nothing inconsistent either with experience or with recognized psychological conceptions. For, in the first place, the body of an affective state being made up of organic sensations corresponding to definite organic tendencies, if our recognition of an affective state as such depends upon the

definiteness of the component sensation we ought to recognize a particular emotion much more easily than those sentiments and moods in which the qualitative elements are less definite. This, as we shall see in our later study of affective recognition, is not necessarily true. Again, the organic sensations of an emotional state, as has been pointed out, vary in the quality of their components. There are different fears, rather than a single reaction of fear. Any one of these particular fear reactions may stand for the generic state of fear, as a particular may stand for a general concept, and yet it represents the generic emotion only in virtue of something constant which makes them all capable of assimilation along the same general paths of motor habit.

As to the concept itself, it is already familiar in other spheres of psychological analysis. Under the term '*Gestalt-Qualitäten*' is coming to be understood such systems of relations as remain permanent amid changes of content. Thus, for instance, a melody may remain constant for recognition though expressed in different keys, in different tone series. This unity is, however, so permanent in all its expressions and in its further use in higher tone creations, that it may be conceived of as a complex which is fundamental in its character, '*ein fundierter Inhalt.*' Among these '*Complexionen,*' as the writers of this school call them (Ehrenfels, Witasek), Ehrenfels suggests that the fundamental æsthetic reactions, the sentiments of the beautiful, the sublime and the comic, may be placed.

In any case we are now in a position to outline a possible theory of emotional abstraction and generalization. We have already seen that those affective states which bear the marks of abstraction—concept feelings, sentiments and moods—are characterized in general by lower hedonic intensity and by qualitative indefiniteness, and yet their unitary quality stands out strongly. This is, however, just what would be expected if the process of abstraction consists of the bringing into prominence by selective attention of a fundamental quality other than the varying elements of hedonic intensity and qualities of sensations. The first stage of this generalizing process is then the generic emotion itself, which, as we have seen, should be properly distin-

guished from the single emotional impulse, it being made up of a number of motor tendencies manifesting themselves in consciousness in various organic sensations, qualitatively different, but each group having the common dynamic constant. The genetic theory that the body of an emotion is made up of survivals of particular impulsive movements conceives our experience of an emotion as generic, as a fusion of organic states coming from varied particular reactions, and Professor Baldwin has expressed this same idea by the theory that the development of sentiment is a result of the assimilation of new content to old motor tracts, thus making of the emotion a generic tendency rather than, as the association theory conceives it, a single element associated by habit with various ideas. It is, however, the very nature of such a process of assimilation to bring out the constant dynamic resonance of an emotion, and it is as such that we cognize it as a definite affective experience.

Still more generic phases of emotionalism may be conceived of as developing through the same processes of assimilation. Like the sentiment of sublimity, the historical origin of which was used as an illustration, the great sentiments of the ethical, religious and æsthetic experience—as for instance faith, reverence, playfulness, the comic—may be looked upon as complexes of a higher order, as assimilations of varying emotional tendencies on the basis of their dynamic constancy.

They represent generic attitudes of emotional response to which new emotions of the same dynamic constant may be assimilated. This enables us to understand the development of sentiments, their expansion, ever including finer *nuances* of particular emotions, and at the same time their relative permanency. Undoubtedly the entire growth of sentiment, as Guyau has described it in his beautiful chapter on the ‘*Evolution des Sentiments*,’ has consisted in the expansion of the sentiment over more and more ideal content; and in this expansive tendency of the sentiments lies the possibility of subsuming, as Guyau hopes, the scientific ideas of his time. But just this expansion, as we have it in art (especially poetry), consists in the development of new concept feelings, those complexes of partial emotional tendencies, of such character that they may assimilated to the



old sentiments. This leads us to a final problem, which can be merely touched upon in the limits of this paper, a problem however, for which the present discussion is largely introductory—namely, what is an emotional value; what relation has this concept of the ‘dynamic constant’ to emotional values, and how are emotional values related in the ethical and æsthetic consciousness? An examination of the more objective and permanent sentiments and moods discloses the fact that their description as well as their valuation—their meaning, in other words—is invariably in terms of their relation to volitional efficiency. Viewed objectively, they have value as dispositional presuppositions of overt acts of impulse and will. As extrinsic values, they are conceived as the causal presupposition of judgments and acts. As intrinsic values, however, viewed inwardly, we have described them as ‘dynamic constants,’ and undoubtedly their meaning and value lies in their dynamical relation to volition, to inner activity, in their efficiency for volition.

On the theory that volition is primary in the mental life, which underlies so much of modern psychology, and, secondly, on the theory that the will manifests itself only indirectly in the affective life—in the complexes of sensations and affections which constitute emotion, sentiment and mood—we may see how some such concept as that of the ‘dynamic constant’ in affective states and the theory of emotional abstracts as here understood would afford a basis in psychology for a doctrine of values. It may be possible, then, to say with Krüger that the value of feelings is measured not only by their hedonic intensity but by their breadth and depth in the personality. But the relation of the extension to the intention of a sentiment and the general question of the relation of sentiment, æsthetic and ethical, to volition, are problems that must be left for another occasion.

## DISCUSSION AND REPORTS.

### MULTIPLE AFTER-IMAGES.

In the July issue of the *PSYCHOLOGICAL REVIEW* for 1899, under the caption of Notes on After-Images, I called attention to the phenomenon of a different after-image for each eye simultaneously, the proof resting on the fact that their colors were different, the one being positive while the other was negative, and vice versa. Now I wish to record another interesting observation in relation to images. The matter started as an experience and became an experiment.

On a recent morning I noticed a bright image on the floor of my bedroom and supposed it was due to the sun at which I had just looked. I tested the matter by looking again at the sun. I projected the image on the wall of my room and was surprised to find not a single image but three, side by side, in the negative stage. They soon became two and then merged into one. All the more curious, I again viewed the sun and returning my eyes to the wall discovered five images, two of which were positive and three negative. Keeping at this process I secured as many as fifteen images at one time, some of which were positive and some negative, and all clear and distinct. Then I turned my attention to the sun. I found a new phase, namely, that instead of being eclipsed by the sun, the after-images persisted while I looked at that orb and appeared in the field of vision all about it. The number increased meanwhile, by what means I did not know, and three different colors appeared among them. These triple colors, however, came after I had projected a group of images upon the wall of my room. Returning my eyes to the field of the sun I discovered an intense lemon color, besides a positive and negative color of the sun itself. On the succeeding morning I found the images could be multiplied indefinitely and that at least five distinct colors could be secured simultaneously.

Such are the data. How are they to be explained?

First, the number of images is due to the great intensity of the stimulus, together with the orientation of the eyes. Images under a bright sun form instantly. Then the rotation of the eyes brings new

regions of the retina under the direct assault of the stimulus and each new spot impressed gives out an image.

Second, as to the visibility of the images under the full light of the sun. At first I thought this was due to the indirect rays of light being sufficient to cause them to persist. Now I believe it is due to the orienting of the eyes, which passes the stimulated regions again under the direct rays of the stimulus and so renews the stimulus. This seems true because by fixating the sun for a minute the surrounding images fade away.

Third, the multiplicity of colors evidently comes from color mixing upon the normal principles of that process as conducted in the laboratory. For instance, the deep lemon color I found to be compounded from the images projected on my drab-colored wall carried over to the blue field near the sun. The other different colored images were made from combination of images in different stages of passage from positive to negative. The possibility of different hues is very great.

One interesting feature of the first experiment I have not been able to reproduce. Upon getting a great number of negative images of the sun projected into the field about it, then slowly turning the eyes upward, the images ran together in a ball and then taking the shape of a balloon floated slowly upward.

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(Signed) EDWARD FRANKLIN BUCHNER,  
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## PSYCHOLOGICAL LITERATURE.

*Grundzüge der Psychologie.* Von HUGO MÜNSTERBERG. Band I., Allgemeiner Theil: Die Principien der Psychologie. Leipzig, 1900. Pp. xii + 565.

Professor Münsterberg is a conspicuous figure among the writers and thinkers of both America and Europe, and whatever differences of view he may encounter he is sure of attention and respect. His latest work, moreover, *Grundzüge der Psychologie*, to judge only from the first volume recently published, would of itself have secured to him this enviable position, if his special work in empirical psychology, and among other things his more popular volume, *Psychology and Life*, so widely read and so much discussed, had not already compassed it. In Professor Münsterberg's history, however, as well as in the history of the science which he has represented, the *Grundzüge* is a peculiarly noteworthy book. Thus it is nothing more nor less than a systematic presentation of its author's rich experience in the field of psychology. Perhaps, at least with reference to this first volume, since some would separate reflection and experience, one should rather say that it is a presentation of his reflections on a rich experience in psychology, the more direct and special treatment of the matter of psychology itself being reserved for the volume that is yet to come; but in any case, whatever one ought to say, it is plain from this first volume that Professor Münsterberg has not been a passive observer, that for him the unexamined life of psychology has not seemed worth living.

And besides having this sense of the need of self-examination, so commonly felt among the sciences of to-day, psychology is to be made timely in another way. Thus it would exact from its followers, or to say the least it has plainly exacted from Professor Münsterberg, the 'strenuous life.' I say this with all due apologies for my profanity. Throughout the present book the 'I want to fight' of the Preface of *Psychology and Life* is distinctly to be heard. Everywhere back of the words one feels in an unusual degree the presence of a strong will. The style is vigorous and, while I myself could never sympathize with those who have chosen to object to Professor Münsterberg's English, the use of the German has undoubtedly enabled a freer and so a more

effective expression. Moreover, with regard to the use of German, the report that the author will not consent to the translation of his work is probably true and may be interpreted as showing how seriously he takes himself. The very life of psychology is at stake, and with more than mere fancy one must say that the German has come to defend what more than any other the German brought into being. In short, from start to finish, this book is a book with a moral purpose; it is no lifeless contribution to the world's stock of knowledge or mere speculative doctrine.

Hence, although perhaps less openly here than in *Psychology and Life*, Professor Münsterberg shows himself to be always thinking, not only of old theories to be criticised and possibly refuted or of new theories to be formulated, but also of men or classes of men to be brought into line and made to go in their right ways, in the different ways of the special sciences or in the way of the 'real life.' It is perhaps only the most external or circumstantial evidence of this that is afforded by the careful and valuable bibliographies given with the different chapters, but such a classification of the books and papers bearing on matters psychological, when taken in connection with the moral purpose and personal spirit of the book, is certainly not without significance. Professor Münsterberg would be a personal leader as well as a scientist or a thinker, and his book, I venture to say, will appeal to its readers from this standpoint, winning disciples or stirring up antagonists in a way that few books are privileged to do. This, of course, is desirable and very refreshing, and it is especially pertinent in this review because, in a way to be pointed out in a later paragraph, it is itself a comment on one of the fundamental contentions of the author.<sup>1</sup>

Now, as has been said, the present volume is an attempt to make psychology self-conscious. Psychology must know what it really is and particularly what it is not, and all the other sciences as well as all the unscientific relations of life need to be informed on these points too. Definition of the field of psychology was, it is true, the purpose of *Psychology and Life*. Here, however, we have no miscellaneous essays for widely different classes of readers, but instead, a uniform treatment, that would be exhaustive and that is addressed especially to

<sup>1</sup> At the 1900 meeting of the American Psychological Association, as well as at that of the Western Philosophical Society, Professor Münsterberg's influence was very obvious. At the former, for example, two of the papers read were, one in part and the other in whole, open criticisms of his standpoint, as presented chiefly in *Psychology and Life*, not to mention certain magazine articles, and others seemed to have that standpoint in mind.



students and teachers in science and philosophy, of the exact province of psychology. And what is the result? The result is a perfectly free object, in the sense of a wholly distinct object for psychology; an object that is wholly divorced in the first place from the will or feeling of the subject—the 'real' subject, of course, not the empirical subject—and in the second place from the proper objects of all the other special sciences as well as from what are not properly objective sciences but historical or normative sciences. "Dieses Buch," says Professor Münsterberg (p. vi), "will der empirischen Psychologie vollkommenste Freiheit sichern und doch das unbegrenzte Recht des Idealismus dathun."

With reference to the latter half of this program it is hardly necessary to say that the cause of a moral Idealism is quite as near to Professor Münsterberg's heart as that of science in general or even of psychology in particular, and what accordingly he proposes is a conciliation, 'auf neuem Weg,' of these two interests. The way, moreover, whether novel or not, in which this conciliation is to be effected is announced thus: "Der Idealismus kann nur dann sein Recht behaupten, wenn er in seinem eigenen Umkreis Platz für eine rücksichtslose konsequente empirische Wissenschaft hat." Science, in short, is to be an act of the will; but in the interests of freedom, of an absolutely free object, it must be wholly innocent of any relation to the will, it must be only an *external* act of the will, will and its acts being separable. This certainly involves a difficulty, but for the moment we will waive it.

The objectivity of psychology is thus presented (p. 56): "Die Psychologie will eine beschreibende und erklärende Wissenschaft sein; sie muss als ihren Gegenstand also vorfindbare, in sich bestimmte und somit unabhängige Objekte verlangen und voraussetzen. Die Wirklichkeit bietet nur abhängige Objekte der Stellungnahme und Akte, die wir verstehen und würdigen; die Psychologie—und dasselbe gilt von der Physik—kann es mit der logisch primären wirklichen Erfahrung also gar nicht zu thun haben. Psychologie und Physik sind somit erst dann möglich, wenn das wirkliche Erlebnis verlassen und ein Abstraktionsprodukt gewonnen ist." The most significant part of this passage is the interjection about physics, 'und dasselbe gilt von der Physik.' When such extremes as physics and psychology are brought together there can be no doubt that the intention to keep psychology in the ranks of the objective sciences is a thoroughly serious one, and so that teachers and preachers and all other creatures of the real life are to have at least no more right to turn psychology into an art of pedagogy or

ethics or of anything else practical, into an ism—or a nostrum?—of any sort, than to abuse physics in the same way. For some reason they can hardly be said ever to abuse physics. Why, then, should they abuse psychology?

But the separation of physics and psychology is quite as important to Professor Münsterberg as the liberation of the object of each from all possible infection with reality or, say more properly, from having anything to do 'mit der logisch primären wirklichen Erfahrung.' Physics and psychology ought to have a common foe, that is to say, physics ought to be no more exempt from interference than psychology, and so to this extent they may be classed together, but within the sphere of the freely objective they are necessarily as far apart as the physical, equal to that which can be thought identical in the experiences of different subjects—"was mehreren Subjekten gemeinsam erfahrbar gedacht werden kann," and the psychical, equal to that which is possible as experience only to a single subject—"was nur einem Subjekt erfahrbar ist" (p. 72), in short, as the universal and the individual. And again, of the free object of psychology we read: "Das Psychische ist uns nunmehr das wirkliche, vom Subjekt losgelöst gedachte Objekt, nach Abzug derjenigen Faktoren, die Objekt für mehrere Subjekte sein können; das Psychische ist also gewissermassen *der Rest, der übrig bleibt, wenn das in verschiedenen Erfahrungen Identifizierbare, und somit das kausal Zusammenhängende, herausgearbeitet und abgezogen ist*" (p. 88). In other words the abstraction of the universal object, 'das identifizierbare Objekt,' is absolutely necessary to physics, in so far as physics is to get any knowledge of causality, while psychology must and, as Professor Münsterberg thinks, can very cheerfully content itself with what is left after physics has been thus richly provided for. Simply the psychical object is the individual object, being 'nur einem Subjekt erfahrbar,' and is therefore without continuity of its own; but this does not rob psychology of any genuine interest, since the discrete object of psychology gets unity, indirectly perhaps but not less effectively, through the physical to which it is parallel. Psycho-physical parallelism, although perhaps to be justified only on grounds of a theory of knowledge, is thus an indispensable presupposition of a science of psychology; and this, instead of robbing psychology of any positive value of its own, as might possibly be imagined, gives it value and interest. The psychical as psychical is certainly not less real or less interesting for being parallel to the physical. But, as regards the separation of physics and psychology, could any two things be farther apart than the universal and

the individual, or the continuous and the discrete, or 'das Zusammenhängende' and 'das Nichtzusammenhängende'? Surely not—unless extremes really do meet. And, in any case, it is not exactly good form in these days to turn anything so chilling as the dialectic on anybody! Moreover, the distinction itself is phenomenalist and only for epistemological purposes.

Of course human societies as well as human individuals have a mental life, a truly psychical life, and so have the animals; and the psychologist accordingly finds his proper object among these, and his field of study is thereby widely extended (pp. 97-103); but psychology is not on this account to be confused with sociology or physiology or biology, and above all psychology must not imagine that it has any right *by mere analogy* to read a psychical life into physical bodies in general, into individual atoms or into the universe as a whole. Thus: "Sowohl vom logischen wie vom psychologischen Standpunkt ist uns der Nebemensch zunächst nicht physisches oder psychisches Objekt, sondern ein aktuelles Subjekt in der Welt der gültigen Beziehungen; er ist als solches Subjekt anerkannt und nur deshalb ist sein Körper für uns mehr als ein physischer Gegenstand. Wollen wir wirklich Analogien bilden, so kann auch die anorganische Natur nur dann uns als beseelt gelten, wenn sie in der wirklichen Welt als Subjekt anerkannt wird und nun aus ihrem Subjektwesen die Doppelheit des psychischen und physischen Inhaltes abgeleitet wird" (p. 101). And further: "Das beseelte Atom als unberechtigte Verkoppelung physikalischer und psychologischer Hilfsbegriffe ist \* \* \* die Konsequenz des Dogmas, dass die Beschreibung der physischen und psychischen Objekte alle Realität erschöpfen könnte. \* \* \* Sobald wir aber im Auge behalten, dass alles Psychische aus der Aktualität wirklicher Subjekte stammt, so werden wir für die Psychologie das Recht zurückfordern, unabhängig von naturwissenschaftlichen Konstruktionen, nur aus dem Zusammenhange des Lebens, über das Dasein psychischer Objekte zu urtheilen, aus ihren eigenen Bedürfnissen heraus ihre Hilfsbegriffe zu bilden und einen atomisierenden Panpsychismus als unberechtigte Grenzüberschreitung der Naturwissenschaft zurückzuweisen" (pp. 102-3). In short psychology is psychology; *even* ontology or cosmology is one of the many things that psychology is not.

So much, then, for Professor Münsterberg's definition of the field of psychology, for his liberation of the psychological object, but this is only a very small portion of the whole book, which comprises in all three parts. Thus the first, bearing the general title: 'Die Auf-



gabe der Psychologie,' besides its contribution to the theory of knowledge, that has now been partly outlined here, contains chapters on 'Psychology and the Historical Sciences,' 'Psychology and the Normative Sciences'—ethics, æsthetics, logic, religion and mysticism, and 'Psychology and Life'—commerce (sozialer Verkehr), jurisprudence and education. I have ventured to translate! The second part is entitled, 'Die psychischen Objekte,' and treats consciousness, space and time, the psychical manifold. And the third part, 'Der psychische Zusammenhang,' is first given to the different theories of the unity of the self, such as (1) the doctrines of psychological *substance*, which simply make the fatal mistake of confusing psychology with metaphysics, (2) the psycho-physical hypothesis, which is of epistemological import only, (3) the theory of apperception, (4) the explanations from biology, and (5) the association-theory, and then, after this clearing of the ground, to (6) the author's action-theory. But, beyond this enumeration of the general contents and in due time some more extended reference to the action-theory, the present notice of the book must confine itself almost entirely to the matter already outlined. This in itself affords more than enough for the work of one review. In general, however, let it be said again that we have here an important study, important for what it accomplishes as well as for what it undertakes, of an epistemological justification of empirical or physiological psychology.

But, to begin with what seems to me a fundamental difficulty of Professor Münsterberg's standpoint, freedom—from the subject, das aktuelle Ich, as well as from the objects of the other sciences—may, indeed, be necessary to the psychical object as the object of a *science* of psychology, and accordingly it may be also necessary that psychology deal with what is only a transformation of reality, a logical construction; but herein lurk two inevitable dangers.

Thus, for the first of these, in epistemology exactly as in ethics, freedom is a very uncertain term. It may mean, and there is always temptation to make it mean, absolute independence and isolation, in short the freedom of indifference; or it may mean, and I think it should mean, only clearly recognized and so clearly defined relationship, the freedom of well-controlled interest and responsibility. The former meaning, however, is that into which Professor Münsterberg's zeal for a freed psychology is constantly leading him. Even in his important chapter, 'Die Psychologie und das praktische Leben,' where the position is by no means so extreme as in the book, *Psychology and Life*, the impression given is that of freedom through divorce rather

than through defined relation, for life as 'real life' does not seem to have any vital need of science in general or of psychology in particular. Successful knowers of men are represented as in no sense psychologists and as not needing to be psychologists. Lawyers and teachers and preachers, too, are similarly independent. And yet we read: "In jeder Situation die [zahllosen sozialen] Verhältnisse es uns aufzwingen, den anderen schlechthin nur als ein Objekt \* \* \* zu betrachten, und nur die Psychologie darf dann unsere Schritte lenken. So lange wir aber den Freund oder Gegner, den Kunden oder den Mitbürger, den Belehrung Suchenden oder den Trost Suchenden als wollende Persönlichkeit anerkennen, liegt seine geistige Wirklichkeit in einer Sphäre, für welche die Kategorien der zerlegenden und erklärenden Psychologie grundsätzlich keinen Sinn besitzen" (p. 199). This is most explicit, but it admits, or virtually admits, too much. If every situation in real life does really involve, and for my part I believe that it does, the regard of one's fellow *both as an object*—whence psychology—and *as a willing personality*—whence all the relations of active life, I have to wonder if it is even possible, physically possible or psychologically possible, for teachers and lawyers and others in real life not to be also psychologists or at least in some degree psychological, and on the other hand for psychologists and even epistemologists not to be also men of affairs—perhaps in some cases strenuous teachers and reformers. Certainly things that coëxist, that are coëxtensive, both being present 'in jeder Situation,' must be very intimate with each other; nay, they must be essential to each other, mutually implicative; and even a superhuman agency would put them asunder with greatest difficulty. Moreover, is not the really 'real life,' of which Professor Münsterberg says so much, not something apart from science, but in itself the very conflict, in which he is taking such a conspicuous rôle, between the two views which he has properly enough opposed to each other? In any case, with such an idea of the real life I referred above to the moral purpose and personal spirit of Professor Münsterberg's book as a comment upon (meaning of course a contradiction of) his contentions, and also I objected to any treatment, open or implicit, of science as only an external act of the will, that is to say, as not itself in and of the will of real life.

For my own part I can think of science and 'life' as separated only as the functions of life in general are separated in what the economists know as division of labor. Things that are separated in this way are never without mutual interest and unceasing competition, each having

and asserting a vital relation to the other and neither ever being free from a disposition to trespass upon the other. This competition, moreover, trespassing and all, is of advantage to both. Professor Münsterberg sees only the danger of the sciences, among them psychology, becoming isms, but no danger is ever single. The threatened development of the sciences into mere ologies needs also to be recognized, for an ology is quite as much to be lamented as an ism. Briefly, science is not for its own sake, and fortunately just such vigorous competition as this of Professor Münsterberg's book, in which science is set so completely aloof, is bound—by inducing an equivalent reaction!—to conserve the relation of science and life.

But science in general and psychology in particular are not only said to be objective; they are also necessarily phenomenistic, dealing with logical transformations or unreal constructions; and here there lurks a second danger of Professor Münsterberg's standpoint. Thus, to find in the phenomenism of science an additional reason for the complete abstraction of the real life or the will, or 'das aktuelle Subjekt,' is to forget, in the first place, that science can never be wholly blind to its own phenomenality, and, in the second place, that it is the very nature of consciousness in general, not merely of the keenly logical consciousness of science, to be phenomenistic. Consciousness in general deals, and always must deal, with symbols, constructions, transformations, and also it *knows* that what it deals with is symbolic or constructed. Science is not a peculiar consciousness; it is only the extreme development of something in all consciousness; and as in the general case, so in the special case, the case of science, due allowance has always to be made by the willing subject for the symbolic or phenomenistic character of its objects. Conscious creatures, from the moment they begin to draw breath, are trained to see one thing and understand another, to use what they see as not reality but only a symbol of reality, and so in volition to allow for the unreality of their objects of consciousness; and just this training, I should say, is what has made the high development of science possible and is also what enables and properly controls the application of science to practical life. The phenomenism of science, then, is no reason for its divorce from life; indeed, it is, on the contrary, one of the motives of its application to life, since it must ever awaken within science a spirit of unrest, a longing for reality. Only application can save science from the charge and the conviction of bankruptcy.

The abstraction of science, accordingly, cannot rest merely on its phenomenism nor for that matter on anything else that would seem



to make the scientific consciousness qualitatively peculiar. Is science 'zerlegend und erklärend'? So is consciousness as 'such; so is all consciousness. Science sees through a microscope, truly, but it still sees; it is moved by the impulses of real life only at long range, but it *is* moved by them; it deals with practical questions, questions of life and death, of personal need and social utility under all sorts of disguises and indirections, in short scholastically, but it does deal with them; and it engages in all of life's conflicts aside and dramatically, but it engages in the conflicts. Am I only fanciful in a way that has no place here when I think that the scientist's laboratory is but a stage on which in various forms and under various conditions, minutely reproduced and carefully organized, man watches the drama of his own life? Well and good. Let it even pass for mere fancy, but at least it hints at a reason for *real* man's absorbing interest in science, and it explains without undoing the abstractness of science and insures as well as justifies the application of science, giving to science as it does the character of only a very critical rehearsal of real life.

The scientific consciousness, too, in a very real sense that has been frequently recognized, is collective or social, and in this character of it one can see a reason both for its objectivity and its phenomenality to the individual, as well as for its exclusion or rather its suppression of the individual will. Kant's *a priori*, for example, so closely related to all modern scientific phenomenalism, has again and again been taken as only his way of giving account of the social mind or say of the abstracted social factor of individual experience. With this understanding, however, the development of an objective and phenomenistic science at any time should be accompanied by social integration and should be a forerunner of some new social movement, the individual will losing itself in the more inclusive process that science has been describing and explaining. Be this as it may, however, suffice it now to say that no theory of knowledge can afford to neglect, as on the whole Professor Münsterberg's theory certainly does neglect, the opportunity for explaining science which the social factor in experience undoubtedly offers.

And furthermore I cannot but think that a keener appreciation of this social factor would have saved Professor Münsterberg from treating not merely psychology and physics but also psychology and both the historical sciences and the normative sciences as at once separate and coördinate sciences. Objectively, perhaps, the sciences are coördinate, just as objectively one might even say that all the parts of the body are of equal rank, being all essential to its integrity; but equality,

like freedom, is a troublesome conception, being too easily taken, among other things, to mean sheer individualism. In reality must not coördination or equality comport with the subjection of each individual to all the others? Science to-day—and this I think is constantly getting truer—is no mere aggregation of separate inquiries, each with its own ‘free’ or distinct object; it is coöperative. Professor Münsterberg would believe that physics and psychology are different in the way already outlined here, psychology dealing with individual objects and physics with universal or ‘over-individual’ objects, and that the historical and normative sciences deal not with objects at all but with will-acts, with individual and ‘over-individual’ will-acts respectively; but such distinctions, full of suggestion as they are, epistemologically worth while as they are, instead of separating the sciences only make them all the more indispensable to each other. Reality, which is divisible in no such way, in spite of, or rather because of, the phenomenalism, is a passion even of science, and is bound therefore to make any special science whatsoever consciously dependent on every other and so prone to use both the methods and the material of every other in its own researches. So psychology uses and needs to use the physical sciences, which strangely enough are most opposed to it, and this use and the need of it Professor Münsterberg recognizes and insists upon emphatically; a psycho-physical parallelism is *epistemologically* necessary to a science of psychology; but does Professor Münsterberg properly appreciate what he recognizes? Is the need only epistemological? For my own part I think not.

Furthermore, apart from the relation of the physical sciences to psychology, history at the present time is notoriously not a science by itself but only a method in science. Even history in the narrow sense of a study of human progress and achievement is after all only a method of general political science, while the ‘historical method’ is a resort of every scientific investigation. In general, therefore, because the sciences are differentiated only to become methods of each other, they are not to be treated in the individualistic way—certainly individualism is his strong leaning—of Professor Münsterberg, but rather they are to be regarded as one and inseparable—and are made so by their very differentiation. How else can we give meaning to such characteristic developments of modern life as physiological psychology, physiological chemistry, physical chemistry, evolution-mechanics (*Entwickelungsmechanik*), and the like? The time has come when scientific specialism by some grim fate or other has actually turned all the different sciences into methods of each other,

and this development, properly appreciated, certainly makes both their separation and their relation something more than an epistemological affair, since it evidently brings the will of real life into the very camp of objective science. Something like a feeling for just this I have liked to read between Professor Münsterberg's lines, but he himself, I fear, would find such a meaning only perversion or misinterpretation, since it makes the volitional attitude and the scientific attitude altogether too intimate. It makes them intimate, of course, because just the will which is necessary to the application of a phenomenalist science to life, and which from its first breath has been trained to make allowance for phenomenism, is a necessity, is even all the more a necessity, to the application of one phenomenal science to another. Morally, or *even* metaphysically, as well as epistemologically, the separation of the sciences is of significance. An epistemologist has no right to expect to escape metaphysical criticism.

But what is to be said in particular of the separation of the objective sciences and the normative sciences, say for example of psychology and ethics? We have seen in the foregoing that the very knowledge which science has of its own phenomenism only shows the will of real life lurking even in the scientific consciousness, as if the scientist, however apparently 'objective' and 'special,' were really in sympathy with something like the mediæval doctrine of reservation, and we have seen too that in the relation of the different sciences, using each other as they do and must, the will of real life is very much in evidence, and now in this matter of the relation of psychology and ethics something similar is to be discovered. Ethics may be one thing and psychology may be another; the two may be very different things indeed, as different even as real life and objective science; but put them side by side, make them contemporary and aware of each other, as any one who would separate them however sharply, even relegating them to different classes or castes of society, certainly must, and their propinquity is sure to beget a mutual interest—I almost said love. Ethics as *only* a normative science, not an objective science, can not fail to entertain doubts of herself and out of her helplessness and insufficiency at last to look with quiet appeal at psychology, and the psychology that pays no heed is—well, it is, as Professor Münsterberg would seem to wish it, lacking in common humanity. But, apart from the romance in the relation of these two, my simple point is this. Psychology and ethics, knowing their limitations, can not avoid, and they have a right to express a conscious



dependence on each other. An only normative ethics must and does feel that its rules are provisional or hypothetical, its standards arbitrary and its outlook narrow, and an only objective psychology must and does chafe under its peculiar world as a mere idea, and the relation between them, that the dissatisfaction of each begets, is a *real reality* of the 'real life.'

Professor Münsterberg's separation of the objective and normative sciences is dangerously near to being only the old story of a science as teaching us to know and an art as teaching us to do. This story could pass muster in the days of 'double truth' and 'double life,' but in these times, when dualism with all its incidents is at best only an epistemological fiction and open to question even as that, the story must take its place among interesting myths. Art is action; science is knowledge; and all knowledge is knowledge how as well as knowledge what. Professor Münsterberg would perhaps admit all this, and his book in spirit if not always in letter has seemed to me to recognize the real value of science to life. Certainly he does not mean to say that science is a luxury, an epiphenomenon, and yet too often he seems to say this and will, I am led to believe, be so interpreted by many who read him. Thus, to review what has been suggested here, he writes of the freedom of the object in a way that is, to say the least, ambiguous; he does not appear fully to appreciate the import of science being not merely phenomenalist but also awake to its phenomenality, that is, of its being qualitatively one with consciousness in general and so quite as much a volitional as an objectively scientific attitude; he fails really to use the opportunity for explaining science's objectivity and phenomenality which is afforded by the social factor, or better the social will, in individual experience; he only partially apprehends the significance of specialism, in which the different sciences are seen to have become methods of each other and in which accordingly the will of real life is manifestly active; and he is blind to the romance always going on between the normative and objective sciences. This is truly a rather long list of criticisms, but lists are easily made and of course the foregoing comprises really only one criticism more or less extensively analyzed. Perhaps the one criticism is this: Professor Münsterberg's epistemology is not sufficiently responsible to metaphysics. *A theory of knowledge and a doctrine of substance are mutually dependent.*

But, finally, there is the *Aktionstheorie*, which is full of interest and is perhaps the most satisfying thing in a most interesting book. As was noted above, this theory is comprised in the concluding chap-

ters of the third part, 'Der psychische Zusammenhang.' To speak after the analogy of a circumference, its three determining points are as follows: (1) "Jedes motorische Centrum steht zunächst zu einem antagonistischen Centrum in Beziehung" (p. 534). Every action, this amounts to saying, has its specific reaction,<sup>1</sup> and very much as the equation of action and reaction in the physical world gives control or conservation, as something immanent instead of imposed from without or instead of resident in any single mass, so here in the sphere of the psychical control is made immanent. Thus: (2) "Das motorische System ist also kein Armeekorps mit einem einzigen General an der Spitze; es hat viel mehr Generäle als Gemeine, aber die Anordnung in übergeordnete und untergeordnete Funktionssysteme muss unbedingt festgehalten werden" (p. 533). So would psychology or physiology apply within its sphere of interest and influence the principles of democracy in place of those of militarism and monarchy. But (3) we read: "Die Aktionstheorie \* \* \* wird \* \* \* behaupten, dass es überhaupt keine Empfindung giebt, der nicht ein motorischer Impuls zu Grunde läge und dass somit in diesem Sinne jede einzige Empfindung und daher jedes Element des gesammten Bewusstseinsinhaltes eine Innervationsempfindung sei" (p. 529), and this third point, explicit as it is, certainly completes the definition of the theory. Moreover, besides completing the theory it is dangerously near to betraying its author's earlier epistemology. Thus who can help jumping in his thought from consciousness as 'Empfindung' to consciousness as science or from 'Innervationsempfindung' to the will of real life? The chasm between them has never been impassable. And who can help thinking not only that the distinction between 'Empfindung' and 'motorischer Impuls' is parallel to that between science and 'real life,' but also that the difference between the sensory system and the motor system is in some way intimately involved in that between 'jedes motorische Centrum' and its inevitable antagonist? 'Real life,' in a word, although merely as physiologists or psychologists we may not call it that, must be supposed to have its special organs, its special localization, in the body and to admit it there even under a scientific *incognito*—so I have to think—is seriously to complicate the consistency of Professor Münsterberg's position. Again, to call the *Aktionstheorie* only a logical construction in science, a mere theory of physiology or psychology, is as far from saving it from being made a theory of real life as the cloister was from protecting nominalism against the real life of religion and politics.

<sup>1</sup> That is, counteraction.

And the *Aktionstheorie*, with its suggestion of control through the opposition of centers—analogue, as hinted already, to physical conservation through the equivalence of action and reaction—is not without a very decided suspicion of Hegelianism, and we have to wonder if Professor Münsterberg is not in danger, in spite of himself, of being carried over to an avowal of relationism and incidentally to the theory of interaction. Certainly, the *Aktionstheorie* is not dualistic in its spirit, whatever it may be in form. This reading of it, too, makes one able to gratify Professor Münsterberg by associating him with Fichte, although throughout the earlier parts of the book the presence of Kant or even of Schopenhauer is felt, and felt strongly. In the preface (vii, viii) we are told that the present book hopes once for all to banish the ghost of Schopenhauer, which so many saw with their very eyes in *Psychology and Life*, and that, historically, the author would prefer the company of Fichte; but this can only mean either that I have not understood the book itself and so that the form of statement is defective, or else that in his concluding chapter the author has really outdone himself.

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*Naturalism and Agnosticism.* The Gifford Lectures delivered before the University of Aberdeen in the years 1896–98. By JAMES WARD, Sc.D., Hon. LL.D., Edinburgh, Professor of Mental Philosophy in the University of Cambridge. Two volumes. New York, The Macmillan Co. 1899.

We are due Professor Ward an apology for the tardy appearance of this notice of his important work. Suffice it to say that circumstances over which the editors had no control, and lying without the knowledge of the present writer, rendered the delay unavoidable.

At this late date it is probably unnecessary to set forth the contents of the work in great detail. One may recall that the lectures deal *critically* with (1) the Mechanical Theory; (2) the Theory of Mechanical Evolution; (3) Biological Evolution as involved, though not always expressed, in the conclusions of Lamarck, Charles Darwin and their followers; (4) the Theory of Psychophysical Parallelism, with its accompaniments, 'mind-stuff,' 'double aspect,' 'conscious automaton' and 'mind as an epiphenomenon of matter.' These critical and destructive excursus are followed by a 'Refutation of Dualism,' in which Professor Ward indicates his own standpoint and his reasons for adopting it; this he accomplishes here by the double refraction process mainly. Finally, a theory, called 'Spiritualistic Monism,' is



advanced; this furnishes forth the lecturer's positive construction, in so far as he finds himself in a position to formulate basal principles.

Despite Dr. Ward's apologetic preface, which applies, no doubt, far more to the constructive than to the critical sections of his work, these lectures must be viewed as a contribution of capital importance for contemporary thought, especially in the English-speaking lands. Moreover, they are of particular interest to the American public, for academic thought in the United States happens to be in a more fluid condition at the moment than in Britain. This peculiar interest must be traced to the fact that our author possesses competent knowledge of science, and perceives, as too few British writers have perceived, the vast import of scientific advances and conclusions for philosophical inquiry. As contrasted with Germany, Britain and the United States remained outside the main current of nineteenth century progress in speculation for nearly three generations. If we take Herder's 'Ideen' (1784-89) as the first conscious embodiment of what was to become the nineteenth century standpoint—historico-evolutionary, we find that, as concerns systematic treatment, the English world remained barren till 1865, when Dr. Hutchison Stirling burst upon it with his 'Secret of Hegel.' During this long interval, the British contented themselves with the quasi-political principles of utilitarianism which, although sensing the organic conception of society—not of the universe—in some ways, remained for the most part under the domination of the static ideas of the seventeenth and eighteenth centuries; with the localized problems of the Scottish school, in which speculation was determined largely by provincial conditions; with the ineptitudes of Mansel, whose fulminations can hardly be said to display vital comprehension of the achievements of Kant and his successors. No doubt Coleridge and Carlyle, to name no others, furnished an earnest of better things. But were they not mystics and misbelievers, perhaps disreputable personages? Accordingly, when Hegel did make entry, his pregnant ideas served to fill an aching void, were thus seized with intense avidity, and transformed into a veritable gospel whose converts still bear strong, sometimes haughty, sway.

Similarly in the United States. Emerson and the transcendentalists, thanks to New England Unitarianism, received more immediate response than Carlyle. But, as strict philosophy went, thanks this time to the denominational colleges, the eighteenth century modes of the Scottish school were impressed upon the callow youth till the seventies. While, after 1875, the Hegelianism of St. Louis and of several among the rapidly growing universities played a part not un-

like that taken in Britain by the idealistic groups at Glasgow and Oxford. Here the parallelism ceased. For, dismissing the consideration that it is easier to dominate a small than a large country, Britain offers no counterpart to the wonderful development of experimental psychology which has effected so much in the United States for a better understanding between science and philosophy. Further, as the American universities maintain intimate relations with those of Germany to a degree unexampled in Britain, the later philosophical movement at Halle, Bonn, Leipzig and the rest—positive and philosophical in character—has had larger influence over cisatlantic conditions. For these reasons Dr. Ward's work may well receive greater approval and attention here than in the land of its birth. Further, it betrays something too much of Lotze's influence to be altogether palatable to the dominant British school. While in this country Lotze's system, evil though its substantialism has been in many respects, counts students enough to be sure of a respectful hearing. On the whole, then, the office of these Gifford Lectures, as the present writer conceives it, is to bring us to a consciousness of the present relation between science and philosophy, a relation grasped more vividly, maybe, by the American than by the British reader.

In his discussion and evaluation of exact science, strictly so called, Dr. Ward shows with great success that modern dynamics and mechanics involve fundamental philosophic (metaphysical and epistemological) presuppositions. Only when these have been separated out from the proper details of the sciences does it become possible to set forth the precise implications adopted *ab initio*. In other words, in so far as the exact sciences have come to contemplate latterly a theory of the universe, rather than a mathematical description of certain aspects of human experience, they have laid themselves open to criticism, to basal valuation, at the hands of philosophy. Thus it becomes simple to trace the reasons for their inadequacy, not within their own chosen, or abstracted, spheres, with which philosophy has nothing to do, but in their explanation of the universe based upon foundations limited artificially to meet the requirements of the content of these sciences. At best, as Dr. Ward proves, we get nothing more than description here; at worst, a series of analogies wherein, from certain 'fictions of the understanding,' a passage is made to ultimate reality. In short, unvalued postulates, not rationally defended principles, govern the cosmic construction.

So much attention has been attracted by the criticism of Mr. Herbert Spencer, in the lectures on the Mechanical Theory of Evo-

lution, that one need do no more than advert to it. I find myself in complete agreement with the majority of Dr. Ward's strictures, especially in that they bring us to clear consciousness of the fact that Mr. Spencer's work has relapsed into a position of historical value only for some years back. On the other hand, the criticism of Biological Evolution is by no means so successful. One reason for this is its extreme brevity. Thirty pages do not afford a canvas large enough for treatment of the 'Lamarckian, Darwinian and ultra-Darwinian theories,' even 'generally,' as the caption runs. In particular, I take it that cautious scepticism is well in place regarding Dr. Ward's elevation of 'subjective selection' to a position of such importance. And, apart altogether from difficulties in the matter itself, these sections are unfortunate as tending to produce misunderstanding on the part of professional biologists, one of whom, for example, reminds our author 'that nothing is easier than for one who is not a naturalist to improve upon the work of Charles Darwin.' Remembering the office which, as indicated above, this work is so well fitted to subserve, one cannot but regret that the enemy—who is really a friend—should find any cause to blaspheme. The truth is that a more extended and thorough treatment of biological evolution from Dr. Ward's pen were very greatly to be desired. In especial, the 'teleological factor'—Lamarckian—in the origin of species demands fresh and further interpretation. For, as it stands now, it seems like to cause a serious, and probably unnecessary, difference between scientific men and philosophers in their consideration of what, after all, cannot but be an identical problem for both.

The first part of the second volume is devoted to the discussion of the Theory of Psychophysical Parallelism. In these pages our author devotes attention to Spinoza, Clifford, Huxley, Du Bois-Reymond, rather than to such psychological writers as Bain, Höffding, Lange, Münsterberg, Jodl, and their fellows. There is a reference to Wundt's article on Parallelism (*Phil. Studien*, x, 33), but to the simple effect that its conclusions are methodological, not systematico-constructive. As against the thinkers with whom he thus takes issue, Dr. Ward is entirely successful. It is interesting to note that he indicates his agreement with James by some pertinent quotations. But here again, as in the case of biological evolution, we would like something bearing on those more recent phases of the controversy, in which criticism of the parallelistic theory has been undertaken by such writers as Busse, Rickert, Wentscher, Erhardt and others. At bottom, the problem is, of course, a metaphysical one. But it cannot be



solved, or even palliated, till we have worked through, not merely the epistemological 'inhibitions,' as one might term them, but also through the physiological and psychological facts in detail. It were much to be desired that a master like Dr. Ward, equipped, not merely with the requisite knowledge, but with a peculiar detachment, insight and cool acuteness, should return to this problem in its most recent phases. I, for one, must hold that, till some thorough treatment be forthcoming, monistic construction cannot avoid the pitfalls of otiose acquiescence. And it is by no means certain that the 'Spiritualistic Monism,' offered by Dr. Ward as his final solution, is free entirely from this very limitation.

Yet, whatever may be said in the way of criticism, one would fall short of his duty did he fail to remind readers that, taken as a whole, these Gifford Lectures must be classed with the most important British contributions to philosophy in recent years—with Green's 'Prolegomena to Ethics,' E. Caird's 'Kant,' F. H. Bradley's 'Appearance and Reality.' True, the office of these last is different, and their initial audience was not of the same character. Nevertheless, in what Dr. Ward implies, more perhaps than in what he actually states, he deserves the closest attention of philosophical students, more especially of those who hold, like the present writer, that the immediate future of philosophy depends on the manner in which it reckons with the positive sciences.

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### COMPARATIVE PSYCHOLOGY.

*Animal Behaviour.* By C. LLOYD MORGAN, F.R.S. Illustrated. London, Edward Arnold. 1900.

The author set out with the purpose of preparing a new edition of his 'Animal Life and Intelligence,' but concluded, and all will agree wisely, to write a new book, into which very little material from the above or his other works has been introduced.

The chapters on organic behavior of plants and animals, with pictorial illustrations, are written in order to show among other things that while there is beautiful adaptation effective to a definite biological end, there is an absence of a guiding consciousness. Among plants there is no evidence of profiting by experience. After a glance at reflex action the discussion of the evolution of organic behavior is considered. To the question, Are acquired modes of behavior inherited? a negative answer is provisionally given, though it is admitted

that if this be not the case 'the method of natural selection in racial progress is curiously indirect.'

Natural selection develops congenital definiteness of response and such innate plasticity as is of advantage under the conditions of existence, uniform conditions tending to emphasize the former, variable conditions the latter.

Professor Morgan concludes that a belief in an accompanying consciousness in the organic behavior of animals is wise, since the associations which take part in the guidance of behavior are so varied and delicate that a skeptical attitude is a greater strain than is a belief in conscious control. And if his remark were extended it would explain very well the attitude of some who are disposed to be somewhat more conservative in regard to many points in animal behavior than Professor Morgan and those of his way of thinking.

Speaking of the explanation of the behavior of a chick, after some experience, the author well says: "Sentience is not sufficient for guidance; there must be consentience involving the presence of several elements; they form constituent parts of the coalescent situation as a whole, of which alone the chick is personally conscious, without analysis of detail," though it is felt that the manner in which consciousness affects behavior is far from clear.

Briefly, consciousness in the first stage of development may be regarded as an accompaniment, in the second as a guide, and in the third as a judge. The latter must of course not be attributed to animals—it is characteristic of man alone.

Instinctive behavior is treated at considerable length. Mr. Marshall's views are subjected to a critical examination, with the general result that while Professor Morgan agrees that instinctive acts tend to the well-being of the individual and the preservation of the species he does not hold that the biological end is the objective mark of an instinct. On the other hand approval is expressed of Dr. Peckham's definition of instinctive behavior: "All complex acts which are performed previous to experience, and *in a similar manner by all members of the same sex and race.*"

Professor Morgan himself would define instinctive behavior as "comprising those complex groups of coördinated acts which are, on their first occurrence, independent of experience; which tend to the well-being of the individual and the preservation of the race; which are due to the coöperation of external and internal stimuli; which are similarly performed by all the members of the same more or less restricted group of animals; but which are subject to variation, and

to subsequent modification under the guidance of experience." Most readers will agree that this leaves little if anything to be desired as a general statement of the case.

Then follows an examination of the subject as it is illustrated by insects and birds.

The characteristics of instinctive behavior in birds are the following:

1. That which is inherited is essentially a motor response or train of such responses. The compound reflex action of Herbert Spencer.
2. These often show very accurate and nicely-adjusted hereditary coördinations.
3. They are evoked by stimuli, the general type of which is fairly definite, and may in some cases be in response to particular objects.
4. They are also generally shown under conditions which lead us to infer the presence of an internal factor, emotional or other.
5. There does not seem to be any evidence of inherited knowledge or experience.

While there is probably in all cases present some internal prompting, it is not equally clear whether a definite external stimulus is invariably necessary. If the latter is indispensable, the reviewer can testify that in some instances 'definite' must be given a very liberal meaning, if not replaced by another term. A very small spark indeed in some cases—if spark at all—is required for the combustion which sometimes seems to be all but spontaneous.

The author does not look with favor on the term 'acquired instincts' of Wundt.

"How comes it, then, that the chick does not instinctively respond by appropriate behavior to the sight of water?" asks the author. Some of us would question this. We hold that the chick does respond to the sight of water under the conditions of its normal existence and frequently under the more or less unnatural conditions we substitute. It is merely a case of a less certain or more tardy response than that to food, so that arguments founded on this supposed fact must carry little weight, whether applied to heredity or other problems.

While frankly admitting that a perfectly satisfactory explanation of intelligent behavior cannot be given at present, Professor Morgan thinks that "from all parts of the automatically working organic machine messages come in to the center of conscious control, and that in accordance with the net result of all these messages, past and present, tinged with pleasure or pain, other messages go out to the



automatic centers, and by checking their action here and enforcing it there, give new direction to resulting behavior."

The physiologists and neurologists at all events will approve of this view of the case, and probably the psychologists equally. When, however, the author goes so far as to say, in comparing intelligent and instinctive behavior, that "in performing the instinctive act, the animal seems to have no more individuality or originality than a piece of adequately wound clockwork," we must dissent. This is far too strongly put for the instincts of the higher animals at least.

Professor Morgan makes generous use of the valuable researches of the Peckhams. Speaking of certain behavior of the wasp, he says: "Here we have intelligent behavior rising to a level to which, some would apply the term rational. For the act may be held to afford evidence of the perception of the relation of the means employed to an end to be attained, and some general conception of purpose."

The author thinks that the question as to whether animal intelligence attains to the 'rational' is more likely to be answered through experiment than by chance observations.

We would say both by experiments and by *carefully made observations*, of which the latter would likely prove the more important, as being more likely to find the animal under natural conditions.

Professor Morgan seems to have based his conclusions in regard to the intelligence of animals, especially dogs, on some observations or experiments with one or two dogs he possessed. He lays great stress on several failures of his fox terrier to get a stick through a fence. Would a well-trained retriever, collie or poodle have had a like difficulty—although their experience had not been in the direction referred to in this case? The reviewer's study of many breeds of dogs leads him to take a much higher view of their intelligence than Professor Morgan seems to believe justifiable. His experience with throwing a ball has been very instructive. In this he has generally tried experiments, sometimes at the same time, with a half dozen dogs of dissimilar breeds. They behaved very differently—perhaps more so than the same number of small boys would have done. The St. Bernard showed a great deal the most of what one might call 'gumption.' He took in the situation by far the best, and adapted to it in a surprising way, while the terriers were altogether more machine-like.

Dr. Thorndike's experiments are reviewed and criticised somewhat unfavorably, though Professor Morgan in many of his positions on the subject of animal intelligence does not differ greatly from Dr.

Thorndike. Says the author, commenting on the behavior of his dog in lifting the latch of the gate: "He did it with the back of his head. I could not get him to do it (more gracefully) with his muzzle." And why should he, seeing that the muzzle is a very sensitive part in dogs?

The author holds that "it may be said that between the method of intelligence and that of fully developed rational procedure there is a wide gap which must have been bridged over in the course of mental evolution. Unquestionably, and in contending that the methods of the animal are predominantly intelligent, I am far from wishing to assert dogmatically that in no animals are there even the beginnings of a rational scheme. \* \* \* We shall probably have to await the further results which must be the outcome of patient and well-directed child-study."

Naturally the reviewer read the above with hopeful satisfaction. With Professor Morgan the case against rational behavior is, at least, not yet closed.

It is believed that growth in intelligence takes place by what the author designates as 'condensation of experience by an elimination of detail and the survival of essential features'—also by the elimination of those modes of behavior which were not efficacious, *i. e.*, by the functional selection of Baldwin. According to this view an animal may come very near to the attainment of the abstract without quite reaching it.

With the development of the higher intelligence instincts decay, which possibly explains why man has so few 'stereotyped instincts'; nevertheless residua remain, which explains much.

Under 'Imitation' Baldwin's 'circular process' is discussed and its truth admitted with certain important reservations; but Professor Morgan does not favor on the whole the extended usage of the term that Baldwin would advocate.

On the other hand the author cannot endorse Dr. Thorndike in his extreme position on the subject of imitation, although he does not apparently believe in 'reflective imitation,' though 'intelligent imitation' is conceived to be of great importance, as also 'instinctive imitation.' In ants, difference in behavior is thought to give rise to suggestive effects on the other members of the community, rather than that their conduct is dependent on communication by any definite system of signs; nor does the author believe that dogs understand words in the proper sense of the term.

While in general Professor Morgan is not convinced that animals reason, he is not prepared to assert dogmatically that they do not, for

he says: " Presumably in the ant, rook, and beaver anything like an ideal scheme of thought based on reflection, if it exist, is as yet exceedingly indefinite."

Under ' Play and Courtship ' the views of Professor Groos are considered, and the author would have the former attach more importance to courtship in generating and strengthening the ardor of the male. Nor would Professor Morgan think it necessary to introduce ' anything so psychologically complex as the conscious illusion of make-believe ' in order to explain certain forms of mock combat, etc. He would differ from those who hold that play is always the outcome of a surplus of energy. He instances the case of a sick kitten attempting to play as evidence to the contrary.

The infrequency with which the term ' association of ideas,' apparently used by many to cover psychological ignorance, occurs in this work is noteworthy.

One lays down this volume with the feeling that it is a psychological and literary production of an unusually high order.

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'The Fur Seals and Fur Seal Islands of the North Pacific Ocean.'  
Part III. Pp. 69-74. Washington. 1899.

These papers, which are either mere notes of isolated observations or minor experimental studies, are of interest as indicating a growing disposition on the part of naturalists to study living animals from a more or less psychological point of view, rather than for any definite results of importance to psychology. Two of them (by trained psychologists) are especially noteworthy because of their employment of a simple experimental method, shown to yield highly instructive results.

Dr. Thorndike used the common *Fundulus*—a bony fish, lacking a brain cortex—in order to test 'the ability of fishes to profit by experience and fit their behavior to situations unprovided for by their innate nervous equipment.' The device used was a large aquarium. The space at one end was shaded, and all food was administered at this end. Along each side were fastened simple pairs of cleats fitted to receive partitions (of wood, glass or wire screening) made each with an opening sufficiently large for the fish to swim through. After a fish had been driven to the illuminated end, a partition was introduced, and its reactions to the situation observed. In its effort to reach the dark, the fish swam repeatedly against the screen, until it chanced to find the opening, through which it swam unimpeded to the shaded end, in which it was allowed to rest for some time. As the experiment was repeated, the fish tended to make fewer mistakes, fewer pauses, until finally the appropriate movement was directly made, and without delay. Several individuals were tried with this and similar experiments (*e. g.*, one in which three slides were used, thus involving a serial multiplication of associations) with a like result. Figures and dimensions of the apparatus are given.

Dr. McIntosh, Professor of Natural History in the University of St. Andrews, communicates a number of interesting miscellaneous observations on several English fishes, which indicate their ability to profit by experience, and that rather rapidly. Such facts are familiar to every fisherman.

Dr. Thorndike's experiments are valuable as providing at once a demonstration and a measure of the associative process.

Mr. Yerkes made experiments of a similar nature upon a single individual of *Chelopus guttatus*, the common 'speckled turtle.' The turtle 'learned' to find its way through a labyrinth, arranged by dividing a box by means of partitions provided with openings so placed

as to compel the animal to take a somewhat irregular route in order to reach the dark 'nest' of damp grass. When placed for the first time at the farther end, the turtle reacted to the unfavorable situation of confinement, light, etc., by 'wandering about almost constantly for thirty-five minutes,' at the end of which time 'it chanced to find the nest, into which it immediately crawled, there remaining until taken out for another experiment two hours later.' After the first three times, the routes, which had been extremely tortuous, became much more direct. Fifty trials were made. The times and tracings of the paths show a rapidly increasing economy of movement. "What at first took minutes, after a few trials took only as many seconds." The sense of smell may have served as a guide. Analogous results were obtained with a complication of the labyrinth by the addition of a blind alley and three inclined planes. In the second labyrinth the turtle gradually learned to shorten its path eight or ten inches by taking a fall (of four inches) over the edge of an incline. Everything in these experiments, it is wisely argued, points conclusively to mere associative capacity to profit by *chance* experience. This popular article is clearly written and illustrated with diagrams. While the results are extremely suggestive, they require to be supplemented by extended experimentation.

The Peckhams, who have rendered a signal service to comparative psychology by their publications on spiders, wasps, etc.—the fruits of enthusiastic zeal and patient industry in observation, together with ingenious reasoning—in the note cited record the 'change in a long-established custom' of the wasp *Sphex ichneumonea*, which they attribute to 'reason.' The animal, at least, is thought to have learned finally to act at variance with the deep-rooted instinct of leaving her prey (grasshopper) at the entrance to her tunnel and then running in and out again before dragging it down, after repeated experiences of finding it at a changed position (due to the disturbing hand of the experimenter). There is not space to discuss it here, but the evidence as stated in this single case, does not seem to us conclusive.

Mr. Shufeldt, after commenting on Professor Whitman's "Animal Behavior,"<sup>1</sup> discusses some of the sense powers of teleost fishes and describes certain observations on living fishes in an aquarium. The peculiar behavior of the young of the snowy grouper (*Epinephelus niveatus*) and the big eye (*Pseudopriacanthus altus*) when ap-

<sup>1</sup>Biological Lectures from the Marine Biological Laboratory, Woods Holl, Mass. (Boston, 1899.)

proached, or under certain other conditions, is explained as an instinctive simulation of death or dying and accorded selective value.

Messrs. Young and Cole record some interesting observations in the methods of nest-building in the brook-lamprey which seem to indicate a high degree of variability in the instincts involved, and which are, therefore, of some interest as data for a genetic theory of instinct.

A like variability on the part of certain birds (notably the robin, cedar waxwing and red-eyed vireo) in respect of their partial habit of swallowing the excrement of their young by way of cleaning the nest, is noted by Mr. Herrick, who suggests that it may serve as food for the parent, since digestion in the young is an imperfect process. Other precautions on the part of the adult bird are mentioned as definite and invariable instincts.

The late Professor Norman's preliminary notes on the habits of *Typhlomolge rathbuni*, posthumously published by Dr. C. H. Eigenmann, contain some observations on the sense powers of this species. The individuals tested showed no reaction against light. The sense of touch seemed highly developed, but could not be experimentally localized. There was no evidence of a sense of smell. The paper is a mere fragment.

Mr. Lucas, 'after a careful study of the behavior of the seal in the field' has been impressed with the absence of intelligence in this animal, which he regards as conspicuous for the blind stability of its instincts. "As it has done in the past so it will do in the future. Its habits, being formed by the slow process of natural selection, can change but slowly, hence the fur seal is not likely to alter its habits, or to adapt itself to changes in surrounding conditions. It may be exterminated, but it will not leave its breeding grounds, and the last seal will come calmly on shore to be knocked in the head." Which would seem of more moment to economics than to psychology.

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### FEELING.

*Die Gemüthsbewegungen und ihre Beherrschung.* C. M. GIESLER. Leipzig, Barth. 1900. Pp. 68.

The aim of this discussion is practical, to control the emotional life. The more we avoid intense and exciting emotions, the longer we shall be able to endure the strains of a life that becomes ever more complex and strenuous. Before we can take up the practical problem, however, many theoretical questions must first be discussed, and in



the first of the six brief chapters various views as to the nature of emotion are mentioned. The author's comments upon these views are almost too brief to be called discussions: I. Psychological definitions. Among these, the first group reduce effective states to other forms of mental life, as the definitions of Wolff, Hobbes, Kant, Herbart. The second group represent emotion as an interaction between a mental image (*Vorstellung*) and a feeling based upon a judgment as to the beneficial or injurious relations of the thing imaged to the individual. Here belong the definitions of Wundt and Stumpf. II. Physiological definitions, reducing emotion to sensations caused by the physiological changes that follow the perception of the exciting object. Here are mentioned Féré, James, Lange and Ribot. III. Psycho-physiological definitions, which, like the genetic definitions of Nalowsky and Alfr. Lehmann, conceive emotion as a complex of feeling and organic sensations. Emotion is a union of feeling and organic sensations, the latter being due partly to the automatic excitation of bodily changes, which always take place involuntarily, and partly to undirected and involuntary innervations of voluntary muscles. This view underlies the further discussion of the author.

Chapter two deals with irritability as the forerunner of emotion. Irritability and its accompaniment, contractility, together play the same rôle in the lower animals that emotion plays in the higher. Affective experience is the first form of experience in the history of the race, the prosthadium for the growth of intellectual processes. In the child ordinary impressions are in time built into objects, and corresponding to this process in the race is the development of the sense-organs. 1. In the service of self-preservation, irritability is what some authors have, I believe, agreed to call *mimetism*—a reaction which tends to repeat its own stimulus (Baldwin). 2. In the growth of a sense of the external world, irritability and impulse furnish, respectively, the material and the formal elements, and the development of these elements runs parallel with the localization of irritability. Dr. Giessler follows the gradual localization of irritability among the lower animals into some detail.

Chapter three deals with the general theory of consciousness, in order to bring out the importance of movement and of motor images and tendencies in all forms of mentality. Quoting from v. Tschisch, "Sensations of movement constitute an indispensable element in every impression, every representation and every abstract idea."

Chapter four contains an analysis of affective states, in which the author develops the conception of a *diremption-threshold*. Affective

states are at first only feeling, but directly we become conscious of motor excitations which are not regulated by the will. These excitations are followed by a greater concentration upon the object, and for a time a voluntary control is exercised over the motor and ideo-motor tendencies. This is the period of tension (*Spannung*): it is followed by the period of discharge (*Entladung*): and between the two lies the diremption-threshold. There is a vital or under-will, a function of the physiological ego, which is instinctive and serves the ends of self-preservation and self-advancement, and there is also a cortical or over-will, which is guided more by reflection and is a function of the psychological ego. Ordinarily the former is subordinate to the latter, but in all intense emotions the lower instinctive will tends to predominate over the reflective will, and the moment when this tendency is realized, when the compensating influence of the will over the motor and ideo-motor processes ceases to be adequate, is the diremption-threshold. The locus of this threshold depends, (1) upon the relative automaticity and excitability of the motor processes, and (2) upon the relative strength of the apperceptive organ and function.

Chapter five deals with precautionary measures to avoid intense and exciting emotions by limiting the sphere of automaticity. The degree of physiological excitability should be kept down, and one should adopt such a method of life that there will be few if any occasions for the appearance of powerful emotions. Repetition is the chief source of motor excitability, and one should see to it that the automatic motor processes involved in emotion are not repeated very early in life or very often. A consequential habit of acting should be cultivated and altruistic motives should be held in the foreground. So conceive thyself inwardly that thou canst always be true to thyself outwardly.

The sixth and last chapter deals with the positive control of particular emotions. One should cultivate the ability to assume an apathetic attitude toward the object of an emotion. Where an affect involves organic or other sensuous imagery, it may be controlled by occupying the attention with other things. Where the affective state has already reached a high degree of excitement, it can be weakened or suppressed only by another affect, and indeed only by one resting upon ideal feeling. Victory can be won by bringing into the field other categories of worth than those of the moment. Moods may be modified best by awaking æsthetic emotions; affects, by ethical ideals with their emotions; but passions can be transformed or suppressed only by religious emotions.

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*Ueber Verallgemeinerung der Gefühle.* THEODOR ELSENHANS.  
Zeitsch. f. Psych., XXII. Pp. 194-217.

The question raised under the above title is briefly this: Do the feelings pass through transformations which resemble the generalization of ideas—are there, that is, ‘general feelings’ in the same way that there are ‘general ideas’? The author finds no difficulty in gathering abundant evidence in support of an affirmative answer to the question. The feelings do, in his opinion, frequently undergo a generalizing process, the results of which are disclosed by certain well-marked characteristics. To enter fully and intimately into the author’s view of how this process is accomplished, one should of course be able to gain first some insight into his general standpoint in reference to the feelings. Unfortunately this standpoint is nowhere clearly revealed. The reader is left to make his own unsatisfactory inferences from the special considerations of the text. And yet, if one is to make an intelligible contribution to our knowledge of the life of feeling, it is certainly most desirable that the reader be first provided with a brief confession of faith. For in the present state of the doctrine of the feelings no contribution to the subject can be correctly understood and properly rated without a knowledge of the general belief from which the view has sprung. The only preliminary supposition that the author sees fit to mention is that there are associations between ideas and feelings, so that when any one of the former is reproduced the corresponding feeling is reproduced also. But this does not admit us very far into the background of the author’s thought. There is, however, one indication of what the latter is. Much of the terminology of the article, many of its illustrations, and some of its divisions irresistibly suggest Wundt and the Wundtian doctrine of the feelings. How far Herr Elsenhans accepts the latter doctrine we have no means of knowing. But this much at least we may conjecture, namely, that he presupposes a *multiplicity of affective qualities*. Whether these qualities are thought of as belonging to more than one affective ‘direction’ is not clear, and indeed is rather indifferent for the present inquiry.

With three guesses at the author’s general position, we may now see how he treats the specific problem before him.

Two possibilities of generalization are conceivable. Either (1) “the feelings participate in the generalizing process of the ideas which they associatively accompany”; or (2) “out of several single feelings there are built up feelings of a more general sort.” Both of these possibilities are realized.

1. The process of passing to a general idea is, of course, that of



reaching a state which may refer indifferently to any one of a multitude of particulars, and which thus comprehends all these particulars within itself. Such an idea is always symbolized by a word, and is represented in consciousness either by a vague and, in a sort, composite image of the individual perceptions or ideas which are included under it, or by a single one of these states, to which the rôle of representative is allowed. How now about the feelings which originally accompanied each of the included ideas? If I rightly understand the author, these feelings are to be regarded as primarily aroused by the representative idea, whose place is in fact so readily taken at any moment by any one of its fellows to which the attention may be turned. Later, however, the verbal expression symbolizing the general idea may itself, by virtue of its close association with the representative image and through it with the feelings, serve to call up the latter directly. Feelings temporally disconnected are thus brought together by the verbal symbol, clustering as it were about this their common center. Thus the expressions 'rustlings of the forest,' 'breezes of spring,' etc., arouse unmistakably the most vivid feeling tones.

But this is only, as it were, the external part of the process of generalization. The essential part relates to two characteristic changes in the feelings. In the first place, the *intensity* of the feelings is reduced to a lower grade; and, in the second place, the *quality* of the feelings, while remaining in general the same, has decreased in definiteness. The intensity of the feelings aroused by the expression 'breezes of spring' is not to be compared with that of those which originally accompanied the single actual experiences. So also the quality of the feelings aroused by the word 'concert' is by no means as definite as is that of those accompanying any single concert heard at a definite time and place. Still, under circumstances, the special idea that stands as representative of its class may carry with it such a clearly marked feeling tone that the quality of the 'general feeling' may be rendered particularly definite and the whole consciousness of the moment be influenced thereby.

The cause of this lessened intensity and diminished definiteness of quality the author seeks, not in the parallel changes that the general idea itself has undergone in becoming general, but rather in the changed teleological relations into which the general idea has come. There is now no such close or definite relation to the weal or woe of the organism, and this fact is immediately reflected in the character of the accompanying feelings. This is the first form in which 'general' feelings may appear.

2. But feelings may come into immediate relations with one another without the aid of a verbal symbol. This may happen when they simultaneously exist. Such single feelings may then be gathered into one of a more general character. A splendid instance of this is the so-called 'common feeling' (*Gemeingefühl*). That this is a *total* feeling, a fusion product made up of numerous partial feelings, is a thought to which Wundt has long since accustomed us. But what the author wishes to emphasize is that the total feeling possesses a *general* character. To be sure, the relation between a 'general feeling' and its constituent parts is not the same as that between a concept and its included ideas. For while any one of the latter may come alone to consciousness, each single feeling has lost its individuality in the fused total. But despite the lack of analogy between the two in this respect the total feeling is seen to possess characteristic marks which make it 'general.' Such are (a) the lack of distinct spatial relation to the objective world; (b) a qualitative indefiniteness, similar to that noted in the first class of general feelings; and (c) no general decrease in the affective intensity, but rather a dependence upon the maximum intensity of any constituent feeling. All these three characteristics are unmistakable in that most comprehensive general feeling that we term 'mood.'

Excellent examples of this kind of generalization are those suggested by Ribot in another connection. (*L'année psychologique*, III., 1-9.) The precipitate of feeling remaining with one after such experiences as travelling through a new country, visiting a cloister, reading a novel, etc., etc., is in every case a 'general feeling' formed out of the more vivid and definite materials furnished from moment to moment by the actual experience. Much the same thing is illustrated by the vague feelings and moods evoked by the modern symbolists.

In the *intellectual* life higher and higher stages of affective generalization may be found, culminating in that feeling which surrounds any successful work of the creative imagination. As to the *aesthetic* feelings, those, for example, that accompany a musical production as a whole are more general than those accompanying specific harmonies and melodies. In the *ethical* life the 'conscience' represents a feeling of general character. And, finally, in the *religious* life such a feeling as that which the idea of devotion arouses is at once seen to be highly composite and general in character.

What is meant by a 'generalizing of the feelings,' and what the two directions are in which such a process may work itself out, may then be clear enough from this brief account. The novelty in this treatment of the feelings is, manifestly, the suggestion that feelings

may come not only into the relation of part to whole, but also into that of particular to general, so that wider and wider fusions of affective complexes may lose more and more the specific and definite characteristics of single feelings. Not all will assent to this way of envisaging the matter, for not every doctrine of the feelings admits of such processes as our author takes for granted. Still, such an attempt as has here been made is certainly a welcome one as assisting, possibly, towards some unanimity of opinion in this difficult and baffling realm of the feelings. It is only to be regretted that Herr Elsenhans has not admitted us more fully and clearly into the several matters of important detail which formed the background for this discussion, in order that we might be sure that the above interpretation of his thought is indeed the correct one.

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*La Tristesse et la Joie.* BY GEORGES DUMAS. Paris, Alcan. 1900. Pp. 426.

Dumas, the translator of Lange's work on the emotions, and the author of another work, 'Les états intellectuels dans la mélancolie,' presents us with a somewhat detailed study of joy and sadness. After a description of the facts from a psychological and biological standpoint, the physiological, chemical, physical and mechanical sides of the phenomena are discussed and analyzed. Some experiments are also introduced. Some of his conclusions are, that in melancholiac depression there is a diminution of organic combustion, as is also the case in active melancholia; that in agreeable excitations there is an augmentation of combustion; that the weight of the body increases in joy and diminishes in sadness; that, in short, joy and sadness are indications on the psychical side of two quite different vital processes. No experiments are reported for the settlement of the question debated by Brugia on the one hand, and Mairet and Bosc on the other hand, as to whether the urine of melancholiacs is more toxic than that of the normal subject. Mairet and Bosc contend that the toxic element is proportional to the intensity of the disease, no matter what its form may be. The question of tastes and odors was also investigated. The author supports Lange in his statement that the phrase 'bitter sorrow' is more than a mere metaphor, the patients investigated suffering in periods of depression from an intense bitter taste, viscous saliva, dry tongue, etc., evidently due to general troubles of nutrition and buccal and stomachic secretions. Fetid odors tended also to disappear upon the alleviation of the depression. Bouchard has investi-



gated the chemical constituents of these odors, so observable especially in the insane. The muscular or mechanical aspect of emotions is also discussed, but nothing new or important is adduced. Experiments on the vaso-motor circulation, central and peripheral, tend to differentiate joy and sadness very clearly. The question of moral pain and sorrow as to its peripheral or central origin is also treated. No mention occurs of the splendid work of Sutherland ('Origin and Growth of the Moral Instinct').

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*Zur Kenntniss des Einflusses einiger psychischer Zustände auf die Athmung.* D. ISENBERG and O. VOGT. *Zeitschrift für Hypnotismus*, 1900, X., 131-158.

*Ueber den Einfluss einiger psychischer Zustände auf Kniephänomen und Muskeltonus.* O. VOGT. *Ibid.*, 202-218.

The point of view is that of individual psychology; each paper presents a careful study of a single individual—not the same individual, however, in the two papers. The first paper considers the effects on respiration of merriment (heiter), sadness (traurig), pleasantness (angenehm) and unpleasantness (unangenehm). These emotions were sometimes awakened by appropriate sensations, sometimes by imagination. Merriment increased the expansion of the chest and the extent and rate of the respiratory movements. Sadness had the opposite effects. Pleasantness decreased the expansion of the chest and decreased the rate, but increased the extent, of the movements. Unpleasantness had the opposite effects. Thus the effect of merriment is more like that of unpleasantness than like that of pleasantness, while the effect of sadness more resembles that of pleasantness.

As to the knee jerk and muscle tonus, Vogt finds them always changed in the same direction. Merriment (aroused by witty stories, etc.) caused a great increase in both; sadness (aroused by imagining a sad situation) a strong decrease; concentrated salt solution, disagreeable and stimulating, a moderate increase; sugar solution, agreeable but a little stimulating, a slight increase; pain (electricity) a moderate increase; a pleasant 'letting one's self go' produced a strong decrease; light hypnosis a decrease; intellectual work (mental arithmetic, etc., without anxiety as to correctness of result) caused a strong decrease, while muscular work (dynamometer) caused a strong increase; also, at the instant of cessation of muscular effort, a strong increase was noted. The author emphasises the facts that mental and

muscular work have opposite effects; and that merriment, though producing opposite effects to sadness, has the same sort of effect as the disagreeably stimulating; whereas sadness and the agreeably relaxing have like effects.

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### MOTOR PHENOMENA.

*Ueber die motorische Einstellung.* LAURA STEFFENS. Zeitsch. f. Psych., Bd. XXIII. 1900. Pp. 241-308.

These experiments continue and amplify a research begun by Müller and Schumann. These authors (Pflüger's Archiv, 1889) found that if a light and a heavy weight be alternately lifted for a number of times, the muscles become adapted to the particular degree of effort necessary for raising each; and if then a weight perceptibly heavier than the lighter, but much lighter than the heavier, of the original two be substituted unexpectedly in the place of the heavier, it is raised with unusual rapidity and regarded as weighing less than the lighter weight; similarly, one lighter than the heavier weight would be regarded as weighing more than it.

Miss Steffens confirms these observations and adds a number of new details. Such motor adaptation, induced in the organs of one side of the body, does not affect the judgment of comparisons executed by aid of the organs of the other side. In case two different motor adaptations are induced in succession, *e. g.*, one for the succession light-heavy and the other for the reverse order or for weights approximately equally heavy, the two adaptations gradually fade away, meanwhile mutually opposing one another, and the older of the two disappears more slowly than the younger, if both are of equal strength. These adaptations often persist for 24 hours or more. Their presence was tested by measurement of the actual rapidity of the lifting-movements, as well as by conscious judgments as to relative weight. A given number of alternate liftings are more effective in establishing an adaptation when spread over a considerable time than when all are performed in quick succession; and a given number, spread over equal times, are more effective when divided into a larger number of smaller groups than when divided into a smaller number of larger groups. In these laws of their behavior the motor adaptations closely resemble psychical memories.

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*Automatic Movements of the Larynx.* H. S. CURTIS. American Journal of Psychology, XI., 237-239.

A tambour, applied to the skin over the larynx, registered its movements during silent reading and during mental repetition of a speech or poem. The records showed that movements took place in 15 out of 20 subjects; in some they were very small, in others nearly as large as the movements recorded during actual whispering. Those persons who made no movements during silent reading usually made none even during whispering. The author seeks to connect his work with the sound-focusing experiments of Hansen and Lehmann, as showing that imagined speech is not absolutely silent, but is accompanied by actual movements of the vocal organs.

*Experiments on the Control of the Reflex Wink.* G. E. PARTRIDGE. American Journal of Psychology, XI., 244-250.

A small rubber-faced hammer was made to spring towards the face and strike on a glass plate directly before the eye. The subject strove to avoid winking. His success was measured (inversely) by the number of winks made before inhibitory control was fully secured. Great variability appeared among the adults tested; some soon learned the trick, while others never gained full control. Alcohol made it easier to inhibit the reflex. About 1150 school children were similarly tested. The ease with which the control was secured increased rapidly from 5 to 7 years of age, and after that more slowly up to 12-14 years. No correlation could be made out between the child's success in this experiment and his school standing or the time of day. The means by which the wink was prevented seemed to consist largely in the preliminary tension of opposing muscles. Young children at first showed general muscular tension over the whole body, but this decreased after practice.

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*De la relation entre le développement des canaux semi-circulaires et la coordination des mouvements chez les oiseaux.* J. P. LAUDENBACH. Jour. de Physiol. et de Pathol. génér., I. Pp. 946-949. 2 plates.

*Expériences sur l'anesthésie des canaux semi-circulaires de l'oreille.* G. GAGLIO. Arch. ital. de biol., XXXI. 1899. Pp. 377-397.

Laudenbach's article is a brief account of some dissections of the



semicircular canals of birds with different motor habits. The two plates accompanying the text show the appearance of the organs in our most common birds—hen, duck, goose, pigeon, hawk, buzzard, owl, crow, heron, etc.

The author assumes that the greatest development of the coördination of movement is evident in birds which fly much, alight often, and have arboreal habits. This class of birds, he finds, is endowed with the canals of greatest size, and he concludes, accordingly, that 'there exists a direct relation between the development of the semicircular canals in birds and the degree of skill which they exercise in the coördination of movement necessitated by the struggle for existence.'

It is well known that any abnormal stimulation or irritation of the semi-circular canal is followed by characteristic motor phenomena. Various investigators have accounted for these motor disorders in two ways, viz.: that they are due: (1) to a disturbance of function, or (2) to the irritation of the nerve endings in the canals. To determine which of these hypotheses is correct was the object of Gaglio's research.

The chief method of previous investigators has been the destruction of one or more of the canals and a subsequent examination of the motor adjustment of the animals experimented upon. Deductions from such an experiment are open to the objection that the results are explained equally well by either of the two above hypotheses. The present research was an attempt to rule out one of the hypotheses. For this purpose the author has applied cocaine to the canals. He assumes that the cocaine will prevent the functioning of any sensory nerves in the canals in the same manner that it acts as an analgesic.

The results of the experiments are as follows: (1) The effects of the anesthesia of the canals, by application of cocaine, are equivalent to those which are observed after section or destruction of the organ. (2) When cocaine is applied to the cut organ the motor troubles, manifested after the lesion, remain. (3) The movements of nystagmus endure after application of the anesthetic, and, moreover, the cocaine applied locally never calms the nystagmus but reinforces it.

These phenomena tend to show, according to its author, that the "characteristic motor disorders consecutive to section of the canals are due to a suppression of function, and not to a reflex action determined by sensory and traumatic irritation."

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## FATIGUE.

*Ermüdungsmessungen.* C. RITTER. Zeitschr. f. Psychol., XXIV., 2. Pp. 401-444.

The author of this unnecessarily lengthy paper is director of the Ellwangen Gymnasium. A desire to find a measure of intellectual fatigue, applicable to classes as a whole, led him to make the measurements here reported upon.

The Griesbach method having been shown to be unreliable, he turned to those used by Ebbinghaus (*Zeit. f. Psy.*, Vol. 13, p. 401 ff.) and selected, to put it to a test, the 'memory' method, although Ebbinghaus had discarded it as not yielding the desired results. It consists in the dictation of a series of numbers to be reproduced in writing immediately after by the subject. Ritter gave it up on finding that series of words were in every way preferable to number-series. The most serviceable word-series he found to be of six two-syllabic nouns, each one accented on the first syllable. The words are to be so chosen that they do not make sense. For the highest class of the gymnasium and for adults he thinks that series of seven two-syllabic nouns, or of five nouns of three syllables, may be preferable. He trained himself to read the series each time in exactly the same way—same rapidity, same rhythmic intonation, etc., and always gave the students a preliminary trial. Two or three series were used for each test. His conclusion from a total of about 140 tests—two successive dictations being counted for each individual as one test—is that this method gives satisfactory results; it is better than all the others with which he is acquainted.

Professor Ritter tried also, but without result, the substitution of sentences for meaningless sequences of nouns. Then, 'mit Unermüdlichkeit,' he devised another method—Professor Ritter does not seem to know that this other method has been frequently used, if not in the study of fatigue, at least for other purposes—the subject is required to underline certain classes of words or certain letters in a piece of printed matter. At first a task was assigned, but it was soon found preferable to set, instead, a time-limit. The results of about 130 tests are summarized as follows: "On the whole, the figures warrant the claim that the underlining of letters and words is also a really practical test of fatigue," but he prefers dictation to underlining, not only because it is much more convenient, but chiefly because of the practice—again not to be completely avoided with the latter. Notwithstanding his apparent success Professor Ritter does not believe

that he has perfected his methods sufficiently to warrant the drawing of conclusions as to the comparative fatigue-producing power of the different branches of study—and for this he is to be commended.

The great advantage of a dictation and also of an underlining method over the æsthesiometric is that one person can test a whole class in a few minutes, and also that several of the sources of error which make the Griesbach method worthless are avoided. But the decisive question is evidently whether or not they are reliable means of measuring intellectual fatigue. This cannot be held to be proven by Professor Ritter; his investigation is not extensive enough, the results are not sufficiently uniform and there are too many contradictory figures to be accounted for on more or less satisfactory ground, or which elicit the remark, "Hier stehe ich vor einem Räthsel."

The fundamental weakness of all these recent attempts at discovering a practical means of determining fatigue is their great deficiency in precision. The only procedure that will give scientific satisfaction, it seems to us, is one in which the value of the tested method will be determined by comparison, not with the expected fatigue produced by one, two or three hours of this or that study, but with an amount of work actually measured. If, for instance, the work done had been the addition of figures according to the method developed by Kraepelin and used with complete success by Lindley in his research, 'Ueber Arbeit und Ruhe,' *Psychol. Arbeiten*, III., pp. 482-534, the chief cause of the dubiousness of Ritter's work would have been removed, as the amount of work done could then have been exactly computed. In this way one could secure in a comparatively short time results having a definite, incontrovertible meaning.

Another methodological criticism must be made: The use of lists of meaningless words offers a very serious difficulty in that it is impossible to make them equally difficult. Similarly the underlining of, for instance, *t* (*T*), is not *a priori* to be supposed, as Ritter does, equally difficult with the underlining of *s* (*S*). There is here, we believe, sufficient disparity to account for divergencies as great as those shown by Professor Ritter's figures. A preliminary investigation should have established the comparative value of the several lists of words and of the several underlining tasks; this could be done by the means suggested above.

There are several other avoidable minor sources of error in these investigations.

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*L' influence de l'âge sur quelques phénomènes de la fatigue.* A. MAGGIORA. Arch. ital. de biol., XXIX. 1898. Pp. 257-286.

This article gives us a comparison of the muscular ability of the author at different ages. Maggiora, it appears, continued the work begun by Mosso, and has given us in these twenty pages the results of single experiments with the Mosso ergograph at intervals of about three years. The first experiments were made at the age of twenty-four and a half, the last experiments were made at the age of thirty-five. A comparison of fatigue curves with various weights at the varying ages shows, according to the author, an ability increasing directly with age. To have the muscles thoroughly recover from the effect of making a single fatigue curve, it was found necessary to have rest periods of two and of one and one-half hours, respectively, for the earlier and later days. From the few experiments described the author concludes that "the neuro-muscular apparatus increases in ability from youth to old age." No daily variation is noticed, the conclusions seem to be drawn from insufficient data, and the results have only a suggestive value.

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## ETHICAL.

*Psychic Rudiments and Morality.* GEORGE E. DAWSON. American Journal of Psychology, Vol. XI. Pp. 181-224.

The aim of this article is to give a scientific account of the genesis of immorality, with a view to discovering the most effective means of dealing with this troublesome and dangerous phenomenon. The discussion is judicious, and while little originality is claimed for it by the author, it renders familiar views more precise, gives them the support of facts and of accepted theories, and thus strengthens them greatly as a basis for practical measures.

After a preliminary discussion of differentiation, or the development of specialized organs as the normal process of evolution, Dr. Dawson points out that the elimination of organs that cease to be useful is an essential part of the process. Useless organs which are not eliminated, at least to the point of becoming rudimentary, stand in the way of the development of the specialized organs that characterize the next stage of evolution. Moreover, there is elimination in this sense as well of psychic qualities as of physical organs, and it occurs alike in ontogenetic and in phylogenetic history.

Dr. Dawson's central thesis is that immorality is essentially due to imperfect elimination of psychic rudiments. In other words, the psychic development of immoral individuals was arrested, either temporarily or permanently, and qualities of character that were useful somewhere in the line of their human or animal ancestry, and that recurred in their own developmental history, failed to be eliminated, thus blocking the way of higher qualities, and throwing their characters out of gear and out of adjustment with their social environment. Many examples of uneliminated organs and psychic qualities are cited by the author. If the nomadic, and feral instincts generally, are not eliminated, individuals are likely to become tramps and paupers; if the predatory instincts and defective sense of property rights characteristic of all animals and savages persist, a thievish nature is the outcome; if development is arrested at the point where the sexual passions are of animal or savage strength, sexual offenders of more or less repulsive types are the result.

In some cases of arrested development there is merely temporary or permanent persistence of subcivilized or animal characteristics in otherwise healthy individuals; in others there is hypertrophy or disease of these abnormally persistent qualities; while in the most serious cases the presence of unwholesome characteristics has the effect of destroying the equilibrium of the moral nature, which is on a high plane of evolution, and peculiarly unstable.

This explanation of immorality, whether it exist in a single direction or pervade the whole nature, as the result of imperfectly eliminated psychic rudiments, is, to the best of the present reviewer's knowledge, the most plausible and scientific so far offered.

But the article also suggests means of treating immorality and immoral individuals, based upon the methods of elimination employed by Nature.

And first, much childish and youthful unruliness and even immorality is due to merely temporarily incomplete elimination of lower traits. Here there is hope. Nature is on the side of elimination, and by observing her methods wholesome moral character will be evolved.

Secondly, childhood and adolescence are periods upon which the stress of moral training should be placed. During the first the craving for food, uncontrolled emotions, strong egoism, etc., are to be dealt with. "The dangers of the second period center in the awakening sex-consciousness."

And coming to methods of elimination, (1) Nature never extirpates an organ or a quality. Atavistic qualities are gradually evolved,

and they must be slowly developed out of existence. Sane education is patient; extirpation is in every case emasculation. While touching on this point, Dr. Dawson scarcely does it justice. At times, by giving an undesirable quality full play, its period of evolution and dissolution is shortened. This view is based on ample evidence, and it can be held without accepting the wild oats theory.

2. But Nature makes use of starving as well as of ample feeding to reduce undesirable organs. Sometimes an organ or quality is allowed to atrophy from lack of opportunity to come into play. "Deliver us from temptation" is at times as fitting a prayer for immature children as for adults. The truth is that the two complementary methods of feeding and starving no-longer-useful characteristics are employed with equal effect by nature, and both should be employed in education. Unfortunately, at present, tact and judgment have little aid from science in determining which method should be employed in any given case; investigation of this point is much needed. Dr. Dawson's plan is unduly simplified through neglect of the importance of giving too persistent qualities full play.

And here it is appropriate to point out that Dr. Dawson's article might well be supplemented by a discussion of one of Nature's favorite devices. In animals, and especially in man, many pairs of mutually antagonistic propensities are implanted to hold each other in check. Fear and trust in chicks, calves, and other young animals furnish the classic instance. It follows that one natural and scientific method of bringing undesirable qualities under control is to develop their antagonists. These antagonists deserve study, especially the organized group of the more reflective among them, which has the name of conscience.

3. The method of transformation of function needs little comment. The familiar pedagogical principle of emphasizing the interesting here finds its foundation. Especial emphasis is laid by the author on the fact that by this method the adolescent sex-consciousness may be healthily developed in domestic, altruistic, artistic and religious directions.

4. Finally, Dr. Dawson shows that Nature destroys unduly abnormal and harmful individuals, or at least prevents them from propagating their kind. And man has learned to remove the vicious and criminal from society, and to prevent them from perpetuating their stock. But much more thorough and effective measures are here called for, especially in the case of habitual criminals and incurables generally. Of course, Dr. Dawson is aware that this method of



elimination can be employed only under the most carefully devised safeguards.

Articles of this type will ultimately place in society's hands scientifically forged weapons with which to combat its chief inner foe.

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### ÆSTHETICS.

*The Origins of Art: A Psychological and Sociological Inquiry.*

By YRJÖ HIRN, Lecturer on Æsthetic at the University of Finland. London and New York, Macmillan & Co. 1900.

This important volume has undertaken the considerable task of bringing into some system the partial theories which, one after the other, have in recent years added to the interest if not to the lucidity of æsthetic inquiry. The genetic method, with its belief in utility-selection as an ultimate criterion of rationality, has been so prolific in hypotheses as almost to discredit itself, and this struggle of theories has brought us to a point where any attempt at unification is welcome.

Although the book before us cannot be looked upon as in any sense final, there being, as I think can be shown, too many subsidiary questions left unanswered, the method and temper of the writer ought to commend themselves to all those who realize the breadth and depth of the æsthetic problem. For, in the first place, Hirn does not hesitate to face the problem of the fundamental and underived nature of the art impulse, and he insists that "even if all other hypotheses be banished, æsthetic research cannot possibly dispense with the fundamental assumption of the unity of art." Although art in its beginnings is not wholly 'autotelic' and disinterested, its present nature requires an explanation which shall relegate particular utility conceptions to a secondary rôle and find in some intrinsic function of the psycho-physical organism its unitary origin.

This unitary origin of the art impulse is to be found in emotional expression—on the one hand for the enhancement of pleasure and on the other for the relief of pain. This expression is in turn conditioned and modified by subordinate social utilities, such as the motives of information, sexual and religious propitiation and stimulation, and magical efficiency through the production of illusion (p. 301).

The revival of the idea that art is essentially expression, bringing the author in line with the best idealistic traditions as Mr. Bosanquet has shown them to us, is in no sense, however, a return to what some

would call unprofitable things. Careful study makes it clear that the method is entirely 'von Unten nach Oben' and to understand what this expression of emotions rather than ideas is, we are led through a careful psychological study.

The fundamental law of hedonic expression, that all pleasurable experience tends to express itself in movements of expansion while painful experience has corresponding movements of withdrawal and inhibition, may be called the primary law of expression. Except in certain cases, where variation can be easily explained, this law holds good of the most complex emotions, as well as of simple algedonic states, whatever our theory of emotion may be. And the weight of evidence is, moreover, in favor of the theory that not only does pleasure thus become associated with movement, but that, within limits, movement is itself a source of pleasure. To this primary law of functional enhancement and arrest must be added therefore the secondary form of motor expression, namely, a tendency to automatically increase pleasure by motor function and to relieve pain by the pleasurable functioning of the expression of painful states. This distinction between primary and secondary expression of the emotions is fundamental for Hirn's theory of art, a distinction which, as he well says, has the merit of alone explaining the pleasure gotten out of painful emotions, a phenomenon which has made itself striking even in social manifestations, such as the crying feasts of the Maoris and the ceremonial wailings of ancient Greece—a distinction, too, which alone makes explicable the contradictory statements of James that sorrow is increased by sobbing and in another place that 'dry and shrunken sorrow is more painful than any crying fit.' In a brilliant chapter Hirn has also made clear the function of apparently pathological and painful motor expression as a secondary form of expression for the relief of more deep-seated and painful inhibition.

Out of such fundamental laws of the psychophysical organism emerges an automatic and autotelic activity of self-expression, upon which the secondary laws of selection can act. But, to quote the author's own words, "as against the 'Spieltrieb' theories it was objected that play never develops of itself into art, so it may now be objected, with as much reason, that all immediate or secondary emotional expressions, however interesting they may be, give us no information on artistic manifestations." And it is indeed a question why formless expression should not have sufficed for algedonic enhancement and relief; why order and form, the very essence of the æsthetic, should necessarily have appeared. It is to my mind doubtful whether the

author's answer to the question which he himself has raised, is adequate. No new principle is necessary, he insists, for we need only apply these same laws found valid in the emotional life of the individual, to the larger emotional life of the race. Sympathetic imitation is the medium for the communication of feeling. These widening 'somatic resonances' from individual to individual tend, in the first place to the enhancement of emotion, and thus further the ends of expression, and in the second place to the objectification and exteriorization of the emotion in such forms as make it socially communicable, or facilitate coöperation in emotional expression. We may agree with him as to the importance of sympathetic imitation, both in its 'outer' and 'inner' form for all kinds of social objectifications. It is probable that the first form of objectification in art is the expression of 'objectless feeling' in the three logically most primordial arts or lyrical forms, gymnastic dance (ordered movement of the body), geometric ornament (ordered movement of the eyes) and simple rythmic music or singing, without melody. No less primordial, doubtless, is the mimetic expression which slips into the earliest forms of dancing and singing, thus adding to moods the particular emotional nuances, which in time find their way into the formative arts. All this we may grant, the essential contagiousness of rhythm and its function in furthering social cohesion and expression, as brought out in the researches of Wallaschek and Bücher—and yet we may well ask ourselves at this point the question whether the sociological conceptions here introduced are sufficient to bridge over the chasm between mere unæsthetic movement and the ordered expression of art. If there is in the individual consciousness nothing to explain why mere instinctive movements did not suffice, it is not clear how social imitation has answered the question more satisfactorily than the play impulse of Groos, acting upon instinctive movements.

In fact, the whole question of the nature of imitative transmission, its mechanism, the material which it transmits (witness the divergence of opinion of Tarde and Baldwin as to whether ideas or emotions are imitated), indubitable as the facts themselves may be, is so uncertain as to make the principle singularly unsuitable, of itself, to explain these phenomena. It is probable that the secret of the passage of merely impulsive expressional movement into purposive artistic expression is not to be disclosed by this sociological principle, but must be found rather in a closer study of the æsthetic attention, in the 'Verinnerlichung' of motor impulses of which Lange and Groos have made so much. Hirn indeed seems to recognize this, for he remarks: "When a savage



has attained so high a state of development as to be able to control the impulse to dance and yell for joy the first dithyramb has been composed." Just what this state is, when the selective attention turns from the object itself to an inner imitation and coördination of its own reactions, just what its significance is in the volitional and emotional life, is a question that still remains unanswered. Some insight into the phenomenon itself Hirn has certainly given us by his beautiful illustration from the Bacchic Candelabra in the Louvre, where we see, in the same orgiastic dance, some of the dancers still seeking relief in violent unæsthetic movements, while others have found it in the gentle rythmic motion of the æsthetic dance.

For the final test of the theory of emotional expression Hirn leaves the genetic and historical method and the psychological analysis of the art impulse for an examination of the 'Work of Art' itself. The traditional origin of classical art in the Dionysic festivals and an introspective study of the creative impulse of to-day would seem to give an easy answer to our problem. But in view of the historical uncertainty of the former, probable as it is from other points of view, and the difficulty of getting definite and credible results in regard to the latter (although in this connection it seems curious that, after mentioning Ribot and Paulhan in his bibliography, the author makes absolutely no use of their interesting researches in a direction which would tend to support his own theory), it seems better to turn to an analysis of the concrete work of art itself.

With Hirn's thesis in regard to the principle of unity in concrete works of art the modern critical sense doubtless agrees. It is emotional rather than intellectual, and the *émotion fixe*, to use Ribot's term, is the dominating and selective principle in both creation and appreciation of most works of art. The bold intellectualism of Hegel, Vischer and Taine and the scientific method of the formalists both fail to satisfy the modern æsthetic sense. "The desire to fix a passing emotional state—in order to facilitate either the revival of the same state or its transmission to others—by the help of intellectual elements"; 'to avoid the transience of feeling' is undoubtedly the *raison d'être* of most artistic effort, as Ribot has learned from his artist questionnaire; and Hirn has made a point of considerable importance when he shows that, not only throughout the range of art itself, but even in the æsthetic appreciation of nature, it is emotion that selects the focal point and makes all other elements subordinate to it. And yet, in spite of the author's fine feeling for artistic values and his critical faculty which makes us realize the truth of his general

point of view, the chapter as a whole remains unconvincing. There is still a *desideratum* much to be desired, from the scientific point of view. What is an emotion detached, objectified? What is the mechanism of subsumption of subsidiary ideas and emotions under the emotional abstract or mood? Is there 'revival' and 'transmission' of an emotional state as such? These problems of 'emotional memory' and of a '*logique émotionnelle*' are involved in every line of the chapter under discussion, as they must underlie every theory of art or emotional expression. The facts of artistic expression may require these notions, and the present writer is inclined to think they do; but, in any case, their position calls for discussion at the present time.

The second portion of the volume, which discusses the concrete origins of art and the effect of secondary non-æsthetic motives upon its development, is an unusually good illustration of the proper use of the data of ethnology. Although it is doubtful whether any but experts should pronounce judgment in this field, yet to the careful reader its main contentions seem to have been made out with more than usual probability. The skillful use of the facts of primitive art to relegate these utility factors to their proper secondary rôle is a distinct contribution to æsthetic theory and ought to clear the air considerably. "Just as the play impulse is a concrete source of art and the chief classes of art forms are already foreshadowed in the various forms of playful experimentations, and as this simple activity furnishes sufficient bulk of raw material of which the art impulse when once developed can then avail itself," so the utility factors can furnish material for the art impulse, but are not its origin.

Especially important are the chapters which deal with the relation of art to sexual selection and propitiation. It is shown that the Darwinian conception of æsthetic attention and judgment on the part of the female (to be sure only guardedly suggested by Darwin but raised to a principle by his successors) is but a round-about way of explaining the facts of sexual selection. Not only can the supposed æsthetic factors, which have developed in the male of animals, be explained as merely secondary sex and species characteristics, but even in primitive man the means resorted to 'to attract by pleasing,' self decoration, for instance, may be genetically explained, without any reference to æsthetic judgment, as secondary sexual and tribal marks, having direct stimulating power. The difficulties in the way of reading æsthetic judgment into animal consciousness, which Groos has also seen and called attention to in his '*Spiele der Thiere*,' are no greater than those involved in connecting the æsthetic judgment in primitive men too

closely with sexual selection. It is probable that the Darwinian emphasis upon the erotic origin of art applies not so much to primitive man as to the special social conditions of barbaric peoples. In any case, striking as the influence of sex upon art development has been, Hirn seems to be justified in considering that influence much overestimated, and to explain all art by the principle is impossible in the light of the results of later ethnological investigation.

In his effort to disclose all the non-æsthetic and secondary motives of art the author has come upon an important connection of art with life—through the belief in magic. Here is at least a suggestion for the genetic explanation of the baffling ‘moment’ of illusion in all art. Psychopathic magic, resting as it does upon the idea that the material contiguity and even similarity of the fetish to the object of desire gives power over the real object itself, has, doubtless, through the exteriorization of the emotional and volitional values of the original, been the source of much of the reality feeling that envelops primitive art. It is, however, Hirn points out, only the principle of similarity in magic that has had any real influence upon art. It is significant, too, that the ‘volts,’ *i. e.*, the dolls and drawings used by primitive peoples to bewitch, are not in any realistic sense similar, nor indeed are they intended to be, for the vaguest resemblance seems to suffice. This vague similarity seems rather to suggest a fantastic, but still natural, belief of the primitive mind in the invisible connection of things similar. Illusion in its full sense is neither expressed nor sought for. One fact about volts, idols and magical symbols in general, extremely favorable to Hirn’s emotional theory of art, and which brings the illusion of magic into close relation with that of art, the author fails to bring out, *viz.* : that however unlike the original they may be they emphasize some emotionally important aspect of the thing or person, which single aspect, just because of its singularity, attracts the attention in a semi-hypnotic fashion and makes it possible to spread the emotion of reality over the entire object no matter how crude it may be. In fact, too much detail here or in the higher developments of art may disturb the illusion.

One is tempted to say in this connection that the passage from the illusion of magic to the conscious self-illusion of art is just such a problem as faced the thinker in an earlier portion of the work in connection with the derivation of purposive ordered emotional expression from mere emotional reaction, and it is only just to the author to say that he does not profess to have given more than a suggestion of kinship. His purpose was, in fact, merely to show how the unæsthetic



motive of a common belief in magical efficiency may have contributed to the development of the 'illusion' aspect of æsthetic expression.

In concluding our study of what is in many respects an important book, it is worth while to emphasize again the critical care and insight with which the author has sought to separate the æsthetic from the non-æsthetic motives in art. It should be noted, too, that the insufficiencies in the treatment of the main problem which it seemed necessary to point out, are just such insufficiencies as are incident to a method which distinguishes too sharply between the problems of 'origin' and 'meaning.' Unless the present meaning of æsthetic experience be taken as a starting point in the investigation of origins, it is doubtful whether one will find more than vague 'kinships,' to use the author's own expression, between primitive forms of emotional reaction and specialized æsthetic experience. Hirn's problem is one of origins, and his theory of the genesis of art, to the extent that it does justice to the problem of art's meaning, has overcome the one-sidedness and fragmentary nature of preceding theories. On the other hand, those who cling to the purely causal point of view may decide that he concerns himself too much with 'meanings.' It is hard to serve two masters!

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### EXPERIMENTAL.

*L'acuité stéréoscopique.* B. BOURDON. *Revue Philosophique*, January, 1900. Pp. 74-78.

Helmholtz's experiments on the accuracy of the binocular perception of depth led him to the conclusion that the just noticeable difference is about equal to the limen of distance between two images in monocular vision—that is, in the neighborhood of sixty seconds. Stratton's work with the pseudoscope indicates a considerably greater degree of accuracy in the depth perception. M. Bourdon here gives an account of some experiments with the Helmholtz needle method, where, however, the movements of the middle needle were not watched by the subject, who judged of its position each time after it had been brought to rest—a better method, because 'the perception of continuous change is always less delicate than that of discontinuous changes.' The results show that the just noticeable difference is very much smaller than that obtained in Stratton's experiments, the minimum being an angle of about five seconds. The author suggests that the correlate of this value in monocular vision is not the just noticeable distance between two images, but the least perceptible difference in

the size of two images. What corresponds to the former in binocular vision is rather the least distance apart at which double images can be observed. Further experiments by M. Bourdon show this distance to be approximately 100", which he considers sufficiently close to the 60-90" representing the limen of distance between two images in monocular vision.

*L'asymétrie sensorielle olfactive.* E. TOULOUSE and N. VASCHIDE. *Revue Philosophique*, February, 1900. Pp. 176-186.

The absolute limen for the smell of camphorated water was determined for a large number of subjects, each nostril being tested separately. Out of sixty-four persons, fifty-six gave the limina both of sensation (recognition of a smell in general) and perception (recognition of camphor) lower for the left nostril. In explanation, the authors point to the probability that there is no crossing of the fibers of the olfactory nerve, the connection of the more important fibers at least being with the hemisphere on the same side of the body. Thus a superiority of the left nostril in smell would be due to the same cause that produces superiority of the right side in other senses. Ferrier, and especially Collet, are quoted in support of the hypothesis that there is no decussation of the olfactory fibers. Further, along with superiority of the left nostril for smell, there was a superiority of the right nostril for touch sensations, tested by solutions of ammonia, in the fifteen persons examined to decide the question. And the subjects who showed asymmetry in favor of the right nostril for sensations of smell were all either left-handed or ambidextrous.

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*Aspects of Mental Economy.* An essay in some phases of the dynamics of mind, with particular observations upon student life in the University of Wisconsin. By M. V. O'SHEA. *Bulletin of the University of Wisconsin*, No. 36. April, 1900.

Professor O'Shea gives in this Bulletin the results of an investigation into the dietaries and methods of life of the students of the University of Wisconsin. The author claims that the primary object in asking the students to report was to direct their attention to these matters. The author enters, however, a wider area of discussion when he enters the subject of mental economy in general. He pleads for an 'energetic' conception and treatment of the relation of mind and body. The essay is largely made up of citations from different authors on matters of nutrition. The relative values of foods in the

production of nervous energy, the preparation of foods, hours for meals, individual peculiarities in digestive capacities, expense of dietaries, exercise, sleep, muscular and nervous waste, etc., are some of the chief topics treated. The style and often the subject-matter are quite popular in their nature. Perhaps they were so intended. This may explain the hortatory nature of some of the concluding remarks. Such phrases as, "Tell me what company you keep and I will tell you what you are," and "The M.D.'s have not yet dealt largely with the subject of nutrition for healthy beings," etc., are probably meant for popular distribution. The work is valuable for the information it contains.

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### PHYSIOLOGICAL.

*A Note on the Significance of the Small Volume of the Nerve Cell Bodies in the Cerebral Cortex in Man.* HENRY H. DONALDSON. *Journal of Comparative Neurology*, Vol. IX., No. 2. 1899. Pp. 141-149.

The substance of this paper by Professor Donaldson, of Chicago University, may perhaps be given in the words of his own Summary:

"(1) The weight of all the nerve cells in the human encephalon is less than 27 grams [although in number many more than 9,200,000,000]. (2) When comparison is made of human encephala grouped according to race, sex, mental power, stature and age, the differences in weight, within each group, are always more than twice that of the nerve cell bodies, and hence these differences depend mainly on variations in the medullary substance. (3) Small variations in the mass of the nerve cell bodies (though physiologically highly important) escape detection by the method of weighing, or may be masked by the greater growth of the medullary substance."

Certain other considerations proposed by the author in the course of his paper have psychological interest, which perhaps warrants their repetition here: "We infer from these observations, and from the fact that the dendritic branches of the cortical cells become more numerous as we ascend in the vertebrate series, that the principal means of increasing the *physiological complexity* of the cortex is represented by the dendrons of the cortical cells, together with the associated terminals surrounding them. As those portions appear to furnish the structural basis necessary to the exhibition of intelligence, variations in their number and extent may be fairly correlated with the variations in mental power. \* \* \* The neurone may be resolved into



three portions. First, the receiving portion; second, the conducting; third, the transmitting. These correspond, first, to the cell body and its dendrons; second, to the axone; and third, to the ends of the branches of the terminals. By means of the first portion, the neurone is rendered responsive to nerve impulses brought into its neighborhood; by means of the second, impulses arising in the cell body are conducted to the ends of the axone; and by means of the third, the terminals, these impulses stimulate other neurones. It is therefore on the dendrons and the terminals, *i. e.*, the receiving and transmitting portions of the neurone, that the complexity of the encephalon is dependent." From this paper it may be seen why it is that the brain of an adult often weighs no more than it weighed when only six years old.

*A Plethysmographic Study of the Vascular Conditions during Hypnotic Sleep.* By E. C. WALDEN. Amer. Jour. of Physiol., Vol. IV., No. III. July 1, 1900. Pp. 124-161.

The object of this research was, simply, to determine if the arm's volume increases from suggestion and during hypnosis as it does during normal sleep. There are reported the results of twenty-five experiments on hypnotic sleep and a like number on suggestion. The instruments employed were Howell's plethysmograph with Bowditch's recorder and a modification of Masso's sphygmomanometer—the latter for recording the blood-pressure.

There was found regularly a comparatively brief and sudden diminution in the circumference due to constriction of the peripheral blood vessels. When suggestion ended the arm expanded from 0.2 to 10.0 cc., lasting from 1 minute to 2 hours. After slowly expanding thus the arm gradually constricted up to the end of the hypnosis, sometimes 3 cc. At the instant of awakening a sudden but brief constriction occurred, 'due to the action of mental and sensory stimuli upon vasomotor centers' during the process of awakening; relaxation to normal then rapidly occurred. Just after the suggestion there was a fall in arterial pressure in the fingers of about 7 mm. of mercury. The pulse-rate was slower during hypnosis, especially at first, followed after waking by an abnormal rapidity, which continued 15-20 minutes. The breath-rate likewise was slower, especially at first, lasting several hours; at times it was above normal. There was a steady, although very slight, fall of rectal temperature during the hypnotic sleep, followed by a maximum rise of 0.4°. The surface temperature of the arms was higher during hypnosis than at the experiment's beginning.

It gradually rose during the first hour about  $0.6^{\circ}$ , then gradually fell about  $0.8^{\circ}$ . Suggestion during the waking state caused a decrease in the volume of the arm, as the plethysmographic tracing demonstrated; this was followed by an increase. 'A pronounced and increasing vaso-constriction in the arm during most of the sleep-period' was then the most positive and suggestive result obtained.

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### PATHOLOGICAL.

*Les causes psychologiques de l'aboulie.* BARON C. MOURRE. Rev. Phil., L., 3. September, 1900. Pp. 276-285.

M. Mourre considers aboulia the inability to transform an idea into its act as the consequence or product of victory among antagonistic ideas. The aboulic person is conscious that she wishes to do a thing, and at the same time quite aware that she does not do it. Opposed to the motor idea is always its inhibitory opposite, and it is this latter tendency that triumphs. Why it triumphs the greater part of this brief article attempts to explain.

The causes of 'psychic paralyses' the author deems of two sorts. Some of them arise in ideas of acts different from those intended by the agent. Others, on the other hand, arise in the fear of the subject that she will be 'paralyzed' if she attempts the act. Of the latter class, in part, are cases of aboulia, the patient realizing and fearing her infirmity. In true aboulia the agent considers her will free, and she fails to act because she chooses not to act, on account of her fear of failure. But this association by contrast is not the sole nor primary cause of aboulia. There is, second, a diminution of the sensations and of the desires, making the pleasurable motogenic balance smaller than in normal organisms. The causes of this diminution, or even loss, of desires to action are almost always contrarieties, chagrins which occasion a profound moral depression, making life distasteful. This state is by no means incompatible with an excess of emotional activity, as is frequently experienced by those who have to do with this class of mental derangement. But effort seems useless, because its usual rewards are absent.

"The psychological cause of aboulia will be then the difficulty of voluntary effort, and the physiological cause an organic trouble of the brain of which the localization is unknown." Although no novelty, such an explanation has a certain usefulness and probably much of the truth in it so far as it goes.

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

## NEW BOOKS.

*Report of the U. S. Commissioner of Education, 1898-99.* Vol. II. Washington, Gov. Printing Office. 1900. Pp. viii + 1249-2578.

*Proceedings of the American Association for the Advancement of Science, New York Meeting, June, 1900.* Easton, Pa. 1900. Pp. xcv + 409.

*Socrate.* C. PIAT. Grands philosophes series. Paris, Alcan. 1900. Pp. 270. Fr. 5.

*Saint Augustin.* L'Abbé J. MARTIN. Grand philosophes. Paris, Alcan. 1901. Pp. xvi + 403. Fr. 5.

*Hume's Enquiry Concerning the Principles of Morals.* Religion of Science Library. Chicago, Open Court Publ. Co. 1900. Pp. 169.

*Kant.* TH. RUYSEN. Grands philosophes. Paris, Alcan. 1901. Fr. 5.

*La Foule criminelle.* S. SIGHELE. 2<sup>me</sup> Éd. Paris, Alcan. 1901. Pp. 300. Fr. 5.

In this new edition of the French translation of Sighele's well-known and influential book the author protests against the use made of his work without credit by certain authors, notably by M. Le Bon. In this he should have the sympathy of his readers; for the case is a flagrant one: Le Bon has had circulation and citation in English without reference to the work of Sighele from which he drew much of his material. The present work is revised and enlarged. J. M. B.

*Psychologie appliquée à la morale et à l'éducation.* F. RAUH and G. REVAULT D'ALLOUNES. Paris, Hachette. 1900. Pp. 320.

A text-book for use in the *enseignement secondaire des jeunes filles*. As we should expect from the pen of M. Rauh it utilizes the latest results. J. M. B.

*Un Siècle: mouvement du monde de 1800 à 1900.* By a committee: President, Monseigneur Péchenard. Paris, Ouden. No date. (5<sup>me</sup> mille.)

A review of all the intellectual movements of this century from a Catholic point of view.

*Le vocabulaire philosophique.* E. GOBLET. Paris, Armand Colin. 1901. Pp. xiii + 489.

*L'organisation de la science.* L. FAVRE. Paris, Schleicher Frères. 1900. Pp. lx + 409.



- La Folie: ses causes, sa thérapeutique au point de vue psychique.* TH. DAREL. Geneva, M. Reymond et Cie.; Paris, F. Alcan. 1901. Pp. 196.
- La musique des couleurs.* L. FAVRE. Paris, Schleicher Frères. 1900. Pp. xiv + 113.
- The Human Nature Club.* EDWARD THORNDIKE. New York, London and Bombay, Longmans, Green & Co. 1901. Pp. viii + 235.
- Problems of Evolution.* F. W. HEADLEY. New York, T. Y. Crowell & Co. 1901. Pp. xvi + 373.
- Le problème de la vie.* Essai de sociologie générale. LOUIS BOURDEAU. Paris, Alcan. 1901. Pp. xii + 372.
- Essai critique sur le droit d'affirmer.* ALBERT LECLÈRE. Paris, Alcan. 1901. Pp. 264.
- Experimental Psychology: A Manual of Laboratory Practice.* EDWARD BRADFORD TITCHENER. Vol. I., Qualitative Experiments. Part I: Student's Manual. New York, The Macmillan Co. 1901. Pp. xx + 214. Price, \$1.60.
- Wörterbuch der philosophischen Begriffe und Ausdrücke.* DR. R. EISLER. 3, 4, 7 u. 8. Lieferungen. Berlin, E. S. Mittler u. Sohn. 1899-1900. Pp. 193-288, 289-384, 705-956.

## NOTES.

IT gives us much gratification to announce that Professor William James has completed his first course of Gifford lectures and intends to read them at Edinburgh University in May. He has been in England during April, much improved in health.

PROFESSOR J. MARK BALDWIN has gone abroad (sailing April 6) in the interest of his *Dictionary of Philosophy and Psychology* and other literary undertakings. He and his family remain in Italy until about June, and summer in Switzerland. (Constant address: Care Controller, University Press, Oxford, England.) He and Professor Wenley of Michigan are among the delegates from the United States to the Ninth Jubilee of Glasgow University.

PROFESSOR R. B. JOHNSON, of Miami University, has been called to Ohio State University, at Columbus. He will enter upon his duties there in the autumn.

WE are requested to state that, owing to continued dissatisfaction with the administration of the Department, the following professors of the Faculty of the School of Pedagogy of New York University announce their resignation from the University: Samuel Weir, professor of history of education and ethics; Edward F. Buchner, professor of analytical psychology, and secretary of the faculty; Charles H. Judd, professor of experimental psychology.

# THE PSYCHOLOGICAL REVIEW.

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## A PROVISIONAL DISTRIBUTION OF THE POPULATION OF THE UNITED STATES INTO PSYCHOLOGICAL CLASSES.

BY PROFESSOR FRANKLIN H. GIDDINGS,  
*Columbia University.*

Descriptive sociology has arrived at a stage at which it seems necessary to attempt to make a distribution of the population of any nation or community into psychological classes. We know, for example, that in the population of the United States are some tens of thousands of instinctive, animal-like creatures, passionate and violent; that thousands of others are imaginative, weakly but persistently emotional, and easily influenced by suggestion; that yet others are more or less fanatical, speculative, devoted to 'causes,' 'reforms,' and so on, without end; and that, finally, some are critical, calculating, inductive, scientific. We know, further, that concerted volition of every description takes its character from the proportions in which these different psychological classes are combined. One combination makes the mob or the lynching party, another combination makes the deliberative assembly; one combination makes one sort of political policy and scheme of legislation, another combination makes a different policy and a different legislation. Great masses of statistics will acquire significance if the distribution of a population into psychological classes can be made. If the distribution is impossible, the statistics must remain meaningless.

The analysis of mental phenomena into motor, affective (or emotional) and intellective aspects, suggests a threefold group-



ing. Other considerations, however, suggest a fourfold grouping, corresponding to the commonly recognized four temperaments. Such a grouping may be found in the writings of many scientific students of social psychological phenomena; for example, in Dr. Starbuck's book on 'The Psychology of Religion,' and in Dr. George A. Coe's study of 'The Spiritual Life.' A more scientific grouping than either of the foregoing may be found, I think, in the six possible arrangements (in order of predominance and subordination) of the three fundamental modes of mental phenomena. Designating each of the three by a letter, namely, Motor reactions by M, Feeling (affection or emotion) by E, and the Intellective aspect by I, we have the following six possibilities:

MEI	EMI	IME
MIE	EIM	IEM.

Of these six arrangements two, in which intellect holds the third place, namely, MEI and EMI are found only among animals, human babies and defectives. Among normal human adults intellect moves forward to the second or the first place, and we have, therefore, four mental types of normal human beings of adult age, namely, MIE, EIM, IEM, and IME.

Starting, then, with these four arrangements, I have grouped under each of them certain psychological characteristics that, so far as my observation has extended, are *usually* combined in the same personality or class of persons. I emphasize the word 'usually.' In the scheme herewith presented there are thirty-six items of psychological importance, distributed into four groups of nine items each. No item can be combined with another item in the same horizontal line. Making allowance for this limitation, there remain 2,665,797,300,224 ways in which the thirty-six items can be combined in a scheme of four columns of nine items each. It is therefore conceivable that no two individuals can be found in the world sufficiently alike to constitute a psychological class. Such a conception, however, is negatived by everyday observation. Certain combinations of traits often recur, and other, mathematically possible, combinations never occur. It is the usual combinations only that

are of fundamental importance for psychology and for sociology. Here, then, is the scheme :

TABLE I.

Type of Mind.		MIE. Ideo-Motor.	EIM. Ideo-Emotional.	DEM. Dogmatic- Emotional.	IME. Critical- Intellectual.
Usual Characteristics.	Promptness of reaction.	Prompt ( $R_1$ ).	Prompt ( $R_2$ ).	Slow ( $R_3$ ).	Slow ( $R_4$ ).
	Continuity of activity.	Persistent ( $A_1$ ).	Intermittent ( $A_2$ ).	Intermittent ( $A_3$ ).	Persistent ( $A_4$ ).
	Kind of movement.	Largely involuntary (instinctive).	Semi-involuntary (imitative, sympathetic).	Largely voluntary (Coefficient of Belief).	Largely voluntary (Coefficient of Judgment).
	Degree of emotion.	Strong ( $E_1$ ).	Weak ( $E_3$ ).	Strong ( $E_2$ ).	Weak ( $E_4$ ).
	Temperament.	Choleric.	Sanguine.	Melancholic.	Phlegmatic.
	Formation of belief or judgment.	Subjectively determined (by instinct, habit and auto-suggestion).	Objectively determined (by external suggestion : personal or impersonal).	Subjectively determined (by emotion, mood, temperament).	Objectively determined (by evidence).
	Mode of reasoning.	Conjectural (guesswork).	Imaginative (analogical).	Deductive, (speculative) May be critical of logical processes, rarely of premises.	Critical, of premises as well as of logic : inductive.
	Disposition. Character.	Aggressive. Forceful.	Investigative. Convivial.	Domineering. Austere.	Creative. Rationally conscientious.

Having regard to all the characteristics of each group I give descriptive names to the resulting type of mind ; calling the first Ideo-Motor, the second Ideo-Emotional, the third Dogmatic-Emotional, and the fourth Critical-Intellectual.

Most of the items entered in the scheme need no explanation. Two or three, however, may possibly have some slight novelty and call for an explanatory word.

First among these are the discriminations made of the different modes of reasoning. Of four intellectual modes the first is called ' conjectural ' and the interpretative word ' guesswork ' is inserted. I have long felt that the transition from instinct to reason has been inadequately explained by even the best of psychologists, who have devoted years of patient research to the subject. Neither Darwin nor Romanes, neither Lloyd Morgan

nor James, neither Henry Rutgers Marshall nor any other writer, has given due attention to the 'wobbling' state of mind that intervenes between instinct at the point of breaking down, and rational discrimination. Both introspection and observation will, I think, convince any student that there is a good deal more here than 'the contrary impulses' so illuminatingly described by James, 'Psychology,' Vol. II., pp. 389-393, and the 'hesitation' mentioned by Marshall, 'Instinct and Reason,' p. 417 *et seq.* The mind begins to try the possible ways of going wrong. It hits out at random, it conjectures, it guesses. Guesswork, then, I am inclined to regard as the first constructive stage of reasoning, as distinguished from a purely negative 'hesitation,' and a conjectural mode of intellection as, on the whole, characteristic of the man who combines prompt responses to stimulus, persistent activity, strong emotion, choleric temper, and an aggressive disposition. At any rate, I provisionally insert it in my scheme, and express the hope that psychologists of competent training will investigate more fully than has ever yet been done the 'guesswork' stage of thinking.

The second stage in the development of reasoning, following close upon the conjectural, is seen, I think, in that process which we call reasoning from analogy. The mind begins to form conclusions based upon observations of superficial likeness. Imagination is an important coefficient of reasoning at this stage, and the reasoning is almost as likely to follow the psychological laws of the blending of mental images, as to obey the laws of logic.

Reason arrives at a third stage of its development when the mind grasps the difference between mere analogy and necessary implication. Deductive reasoning now begins. But, having acquired the art of logic, the mind is so enamoured of its new power that, for a long time, it concentrates its attention almost entirely upon the formal steps of the reasoning process, to the entire neglect of any searching criticism of the premises upon which the whole superstructure rests. Such, for example, was the thinking of the Mediaeval Schoolmen. The premises of the most pretentious system of thought may be utterly childish beliefs that have acquired sacredness through mere age. The in-



tellectual mode of minds in this stage of reasoning is therefore speculative and dogmatic, rather than scientific.

The highest development of reason is attained when the mind presently transfers a measure of attention from the reasoning process to its premises, and begins to subject them, as well as the successive steps of formal logic, to a rigorous criticism. Inductive methods now begin largely to supplement or to displace *a priori* methods. The mind becomes scientific.

The remaining discriminations in my scheme that possibly call for explanation are those relating to the formation of belief. The serious discussion which has been conducted by Miller, Marshall and others over the doctrine set forth in James' 'The Will to Believe' has its ground, I think, in a fundamental difference between two modes of determination of both choice and voluntary movement. There is a type of man who undoubtedly is cock sure that he 'believes as he's a mind to.' He *feels* that his belief and his choice are shaped within himself and by himself, and the psychologist, analyzing him, is unable to doubt that he is quite right in this conviction. Inherited instinct, emotion, mood, temperament, are all discoverable in his volition; and not only in his volition, but also in his hobbies, and pet isms. Another sort of man *knows* that his judgment, conviction, conclusion, has been *objectively* determined—by evidence critically weighed. The evidence has perhaps upset the hypothesis which, for half a life-time, he has been hoping to demonstrate. Nevertheless, he accepts the evidence and changes his theory. These two types of men of course fall into different groups in the foregoing scheme.

May the minds that are neither critical nor strongly dogmatic in like manner be divided into two classes, namely, those whose beliefs are on the whole *subjectively*, and those whose beliefs are on the whole *objectively* determined? Such a division is here assumed to be a natural and important one, and, accordingly, in the recognition of suggestion as a belief-making factor, objective or external suggestion is discriminated from subjective or auto-suggestion. Objective suggestion, in turn, is marked as proceeding from a personal or as proceeding from an impersonal agent.

From the sociologist's point of view these distinctions are necessary and significant. Looking at men in masses, we commonly see those who are subject to auto-suggestion acting, on the whole, with the dogmatists and the martyrs, and those who are susceptible to impersonal objective suggestion acting on the whole with the explorers, the inventors, and the scientific investigators. In a word, reviled 'suggestibility' is not always and uniformly a bad thing, and there is much reason to think that one sort of suggestibility is genetically antecedent to the scientific temper.

Supporting these views is the fact that the present drift of opinion among students of folklore, mythology, and comparative religion, is towards the conclusion that, from the days of primitive man onwards, there have always contended in society two types of mind, in one of which belief is subjectively determined, and in the other of which belief is objectively determined, and that the struggle, therefore, between the priests and the prophets in the development of religion, and between dogmatic supernaturalism and naturalistic science, began with the very dawn of human reason. Frazer, in the new edition of 'The Golden Bough,' has clearly explained how, in the primitive mind, the thought of imitative and sympathetic magic, although not dissociated from animism, was yet different from spiritism. The doctrine of magic was an affirmation that natural law of some kind runs through the totality of natural objects and binds the world of objective things together. Crude as it was, magic was the beginning of a natural philosophy. The magician and the ghost-worshipper did not clash at first, but they were bound to clash at last. Greek naturalism and Semitic supernaturalism are not mere historical episodes, nor even mere cultural differentiations. They are fundamental psychological distinctions. This thought may be found further developed in an instructive article on 'The Law of Historical Intellectual Development,' by J. S. Stuart-Glennie, in the *International Monthly*, Vol. III., No. 4, April, 1901.

On the basis of the classification above presented I have made a provisional distribution of the population of the United States. My procedure was as follows:

I first divided the native born whites of native parents into approximately homogeneous groups, by states and census divisions. These appear in the left hand space of the following tables. In the same space I entered the various nationalities of foreign born and native born of foreign parents, and the colored.

Taking, then, in order the successive divisions of native whites, foreign whites, and colored, I tried to determine from objective indications the mental qualities of each division. The manifestations of type of mind that appear in religion I purposely left out of consideration. There remained as important indications the manifestations in politics, in law and order, in occupations, and in literature.

These indications are more positive and exact than one might on first thought suppose. Take, for example, the indications found in literature. The literature produced by New England native whites has been from the earliest time chiefly dogmatic and polemical, secondarily critical, and only in a third degree imaginative and emotional, since even the poetry and other *belles lettres* products of New England have always had a strong dash of the critical and moralizing element. The New England white civilization has never produced the original materials of a folklore or of an epic, and her legendary literature is neither abundant nor richly varied in form. On the other hand, the literature proceeding from authors of Irish nationality is chiefly imaginative and emotional, secondarily polemical, and only in a very slight degree critical and scientific. Estimated by the same objective standard the Scotch are on the whole like the New Englanders, while the English, French, Scandinavians, and Germans are intermediate between the New Englanders and the Scotch on the one hand and the Irish, the Italians, and other Celts and Latins on the other hand.

The determination of mental type on the basis of literary indications may not perfectly correspond to the determination on the basis of politics and law, and a compromising judgment must then be made. Thus, in the population of Kansas, a large element of New England origin is found, but the chronic emotional insanity of the Kansans in their law and politics indicates a strong lean of their mental type towards the ideo-emotional.



Occupations are an excellent indication of character and of disposition. Forceful and aggressive men are found in the difficult and dangerous vocations; convivial men are found in the safe, relatively profitable and easy occupations; instigative and domineering men in politics, the priesthood, and the ministry; austere men of whatever vocation are habitually devoted to 'reforming' activity as an avocation.

TABLE II.

## DISTRIBUTION BY NATIONALITIES.

Type of Mind.		MIE.		EIM.		IEM.		IME.
		Ideo-Motor.	IM to IE.	Ideo-Emotional.	IE to DE.	Dogmatic-Emotional.	DE to CI.	Critical-Intellectual.
Native born whites of native parents.	New England.				$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$
	Mich., Iowa, Kans., Neb.			$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$	
	New York, N. J., Penn.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{8}$
	Remaining North Cent.			$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{4}$		
	Western.		$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$		
	South Atlantic.		$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{8}$	
	South Central.		$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$		
Foreign born whites and native born whites of foreign parents.	Scotch.				$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$
	English.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{8}$
	French.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{8}$
	Germans.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	
	Scandinavians.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	
	Other Teutonic.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	
	Russians.			$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{4}$		
	Welsh.		$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{4}$		
	French Canadians.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			
	Poles, Bohemians, Hung.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			
	Italians.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			
	Other Latin.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			
	Irish.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			
	All others.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			
Col.	Negroes and other col.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$			

Guided by such indications I decided to which of my four psychological groups I would on the whole assign as many as one half of all persons belonging to any given division or nationality. Having answered this question I put the fraction  $\frac{1}{2}$  in the column to which I had assigned it. Then assuming that the distribution to the right or the left of the column to which the one half had been assigned should follow a smooth

curve (that is, dropping successively to  $\frac{1}{4}$ ,  $\frac{1}{8}$ ,  $\frac{1}{16}$ , and so on) I decided whether I would put the  $\frac{1}{4}$  to the right or to the left. This done I next asked myself whether, proceeding in the same direction, I should enter  $\frac{1}{8}$ , or should rather go back to the other side of the sheet, and enter  $\frac{1}{4}$ . This method I followed until for every division of the native whites and for every foreign nationality I had distributed fractions amounting to unity. The result was the scheme given in Table II.

Following precisely the same method as that used in Table II., except that this time I regarded only the manifestations of

TABLE III.

## DISTRIBUTION BY RELIGIONS.

Type of Mind.	MIE. Ideo-Motor.	EIM. Ideo-Emotional.	IEM. Ideo-Emotional.	IME. Ideo-Emotional.	IME. Ideo-Emotional.	IME. Ideo-Emotional.
Adventists.			$\frac{1}{2}$	$\frac{1}{2}$		
Baptists, white.		$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{8}$		
Baptists, colored.		I				
Catholics.	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{4}$			
Catholic Apostolic.				I		
Christians.					I	
Christian Scientists.			$\frac{1}{2}$	$\frac{1}{2}$		
Congregationalists.					I	
Disciples of Christ.			$\frac{1}{2}$	$\frac{1}{2}$		
Dunkards.			I			
Evangelical Association.			$\frac{1}{2}$	$\frac{1}{2}$		
Friends.			I			
German Evangelical Prot.				I		
German Evangelical Synod.			$\frac{1}{4}$	$\frac{3}{4}$		
Jewish Congregations.				I		
Latter Day Saints.			$\frac{1}{2}$	$\frac{1}{2}$		
Lutherans.				I		
Mennonites.			I			
Methodists, white.		$\frac{1}{8}$	$\frac{3}{4}$	$\frac{1}{8}$		
Methodists, colored.		I				
Moravians.			I			
Presbyterians.				$\frac{3}{4}$	$\frac{1}{4}$	
Protestant Episcopalians.			$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{4}$	
Reformed.				$\frac{3}{4}$	$\frac{1}{4}$	
Salvation Army.	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{8}$		
Society for Ethical Culture.					I	
Spiritualists.			I			
Theosophical Study.				I		
Unitarians.					I	
Universalists.					I	
All Others.			I			

mental type that appear in religious preferences, I next distributed the membership of the various religious denominations of the United States as tabulated by the Eleventh Census, paying no attention, however, to the actual numbers reported as belonging to the several denominations. The resulting scheme turned out as in Table III.

This work done, I placed the two schemes in the hands of my son with instructions to obtain from the Census tables the actual numbers corresponding to the fractions in the two schemes, to enter them in adjacent columns, foot up the columns, and obtain percentages—in the nationality table on the basis of total population, in the religious denominations table on the basis of total religious population. The results are presented in Tables IV. and V.

The close correspondence of these percentages does not prove that my provisional distribution is the right one. I think it shows only that the distribution is approximately right and may be taken as a basis for further investigation. I should be glad if psychologists would examine their accumulated statistics of reaction time, emotion, mode of thinking, and so on, with reference to a possible distribution and tabulation by nationalities. Such a tabulation should check evidence that may be obtained through sociological observations and historical study.

One single conclusion may, I think, be accepted as already established. The mental 'mode' of the American people as a whole is ideo-emotional to dogmatic-emotional. This is independently indicated by the statistics of the annual output of books by American publishing houses as compiled by the *Publishers Weekly* and reproduced in Appleton's 'Annual Cyclopaedia.' Assigning the different classes—fiction, poetry, biography, theology, etc.—to their proper psychological columns, we have the results presented in Table VI. Substantially the same result, again, is independently reached by an analysis of state and national election returns.<sup>1</sup>

<sup>1</sup> See 'The Nature and Conduct of Political Majorities,' *Political Science Quarterly*, Vol. VII., No. 1, March, 1892; republished in 'Democracy and Empire,' by the present author.



TABLE IV.

[illegible]

TABLE V.

Type of Mind.	MIE.		EIM.		Ideo-Emotional to Dogmatic-Emot.		IEM.		Dogmatic-Emot. to Critical Intel.		IME. Critical-Intel.	Total Members.
	Ideo-Motor.	Ideo-Motor to Ideo-Emotional.	Ideo-Emotional.	Ideo-Emotional to Ideo-Emotional.	Ideo-Emotional to Dogmatic-Emot.	Dogmatic-Emotional.	Dogmatic-Emotional.	Dogmatic-Emot. to Critical Intel.				
Adventists.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				60,490
Baptists, white.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				2,363,478
Baptists, colored.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				1,348,989
Roman Catholics.			$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				6,257,870
Catholic Apostolic.		$\frac{1}{4}$										1,394
Christians.												103,722
Christian Scientists.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				8,724
Congregationalists.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				512,771
Disciples of Christ.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				641,050
Dunkards.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				73,795
Evangelical Assoc'n.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				133,312
Friends.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				107,208
German Evan. Prot.					$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{4}$				36,156
German Evan. Synod.					$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{4}$				187,432
Jewish Congregations.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				130,496
Latter Day Saints.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				166,124
Lutherans.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				1,231,072
Mennonites.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				41,541
Methodists, white.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				3,648,077
Methodists, colored.			$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				941,206
Moravians.					$\frac{1}{8}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				11,781
Presbyterians.					$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				1,278,232
Protestant Episcopal.					$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				540,508
Reformed.					$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				309,457
Salvation Army.		$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				8,742
Ethical Culture.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				1,064
Spiritualists.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				45,030
Theosophical Society.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				695
Unitarians.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				67,749
Universalists.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				49,194
All Others.					$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$				305,338
Totals.												20,612,806
Percentages.												100%
		7.6%	29.9%	35.8%	20.8%	6.1%						

TABLE VI.

Type of Mind.	EIM. Ideo-Emo- tional.		IEM. Dogmatic- Emotional.		IME. Critical- Intellectual.		
Class of Books.	Books that aim to please, amuse, inter- est: appeal- ing to imag- ination, emo- tion, senti- ment.		Books that aim to con- vert, influ- ence, in- struct; ap- pealing to belief, self- interest or ethical emo- tion.		Books that aim to criti- cise, or to make posi- tive inductive additions to knowledge: appealing only to criti- cal reason.		Totals.
Fiction.	I	749	I	454			749
Law.							454
Juvenile.	I	434					434
Education and Language.			I	397			397
Theology and Religion.			I	393			393
Literary History and Misc.	I	304					304
Poetry.	I	302					302
Biography and Memoirs.	I	288					288
History.	$\frac{1}{4}$	62	$\frac{1}{2}$	123	$\frac{1}{4}$	61	246
Political and Social Science.			$\frac{3}{4}$	170	$\frac{1}{4}$	56	226
Fine Arts and Illustrated B.	I	194					194
Description, Travel.	$\frac{1}{2}$	95	$\frac{1}{2}$	95			190
Physical and Mathematical.			$\frac{1}{2}$	60	I	176	176
Medical and Hygiene.			I	99	$\frac{1}{2}$	60	120
Useful Arts.			$\frac{3}{4}$	47	$\frac{1}{4}$	16	99
Mental and Moral Philos.			I	55			63
Domestic and Rural.							55
Sports and Amusements.	I	43					43
Humor and Satire.	I	26					26
Totals.		2497		1893		369	4759 <sup>1</sup>
Percentages.	52		40		8		100

<sup>1</sup> The figures are for the year 1899. They are wrongly footed in Appleton's 'Annual Cyclopædia' and given as 4749.



## ON THE VOLUNTARY CONTROL OF THE FORCE OF MOVEMENT.

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The muscle sense, if the term be used in a comprehensive way to include all sensations arising in the interior of a muscular member of the body, furnishes sensations that form the basis of a variety of perceptions. Judgments of weight and resistance, of extent of movement, of speed and force of movement, and of position, are all based in large measure on this class of sensations. There have been more or less definite attempts to reduce some of these types of perception to others, as for instance to explain the perception of the extent of movement as a derivative from the perception of the time of movement.

In another place,<sup>1</sup> I have brought together a variety of evidence all going to show that the extent of a movement is not inferred from judgments of the force and duration of the movement or from a comparison of the initial and terminal positions of the movement. These other judgments may indeed assist in certain cases. But there exists a judgment of extent, independent and in its own right.

The present paper is an attempt to examine the force of movement in a similar way. The main question is whether the force of a movement is perceived directly or inferred from a perception of extent.

Any form of apparatus in which force is applied gradually to a spring—as in a dynamometer or ergometer—serves admirably to raise this question, but not to answer it. In such an apparatus, increase in the force of movement is only to be attained by increasing the extent; and it does not appear whether the force is perceived directly or in terms of the extent.

<sup>1</sup> PSYCHOLOGICAL REVIEW, Monograph Supplement, 13; pp. 76-86.

I have, accordingly, had recourse to another form of movement, namely, a *blow*. Its advantage for our present purpose lies in the fact that, when the blow is struck on a firm object, the actual exertion of the force occurs practically in an instant of time and in a point of space. The factors of extent and duration are thus eliminated from the actual exertion of the force. On the other hand, the distance and time during which the force of the blow is *developed*, vary widely; but they can also be measured separately from the force of the blow. And when we have obtained separate but corresponding records of the force of the blow, and of the extent (and duration, if need be) of the preliminary movement or 'start,' we shall be in a position to discover any relation between the two.

The apparatus used consists, then, of two parts, one for recording the back movement that precedes the blow, and the other for recording the blow.

The preliminary movement is recorded on a rotating smoked drum by the short arm of a lever, the long arm of which is attached to the hand. The attachment consists of a light silk thread, running over pulleys above. As the hand rises to get a start for its blow, the lever falls of its own weight and so records the extent of the start; as the hand descends to strike the blow, the lever is of course raised. The lever that has been used is of wood, 56 cm. long, pivoted at the end, with the writing point one quarter of the distance out from the pivot. The movements of the hand are thus reduced to a quarter of their true extent. The weight of the lever is sufficient to make it fall promptly when the hand rises, but not enough to impede free movement.

For recording the force of the blow two devices have been used. At first an instrument was constructed in the form of a piston, with the plunger projecting upward and capped with a flat metal disc 3 cm. in diameter. The blows were struck on this disc against the resistance of a stiff spring placed in the cylinder beneath the plunger. To record the blow, a little additional cylinder was attached to the wall of the main cylinder, and in it moved a little plunger attached to the main plunger. The little cylinder and its piston were air-tight, and were con-

nected by rubber tubing with a Marey tambour writing on the same drum as the lever that recorded the movements of the hand. The drum turned at a slow rate, and simultaneous records were taken of the preliminary movement and of the force of the blow.<sup>1</sup>

This instrument, while possessing great advantages in recording a rapid series of blows, on account of the quickness of its return to the zero point, had also a disadvantage in that it was necessary to strike it very squarely in order to have the full force of the blow recorded. A simpler device that obviates this difficulty, and works well when the interval between the blows is one second or longer, consists in simply making use of the momentum of the lever above described. If, as the lever is being carried upward by the downward motion of the hand, the hand is suddenly stopped by striking a table, the lever will continue to rise by its own momentum. The height to which it will rise is proportional to the square of its velocity, or, what is the same thing, the velocity of the hand on striking the table. As the *vis viva* of the hand is also proportional to the square of its velocity, it follows that the height to which the lever rises by its momentum is directly proportional to the energy of the blow. This proportion will be modified somewhat by the friction of the writing point on the drum, but is still accurate enough for the present purpose. By this second device can be obtained records like Figs. 1 and 2, that give at a glance a record of the blow and of the start.<sup>2</sup>

The procedure was usually as follows: the person tested sat before a table of ordinary height—or before a low table on which the spring instrument was clamped—and the thread of the lever was attached to his thumb. He was directed to make with his fist a series of downward blows on the table, of no great force but as uniform as possible. Each blow was to be made the standard for the next. The interval between them was prescribed by a metronome, and was usually one-and-a-half

<sup>1</sup> This instrument was constructed for me by E. Horstmann, instrument maker to the department of psychology, Columbia University.

<sup>2</sup> The graduation of either of these blow-recorders must be made empirically, and is attended with some difficulty. I have no occasion to use the absolute values in this study.



seconds. The movement with which the blow was made consisted chiefly in extension of the elbow.

The question to be tested in the records was: Is there a direct correlation between the force of a blow and the extent of the preliminary movement? If every increase or decrease in the force is accompanied by a corresponding increase or decrease in the extent, the reasonable conclusion would be that the force is regulated by the extent.

Two facts are obvious from the outset. Large differences in the force will surely correspond with differences in the extent, since on the one hand it is physiologically impossible to strike a strong blow from a short start, and on the other hand it is inconvenient to strike a weak blow from a long start. What we have to consider is therefore the fine discrimination of the force, to see whether that is dependent on discrimination of the extent. The other obvious fact is that it is perfectly possible, within narrower limits, to strike a blow of given force from starts of different extent. It is possible to strike alternately strong and weak blows from the same start. This fact shows at once a certain amount of control over the force independent of the extent.

On examining the records, we find both of these facts exemplified. In Fig. 2, for instance, large changes in the force are accompanied by changes in the extent; yet the same blow is sometimes struck from different extents of start, and different blows from the same extent. On examining a large number of blows I find that there is no one extent that can be regarded as the normal start for a blow of a given force. In a series where the blows average stronger than in another, the starts may average shorter.

What we wish specially to examine is, however, the fine gradations in force, in order to see whether or not they are correlated with gradations of extent.

Fig. 1 gives a fair idea of the degree of correlation observed. The blows here recorded were intended to be equal, each to the preceding. We notice in the majority of instances that an increase in the force is associated with an increase in the extent of the start, and a decrease with a decrease. But we

notice also a number of exceptions. If there were no correlation between the two, the force and the extent would be equally likely to change in the same direction or in opposite directions: 50 per cent. of the cases should show like changes in the two,

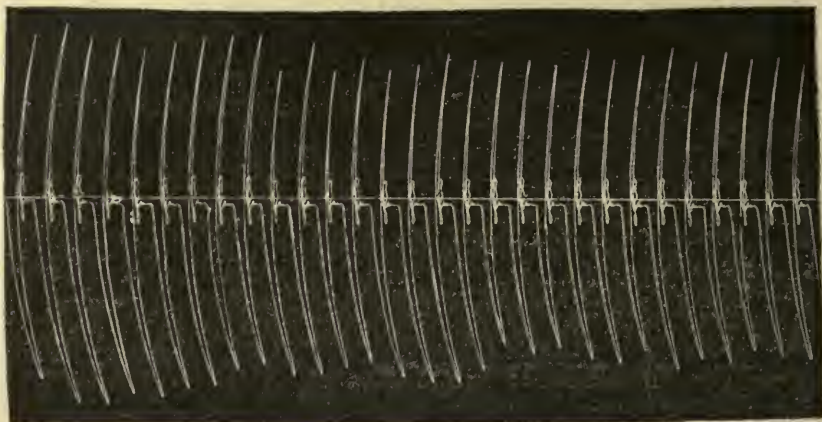


Fig. 1. Simultaneous record of the force of a series of blows, and of the extent of the movement preliminary to each. The portion of the tracing below the horizontal line records the extent of the preliminary movement. The further down the tracing extends, the higher the hand was raised. Above the line is recorded the force of the blow, the height of the tracing being proportional to the force of the blow. In this series each blow was intended to be equal to its predecessor.

and 50 per cent. unlike.<sup>1</sup> If, on the contrary, the force were completely dependent on the extent, then 100 per cent. of the cases would show like changes. The truth is found to lie between these extremes, and much nearer to the 'no correlation' mark. The like changes were found to make up the following proportion of the total number of cases:

With subject	<i>M</i> ,	62.8 % of	670 cases examined.
"	"	<i>R</i> ,	54.6 " 928 " "
"	"	<i>S</i> ,	52.6 " 207 " "
"	"	<i>W</i> ,	61.7 " 2115 " "

The correlation between the fine gradations in the force of a blow and in the extent of the preliminary movement, is therefore direct, but rather slight in degree. The force is not closely dependent on the extent. The extent of the start is apparently one of several causes that influence the force of the blow.

<sup>1</sup> The equalities can be eliminated by minute measurement.

The error in reproducing a blow of given force is not to be regarded as in the main an error in the extent of the preliminary movement.

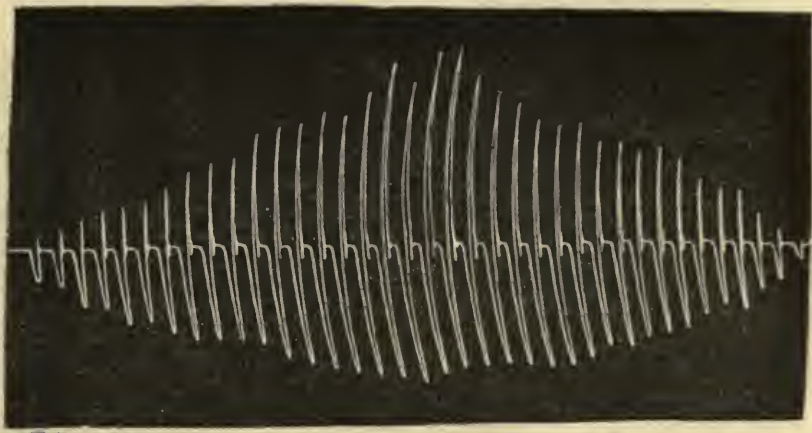


Fig. 2. In this tracing, each blow was intended to be just stronger than the preceding blow, up to the maximum, after that just weaker than the preceding. No heed was paid to the preliminary movement.

If instead of calling for equal blows, we call for blows progressively stronger or weaker, we obtain such tracings as Fig. 2. The increase in force is usually accompanied by increase in the start. The correlation between the two is sometimes, but not always, stronger here than in the former style of experiment. The most significant question is whether the mistakes made in graduating the force can be explained as the result of unsuitable changes in the extent. It is found that mistakes are relatively more frequent when the change of extent is contrary to the desired change of force, but that, even so, the force is rightly graduated in the majority of such cases; and, finally, that less than half of the mistakes made are associated with like changes in the extent. While, therefore, the proper modulation of the force is favored by a corresponding modulation of the extent, the latter is not necessary: the force will usually be made right without the help of the extent.

This conclusion is helped out by the results of experiments in which there was a deliberate attempt to vary the force as in the last experiment, but keep the extent constant; or, to vary



the extent, while keeping the force constant. This experiment was difficult and 'unnatural' at first, but after a little practice it could be carried out with a fair degree of accuracy.

We may approach our general question in still another way. If the force of a blow were regulated mainly by the extent of the preliminary movement, the proper means of *improving* the accuracy of the blow would be to increase the uniformity of the extent. This can be tested in several ways :

1. If a bar is placed over the hand so as to limit its upward movement, the start will always have the same extent, and the accuracy of the blow should be increased. This was tried on three subjects, and succeeded to a certain degree with one, but failed with the other two. The average errors of the force were as follows :<sup>1</sup>

Subject *S*, without bar,  $2.50 \pm .10$ , with bar,  $1.80 \pm .10$ .

" *M*, " "  $3.72 \pm .23$ , " "  $5.62 \pm .35$ .

" *W*, " "  $1.41 \pm .04$ , " "  $1.82 \pm .05$ .

For the latter two subjects, the uniformity of extent was not so important as a free, unimpeded movement of the arm.

2. To avoid the impeding of free movement by the bar, another means was adopted for making the start uniform. The drum was so placed that the subject could see the record of his preliminary movements as he made them, and was told to make them as equal as possible. On the whole, this did not increase the accuracy of the blows.

This may be seen by reference to the following table, which gives, besides the errors in force, the average change in the extent of the preliminary movement. The first subject, it will be noted, did not succeed in making the extent more uniform by attending to it.

	Attention to force.		Attention to extent.	
	Extent.	Force.	Extent.	Force.
<i>M</i> , .....	$2.61 \pm .18$	$3.55 \pm .25$	$3.16 \pm .25$	$5.16 \pm .40$
<i>R</i> , right hand,	$2.83 \pm .13$	$4.59 \pm .21$	$1.85 \pm .11$	$5.23 \pm .31$
left hand,	$3.45 \pm .21$	$5.87 \pm .37$	$1.49 \pm .10$	$4.58 \pm .30$
<i>W</i> , right hand,	$1.40 \pm .04$	$1.53 \pm .04$	$1.08 \pm .03$	$2.03 \pm .05$
left hand,	$1.80 \pm .04$	$2.13 \pm .05$	$1.46 \pm .03$	$2.82 \pm .06$

<sup>1</sup>The unit is purely arbitrary, being simply a millimeter applied to the record. The numbers following the averages furnish a measure of their reliability. The chances are 68:32 that the true value of the average does not differ from the observed value by more than this amount.

On the whole, if the object is to keep the force uniform, it is of no advantage to attend to the extent. It is better to attend directly to the force.

3. Even if no attention is paid to the preliminary movement it will be sometimes much more uniform than at other times. If controlled by the extent, the force should be most accurate when the extent is most uniform. But this is by no means always the case. There are series in which a high degree of accuracy is accompanied by great variability in the extent, and others with the contrary combination. What is the general rule? To answer this question we may take the separate sets of 20 trials each, into which the experiment was originally divided, and discover what correlation exists between the accuracy of the blow and the uniformity of the extent. We correlate the average error of the 20 blows with the average change in extent of the 20 starts. We must avoid the interference of practice, which makes both the blow and the start more uniform—but probably in independence of each other—and so introduces an artificial appearance of correlation into the statistics. I have therefore taken the last 60 sets of 20 blows each belonging to one series, and grouped them to bring out the correlation.

First, they are grouped according to uniformity of extent, and the average error in force computed for each group. The numbers represent the total error or change of a set of 20 blows. In the line labeled 'Range' are given the limits which served as the basis of division; below this is the average change in extent per set; and below that the average error in force for the same sets.

Range,	—25 mm.	25-30	30-35	35-40	40 +
Average,	20.5	26.9	31.7	37.2	44.9
Average error in force,	32.7	34.0	40.0	40.6	37.0

As the extent becomes less uniform, the blow becomes somewhat, and on the whole, less accurate in force. But the accuracy of force does not follow the uniformity of extent with great closeness.

Again, the same series is grouped on the basis of accuracy in force, and the average change in extent is computed for each group.

Range,	—30 mm.	30-35	35-40	40-45	45 +
Average,	25.9	32.6	37.3	42.0	51.2
Average change in extent,	29.5	30.3	30.7	34.4	34.0

The correlation here is even slighter than on the other basis of division. Where the error in force is great, the extent is somewhat less uniform, but not much less. The different accuracy of different sets can only in small measure be explained in terms of extent.

The different ways in which we have approached the problem have led to the same conclusion. There is a certain amount of correlation between the extent of the preliminary movement and the force of a blow, but the correlation is not strong. The force can perfectly well be varied while the extent is kept constant, or kept constant while the extent is varied. The error of the blow cannot be explained as a result of variation in the extent, nor can the accuracy of the blow be attributed to uniformity of extent. There is a certain amount of cause and effect between the two, but there is also a large amount of loose play.

The conclusion to which these experiments lead is that the regulation of the force of movement, at least in the case of a blow, is not simply a derivative function, dependent on regulation of the extent. Nor is it dependent on the duration of the movement, since that varies—though not in exact proportion—with the extent, and whatever has been said of extent would apply with slight change to duration. And if the regulation of force is an independent function, so is also the perception of the force of movement, since the control of the force of any one blow is based on the perception of the preceding blow. The muscle sense informs us of the force of a movement directly.<sup>1</sup>

What the muscle sense primarily gives us is not definite sensations of force, extent, duration, or position. The rudimentary quality of each of these abstractions is present in the muscular sensations, but they come to have definite meaning and use only by association, and especially by association with

<sup>1</sup> We may substitute 'speed' for 'force' in most of our discussions, since the force of a blow made with a hand and arm of constant weight varies only with the speed.



the production and results of movement. Any other sensations, such as those of sight or hearing, may join with the muscle sensations in forming the basis of a given judgment. And, as the present studies show, the groups of sensations on which two different kinds of judgment are based are in great measure separable from each other.

## THE PROBLEM OF A 'LOGIC OF THE EMOTIONS' AND AFFECTIVE MEMORY. II.

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The second portion of our discussion concerns itself with the problem of affective memory, the existence of which had to be assumed at various points in the preceding portion of this paper. Affective memory is the presupposition of the doctrine of emotional abstracts, as indeed of any theory of emotional continuity and evolution. It is unnecessary to point out that this has been assumed in all the English psychology which underlies hedonistic ethics and indeed affective memory in its one untenable form. It is this very assumption of a hedonic memory which makes the conception of hedonic calculus untenable. Yet, as M. Pillon has brought to our notice, both Spencer and Bain assumed the existence of a memory of emotions without going into careful analysis.<sup>1</sup> It is the modern 'doctrine of

<sup>1</sup> *Revue Philosophique*, Feb., 1901. In addition to showing historically the extent to which affective memory is presupposed by the earlier descriptive psychologists in their reconstruction of the mental life, Pillon reconsiders the grounds for the doctrine. He points out that the distinctions between real and ideal emotions (James), abstract-false and concrete-true emotional memory (Ribot), are distinctions of degree, not of kind. What distinguishes the memory of a concrete particular from an abstract, ideal emotion is merely the tendency of the former to localization in time, through ideal associations. The recurrence of particular emotions without ideal links is explained by immediate association. While the tendency of abstract emotions is always to pass over into particular through concretion or simultaneous association of ideal content—and not until then are they refelt (*ressenti*)—they may however be recognized (*reconnu*) without particular ideal content. This distinction between the mere recognition of a *re-presented* emotion and the feeling again of a particular, is indeed important, and the peculiar warmth of the latter seems to presuppose ideas as their condition.

In answer to the criticism that the so-called remembered emotions are really new, Pillon quotes from a passage in the 'Nouvelle Héloïse' (Part IV. Letter XVII.) where Saint-Preux describes himself as reviving, in the presence of the

elements' which develops *a priori* arguments against affective memory.

That doctrine of elements, in finding the constant element of all affective states in the two abstract qualities, pleasure and pain, as ultimate reactions of the organism upon presented content of consciousness, has been unable to conceive these as in turn presented and subject to the laws of presentation, memory, association and generalization. Of course, logically, the same *a priori* arguments which oppose themselves to affective memory would be equally strong against the notion that ideas may be associated through the mediation of an affective state common to both. Can an emotional state be presented and represented to consciousness—that is the problem. It need not be pointed out here that the æsthetic theories of *Scheingefühle* and *Einfühlung* assume this. Let us first recall briefly the phenomena of ideal memory and recognition and compare them with the alleged phenomena of affective memory. In the first place, then, there is a voluntary recall of sensational images and corresponding to it there is a so-called voluntary recall of emotional states as such, characteristic of what Ribot calls the affective type. The peculiarity of this phenomenon is that, while use is made of ideal associations or of organic attitude to bring up an emotional state, the object of the voluntary recall is a past emotion which is recognized as such when it comes.<sup>1</sup> This ability of voluntary recall of emotions may quite conceivably be

old scenes of love, the same emotions, but upon recognizing their futility he falls into rage and despair. He considers this passage a distinct proof of a recognized difference in the mind of Rousseau between the revived emotion, which appears according to the laws of association, and the new and full reaction of which they are the presupposition.

<sup>1</sup> Among others M. Sully-Prudhomme discloses that in his literary activity he does not seek primarily to objectify memories of sensations and ideas, but rather to reinstate a certain emotion, sentiment or mood, careless of the previous ideal elements if the emotional attitude be retained. The fact of importance here is that the objective value which is to be the center of the work of art is the emotional attitude which becomes both the center of unity for the work itself and a test of the reality of the result. In his '*Imagination créatrice*' Ribot finds certain forms of art where the dominating unity is an '*émotion fixe*' and more subtle forms of creation where the chief element of unity is an emotional abstract, a mood or sentiment, which acts as a universal, gathering under it, by way of subalternation, particular feelings and emotions.



limited, and memory, in the sense of recognition, still remain, just as we find memory of sensations in those who can recognize, for instance, various shades of red perfectly but cannot voluntarily recall them.

In the second place, parallel with the spontaneous appearance of images in consciousness there are well-authenticated cases of a spontaneous appearance in consciousness of particular emotional states which are recognized as familiar before the appearance in consciousness of the ideal content which shall definitely locate them in time. In this connection may be mentioned the classical case of M. Littré given by Ribot and a case given by Mauxion in a recent article in the *Revue Philosophique*,<sup>1</sup> and I shall add to the group one from my personal experience. A few years ago, while living abroad, there came into my consciousness entirely without associational conditions that were recognizable a peculiar emotional tone which I recognized as having been experienced before with peculiar intensity. I located it finally as the emotional overtone of a peculiarly desolate bit of anthracite coal region. So strong and marked was it that it developed into particular emotions of great vividness and sufficient to lead immediately to a bit of descriptive writing. The point of psychological interest is that

<sup>1</sup>In a second article in the February number of the *Revue*, entitled, 'La vraie mémoire affective,' the writer, M. Mauxion, is disposed to class all emotions which are connected with revived images as new, and to explain the judgment of such men as Sully-Prudhomme and Saint-Preux (as described above) as an illusion of auto-sympathy. The true emotional memory consists, he hints, in the recurrence of an emotional state independent of ideal content, for which he gives illustrations similar to the spontaneous recurrence of emotions described above. These recurrences he thinks of as conditioned wholly by the vital feeling or dominant mood. This true affective memory is thus wholly motor, like the motor memory of speech. Motor tendencies become organized into fundamental somatic resonances, or systems, whose recall the faintest suggestion can mediate. The author calls attention to the tendency of certain natural phenomena to develop definite organized rhythms in the beholder, which become emotional concomitants. Such, for instance, are the moods of the sea, the rising and setting of the sun. He also notes the dependence of emotional result upon the dominant organic resonance—the mood or vital feeling—the phenomena to which in my earlier paper was given the name of subsumption. True as this concept of the motor basis of emotional memory is, the writer does not seem to recognize the possibility of abstracting the systems of relations from the organic content itself.

with the closest search no ideal content could be found which would account for its revival. It is probably explained by the fact that a somewhat similar feeling had that day been generated by wholly different content—the squalor of a certain quarter in a foreign city, and that there had been direct emotional recall through emotion.

In the third place, we find simple recognition of new emotions or moods in life or in art as like some old experience or at least as familiar. Although the facts in favor of cases of direct recall seem sufficient, however its mechanism be explained, it is rather upon the phenomenon of recognition of affective states, as such, that the continuity of affective life through generalization of the emotions is based. It would seem that this point has not been sufficiently understood in the current discussions of this problem. For it is upon recognition rather than direct recall that the continuity of the ideal content of consciousness rests, and the case should not be made any more difficult for affective continuity.

An analysis of the methods which underlie experimentation upon sensational memory discloses the fact that it is with phenomena of recognition that we are really concerned. In the methods of identification and reproduction as applied, for instance, to experimentation upon the memory for sound and muscular movement it is really the accuracy of recognition that is measured. Of course it is possible to voluntarily reproduce sensations without the stimulus for recognition, but as in the case of Külpe's subject who could not recall color tones, 'the process of recognition remains perfectly normal,' that is, it is possible to carry out the memory experiments by the method of identification without the least trace of voluntary recall. In fact, in my own laboratory work I found a subject without the so-called auditory memory, whose memory curve for tones was perfectly normal. Likewise in the recognition of reproduced arm movements I cannot find any moment of comparison between a memory image and the arm movement reproduced. The point is that in much of memory experimentation there is no conscious comparison of images but only recognition of the new sensations as same or different or by means of verbal association. As

Külpe says, in his searching critique of the traditional doctrine of memory and association, "we are ordinarily unable to institute a direct comparison of memorial image with perception; \* \* \* conditions are unfavorable to comparison." If then, after Külpe's critique, memory reduces itself largely to recognition, and this to the liability of reproduction by peripherally stimulated sensations of the organic element making up the mood of recognition, the whole problem of affective memory becomes this: Can an affective state become the presupposition of another affective state, that is, the mood of recognition? Is feeling, emotion, capable of becoming a presupposition of a judgment feeling of familiarity? This is, of course, a question for empirical research, and recent investigators, Meinong, Ehrenfels, Schwartz, Witasek, have pronounced themselves affirmatively on the question.<sup>1</sup> To take one crucial case: when Professor James tells us that we may produce an emotion by putting ourselves in the appropriate bodily attitude and when we set out to do so and find it to be true, certainly we have meant to reproduce that emotion, whatever it is theoretically, and we recognize it as the true one when it comes.<sup>2</sup> It is then the function of assimilation in recognition which underlies the formation of generalized emotions just as it underlies the formation of general ideas.

As to the facts of affective recognition, it follows exactly such laws as would be expected on the assumption that the different orders of affective experience relate themselves as general and particular. It is a well-established law of revivability and recognition in the sphere of sensation and ideas that the general recurs before the particular and is recognized before

<sup>1</sup> Cf. Part I. of this article.

<sup>2</sup> In this connection I have to record a case of 'Sprachgefühl' which shows beautifully the way in which a remembered feeling without any ideal content may act as a criterion for a remembered word, and may mediate or prevent the recall of the word. Having, by intercourse with a particular group of people become accustomed to the more or less continuous use of the expression *schablonenhaft*, with all the peculiar Sprachgefühl connected with it, I had occasion to use the English word 'conventional.' I could not remember it. The feeling, partly rhythmical, partly a suggestion of distaste, connected with *schablonenhaft* appeared, though I could not remember the word. The feeling itself, however, was so distinct that when 'conventional' appeared, it was at first rejected, since it did not have the exact emotional connotation of the feeling already in consciousness.



the particular.<sup>1</sup> Thus, in the recall of a word that is particular, it frequently occurs that the class word appears first, and it is well known that in experimentation perception of difference precedes the perception of the direction of the difference. It is but a particular case of the general law that frequency of excitation exercises an influence upon the reproduction of an experience. In like manner, in most cases of spontaneous or volitional revivability of affective states the tendency is for the emotional abstract sentiment or mood to precede the revival of the particular emotion which depends more or less upon particular memory images. Thus in the case cited by Mauxion and in the case cited from my own experience it is the general mood which is recognized first, to be followed by the surging into consciousness of the particular emotion reactions subsumed under it. Analysis of emotional memories of places, their general mood in the sense that the term is used by Ribot, precedes the particular emotionally toned images. Again, in memory of dreams, the emotional overtone, as it has been called, may be frequently recognized on succeeding mornings as familiar, and as the same although the particular images with their emotional tone may be varied, and the general emotional tone of the dream may be recognized as familiar before any of the particulars. From my own experience it seems to be a clear case of recognition of the emotional abstract or fusion of a series of emotional experiences now fallen into obscurity.<sup>2</sup>

It is likewise true that in the presentations of art the same law holds, except that it is modified in certain ways by the technical conditions in the different arts. Thus, in some inves-

<sup>1</sup>Külpe, 'Introduction to Psychology,' p. 174.

<sup>2</sup>I have in mind a peculiar form of dream which there is reason to believe is quite general, the dream of mounting an endless flight of steps, or a series of mountain peaks, each higher than the other—various forms of continued effort—a dream doubtless brought about by organic sensations of weariness acting as stimuli. I have had the dream at various times—until its emotional tone became quite familiar, although the actual particular content of the dream, *i. e.*, the nature of the effort, was so vague that I could barely recall what it was, steps or mountains. And quite recently (of course it may have been an illusion) I recognized it as *the* dream without remembering any of the details.

tigations which I carried out in a class of students in the fine arts to discover wherein their sense of the reality of the work of art lay, it became clear that the criterion of reality, at least for the artistically undeveloped, is the familiarity of the emotional resonance that the work of art produces. It may be true, as Professor James insists, that the 'spurious' sentiment of the young couple before his picture of the Virgin 'would have fairly made old Titian sick.' It is probable that the expression of a sentiment was not the sole end of Titian's technique. The point of psychological interest is that a sentiment was called up and it was recognized as familiar, illusion though it may have been in comparison with Titian's purpose. Careful observation of those in my classes, all of them entirely undeveloped artistically, confirmed me in the view that for the tyro the test of artistic truth is the familiarity of the total emotional resonance which the first impression awakes. The emotional unity must dominate, even to such an extent as to be almost schematic and insipid. The crude sentimentality of popular art and literature is proverbial. Except in the cases of one or two, who were notably intellectual in type, with little natural feeling for art, I found that a total sentiment or mood is recognized as familiar and true, not only temporally before representations of particular emotions, but with much more certainty. For instance, the Dancing Boys of Donatello suggested feelings of pleased familiarity which were easily traceable to memories of childish games. The response was immediate. Nevertheless, the bodily and facial emotional expressions, true as they are to nature in an artistic sense, seemed to the observers unfamiliar and almost caricature, showing clearly that the recognition of suggested organic sensations was weak. In fact, the unfamiliarity of the particular emotional expressions had a tendency to break up the first unitary emotional impression. On the other hand, the wonderful nature moods which Corot has caught rarely failed to be recognized as familiar—as having been experienced—before the details of the landscape with their particular associations and emotional tendencies were taken account of. These are merely two illustrations of what appeared to be a common occurrence.

In literature and music the same direct investigation of rec-

ognition is not possible. The sentiment or mood, as total feeling, is rather a resultant of the entire reading or hearing, while the reality of the particular feelings may be recognized in passing. In the first reading, however, the judgment remains more or less in suspension; it is in the return after the total feeling has been caught that its particular emotions are really first fully recognized, emotionally, as part of the total feeling. It is a phenomenon of frequent occurrence that the emotional overtone of a poem may be caught in its unity, even when the energy of consciousness is so lowered that the particular images with their emotional tendencies barely enter consciousness.

Then, too, it must be remembered that in lyrical verse and music, especially if it be of a symbolic or impressionistic sort, the dominant mood is caught almost immediately and determines by its unity the recognition of the reality of the particular emotional tendencies. The 'Lotos Eaters' and the 'Raven' are good examples of this, as the first part of this article pointed out.

If sentiments and moods are generic phases of emotionalism, or definite attitudes, their liability to reproduction and recognition should be greater than that of particular emotions. Likewise, just as all schematic complexes of consciousness (general concepts, for instance) become susceptible of extension over varied stimuli, and may be reproduced by insufficient grounds, giving rise to illusions, so it is found that a schematic representation of sentiment, in general, will call it forth when the production or recognition of a particular emotion is a difficult matter.

On the ordinary theory of 'Scheingefühle,' that they result from an organic imitation of emotional expression in the work of art, recognition being based upon the definiteness of the quality of the organic sensations aroused, one should expect that the particular emotion, being more definite than the sentiment and mood, should be more easily recognized. Our experience, which was indeed the starting point of this entire discussion, is not in accord with this expectation, and we must conclude that recognition of affective states rests, not upon the definite quality of the ideal or organic elements, but rather upon the dynamic



relation of the elements of the emotional states, and this revivability goes back ultimately to the dynamic relation to volition. But this is a question which goes over into the problem which it is proposed to discuss in the paper on the 'æsthetic attention.'

These results are, of course, not wholly in accord with the two empirical laws of emotional revival that Ribot has formulated in his 'Psychologie des sentiments.' Emotional revival, he thinks, varies directly (*a*) with the complexity and development of the emotional state and (*b*) with the presence of motor elements.

Revival, in the sense that it has been conceived in this paper—namely, recognition of a state as familiar (whether it is new or old in any other sense has very little to do with the question), does seem to follow the first law. Sentiments, the subtler emotions, appear more susceptible to revival—both voluntary and involuntary—than particular feelings and emotions. This, as we have seen, is almost a truism in art and art criticism. Being relatively vague in particular motor content, as the result of abstraction, and such in meaning, *i. e.*, in constant dynamic relationship to the volitional life, they are much more easily revivable and recognizable than particular emotions, which must depend for recognition upon the definiteness of particular organic and muscular sensations. If this explanation were true, it would carry the negative of Ribot's second law with it. And, in so far as I have been able to investigate my own consciousness, experience, as well as theory, seems to be against it. The more definite and particular the organic and muscular sensations, the more they are conspicuous to consciousness, the more the subject seems to be *in mediis rebus*, in the midst of an emotional state, and the mood of recognition of which it is the presupposition does not come to consciousness. In the case of particular emotional states we are often rightly uncertain whether it is a new or old experience. The liability to reproduction in emotional states seems rather to depend upon the extent to which they have become generic, or of constant dynamic value for volition.

The ordinary forms of speech seem to indicate this, for we say: "I am angry again"; "He has the sentiment of obligation

strongly developed"; "I am in the same old mood again." In this matter, I can not help coming to the conclusion that Höffding's formulation of the law of affective memory is truer to the facts. "The feelings which are linked with the higher senses and ideational activity are more easily reproduced than those connected with the lower senses and organic feelings" ('Outlines,' p. 242). This formulation, to be sure, does not take account of recent discussions of the question—and uses feeling in a sense not in harmony with the present discussion, but it recognizes the same facts of introspection which have just been taken into account.

In connection with the discussion of the problem of this paper, Mr. Rutgers Marshall asked the writer what he meant by an emotion, that it could be represented to consciousness, remembered. Surely the answer to this question is not difficult, for it would be in Mr. Marshall's own terms. The breaking up of the false confusion of emotion with pleasure and pain, begun by the Lange-James theory and critically carried out by Marshall, has opened the way for the consideration of the emotion as content, as a system of organic sensations. Pleasure and pain, by their very nature, cannot be represented, but a relatively permanent group of 'instinct feelings' can, and with different algedonic coefficients at different times. All that is necessary to make conceivable theoretically what the facts of emotional memory or recognition assert empirically, is some such conception as that of the *dynamic constant* developed in Part I. of this paper. In the same sense that an idea is a complex of sensations, in various relations of time, space and intensity, an emotion in representation is an ideal feeling, a definite system of time and intensity relations among organic-sensation content. Representation, however, involves a degree of abstraction in which the algedonic intensity is lowered and the organic sensations dampened.<sup>1</sup> Grant that the body of an emotion is a system of relations among a group of ideas and sensations, and by that very fact it becomes capable of ideal representation, and subject to its laws.

<sup>1</sup>In this connection attention may be called to a critical review, by the present writer, of Witasek's paper, 'Zur psychologischen Analyse der Einführung,' in this issue of the REVIEW.

The emotional theories of art have taken the emotional state out of the sphere of mere emotional reaction to content (the sphere of pleasure and pain) and placed it in the sphere of content itself. This is the meaning of the æsthetic theories of *Einfühlung* and *Scheingefühlen*. The condition of representation to consciousness of any content is that it shall be segregated from the rest of consciousness by attention. When æsthetic theory, in its psychological analysis, shall have made clear the processes and the reasons for the processes which underlie the passage of the attention from the object to the complex of relationships which constitute the organic reaction to the object, the most difficult question of æsthetics will be solved.



## THE PSYCHOLOGY OF NUMBER—A GENETIC VIEW.

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The diversity of views regarding the teaching of arithmetic, some of them fundamentally different from others, impresses one with the lack of agreement among educators respecting the way in which the individual most naturally acquires the adjustments to the world which this subject gives. Yet there is no topic of study concerning which more has been said in the attempt to expound its psychological character, for the reason, doubtless, that arithmetic has always been esteemed to be of vital consequence in the life of every person. One can scarcely perform an action, it is said, which does not in one way or another involve quantitative judgment; and if he be not quick and accurate in his estimation of situations presenting quantitative aspects, he must be seriously handicapped in all his activities. And then, too, reasoning in the sphere of quantitative relations is such an excellent discipline; it trains to such exactness, such precision, such definiteness. One can not make an error in adjusting himself to the quantitative side of things without becoming aware of it at once. He can not manipulate figures and reach right results without being correct at every step. So it is but natural that people from the earliest times should have felt a deep interest in the study of mathematics, and especially of arithmetic; and, as we should expect, they would be most eager to discover by what processes the learning of it would be made most facile, and effective in the learner's life.

First among the theories regarding the manner in which ideas of number are acquired is what might be called the *symbolic* method. This is based upon the conception that number consists of the art of manipulating figures according to principles which govern the operations that can be performed upon

them. The learner must at the outset become familiar with certain of these principles, as of addition, subtraction, etc., and then in the course of his learning he must be led to see their application, first in simple, and then in more and more complex situations. But the process in all its aspects is simply one of handling symbols; and the end to be attained is to acquire the ability to manipulate them in an automatic manner. Now, it may be said that the figures are but the representatives, so to speak, of concrete realities, and the operations performed upon them could be performed upon the realities themselves, so that the figure presenting the result of the operation is the symbol of an actual situation which could be produced by treating things in the manner indicated in the symbolic process; and while, then, this method seems to make the learning of number mean the acquisition of automatic processes in the handling of figures, still at bottom this is but operating with things as they are found in the world. This is of course logically true, but the learner is not aware of it, at least in the early stages of his learning. He generally does not know what his manipulations mean, beyond the simple fact that they produce certain results in terms of the symbols with which he works. He is often, perhaps usually, not able to translate the results of his operations into their concrete significance. Surely, many a pupil goes through his arithmetic, solving all the problems readily, without having the faintest notion that he has been performing operations with figures that could be duplicated with the tangible things about him. He can, for instance, extract the cube root of a given number, wielding his figures according to a mode he has gained either from some example he has witnessed, or from a rule that has indicated the steps he must take; but when he is thrown into the necessity of dealing with those situations in his environment concerning which he should ascertain some property or attribute by the extraction of cube root he is utterly at sea. He is called upon, it may be, to build a bin of just the size required to contain a thousand bushels of wheat, and he has learned the amount of space which a bushel of wheat occupies: but he stands sicklied o'er with the pale cast of lack of thought in such a predicament. Arithmetic has conferred upon him simply the

ability to manipulate figures in an automatic way in the several relationships into which they are brought in the ordinary text on the subject.

Growing out of this conception of the function of arithmetic is a doctrine concerning the nature of individual numbers, and the manner in which familiarity with them is attained. If the end of number-study be to acquire the power of manipulation with figures, then it seems to follow easily enough that the best mode of learning is to master all the combinations possible for any one number before passing to the other. Each number is a fixed quantity, an individual thing possessing certain 'properties,' and the acquisition of number consists in the learning of these properties. The Grubé system is the best illustration of this conception of number, since it carries it to its logical extremes in presenting it to the learner.

In recent times people have come to feel that arithmetic ought to be made in the learning to relate to the world of things. The results of a course of training based upon the views of number indicated above have not been very satisfactory in giving the pupil a capacity to adjust himself well to that phase of the world about him with which number alone deals. He has not been made able to buy or sell much better because of his experience with book arithmetic. He has not been made much more ready in ascertaining the area of fields to be purchased or cultivated. He has had to depend upon the cashier at the bank to cast up his interest; and indeed in none of his activities which his arithmetic was supposed to perfect him in has he acquired much agility. The reason for this shortcoming, it is urged, must be that there has been too much reverence for figures, and figures are meaningless except they be connected with objects. So objects must be given the learner, and he must be made to see number in them, or rather, perhaps, to get number from them; and when he performs operations with figures he will know what he is doing. So the method of teaching number by objects has got a foothold; and this method aims really to employ objects to illustrate the symbolic processes, rather than to present actual situations which occur in the everyday world of the pupil, and which may later, for the sake of convenience in many ways, be



indicated by signs which have come through association to represent them. It should not be thought that this method is designed to confer upon the learner the power of adjustment to the quantitative side of the world by leading him to deal at first hand with it, and employing figures only in a true symbolic way wherein the symbols reinstate actual experiences, or at least tend to do so and could always do so if occasion demanded.

The developing conviction that arithmetic should relate to the concrete world, giving the learner the power to deal with it in the way in which he will need to handle it in order to perfect his adjustment to it, finds expression in a method of teaching the subject which has come to the front in the last few years. It is maintained now by some that adjustment to one's quantitative environment is determined by the ability to measure it—to ascertain how much there is in a whole of anything by finding out how often some unit of experience is repeated in it.<sup>1</sup> One wishes to know, for instance, how much land there is in a field; and he proceeds to find out by measuring it by some smaller part, as an acre or a lot, which can be easily grasped in an act of perception, or which is employed frequently in the affairs of daily life. Almost any one can get an acre of ground, if he is a farmer, or a lot if he is a city man, and this is the quantity which is most commonly acquired, or at least the quantity which was the most generally acquired when the unit of land measurement was established. So the farmer measures his land by the acre, repeating that unit times enough to cover the whole, and the city man buys his piece of ground by ascertaining how many lots it contains. A cistern contains a given number of repetitions of a gallon as a convenient unit. A pile of apples contains many repetitions of a bushel, which quantity can be

<sup>1</sup> "That which fixes the magnitude or quantity which, in any given case, needs to be measured is some activity or movement, internally continuous, but externally limited. That which measures this whole is some minor or partial activity into which the original continuous activity may be broken up (analysis), and which repeated a number of times gives the same result (synthesis) as the original continuous activity.

"This formula, embodying the idea that number is to be traced to measurement, and measurement back to adjustment of activity, is the key to the entire treatment of number as presented in these pages." McLellan and Dewey: 'The Psychology of Number,' p. 52.

easily handled. The distance from one town to another involves a certain number of repetitions of a mile as a unit of distance, which unit can be with some degree of accuracy measured by experience in walking. According to this view of arithmetic the pupil must be impressed in all his work in this field with the idea of measurement, or, as some say, of relative magnitude, regarding the two as the same; and he must be disciplined in the art of ready, accurate measurement. He must come to regard figures as existing for the purpose of making operations with real things simple and handy, and he must get in the habit of interpreting them as referring to things on their quantitative side, as he interprets the most of his words to refer to things on their qualitative side.<sup>1</sup>

#### GENESIS OF THE NUMBER IDEA.<sup>2</sup>

These different conceptions of the nature of number, together with the practical importance of the subject, will justify entering in a little detail into a consideration of the first steps which the child takes in adjusting himself to the quantitative aspects of his environment. Now, it will doubtless be granted as a general proposition that the processes involved in learning number (by which is meant the body of facts relating to the nature and potencies, as it were, of the quantitative *milieu* in its diverse aspects in the midst of which the individual is placed) will be determined by the needs of efficiency, economy, and expediteness in reacting to this quantitative environment. What sort of a relation, then, must the child come to assume to this phase of the world in order that he may bring himself into correspondence with it? What should he be able to do with it, and how does he acquire the requisite ability? How does he become possessed of skill in estimating numerical situations so as to determine what sort of meaning they have for him, and how he ought to conduct himself with reference to them?

<sup>1</sup>This principle is expressed in the words of McLellan and Dewey, "To teach symbols instead of number as the instrument of measurement is to cut across all the existing activities, whether impulsive or habitual. To teach number as a property of observed things is to cut it off from all other activities. To teach it through the close adjustment of things to a given end is to reinforce it by all the deepest activities."—'Psychology of Number,' p. 66.

<sup>2</sup>The present article discusses this phase of the subject simply and is but an introduction to the treatment of the learning of arithmetic as a whole.

Now, if one will follow the development of a child from the earliest weeks he will see that when it first begins to be aware of the environing world there is in all likelihood no appreciation of the quantitative value of things,<sup>1</sup> although there is from the start a sort of reflex response to experience with quantity, as when a well-fed child refuses to take more food, or when one deprived of his usual allowance cries for more. But in due season, perhaps after the lapse of fifteen months, there begin to appear evidences that the child is making some sort of judgments relating to the quantitative values of the things which affect him in his daily life. However, these judgments must be exceedingly indefinite; they must be greatly lacking in preciseness—judgments simply of muchness. As McLellan and Dewey say, referring to the general process of the development of exactness in quantitative judgment,<sup>2</sup> the muchness of things has a vital significance for him, since it determines the amount of a given experience which he may get from them. The muchness of his milk indicates the amount of pleasure he will derive from this source, and so with his sugar and all the other things which relate to his pleasure and his pain experiences. At the start, though, this is all indefinite, instinctive wholly, using the term instinct in the broadest way. But at the age of two or thereabouts the child is forming ideas of more or less with reference to given objects which he frequently experiences, these being determined now not alone by the indefinite sense of fulness from having eaten, or of pain from having actually experienced unpleasant sensations, but he

<sup>1</sup> It is doubtless true, as Spencer has said, that qualitative reasoning has always preceded quantitative reasoning in the development of the race, and in the growth of the individual mind progress must be made through the qualitative to the quantitative. Moreover, all quantitative reasoning is *qualitative in its initial stages*.

<sup>2</sup> We make a start with what is vague and indefinite respecting bulk, size, weight, etc., and then we go on to determine these exactly. We pass from the indefinite how much to the definite so much. It is the difference between saying that iron is heavy and that so much iron at a given temperature and a given latitude weighs just so much; or between saying that the blackboard is of moderate size and that it contains so many square feet. The development from the crude guess to the exact statement depends upon the selection and recognition of a unit, the repetition of which in space or time makes up and thus measures the whole.—'The Psychology of Number,' p. 45.



knows from ocular examination of things, as of the milk in his bottle, that it is more or less than he ordinarily has or desires, this ability having been acquired, of course, through association of visual and organic impressions, so that visual data now reinstate organic experience. He is active at this age in estimating the how much or the how little of things, with the motive of increasing the number of pleasure-giving objects, and of decreasing the number of those causing pain. Without doubt the ideas relating to increase of experience are more definite in the early years than those relating to decrease, since as a matter of fact the child is concerned more with having pleasurable experiences repeated than with lessening the number of repetitions of any disagreeable experience.

Quantitative ideas during this early stage relate to muchness in just the meaning which this word conveys to one.<sup>1</sup> There is probably in the first years no process of measurement in the way of singling out a unit of experience and applying it to a total situation of any kind; what there seems to be is an association, rapidly reduced to automatic readiness, with the consequent dropping out of elements which were present at the start, between the bigness or littleness of things and a certain amount of experience, either of sensation or of effort, which they occasion him in reacting upon them. The child does not think of three bottles of milk as one bottle repeated three times, nor four sugar-lumps as one sugar-lump repeated four times, nor the stairs he climbs as one stair repeated fifteen times. In the latter case there is simply a valuation, more or less explicit, of the amount of effort required to get up the stairs. But as the reactions upon the world are multiplied, and probably by the

<sup>1</sup> The idea of the exact adjustment of things is of very slow growth. As Fiske has said, bearing upon this point, "The conception of exact likeness is a highly abstract conception, which can only be framed after the comparison of numerous represented cases in which degree of likeness is the common trait that is thought about."—'Cosmic Philosophy,' Vol. II., p. 316.

The adult mind is not generally exact in its quantitative thinking either. Spencer's example illustrates this: "A man who has walked a mile in fifteen minutes, and, observing that he has a quarter of a mile still to go, infers the time it will take to reach his destination, does not primarily infer *three minutes and three-quarters*, he primarily infers a *short time*, a time indefinitely conceived as less than ten minutes and more than one."

age of four with most children, quite definite ideas of limitation of quantity, due to a growing power of attention to, and appreciation of, the boundary lines, so to speak, of experience occasioned by contact with the quantitative environment, are beginning to arise. At the table the child is limited constantly in the amount and number of things that may be eaten. He is limited with regard to the hours and minutes he may spend with play-mates, that he may sit up at night, and the like. He comes up incessantly against limitations in regard to the distance he can walk, the size of his playthings; and in innumerable ways there is constantly impressed upon him the idea of boundaries to everything.

Then there is arising, too, the notion of the repetition of a unit to produce a whole. The child has three oranges and he is to eat one in the morning, one at noon, and one at night. He has ten pieces of candy which are to be divided between himself and his brothers and sister. The general impression of muchness is now broken up into ideas of particulars; and all this time the learner is hearing people about him make use of number terms to define quantities, and he comes to employ these terms himself, and his attention is thereby drawn in some measure at least to their connection as it is displayed in the concrete situations in which he is placed, at first in all likelihood in an indefinite, general way; but still the words two and three and four come to reinstate with greater or less fulness of detail situations in which the number idea is the prominent one. Four apples will give him more pleasure than three; five pieces of gum will be more enjoyable than four. The association of number terms with quantity in this way, and the comparison of quantities through the terms thus used to denote them, can not fail to bring to the attention the thought of wholes being made up of units comprising the same 'properties.'

This growing sense of the composition of groups or wholes of the child's daily experience by units is indicated by the appearance of the tendency to count objects; and the process of counting reacts to develop the mode of conceiving wholes in this way. Counting is without doubt first suggested to the child by the parent who counts out his lumps of sugar and his apples.

In this counting the attention of the recipient is, of course, turned more or less fully upon each unit on its quantitative side as he receives it, for he is brought into a situation where he is getting a whole by parts or units. When this is repeated often enough the attention is spontaneously attracted by the units which compose a group, and the child comes himself to count off his objects. At the outset counting has no more significance, probably, than that it is a case of mere imitation; but its effect is to modify the initial way of regarding quantity as muchness, and develop the consciousness of repetition of units. After a time this comes to be the way of regarding a whole, and so of comprehending it. The child of five has a handful of sugar-lumps; before he devours them he counts them out and he discovers there are six. Now in all likelihood he does this at the start through imitation of an act he sees other people perform, and in so far the act has no mathematical significance; but in time he arrives at the point where he needs to state an unmeasured whole in a term which, through previous experience, he has come to feel has a certain value for him. Six sugar-lumps mean a good many or only a few. It is not exact, yet it means something, it means more than three, for instance; and as you observe the child counting and arriving at a conclusion you can readily see the influence which the result has upon him. He is either happy or depressed, according as the number expressing the total suggests to him much or little pleasure. And then the motor processes involved in handling units, the acts of speaking the names of numbers and laying down each unit, gives him a certain clue to the value which any given whole will have for him. It may seem to some that these motor factors would have no psychological value, but when you watch a child going through these processes day after day, and apprehending more or less consciously the amount of pleasure many or few repetitions of movements give him, it can be seen that the mere act of counting six apples, for instance, regarded simply as a complex of vocal and manual activities, will reinstate old associated gustatory experiences, and so will be an index to the amount of pleasurable taste this whole will afford.<sup>1</sup>

<sup>1</sup> Cf. Speer, 'Arithmetic, Book for Teachers,' p. xvii.



McLellan and Dewey<sup>1</sup> make counting a more distinctive and peculiar process than it really seems to be. They say : "The concept two involves the act of putting together and holding together the two discriminated ones. It is this tension between opposites which is largely the basis of the childish delight in counting. Number is a continued paradox, a continued reconciliation of contradictions. If two things are simply *fused* in each other, forming a sort of vague *oneness*, or if they are simply *kept apart* from each other, there is no counting, no 'two.' It is the correlative differentiation and identification, the holding apart and at the same time bringing together, which imparts to the operation of counting its fascination." But the concept 'tree' involves the act of putting together and holding together the discriminated leaves, branches, and trunk, in as true a sense as in the case of the concept 'two.' In either instance the word comes by repeated experience to reinstate previous situations, the details of which have become submerged in subconsciousness, but which may be restored by concentrated attention. When we of adult years hear the word 'two' we have a feeling that we know it; if we attend closely enough there appears in the mind's eye two centers of attention (I say 'centers of attention' for the reason that in most minds probably no distinct objects are presented, only the remains of objects, as it were, or the place which the objects would occupy if one were viewing them); and on prolonged attention definite objects are presented, as two men or two chairs. Now, the establishment of this concept certainly proceeds in no different way in principle from the establishment of any other concept; situations are encountered and words associated with them; their details are at first noted, but by repetition of instances wherein the principal characteristics are similar but the details different, the latter gradually fade away, leaving the word finally as a sufficient guide for the purposes of ordinary adaptation. The child's delight in counting is not due to a process of 'correlative differentiation and identification,' but to the accomplishment of a feat which others are able to perform, and to the satisfaction of having gotten at an idea of what the whole he is

<sup>1</sup> Op. cit., p. 31.

counting is worth, in terms largely of verbal and motor experience.

Counting, for the sake of the act largely, is the earliest mode of measuring a certain indefinite whole or group, but later the measuring process is made more explicit through the exigencies of the child's life. He has five sticks of candy and he may have one stick a day; now, how many days will the candy last? He has six dolls and he wishes to have a doll party, how many children can he invite? He is to have a birthday party and a certain number of children can sit about the dining table; how many children? So there is constant occasion for the child to count up a whole and become aware of the number of units it contains; and the personality of units, so to say, being thus forced upon the child in his daily adaptations, attention comes to reckon with them, to get into the way of noting them in their functions as composing a whole, and so the individual comes to learn them, to react appropriately to them.

It has been implied, of course, in what has been said, that in his counting the child considers only objects of the same kind. He will not count dolls and sugar-lumps together, and the reason is obvious. Dolls and sugar-lumps do not afford him the same experience. There is no reason why he should try to ascertain the value of a whole made up of these different things. What he is after, ever and always, is to find how often a certain kind of experience will be repeated, or what it will be in its totality. Dewey and McClellan have maintained that this counting of like objects only together depends upon a process of abstraction, whereby the child consciously groups in one class objects possessing common characteristics and isolates them from all other things. Now, looking at it from the outside, and logically, this seems to be true. Chairs are distinguished from people, and people from dishes, but this phenomenon is not at all peculiar to the formation of number notions, and does not involve what is ordinarily understood by abstraction at all. The child reacts in the same way to objects that resemble one another; an activity set up by one thing will be revived when another thing like it is presented. Then each of the members of any class of objects will stimulate a certain characteristic kind of

reaction, and each of the members of another class of objects a certain other characteristic kind of reaction, and so the world will get classified in the individual's behavior toward it, but not by conscious abstraction and isolation. Well, then, in dealing with the quantitative aspects of his environment, the child considers in his counting only like objects, pursuing the same psychological process in principle that he would if he wanted to eat something sweet. He would choose those objects which on previous occasions have afforded him certain desired-to-be-repeated experiences. In counting he sticks to his beads or to his blocks because he wants to know what he can do with this certain sort of thing. When he wants a sweet taste he helps himself to sugar and passes by the lemons on the table; and when he wants to show how big a house he can make he counts blocks and does not include tables. It is obvious, of course, that in order to become adapted at all to his environments this is just what he must do. He must think of things together that have similar properties, because they will affect him in the same way, or he can do the same things with them.

x It is maintained, again, by Dewey and McClellan that in his counting the child synthesizes units into a whole. He thinks of three apples after he has counted them as *a three*, and not as one apple, one apple, one apple. One ought not to dogmatize on this subject to-day, seeing that it is under dispute in many quarters; but regarded from the genetic standpoint it seems as though the only sense in which this synthesizing process can be at all true is that the child in time acquires the power to hold the one apple, and one apple, and one apple, in the focus of consciousness at the same time; and as a result of oft-repeated experience with three objects of various sorts this mental content revives a certain effect or mood which gives the individual the impression that he knows it. By continued repetition of the presentation of a concrete *three*, together with the word denoting the same, the latter comes in time through a process of economy, so evident everywhere in the mental life, to stand for, to represent, the idea, calling into consciousness only a small part, perhaps not any, of the original elements of the idea. Now, what the individual needs for right adjustment is to get



some kind of a meaning for the three apples, and this is attained by responding to some extent to each unit in the group, the whole fusing together into a total effect, the more easily and perfectly the oftener the experience is repeated. In time, to repeat, the word 'three' comes to reinstate an attitude or adjustment, or awaken a mood, the particular items of which have disappeared in subconsciousness. But this is not a different process in principle from that which is involved in learning other aspects of the world. There is a disposition in the mental life to condense, to short-circuit adaptive processes whenever possible; and mental development really requires this as a primary factor. One sees this illustrated in the learning of words. Take, for instance, the word 'honesty'; when spoken in the presence of an adult it awakens simply a general mood, which gives the feeling that the thing is understood, and this is enough for purposes of adaptation. But to a child this word, if comprehended, must revive a lot of particular things. If one will watch such a child learning the word, and trying to grasp its meaning when he hears it, he will see the fact here mentioned clearly illustrated. He will be impressed with the number of times the word must be repeated to the child, and the apparent effort he makes to get reinstated particular experiences. The purpose of bringing this matter up in this connection is to show that the learning of number is no exception to the general law of learning, that there is no process of abstraction or synthesizing which is not present in acquiring adjustment to any other phase of the world. One cannot say that the process of learning the significance of the term 'five apples,' and the psychological content aroused when it is heard, is any different in principle from the process of learning the significance of the term 'maple-tree,' or the response produced when this term is heard. Both terms reinstate experiences more or less complete in conscious detail, only one reinstates characteristics of form, it may be, or quality, as taste of sap, or color of the leaves, characteristics which were concerning the individual when the term was being learned, while the other relates to quantity of experience, the characteristic which was being attended to when this term was being gained.

# THE INFLUENCE OF IMPROVEMENT IN ONE MENTAL FUNCTION UPON THE EFFI- CIENCY OF OTHER FUNCTIONS.

## II. THE ESTIMATION OF MAGNITUDES.

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In a previous paper we considered in detail a typical experiment on the influence of training in delicate estimation of magnitudes. The present paper will summarize all the experiments of that sort which we have made with individuals.

Before and after training in judging the areas of rectangles 10-100 sq. cm. in size, four subjects were tested as to their ability to judge :

1. Triangles within the same limits of size.
2. Areas between 140-200 sq. cm. of similar shape to those of the training series.
3. Areas between 200-300 sq. cm. of similar shape to those of the training series.
4. Areas between 100-140 sq. cm. of various shapes; circles, trapezoids, etc.
5. Areas of 140-200 sq. cm. of various shapes; circles, trapezoids, etc.
6. Areas between 200-240 sq. cm. of various shapes; circles, trapezoids, etc.
7. Areas of 240 sq. cm. and over of various shapes; circles, trapezoids, etc.

Table IV. represents the results with these subjects. The figures after each name in part A represent the average errors for the kind of area stated at the head of the column, in the before- and after-training tests. In part B are given percent-

TABLE IV.

SAME SHAPE.										DIFFERENT SHAPE.									
Subject.	10-100 in training series at end.	20-100 without correction factor.		140-200 without correction factor.		200-300 without correction factor.		Triangles 10-60.		100-140.		140-200.		200-240.		240 and over.			
		Before training.	After training.	Before training.	After training.	Before training.	After training.	Before training.	After training.	Before training.	After training.	Before training.	After training.	Before training.	After training.	Before training.	After training.		
A.	Be.	21.9	6.4	43.5	20.5	30.2	43.0	14.7	4.7	27.5	14.5	62.6	12.6	63.8	28.0	78.8	27.7		
	Br.	21.0	7.7	47.8	26.8	33.0	33.7	10.6	9.0	27.5	16.2	40.3	58.9	57.4	79.7	86.0	104.0		
	E.M.T.	2.3	6.0	5.0	31.0	12.6	44.8	11.5	5.9	37.8	12.8	23.7	30.7	33.8	28.2	52.9	45.9		
	Ber.	0.4	4.1	41.6	14.0	47.4	4.0	4.5	5.9	20.1	12.2	39.6	27.7	47.0	38.0	85.0	55.0		
	Total.	61.7	24.2	137.9	82.3	123.3	125.5	40.3	25.5	112.9	55.7	166.2	129.9	202.0	173.9	302.7	232.6		
B.	Be.	29	48	48	143	143	85	32	52	52	39	20	44	44	35	35	35		
	Br.	37	56	56	100	100	85	85	39	39	34	146	139	139	121	121	121		
	E.M.T.	66	602	602	355	355	131	51	34	60	60	129	83	83	83	97	97		
	Ber.	42	34	34	8	8	131	131	60	60	60	70	81	81	81	65	65		
	Total.	39	60	60	102	102	63	63	50	50	50	78	86	86	77	77	77		
			3	3	3	3	3	3	3	3	3	4	4	4	4	4	4		



ages showing the proportion of the late to the early errors. The percentages after 'total' represent the proportion of the sum of the average errors of all four in the after-training tests to the sum of the average errors in the before-training tests. The figures beneath represent the number who make a smaller proportionate improvement, in the case of each category, than they did in the case of areas exactly similar to those of the training series but estimated without the correction factor, *i. e.*, in just the same way that they estimated the triangles, irregular areas, etc. It has seemed unwise to attempt in detail the calculation of the reliability of each of these and of following results. The labor would be enormous and in many cases the laws of chance not easily applicable. In these preliminary studies we have tried to discover only general tendencies, not their exact amount.

From the figures given for these subjects it seems clear (1) that the improvement in the estimation of rectangles 10 to 100 sq. cm. is not equalled in the other functions; (2) that change in size without change in shape decreases the amount of improvement in proportion, in general, to the amount of the change, and (3) that the same tends to hold true when both size and shape change. The score for areas 240 and over presents an exception to this which cannot, we think, be due to chance. (4) The different influence of the training on the different subjects is apparent from the last column. It teaches, as was pointed out in a previous article, that there is no inner necessity for improvement of one function to improve others closely similar to it, due to a subtle transfer of practice effect. Improvement in them seems due to definite factors, the operation of which the training may or may not secure.

Two subjects took the training in the same manner as did these four but were tested with only parts of the series. Their records were as in Table V.

Experiments similar in their general plan to these were carried on in the case of several other sorts of estimations of magnitude. A detailed account of their administration is out of the question. As has been pointed out, an exact measure of the improvement in the case of the different training series has not

TABLE V.

	20-100 in training series at end.		20-100 with- out correction factor.		Areas of different shape.							
					100-140.		140-200.		200-240.		240 and over.	
J. W. W.	1.8	1.8	7.7	8.6	27.8	42.1	36.1	42.3	83.3	82.6	116.8	87.3
M. W.	4.2	2.2	11.6	3.3	18.	1.8	30.8	14.1	42.4	23.6	42.2	33.7
Total.	6.0	4.0	19.3	11.9	45.8	43.9	66.9	56.4	125.7	106.2	159.0	121.0
J. W. W.	100		111		151		117		99		80	
M. W.	52		28		10		45		56		75	
Total.	66		61		96		84		84		76	

been possible. In the following results whenever a measure of such improvement is given, it means the change from the average of the first trial of the whole series to that of the last trial.

*The influence of training in estimations of magnitude within certain limits on the ability to estimate similar magnitudes in case of objects qualitatively different.*

1. The influence of training in estimating the areas of rectangles from 2 to 12 square inches on the ability to estimate triangles from 1 to 5.5 square inches.

The general method was the same as has been described. The series used for the training was a set of 60 rectangles of various shapes, ranging from 1.5 to 12 sq. inches. It was expected that the number would be so great as to prevent any from being known by their shape, and the records are free from any proof that such was the case. It may have been, however, that the subjects were to some extent unconsciously guided by other factors than the mere magnitudes.

Subject W. in the rectangle series, being allowed to note the real lengths after each judgment, made sum of deviations 30.1 square inches (approx.).<sup>1</sup>

After 20 trials, 5 with about two-thirds and 15 with the whole series, he made 11.5, being 28.3 per cent. of his first trial (average errors approximately .5 and .2). With the tri-

<sup>1</sup> The areas from 8 to 9.5 were added after the 5th trial. The sums of deviations for the first five trials were 16.5, 9.5, 6.5, 8.5, 7. They then rose to 17.5, 25.0, 21, 19.5, etc. By calculating what the sum of deviation would have been had the series been full from the start, we get 30.0 square inches.

angle series W. was tested before and after this training, the results being sums of deviations 2.5 square inches and 5.75 square inches, the latter being 230 per cent. of the former (average errors .11 and .26 square inch). The average error of areas in the training series of corresponding sizes at the end of training was approximately .07 square inch.

Subject T. in a similar way made 30.0 (approximate) at the start and after approximately 20 trials made 39 per cent. of the former (average errors approximately .5 and .2). In tests with the triangle series before and after this training T.'s sums of deviations were 15.0 and 6.5, 43.3 per cent. of the former. Average errors were .68 and .30. The average error for areas in the training series of corresponding size was at the end of training .07 square inch approximately.

2. The influence of training in estimating the areas of rectangles and triangles from 0.5 to 12.0 square inches on the ability to estimate various shapes between the same limits.

The general method was the same that has been described. The series used for the training was the set of rectangles used in 1, plus 42 triangles of different shapes, ranging from 1.5 to 5.5 square inches by .5 square inch steps.

The note on page 387 is equally applicable here. Before and after this training the subjects were tested with 17 areas of various irregular shape running from 3.1 square inches to 11.8,<sup>1</sup> and averaging 6.4.

Subject W., starting from the point of ability given by experiment 2, and being allowed to note the real lengths after each judgment, made in the first trial sum of deviations 21 square inches, at the end of 32 trials 8 square inches, 38 per cent. of the former (the training was at intervals of about a week during over a month, hence the slow progress). The average errors were approximately .21 and .08 square inch. Before and after this training he was tested with the irregular shape series, the results being sums of deviations 17.17 and 16.83 square inches, or 98.0 per cent. of the former. Average errors,

<sup>1</sup> These areas were determined by careful weighing, but their accuracy is conditioned by such slight variations as there were in the thickness of the paper used.



1.01 and .99. The average error for corresponding sizes in training series was at the end of training approximately .2.

Subject T. in a similar manner made in the first trial sum of deviations 26.5 square inches, at the end of 41 trials 9.0 square inches, 34 per cent. of the former (the training was over a similar time to W.'s). The average errors were approximately .26 and .09. Before and after this training his results with the irregular shape series were sums of deviations 34.1 and 11.7, the latter being 31.3 per cent. of the former. Average errors 2 and .69. The average error for corresponding sizes in the training series was at the end approximately .2.

Subject N. was tested with the same series as W. and T., but estimated the areas in square centimeters. She was trained with a series of rectangles of 20 to 60 sq. cm. varying each from the next by one sq. cm.,<sup>1</sup> there being two of each size.<sup>2</sup> With the 20-60 sq. cm. series, being told only that the limits were 20 and 60 cm. and that 1 inch equalled 2.54 sq. cm., N. made an average error of 4 sq. cm. Being then allowed to note the real area after each judgment, she made in her first trial with the series an average error of 2.2 sq. cm. At the end of 28 trials her average error was 0.55 sq. cm., 14 per cent. of the first error, 25 per cent. of the second.

Before any knowledge save that 1 inch equalled 2.54 cm., N. made with estimates of ten of the test series an average error of 63.0 sq. cm., the average real size being 122.3. Of these, four were under 60 sq. cm., averaging 38.4. The average error for these four was 22.7. After two minutes' observation of a sq. cm., a 10 sq. cm., a 50 sq. cm. and a 100 sq. cm. area, N. made for these four (when mixed in the total series) an average error of 8.8. For the series of varied shapes (12 being used) she made under similar circumstances average error 12.4, sum of deviations 148.6. After the 28 trials with the training series her average error was 3.6, sum of deviations 44.8, 30 per cent. of the former. For the four areas previously mentioned her average error was 6.0. In brief, her improvement due to

<sup>1</sup> This series was intended to be made up of areas indistinguishable save by size, but their shapes did perhaps afford some opportunity for unconscious influence on the estimations.

<sup>2</sup> Save in the first few trials, where 25 per cent. were unduplicated.

the slight chance to acquire a standard was nearly twice that due to the actual training, in so far as the four determinations were a fair test. For areas in the training series of sizes corresponding to the varied shapes of the test series her average error at the end of training was approximately .6.

3. The influence of training in estimating weights of 40 to 120 grams on the ability to estimate the weights of miscellaneous objects of similar weights.

The test weights were eight in number, averaging 95.8 grams. The objects were a cup, umbrella handle, pack of cards, etc.

The training was of the general method described, a series of weights 40, 45, 50, 55, \* \* \* 120 grams, differing no wise save in weight, being used.

Subject W., being allowed from the start to note after each judgment the correct weight, made at his first trial with the series sum of deviations 245, after 50 trials with the series sum of deviations 125. Average errors, 14 and 7.

In tests with the eight weights before and after this training he made sum of deviations 377 and 142, the average errors being 47 and 18. Six judgments improved, 2 were worse. With corresponding weights of the training series the average error at the end of training was 9.

Subject T. in a similar experiment made at his first and last trials (T. took 100 trials) with the 40-120 series sums of deviations 135 and 80, 59 per cent. of the former.

In tests with the eight weights before and after this training T. made sums of deviations 182.5 and 159.5, 87 per cent. of the former, the average errors being 22.6 and 19.9. Three judgments improved, 5 were worse. With corresponding weights of the training series the average error at the end of training was 3.

*The influence of training in estimations of magnitude within certain limits on the ability to estimate magnitudes outside those limits.*

1. The influence of training in estimating lengths from .5 to 1.5 inches on the ability to estimate lengths of 6.0 to 12.0 inches.

The training was of the general type described on page 250, the series used being a set of cards on each of which was drawn a line. The series contained 5 lines of  $\frac{1}{2}$  inch and 4 lines of each of the following sizes,  $\frac{5}{8}$ ,  $\frac{3}{4}$ ,  $\frac{7}{8}$ , etc., up to  $1\frac{1}{2}$ . The subject was permitted from the start to note after each judgment the real value. In W.'s case the sum of deviations for the first trial was 9 (eighths of an inch). In the last of 40 trials it was 2. The inaccuracy in the last trial was thus 22 per cent. of that in the first.

Before the 1st and after the 40th trial, W. estimated the lengths of 28 lines from 6 to 12 inches long. His sums of deviations before and after the training were 7.5 inches and 11 inches respectively, the number of errors being 13 and 19.

Subject T. in a similar experiment made with the training series in the first trial sum of deviations 8 (the average of the first three trials was  $10\frac{1}{3}$ ). In the last of 24 trials the sum of deviations was 2 (the average of the last three trials being  $1\frac{2}{3}$ ). The inaccuracy of the last trial was thus 25 per cent. of that of the first. With the test series T. made sums of deviations 7.5 and 7.5, the number of errors being 15 and 14.

For four other subjects the records were as follows :

	Training Series.—Sums of Deviations.			Test Series.—Av. Error.		
	Early.	Late.	Percentages.	Before Training	After Training.	Percentages.
L. S. W.	15	8	53	2.77	2.02	73
A. V. W.	33	8	24	2.07	1.61	78
E. J. H.	47	16	34	.72	1.13	155
J. W. W.	20	3	15	.98	1.46	149

2. The influence of training in estimating lengths of 6.0 to 12.0 inches on ability to estimate lengths of 15 to 24 inches.

The method of the experiment was the same as in 1. The series used for the training was 20 lines from 6 to 12 inches long. The series used for the tests was 13 lines from 15 to 24 inches long.

Subject C. in the 6–12 inch series made sum of deviations 40 when estimating the lengths without aid, save the knowledge that they were between 6 and 12. In the next trial, being allowed to note the real length after each judgment, she made



sum of deviations 23. After 40 trials with the set (roughly 80 units of trial) her sum of deviations was 5.6, 24.3 per cent. of her second trial, 14 per cent. of her first trial. With the 15-24 inch series she was tested before the 1st and after the 40th, the results being sums of deviations 31 (all minus) and 9 (8 -, 1 +), the latter being 29.0 per cent. of the former.

Subject N., being allowed to note the real lengths after each judgment, made sum of deviations 23. After 32 trials (roughly 64 units of trial) her sum of deviations was 1.0 (approximately), 4.0 per cent. (approximately) of her first trial. With the 15-24 inch series she was tested after the 8th and 32d, the results being sums of deviations 16 and 13, the latter being 81.0 per cent. of the former. During the period from trial 8 to trial 32 her improvement on the 6-12 inch lines was such as to reduce the sum of deviations from approximately 10 to approximately 1, that is, to 10.0 per cent.

*The influence of training in estimations of magnitudes within limits on the ability to estimate magnitudes outside those limits, the objects being in addition qualitatively different.*

1. The influence of training in estimations of areas of rectangles and triangles of 0.5 to 12.0 square inches on the ability to judge areas from 12 to 65 square inches of different sorts of shapes.

The training series has been described on page 388. The test series contained 10 areas 12 to 18 inches, averaging 14.1, 6 areas 18 to 24 square inches, averaging 20.9, 6 areas 24 to 30 square inches, averaging 28.5, 5 areas 30 to 36 square inches, averaging 34.1, 11 areas 36-65 square inches, averaging 44.5.

Before Training.			After Training.	
	Sum of Dev.	Av. Er.	Sum of Dev.	Av. Er.
12-18	27.4	2.7	19.1	1.9
18-24	17.8	3.0	23.5	4.0
24-30	19.4	3.9	21.1	4.2
30-36	30.5	6.1	28.9	5.8
36-65	63.6	5.3	35.0 <sup>1</sup>	2.9 <sup>1</sup>
Total.	158.7		127.6	

<sup>1</sup> The notable decrease in error here was due to a few very great improvements. Out of 12 judgments 4 were worse than before training, 1 was the same and 7 were better.

Subject W. was tested before and after the training described on page 388. The results were as shown on previous page.

The total error after training was thus 80 per cent. of that before training.

Subject T. was tested before and after the training described. The results were :

Before Training.			After Training.	
	Sum of Dev.	Av. Er.	Sum of Dev.	Av. Er.
12-18	39.8	4.0	12.1	1.2
18-24	23.2	3.9	11.3	1.9
24-30	27.3	4.5	30.4	5.1
30-36	23.0	4.6	24.1	4.8
36-65	73.4	6.7	76.5	7.0
Total.	186.7		154.4	

The total error after training was thus 83 per cent. of that before training.

Subject N. was tested with the large areas at the same times and in the same manner as described on page 389, before and after the training there described. Her estimates were made in square centimeters. Dividing the areas used in the test into those between 60 and 100, 100 and 140, 140 and 200, 200 and 240, and 240 and over we get the following results :

	Sum of Dev.			Sum of Dev.	
	No.	Early.	Av. Er.	Late.	Av. Er.
60-100	14	202.6	15.5	150.9	11.6
100-140	3	87.6	29.2	72.2	24.1
140-200	8	391.6	49.0	251.6	31.5
200-240	6	441.6	73.6	288.7	48.1
240+	10	1053.8	105.0	574.4	57.0
Totals.	41	2177.2	53.0	337.8	33.0

The inaccuracy in the late test was thus 61 per cent. of that in the former.

Before any knowledge save that 1 inch equals 2.54 cm., N. made with 10 of the test series an average error of 63 sq. cm. Of these, 6 were above 60 sq. cm., averaging 178 sq. cm. Her average error for these was 90.1. After two minutes' observation of a sq. cm., a 10 sq. cm., a 50 sq. cm. and a 100 sq. cm. area, N. made for these six (when mixed in the total series) an average error of 55.5, 62 per cent. of the former.

2. The influence of training in estimating weights of 40 to 120 grams on the ability to estimate the weights of miscellaneous objects of weights outside 40-120 grams.

The test weights were 12 in number, averaging 736 grams. The objects were books, a shoe, a bottle, etc.

The training was that described on page 390.

W. was tested before and after the training with the 40-120 series. The sums of deviations were 1438 and 958, the average errors being 120 and 80. Of the 12, six estimations were improved, two equal, four worse. One case of improvement was from 390 to 90.

T., in a similar experiment with training as described on page 390, made before and after training sums of deviations 1128 and 1142, the average errors being 94 and 95. 6 judgments improved, three were equal and three worse.

3. The influence of training in estimating lengths of lines from 0.5 to 1.5 inches on the ability to estimate the lengths of objects qualitatively different of 2.5 to 8.75 inches.

Subject W. before and after the training described on page 391 was tested with 12 objects, *e. g.*, an envelope, a brush, a wrench, the average length being 5.8 inches. His sums of deviations were 5.0 and 5.0, being the same. The average error was 0.42— in both cases.

Subject T. in a similar experiment made with a series of ten such objects of nearly the same average length, sums of deviations 2.75 and 3.25, the average errors being 0.275 and 0.325.

When one undergoes training in estimating certain magnitudes he may improve in estimating others from various causes. Such training as was described in our previous paper gives one more accurate mental standards and more delicacy in judging different magnitudes by them. In the case of estimations of magnitudes in terms of unfamiliar standards such as grams or centimeters, the acquisition of the mere idea of what a gram or centimeter is, makes a tremendous difference in all judgments. This will be seen in the case of N.'s estimation of areas. She was told that an inch was 2.54 centimeters, and with that as practically the sum of her knowledge of the size of a centimeter



made judgments of a certain inaccuracy. The mere examination for two minutes of areas 1, 10, 50 and 100 sq. cm. in size reduced this inaccuracy to 38 per cent. of what it had been. The acquisition of definite ideas is thus an important part of the influence of improvement in one function on the efficiency of other functions. Even this, however, may not be operative. With some subjects in some cases the new ideas or the refinement of old ideas produced by the training seem impotent to influence judgments with slightly different data.

It is hard to prove whether or not or to what extent the delicacy in judging by means of such ideas in the case of one set of data, is operative with the different data used in the test series. Surely it sometimes is not.

The training might also give ideas of how to most successfully estimate, habits of making the judgments in better ways, of making allowance for constant errors, of avoiding certain prejudices. These habits might often concern features in which the function trained and the functions tested were identical. For instance, the subjects who judged areas of various shapes made their judgments before training by looking at the 10, 25 and 100 sq. cm. areas given them as guides; after training they never looked at these but used the mental standards acquired. This habit is a favorable one, for a person can look at a 25 sq. cm. area in the shape of a square and still think various-shaped areas from 30 to 50 sq. cm. are under 30. The mental standard works better.

The training might give some mysterious discipline to mental powers which we could not analyze but could only speak of vaguely as training of discrimination or attention. If present, such an effect should be widely and rather evenly present, since the training in every case followed the same plan. It was not.

For functions so similar and for cases so favorable for getting better standards and better habits of judging the amount of improvement gotten by training in an allied function is small. Studies of the influence of the training of similar functions in school and in the ordinary course of life, so far as we have made such, show a similar failure to bring large increases of efficiency in allied functions.

## DISCUSSION AND REPORTS.

### COLOR-INTROSPECTION ON THE PART OF THE ESKIMO.

The color-vocabulary of any language has its source in considerations of two very different kinds: (1) It may be due to an accurate introspection of color-sensation, in which case certain regions of the color-scale (wholly unlike what we have in the tone-scale, in which one region differs from another always in the same way) are felt to be turning points, such that elements on one side of them differ from each other by a different sort of difference from that which distinguishes elements on the other side of them. Thus, all the colors which resemble yellow, but are not quite the same thing, fall into two sets, in one of which elements differ from one another by being more or less like green, and in the other by being more or less like red. Now the construction of any vocabulary is purely a matter of interest, and where colors become interesting first in an intrinsic way, so to speak, we should expect color-names to exhibit the same four-fold demarcation into red, yellow, green and blue (with compound names expressing the 'likeness' of the intermediate color-tones to the principal ones) which introspection reveals to exist in color sensation. Most frequently, however, (2) certain easily accessible dye-stuffs or paint-stuffs become at an early date objects of barter, and the necessity having arisen for giving a definite name to the stuff, the color which it represents is unavoidably designated by the same name, and the fact that it is not one of the colors of the turning-points is then not of sufficient interest to get itself represented in language at all. What usually happens is that both principles are operative more or less—that is to say, the conveniences of the language of barter do not completely obliterate the requirements of a scientific reflection of the facts of consciousness. For instance, in the language which has come down to us from various remote sources, red, yellow, blue and green have, to a considerable extent, the distinction which is their due—these names are all of such ancient origin that their original meaning has completely vanished from the common consciousness. But the terms which stand for the even intermediates between these end-members of series, orange, olive, peacock, violet, bear upon their face their recent origin—their color-reference is only a secondary element in their mean-

ing. In fact, for two of these intermediate color-tones we more frequently renounce the use of any special name at all, and are content to say *blue-green* and *yellow-green* (wherein we make an unfair distinction against *green-blue* and *green-yellow*); but *peacock* is a perfectly good name for blue-green, and *olive* (though in its actual signification it is rather too dark and too unsaturated), may easily be forced into supplying what is a crying lack in scientific nomenclature; and, since it is not too difficult, it is distinctly desirable to have well-recognized simple names for the colors which are evenly intermediate between the turning-point colors.

It is one of the most remarkable facts which the psychologist has to deal with, that the separation of the total color gamut into four different rectilinear series (to use Professor Müller's term), (*a*) red to yellow, (*b*) yellow to green, (*c*) green to blue, (*d*) blue to red, (strikingly different from the gamut of tones, where we have but a single rectilinear series), which is so distinct a *datum* of consciousness for many psychologists, is affirmed by others to have no real existence. That it should not force itself upon the attention of one interested in physical science is not so surprising, especially since so great a physicist as Helmholtz has set the example in feeling no necessity for its recognition. But that the psychologist, whose *métier* is introspection, should be able to ignore it is certainly remarkable: there is probably no other so simple a point in consciousness in which difference of opinion is so distinct. For instance, to take a recent case, Professor Scripture, who has given more attention to color than most English-speaking psychologists have done, says in regard to this four-fold root of the color series (PSYCHOLOGICAL REVIEW, 1897, IV., 544): "It omits violet, which is to the eye as different from blue as yellow from red. [This is a misconception of what is claimed by the advocates of a four-color system: it is not the *amount* of difference that they insist upon but the *kind* of difference; red and yellow are end-members of series, violet is an intermediate sense-quality; on either side of violet, the members of the series are *more* or *less* like blue, and *less* or *more* like red; on the two sides of yellow they must be differently described—they are at once *less* like yellow, and *either* more like green, or more like red. There is, that is to say, a change in *sort* of difference in passing through a *Wendepunkt*. Surely this is too obvious a difference for the scientific mind to ignore.] 'In purple we see blue and red' is true only of those persons who have seen purple produced by mixtures of blue or violet and red. \* \* \*

This whole seeing of primary and secondary colors and their relations



is a matter of education; it is absolutely lacking in children, to whom orange is as much a primary color as red is."

Whether children have any perception of the difference here in question is something which it is difficult to find out, because our color-names are imposed upon them long before those portions of their brains have been developed with which they reflect upon their sensations as such, or other than as signs of connections in an external world—a world which they do not yet know to be hypothetical. Naturally, if a child has already been taught to call a given sensation orange, he will not of his own accord think of it as a compound of yellow and red. But occasionally it may happen that favorable conditions present themselves for making the experiment; that happened, in fact, in the case of my own little girl. She had never had her attention called to purple until one day when she suddenly came upon a large and brilliantly lighted up surface of that color. She stood still, her interest fully excited, and said, in what we used to call her hypothetical tone of voice: "B'u!—Wed!—Wed!—B'u!"—the meaning being, "Perhaps I should call this blue!—Perhaps I should call it red!" It was impossible to doubt that she, at least, saw in purple the blue and red of which it is composed. To make the experiment complete, of course, it would be necessary to have for comparison a child brought up in an æsthetic atmosphere of nothing but blue-greens and green-yellows, and to see if, when suddenly confronted for the first time with a pure and brilliant surface of green, it exclaimed: "Blue-green!—Green-yellow!—Blue-green!—Green-yellow!"

An observation of ideally high value could be made if we were able to come upon some half-civilized race of beings who had had no practical concern with colored objects to distract their interests, but who had seen spread out before their eyes from time to time surfaces of color large and brilliant enough to be interesting in themselves. This is exactly the race that we have at this moment the good fortune to have described for us by Mr. W. H. R. Rivers.<sup>1</sup> He says (p. 149): "So far as I can gather from reading accounts of Eskimo life, color does not seem to be largely used in the dress or decorations of these people. [Hence their interest in color as a sensation would not be overlaid by their interest in pigments and dye-stuffs.] \* \* \* My previous experience \* \* \* has been derived from races inhabiting the tropics, and it seemed somewhat unnatural to find a far more highly developed language for colors in the inhabitants of a sub-arctic country

<sup>1</sup> 'The Colour Vision of the Eskimo.' Extracted from the *Proceedings of the Cambridge Philosophical Society*, Vol. XI. Pt. II.

such as Labrador. The Eskimo, however, told me that in the autumn they could see all the colors I had shown them in the hills of their country, and it is possible that when color is only a transient occurrence in the year's experience, it may excite more attention and therefore receive more definite nomenclature than in those parts of the world where luxuriance of color is so familiar that it receives little notice." What, now, is the effect upon the color-names of these Eskimos of the detachment of their color sensations from practical needs? "If one excepts three words, *sinanuk*, *achjangrtuk* and *kaijuk*, which were comparatively rarely used, *all hues, shades and tints of color were named by various modifications of the six words for red, yellow, green, blue, black and white.*" There were numerous affixes signifying 'ish,' and 'real, pure, light, dull,' etc., as well as mid-color names, but there were no other names indicating fundamental color concepts. Again, "the definiteness of the word for blue was shown in a very striking way by the fact that several individuals called purple (magenta) *aupaluktaktungalangaijak* (bluish red), and that one called violet *tungajuktakaupalangaijuk* (reddish blue). *These individuals seemed to have recognized in giving these names that both colors contained red and blue, and that one contained more red and the other more blue.*" [The Italics are mine.]

This coincidence of scientific color scheme with an impersonal character of color experience is certainly very remarkable; it is not simply that the prominent colors have the prominent names, for that state of things is more or less closely approached to always, but that such a perfect reflection of the facts of consciousness should occur with a people who have no occasion to subordinate color as a pure sensation to color as a sign of a definite object.

Belief in the value of introspection in this question received a severe blow in the fact that, owing to an accident of pigments, green is, for the painter, made up, physically, by mixing yellow and blue. It is one of the minor triumphs of the human intellect that it has finally been made out that this is not the same thing that is meant by a mixture of sensations, but the fact that it misled acute observers into thinking that green is, for sensation, a mixture of blue and yellow, has had a most untoward effect in retarding the progress of right views on the subject. Thus Helmholtz says (*Physiol. Optik*, p. 380): "Welch trügerisches Mittel die angebliche innere Anschauung in solchen Dingen ist, zeigt am Besten das Beispiel von zwei solchen Autoritäten wie Goethe und Sir D. Brewster, die beide glaubten im Grün das Blau und Gelb zu sehen aus denen sie es, getäuscht durch

die Erfahrungen an Malerfarben, gemischt glaubten." What is shown by this fact is that the power of a vivid suggestion is irresistible not only on the minds of women and children and Christian Scientists, but on those of the most distinguished thinkers as well. There is no illusion that has ever prevailed, in a small way, that has been the cause of such a long train of consequences of error as this. Think of a Helmholtz who is able during a long life to exclude from his consciousness the fact that no one can even *deceive himself* into thinking that he can see in the yellow the red and the green which he must see in it, if his theory of mixed *sensations* is true, and still less that he can detect the green sensation-element in orange, or the red sensation-element in the olives, which he *must* detect if all colors which lie between red and green are sensation-mixtures out of red and green! The fact that language is in such a primitive state that it does not permit us to distinguish between color in the physical sense and color in the psychological sense is largely responsible for the long-continued acceptance on the part of thinking people of this illusion—what may be called the Helmholtz illusion. It has needed much labor on the part of Hering and his followers to put it to flight. But the fact which the acute tribe of Eskimo examined by Mr. Rivers have discovered for themselves—that red, yellow, green and blue (and no other colors) are of a unitary character<sup>1</sup>—is not a fact which was first discovered by Hering; it was expressly insisted upon by Leonardo da Vinci, in spite of his familiarity with the fact that blue and yellow paints when mixed give a green paint; a four-color theory, as such, might well be called by his name.

But no less discreditable to the thinking man than the Helmholtz illusion that yellow and white are *sensation-mixtures* is what we may call the Hering illusion (following the rule which holds in the space-sense, where an illusion is named after its distinguished discoverer), the illusion that the four colors which are *unitary* are also *complementary*. It is no less a willful ignoring of plain facts, which are known to every school boy who has ever played with a color-top, to say that red and green are complementary, than it is to say that they are the psychological constituents of yellow. Is this fact, that the colors which are unitary are not the same thing as the colors which are complementary, a fact of too great complexity to be taken account of by the human mind—is there no way out but to deny

<sup>1</sup> The derivations of their color words are as follows: red relates to blood, green to grass, and yellow to the yellow fox (another word for yellow refers to bile, a common source of color words for yellow or green); they could attach no meaning to the word for blue.



it? The color which you must mix with green to make white is purple, the color which you must mix with red to make white is blue-green, and red and green when mixed together make, not white, but yellow; these three facts, which are the very groundwork of our knowledge of color, are alone totally destructive of the theory of Hering, and it is only by rigidly shutting one's eyes to them that the theory can be upheld for a moment. If the Eskimo, with their acute conscientiousness in matters of color, could be provided with color-tops by the next deputation of missionaries which visits them, they would doubtless be able to demonstrate to us how far we are astray in this matter. The theories of Helmholtz and of Hering may be correctly described in these terms:

(1) *A theory which explains some of the physical facts of color, at the cost of wholly ignoring the principal psychological fact of color.*

(2) *A theory which explains some of the psychological facts of color at the cost of wholly ignoring the principal physical fact of color;*

—or better still, perhaps:

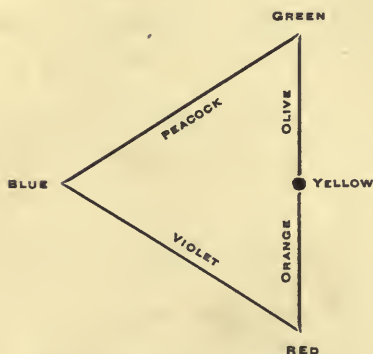
(1) *The theory of the physical color-triangle (which ignores the psychological color-square).*

(2) *The theory of the psychological color-square (which ignores the physical color-triangle).*

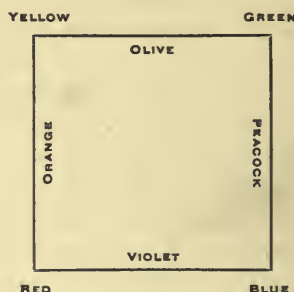
There was never a better case of two rival groups gazing each at its own side of the shield and absolutely refusing to go around and look at the other side. And it would be a difficult matter to say whether it is the ignoring of the color-square (the Helmholtz illusion) or the ignoring of the color-triangle (the Hering illusion) which is the greater crime against the spirit of science. It is probably the latter which is the more self-contradictory. The Helmholtz theory rests its claim chiefly upon color-equations, in the carrying out of which one is simply required to say, 'these two semi-circular color fields look alike.' But the Hering theory purports to restore to its rights our feeling as to what a given color really looks to be; how then, having got us into the mood of conscientious self-inspection, can it ask us to swear that if we flick away a bit of red from a white background, the color that remains behind is green, when everyone knows that it is peacock (a single word for the name of this blue-green is very necessary here); or that, if we look at green, the after-image that results is red, when every one knows that is purple? Either we *can* distinguish between a unitary color sensation or we can *not*; if we cannot, all necessity

for a four-color theory falls away and a three-color theory is good enough; if we can, we cannot (except through our own pure perversity) be juggled into believing that what we add to red is anything but peacock, and that what we add to green is anything but purple, when we wish to produce, for instance, a wholly colorless field in the splendid König-Helmholtz *Farbenmischapparat*, nor that there is any proportion in which a psychological red and a psychological green can be compounded so as to produce a colorless mixture.

Here they are, then, in diagram—the insignia of the Helmholtz and the Hering theories—the Physical Color-Triangle and the Psychological Color-Square—each the emblem of what would be a ten-



The Physical Color Triangle.



The Psychological Color Square.

able working hypothesis, were it not for the invincible stumbling-block exhibited by the other. Helen Keller (who is without doubt the most marvelous human being that has yet existed) has lately said, in a very beautiful passage which I will not quote here, that there are disadvantages connected with the process of acquiring much learning—that, in the first place, one has no time to *think*: it must be that the plethora of facts regarding color-vision has become so burdensome that the adherents of the two rival color-theories have no time to think, for otherwise it is difficult to see how two theories, each of which is absolutely contradicted by the facts which constitute the central position of the other, could either of them continue to exist within a scientific world. Brilliant achievements are constantly chronicled in all other regions of the several sciences upon which the doctrine of color-vision borders—is it possible that here alone there exists a hopeless *impasse*?

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## PSYCHOLOGICAL LITERATURE.

### *Experimental Psychology: A Manual of Laboratory Practice.*

By EDWARD BRADFORD TITCHENER. Volume I., Qualitative: Part I., Student's Manual, pp. xviii + 214, \$1.60 *net*; Part II., Instructor's Manual, pp. xxxiii + 456, \$2.50 *net*. New York, The Macmillan Company. 1901.

"This course aims at two things: first, and more especially, to teach the student to psychologize, and secondly to acquaint him with the most reliable methods and most securely established results of experimental psychology." (Part II., p. xix.) "I have selected a number of the 'classical' experiments of Experimental Psychology, and have tried to present them in such a way that the performance shall have a real disciplinary value for the undergraduate student. Within this general purpose my aim has been twofold. I have sought to show, in the first place, that psychology is above the laboratory; that we employ our instruments of precision not for their own sake, but solely because they help us to a refined and more accurate introspection. And secondly, \* \* \* I have treated the selected experiments not as separate exercises, but as points of departure for systematic discussion." (Part II., p. vii.)

The author announces that the present volume, in two parts, is to be followed by a companion volume, also in two parts, on the quantitative side. Thus the work is divided into qualitative and quantitative, and one student's and one instructor's manual is to be devoted to each. The chapters run parallel in the two parts of Volume I. Part I. contains specific directions for thirty-seven groups of experiments, and Part II. contains discussions of each of these, giving theories, references, results, explanations of technicalities, related experiments, and in general the setting of each experiment. The following subjects are treated: visual, auditory, cutaneous, gustatory, olfactory and organic sensations; the affective qualities; attention and action; visual, auditory and tactual perception, and ideational types and association of ideas. Part I. also contains introductory directions to the student, and Part II. contains suggestions to the instructor, a selected list of books, and a directory of laboratory-supply houses and instrument makers.



These manuals embody the results of a decade's experiment upon the methods of teaching experimental psychology. Their pages show that the long time of preparation has been spent in painstaking and profitable work by the author. His effort receives double significance in view of the opportunity: the subject matter has ripened into presentable form through the efficient efforts of many laboratory workers; the preliminary survey has been made by other manuals and texts, although this will rightly receive the credit of being a pioneer work; and the author is favored with a most excellent laboratory equipment and a desirable class of students to work with. Writing a laboratory manual is not like writing a syllogism. The exercises may seem ideal on paper and yet suffer from the weakness of being fit only for the ideal student. The patience and exhaustiveness with which the author has dealt with the small hitches is highly commendable. He has given us the exercises in the form in which they have been successful in his laboratory. It remains to be seen whether the manuals will serve their purpose as well in the hands of other instructors and in other laboratories. An adequate review can be given only after a thorough trial of the whole plan.

The three most striking features of these manuals are the distinction between qualitative and quantitative experiments, the division into student's and instructor's manuals, and the pedagogical adjustment of the exercises.

"The experiments in this volume are termed 'qualitative' and emphasis is laid throughout upon 'qualitative' as distinct from quantitative work. \* \* \* The student's attention is directed not to the 'How much?' or the 'How well?' but to the 'How?' of mental structure." (Part II., pp. xx, xxi.) Experimental psychology is aided introspection. In the beginning of the modern movement it was found necessary to make the aids the primary object of study and to develop power to govern the conditions of the experiment. The experimental method was then revealed to the few who should become investigators. The reform period has passed and the value of the quantitative method is recognized and understood. Now the facts of psychology are taught to classes by the experimental method. By dividing his work according to the analogy of chemistry and making the first course qualitative Professor Titchener has raised the current emphasis on the qualitative method to a climax. But he does not insist upon a rigid line of demarcation, for such does not exist, and he constantly upholds the dignity of careful and exact methods. In fact, he employs quantitative methods freely and effectively. The author's method and point of view give a richness and refreshing attractiveness to the subject.

The plan of furnishing separate manuals for student and instructor is good. The directions to the student are not encumbered by full discussion of the problem, but the student is permitted to form his own unbiased conclusion, acting upon clear and isolated directions. The instructor's manual is not a 'key' to be locked in the teacher's desk. It is adapted to the use of the student, and here lies its chief value. It is a compendium which the student may study section for section, provided he performs each experiment first. It is a legitimate aid to the teacher and is exceedingly helpful and stimulating. Those who are afraid of acquiring faith in experimental psychology had better not open this book, because on every page of it there stares one in the face the reasonable and enticing challenge, "Try this experiment yourself." He who seeks in this compendium an answer to the question, "Is there anything in experimental psychology?" will never ask the question again.

The cast of the student's manual is calculated to make the student independent of the instructor. The form of the directions is spirited and economical, containing only what is essential for the performing of the experiment. The author has sacrificed uniformity in the length of the exercises for comprehensiveness of view. The instructor must select the points to be tried in a given laboratory period and after trying these the student will profit by reading the rest. It is the author's plan that the present course should be preceded by an introductory course of lectures. But, even with this preparation, the student may make a failure of the course if he is required to perform the experiments *seriatim* without any aid from the instructor. The reviewer has found the plan of conducting every alternate exercise as a *seminar* devoted to the interpretation of the experiments performed in the immediately preceding period very satisfactory. One experiment performed and discussed in its various relations is more profitable than two experiments performed without the systematic interpretation, the time spent being equal in the two cases. These manuals are well adapted for use in such a plan.

The author has here adopted the term 'observer' instead of the term 'subject.' It is unfortunate that so many of the laboratory men in this country have so long persisted in using the term 'subject' which is borrowed from the clinic and the hypnotic séance. The term observer gives respectability to the laboratory student, and that term alone is consistent with the view that the method of experimental psychology is introspection. Where introspection is not employed some other term may be used.

The points that are open to criticism in this work are trivial in comparison with its points of excellence. There are cases of delightful clearness at the expense of fact, as in the adoption of the Hering theory of color-vision, in the adherence to the Helmholtz theory of the function of the parts of the organ of Corti, and in the extent to which he assumes the specialization of function of the end organs in the skin. There is an unnecessary confusion in the use of the terms 'Volume,' 'Part,' and 'Experiment.' Thus, there are two volumes in the series, and each volume contains two parts and each of the parts contains two parts, and there are thirty-seven experiments and each of these experiments is divided into several experiments. Several of the exercises could be improved, but an adequate discussion of that matter would require a special treatise. It is desirable that some one should publish a detailed criticism of the exercises after they have been thoroughly tried. Wherever they are used they must be adapted to the laboratory equipment.

This is an American work. Europe has taught Americans to become investigators in psychology: America is showing Europeans how to teach psychology. The work is well illustrated. Its tenor with reference to philosophy and the sciences to which psychology is related is scholarly and generous. The absence of loose metaphysics and biological speculation is gratifying. The author states theories and freely gives his own view as a starting point. The economy in the use of apparatus in the set exercises is remarkable. The present installment of Professor Titchener's work is the clearest mark of the achievements of modern psychology extant.

C. E. SEASHORE.

UNIVERSITY OF IOWA.

*Les Grands Philosophes: Kant.* Par THÉODORE RUYSSSEN, Agrégé de Philosophie, Professeur au Lycée Gay-Lussac (Limoges). Paris, Félix Alcan, 1900. 8vo, pp. xi + 391. Price, 5 francs.

Inspection of publishers' lists might well afford us much evidence for calling our time the 'Age of Series.' In English we have had Blackwood's 'Philosophical Classics,' edited by Professor Knight; 'German Philosophical Classics for English Readers and Students,' edited by my predecessor, the late Prof. G. S. Morris, and now we have in progress the series, entitled 'The World's Epoch-Makers,' undertaken by the great Edinburgh firm, T. & T. Clark, and handled in this country by the Scribners; a series initiated by one of the best works in the language on 'Luther,' by Professor Lindsay, of Glasgow.



and advertised to contain, amongst others, studies of Socrates, Plato and Aristotle, Marcus Aurelius, Origen, Scotus Erigena, Pascal, Descartes and Spinoza, Hume, Rousseau, Kant, Hegel, by British and American scholars. In Germany, Frommann's 'Klassiker der Philosophie,' edited by Professor Falckenberg, has obtained an enviable reputation in the last year or two, thanks to several brilliant performances, of which Paulsen's 'Kant' may be mentioned in the present connection. And now France follows suit with 'Les Grands Philosophes,' a series edited by M. Clodius Piat, and initiated by him with his excellent volume, 'Socrate.' Professor Ruysen's 'Kant' is the second volume of the series, and a third has now been published on 'Avicenne,' by M. le baron Cara de Vaux, while the well-known M. Henri Joly has a fourth in preparation on 'Malebranche.'

When one opens a new work on Kant to-day, he may be pardoned a good deal of curiosity. What portion of the enormous and unmanageable mass of Kant literature will the author be found to have mastered? What method will he adopt in dealing with the complicated materials furnished by the system? What view will he favor regarding the internal evolution of Kant's thought? What conclusions will he reach with respect to the historical value and permanent significance of the critical philosophy? Such is the contemporary atmosphere that these questions emerge immediately, and will take no denial. Now we know that, till within recent years, Kant was none too well understood, perhaps none too thoroughly studied, in France; accordingly, we may be inclined still to approach French work on the subject with suspicion. But we must call to mind that a complete change has overtaken academic France, in the matter of *Wissenschaft*, since the time, twenty years ago, when some of us enjoyed the privilege of spending a part of our *Wanderjahre* at Paris. To this, it may be said at once, M. Ruysen's book affords eloquent testimony. The author moves with confidence among the relative literature which, in the circumstances, one would expect him to have mastered. And so our first question is answered. Once more, in a general treatment of the Kantian philosophy, drawn for the purposes of a series, the philological and critical methods are ruled out, and the standpoint of history becomes imperative. Thus M. Ruysen furnishes a straightforward account of the historico-intellectual conditions under which Kant labored; of the 'Pre-critical Period,' of the 'Pure Reason,' of the 'Metaphysic of Nature,' of the 'Practical Reason,' of the 'Metaphysic of Ethics,' of the 'Critique of Judgment,' and of the 'Philosophy of Religion'—these being his chapter-headings. Ap-

pendices contain a chronological list of Kant's works, a statement of the principal editions and translations, and, finally, a bibliography, by no means exhaustive, but judicious and careful so far as it goes, and adapted admirably to the purpose of the series. Coming to the third question, regarding M. Ruysen's attitude toward Kant, we find him dissenting strongly from the Neo-Kantian position, from the interpretation which, 'from different points of view, is the opinion of Kuno Fischer and Benno Erdmann' (*cf.* p. 65 f.). On the contrary, M. Ruysen agrees very naturally with M. Em. Boutroux (*cf.* 'Cours sur la Critique de la Raison pure,' dans *Revue des cours et conférences*, 1895-6, pp. 464 f.), in viewing the internal unity of the critical philosophy as its most outstanding feature, in insisting that the three 'Critiques' and the 'Religion within the Limits of Pure Reason,' form a single whole, in which no changes of material moment occur. In this connection he speaks of 'la clef du développement logique qu'a subi sa doctrine après la découverte de l'idée critique' (p. x), and he concludes his volume with the valiant declaration, "A aucun moment du système la science n'atteint la chose en soi, à aucun moment non plus la croyance n'empiète sur le savoir. Le criticisme a donc bien maintenu une opposition radicale entre la science et la foi, le phénomène et la chose en soi, la nature et la liberté. On peut contester la valeur de cette distinction, mais on n'y saurait voir une contradiction interne entre deux moments successifs d'une même pensée; c'est délibérément avec la claire vision du dualisme qu'elle introduisait dans l'esprit et dans les choses, que Kant l'a adopter comme l'hypothèse fondamentale et le principe d'unité de tout son système" (p. 366). M. Ruysen makes no definite attempt to elucidate the historical position of the Kantian system; limits of space did not admit this. Consequently, the commentators are set aside, and an effort is made to let Kant speak in his own words. And so, although M. Ruysen maintains the unity of the Kantian thought, he does not foist an interpretation upon it. Accordingly, in this respect, his procedure is akin more closely to Paulsen's than to E. Caird's. Further, he emphasizes Kant's epistemological point of view in a refreshing way, although, on occasions, he seems to forget that epistemology and metaphysics are hardly identical.

In the body of the exposition I think it evident that M. Ruysen takes the 'Critique of Pure Reason' too easily. That is to say, his presentation of it is too compressed to be satisfying, mainly because he follows out the work in its ramifications; does not attempt to present it by what may be called the problem method, which, all things

considered, seems the better way when space must needs be saved. Of course, ideas of proportion must vary from writer to writer, but I confess that it is hard to see why, in a small work on a big subject, the 'Metaphysic of Ethics' should receive sixty-three pages, the 'Pure Reason' but ninety-four. On the other hand, when the materials are manageable, M. Ruyssen scores a clear success. The chapter on Kant's 'Philosophy of Religion' is an admirable bit of work, and seems to me to show a truer sense of the philosopher's situation than some other discussions, for example, that of the 'Transcendental Dialectic.' One is pleased to note also the care bestowed on the 'Metaphysic of Nature,' a subject to which it will be necessary for Kantian scholars to return ere long. All things considered, it is probable that the book will be more effective for those who know something of Kant already than for a popular audience. It does not suppress enough, especially it displays its methods too fully, for the latter. And this is tantamount to saying that it is an excellent performance, one, in certain respects, clearer and fresher than any other available. It bears striking testimony to the advance of accurate philosophical study in France. What a distance from Victor Hugo or even from Em. Caro, to whom some of us listened, to such definite scholarship as this!

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*What is Thought?* By JAMES HUTCHISON STIRLING. Edinburgh. 1900. Imp. by Chas. Scribner's Sons. Pp. 423.

In the reasoning of daily experience, as in the transformations that make up the deductive processes of science, much depends upon our ability to state the conditions under which we may substitute one term for another with an assurance of their sameness. Looking closely, we see that the conditions sought are those that prevent ambiguous use of a middle term: the conditions under which the 'thing' and the 'itself' of the law of identity may be taken to be indistinguishable. Respecting these, it is clear that deduction is development only in so far as the 'thing' and the 'itself' are not indistinguishable, while the truth of the development would seem to require that a certain sameness between the two be retained. To avoid contradiction the sciences point to 'respects' in which the sameness in question exists, and these 'respects' define the only part of the gross phenomena reasoned about that has been subjected to scientific treatment. There would seem to be legitimate matter for reflection raised by the question: What would a state of knowledge be



like in which this relativity had been overcome? And one would not be without excuse for seeing in this notion of an absolute middle term a way of stating the problem of knowledge with which metaphysics concerns itself.

From such a viewpoint one may be able to catch something of the motives back of Mr. Stirling's phrase: "It is alone the Middle Term that is the entire secret of the universe" (p. 82). If, now, the kind of identity between differentials that is suggested partly by the form, partly by the use of the 'thing'-'itself' relation, could be brought home to us by a familiar experience, one would expect to find this experience playing an important part in the method of a thinker who had stated his problem in the way indicated. To have the problem 'brought home' to one, it is only necessary to state one's law of identity in the first person: "I am myself." "That middle term," says Mr. Stirling, "is the *Ego*" (p. 83), and this is one of the ways in which the hero of the drama is introduced.

If one is content to regard a problem thus 'brought home' as a problem disposed of, one may feel no discontent with such a gradual expansion of the insight gained as is hinted at in the following typical bit of reasoning. Putting as the net result that "thought is the ratio of the *I* to *me*," we proceed to ask: What 'I'? "You yourself are 'I,'" the author explains. "But I, too, when I say of myself 'I,' mean precisely the same thing \* \* \* that you mean; and he, too, when he says of himself 'I,' means precisely the same thing that we both mean. In short, when I say 'I,' when you say 'I,' when he says 'I,' there is but a single *I* between us. Let us call it  $x$ , then this  $x$  does not in the slightest differ in either of the three cases: it is absolutely the same in all of them. \* \* \* The question who? \* \* \* so put to any subject—to God himself—can only be answered by 'I.' To his own self \* \* \* God himself can only be 'I.' He, indeed, it is who said, 'I am that I am,' (p. 42). This, in the end, turns out to be the 'secret of the universe': "To realize *I*—that is the purpose—and that is the history of the universe" (p. 417).

This theme, varied in ways too manifold to admit of reference, is the thesis. Three-quarters of the work is devoted to a survey of the history of philosophy—a survey hasty as to all that precedes Kant, detailed and largely anecdotal as to what concerns Kant, Fichte, Schelling and Hegel, silent as to the very existence of one Schopenhauer. It is in Hegel, particularly in Hegel's treatment of the 'notion,' that philosophy culminates. Only, Hegel, with curious perversity, hid his meaning—that, namely, this 'notion' was nothing

other than the Ego. This was *his* 'secret,' and Mr. Stirling, in divulging it, gives it to us as his own last word.

We have, then, an introduction to, an appreciation and interpretation of Hegel. Apart from the historic context the work is unintelligible: Kant himself could only have read it with bewilderment. As for the scientist, he would fail to detect any analogy between the sequence of ideas here presented and that which he has been accustomed to regard as proof. All of which makes one think that if the admirers of Hegel would give him a place in the really effective thinking of our day they would better devote themselves to bridging the gap between Hegelian dialectic and the processes of reasoning that constitute scientific method, rather than struggle after pleasant ways of restating Hegel's results. What the scientific thinker really craves is the ability to move continuously over the ground of experience; he sees no reason for distinguishing between the labors proper to six days of the week and those sacred to the seventh. But this is exactly the distinction Mr. Stirling insists upon: "Man, in that he is of *sense*, is finite: but man, in that he is of *thought*, is a spirit and infinite. So it is that, if he has his weekday of work, he has not lost his Sabbath-day of religion. And so it is also that, if he has his scientist to minister to the commodity of the finite, he is not without his philosopher to minister to the necessity of the infinite" (p. 51). It would require a much more patient sympathy, a much more penetrating clearness than the author has anywhere displayed to convince the laborer in the 'finite' that in the six days he has not done all that there is to do.

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*The English Utilitarians.* By LESLIE STEPHEN. New York, G. P. Putnam's Sons; London, Duckworth & Co. 1900. 3 Vols. Pp. 326, 382, 525.

This work is intended as a sequel to the author's *History of English Thought in the Eighteenth Century*. The history of the English Utilitarians is regarded by him of special moment because it represents a force which affected deeply the development of modern thought, in its political, moral and philosophical bearings. The three volumes are devoted for the most part to the lives and teachings of three men, Jeremy Bentham, James Mill and John Stuart Mill. In addition there is also mention of Tooke, Dugald Stewart, Malthus, Ricardo, Brown, Beauchamp, Austin, Grote and Buckle, and their several contributions either directly or indirectly to the doctrines of

Utilitarianism. Mr. Stephen, in the preface, draws attention to his special qualification for the task which he has set himself, inasmuch as he had the advantage of being himself a disciple of the school during its last period. This indicates the author's general point of view, and the sympathetic treatment which is given to his estimate of the Utilitarian movement. Mr. Stephen's method is to present the history of the sect rather than any attempt to establish the various arguments for Utilitarianism. To this end he has sought to reproduce the social and political atmosphere which affected the lives and thoughts of the men who may be regarded as the founders and chief apostles of this school. It is needless to say that this purpose has been most admirably carried out. He has very clearly shown that the leaders of Utilitarianism were the product of the existing social order as well as the formulators and organizers of a new regime. The two points of view are given their proper places in his discussion, and the reciprocal relations of social environment and leadership are prominently indicated. The Utilitarian philosophy is essentially the outcome of the practical conditions of human life which confronted the thinking men of the eighteenth century and not the result of speculative theory. For the students of psychology the present work is of special interest because it contains a thorough analysis of the psychological basis of Utilitarianism, both individual and social. It moreover illustrates the scientific method of inductive inquiry, which starts with a systematic examination of the facts of human nature in their concrete setting, and repudiates the 'vague generalities' against which Bentham was wont to vent his wrath.

Beginning with the study of society as it existed at the time of Bentham, the master of Utilitarianism placed such emphasis upon the legal relations of man to man as to absorb morality in a pure type of legality. The difference between legality and morality proper he found to exist only in their respective sanctions. The idea of obligation and that of the moral worth of the motive were both eliminated from the system of Bentham and his followers. Mr. Stephen has shown how, by the very nature of Bentham's point of view and his legal type of mind, the essentially legal system of morality was inevitably developed. Given the age, the man and the specific problem of legislation which he set himself to solve, there was but one logical result that could be expected.

When we come to the life and work of James Mill we find an attempt to ground a philosophy of Utilitarianism upon the foundation of an associational psychology. The idea of obligation, the



worth which we ascribe to certain motives irrespective of the consequences pertaining to the resultant act, were explained in the psychology of Mill by reference to the doctrine of the association of ideas. Indissoluble association was the universal solvent in which the ideas of obligatory morality were completely to disappear. Mr. Stephen's most trenchant criticism is brought to bear upon this attempt of James Mill to explain obligation and the various judgments of moral worth. The author's contention is not that these ideas have absolute worth and are irreducible, but that the lifetime of the individual is far too short to establish such indissoluble associations. Even John Stuart Mill did not appreciate the weakness of the traditional position of the Utilitarians in this respect. Mr. Stephen refers most pointedly to the so-called Utilitarian paradox, 'the indifference to history combined with the appeal to experience.' (Vol. III., p. 373.) He goes on to say, by way of summing up his criticism upon this phase of Utilitarianism: "The difficulty of the Utilitarians was all along that theories of evolution appeared to them to involve something mystical and transcendental. They proposed to analyze everything till they could get to single aggregations of facts, or in their sense ideal, to a thorough-going atomism. This leads to the paradox indicated by Hume's phrase. The atoms, things and thoughts, must be completely separate and yet invariably conjoined. Causation becomes mere sequence or conjunction, and 'experience' ceases to offer any ground for anticipation. I have tried to show how this affected the utilitarians in every subject; in their philosophical, legal, ethical and economical speculations; and how they always seem to be in need of, and yet always to reject by anticipation, some theory of evolution." (Vol. III., p. 374.)

In the development of philosophical thought, it is of special interest to note whenever any controverted questions may be said to reach a final solution. We may mark the doctrine of indissoluble association expounded by James and John Stuart Mill in defense of Utilitarianism, as one of these questions which has been definitely settled. The strategic point of the Utilitarian position has been of necessity shifted from the individualistic to that of a race experience.

In the system of the younger Mill, Mr. Stephen regards the distinction which was introduced concerning 'quality' of pleasures as contrary to the quantitative basis of the Utilitarian system as insisted upon by Bentham. Mr. Stephen, however, does not consider the distinction of Mill's to be a concession to the theory of the transcendental factor in morality. The distinction between higher and lower

pleasures is explained by Mr. Stephen in the following paragraphs:

"As men advance intellectually, intellectual pleasures will clearly fill a larger place in their ideal of life. The purely sensual pleasures will have their value as long as men have bodies and appetites; but they will come to have a subordinate place in defining the whole ends of human conduct. The morality of the higher being will include higher aspirations." (Vol. III., p. 306.) "As the society progresses the individual will himself be altered, and the type which implies a greater development of intellect, sympathy and energy come to prevail over the lower, more sensual, selfish and feeble type. Though happiness is still the ultimate base, the morality applies immediately to the social bond, which contemplates a general development of the whole man and a modification of the elements of happiness itself." (Vol. III., p. 308.)

When we analyze this statement of Mr. Stephen's we see that it in reality implies some ultimate distinction between higher and lower in pleasures that can not be translated into a mere quantitative one. It opens the way to the introduction of a transcendental element in morality which is wholly repugnant to Mr. Stephen's point of view and type of mind. And yet Mr. Stephen's explanation still leaves an irreducible element in the moral consciousness that can not be referred to a strictly Utilitarian origin. Mr. Stephen contends also that there can be no place for religion in a scientific system of ethics. Yet here again Mr. Mill has a clearer insight. As he allows, by implication at least, the possibility of a transcendental element in morality, so also in his posthumous *Essays on Religion* he concedes the possibility of a religion which is the logical outcome of the nature of man when considered in the light of its totality. Mr. Stephen is himself aware of Mill's feeling in this particular; for he says: "Of Mill's position it must be frankly admitted that his desire for a religious and even supernatural belief is a proof of dissatisfaction with his own position. He felt here, as elsewhere, that something was wanting in his philosophy." (Vol. III., p. 451.) This reference to Mill may be taken as a just estimate of the Utilitarian position in general. While there is much to admire in the scientific method of inquiry, in the frank appeal to facts and the earnest endeavor to discover them, in the sincere regard for the sovereignty of the truth, and its compelling power over thought and conduct, in the wide sympathy for the needs and ills of humanity, and in the many other noble characteristics of the Utilitarian school, we are still conscious of 'something wanting in their philosophy.'

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*Le vocabulaire philosophique.* By EDMOND GOBLOT. Paris, A. Colin. 1901. Pp. xiii + 489.

One is surprised that a single individual should attempt, alone, to compass the entire realm of philosophy in a terminological dictionary. The boldness of such a venture will rouse admiration if the effort attain a certain degree of excellence, while it can but excite ridicule if the result fall below the same instinctively felt level. M. Goblot's work happily belongs to the former category; still, we may liken his success to that of an experienced waiter, who manages to meet the wants of a roomful of guests after a fashion—whereas if several waiters had responded the work would have been done with more thoroughness and greater ease. In the same way, we feel that the coöperation of a certain number of specialists is essential to the completeness and accuracy of any philosophical dictionary. Whether the author had any such assistance we do not know; there is no hint of it in the book itself. Of indirect aid, such as citation and use of authorities, there is abundant evidence; but this is satisfactory only where the lexicographer himself is thoroughly familiar with the subject.

In view of the wide range covered, one cannot but feel satisfaction over the general exactness of the definitions (a large number of which from every department were selected as a test), tempered by a shade of uncertainty regarding any crucial points which one might wish to settle by the aid of this work. M. Goblot himself, in the introduction, thus expresses himself regarding the aims and limits of his dictionary: "1. Nous nous sommes proposé de faire connaître aux profanes, aux élèves, aux étudiants—peut-être même aux maîtres et aux philosophes, lesquels sont étudiants toute leur vie,—le sens usuel des mots. Ils devront donc avoir constamment sous la main notre *Vocabulaire*, et le consulter fréquemment, soit en lisant, soit en écrivant. Et nous ne saurions trop leur recommander d'y chercher, non seulement les mots étranges et d'aspect technique, mais surtout les mots les plus familiers de la langue vulgaire, dès que la moindre obscurité se rencontre dans leur emploi; car ces mots usuels reçoivent souvent, en philosophie, une signification toute spéciale, et sont les plus équivoques. \* \* \* 2. Nous avons parfois,—prudemment, timidement même,—proposé des réformes du langage reçu. \* \* \* Le travail que nous présentons au public est, à la vérité, fort imparfait. On voudra bien le considérer comme un essai. Il se perfectionnera, s'il a l'heureuse fortune d'avoir des éditions successives." A mixture of confidence and diffidence which leaves one untrammelled in the formation of his own opinion of the work.



The field which M. Goblot aims to cover is, thus, nothing less than the entire domain of philosophy. It will be interesting to note his conception of the extent of this domain and his ideas as to the relative importance of the several branches included. The themes connected with logic are treated with the greatest fulness. The various forms of judgment and inference, and the endless array of terms associated with these processes by Scholasticism, are given; among the A's alone, *e. g.*, *abduction*, *abstraction*, etc., *absurde*, *per accidens*, *méthode d'accord*, *a contrario*, *in adjectivo*, *alternative*, *ambiguïté*, *analyse*, *antécédent*, *jugement d'antériorité*, *antithèse*, *raisonnement apogogique*, *apodictique*, *argument*, *assertion*, *assertorique*, *axiome*. All the types of syllogism and their formulas (*barbara*, etc.) are discussed. In fact, so far as logic is concerned, the work amounts almost to a text-book. Metaphysics is almost, if not quite, as well represented; the chief ontological and epistemological concepts are included, together with many less important ones, and some possessing merely historical interest. The article on *dme*, one of the longest in the book, discusses the principal theories of the soul, though without doing full justice to so broad a topic. Next in order, perhaps, should be ranked psychology. If the large number of terms relating to mental pathology be included here, the psychological vocabulary will appear extremely full. Yet when we come to examine this topic more minutely we shall find some astonishing omissions. In the evolution terminology the work is very thorough (see especially *évolution*, *variabilité concurrence vitale*, *espèce*). Sociology and philosophy of religion are well represented. It is gratifying to find the basal concepts of mathematics and physics included (*continuité*, *fonction*, *intégration*, *non-euclidien*; *force*, *moment*, *mouvement*, etc.). Terms connected with the anatomy of the nervous system are also numerous, but the ground for inclusion or omission is not clear in many cases. Ethics is the branch which suffers most in treatment. The definitions are noticeably brief; often the ethical significance of a term is hopelessly subordinated to the psychological (*e. g.*, *motif*, *intérêt*, *déterminisme*).

This leads us to speak of another matter in which the *Vocabulaire* is extremely deficient, viz., a broad historical grasp of terminology. We can scarcely criticise the omission of particular school names (Stoicism, Cartesianism, etc.) which cover a whole mass of separate doctrine; we refer rather to the general disposition on the part of the author to regard these terms from a fixed standpoint, instead of tracing them through their successive phases of historical development. Occasional exceptions (*e. g.*, *durée* and *temps*) serve only to accentu-

ate the general practice. The influence of Maine de Biran is too predominant in the discussion of contemporary thought, and that of Descartes in the early modern phases, while the Scholastic interpretation colors too strongly the products of Greek thought. Seldom is there an endeavor to distinguish the meaning of terms as used by different schools of thought; a single authority usually suffices.

On the other hand, too much can not be said in praise of the distinctions, sometimes subtle and original, which the author draws between certain cognate terms. Thus, his suggested differentiation between dissociation and abstraction, making the former a general process and the latter a resolution into mental atoms, might commend itself to English-speaking psychologists as well as French. His careful delimitation of universal and general, on the one hand, as opposed (respectively) to particular and individual (or singular), on the other, might also prove of advantage. So, too, the distinction between *mémoire*, *souvenir* and *réminiscence*, though in part inapplicable to English terminology, suggests a valuable distinction.

The psychological side of M. Goblot's work merits some special attention on our part. And here we find ourselves at once pleased and disappointed. In many respects exhaustive and abreast of the times, there is occasionally an omission so surprising in character as to cause us no slight astonishment. Let us take the sense of vision as an illustration. In physiology we find *adaptation* and *accommodation*, but not *convergence*; in anatomy, *chiasma* and *couche optique*, but not *œil* nor *rétiline*. There is a brief definition of *vision* as 'acte du sens visuel,' and of *vue* as 'faculté de percevoir les sensations de lumière et de couleur,' but *lumière*, *optique*, *couleur* and even *sens visuel* are wanting, though the *sens thermique* and *sens de l'équilibre* are treated; on the other hand, *cécité*, *achromatopsie*, and *hémianopsie* are allowed a place. Under the heads of *binoculaire* and *distance* the visual space perception is discussed, though inadequately, and this is supplemented, under *acuité des sens*, by a discussion of near- and far-sightedness. This will serve to indicate the disproportionate treatment. The articles on *conscience* (consciousness), *attention*, *association*, *sensation*, *perception*, *intellectuel*, etc., are very full, while the affective and motor sides are only imperfectly represented.

The present reviewer will not presume to make an estimate of the terminological distinctions which belong to the genius of the French language; neither will he pass upon the technical exactness of some of the definitions. For the beginner in philosophy and for the general student, the work should prove serviceable; its omissions and limita-

tions, however, prevent its acceptance as an authority in any case; nor is it likely to prove of any great value to the English reader.

We close with an example chosen at random from among the shorter articles; it is typical of the style of treatment adopted in a majority of cases; a few definitions are limited to a single sentence; some discussions extend to 300 words or more, but this is exceptional. "Compossible: Mot de Leibnitz. Un événement qui, pris séparément, est possible, peut n'être pas *compossible* avec le reste de l'univers. Dieu conçoit 'tous les mondes possibles,' des systèmes d'êtres et d'événements qui sont cohérents en eux-mêmes, mais dont chacun exclut tous les autres. Chacun d'eux est donc un système complet de compossibles, et Dieu, qui les connaît tous, choisit *le meilleur*."

H. C. W.

### LOCALIZATION OF BRAIN FUNCTION.<sup>1</sup>

1. *Experimental researches upon the cerebral localization of the tactile, olfactory and gustatory senses.* G. ANDRIANI. Ital. Cong. of Psychiatry, 1896.
2. *Ueber die Entwicklung der Rindencentren.* A. BARY. Arch. f. Physiol., 1898, 341.
3. *Ueber die Lage der motorischen Rindencentren des Menschen nach Ergebnissen faradischer Reizung derselben bei Gehirnoperationen.* W. VON BECHTEREW. Arch. f. Physiol., 1899, 543.
4. *Ueber die sensiblen Functionen der sog. motorischen Rindenzone des Menschen.* W. VON BECHTEREW. Arch. f. Physiol., 1900, 22.
5. *Zur vergleichenden Physiologie des Grosshirns.* A. BICKEL. Arch. f. d. ges. Physiol., 1898, LXXII., 190.
6. *Ueber corticale Reizung der Augenmuskeln.* R. DU BOIS-REYMOND und P. SILEX. Arch. f. Physiol., 1899, 174.
7. *A note on the comparative intellectual value of the anterior and posterior cerebral lobes.* C. CLAPHAM. Journ. Med. Sci., 1898, XLIV., 290.
8. *Cortical motor centres of the Opossum.* R. H. CUNNINGHAM. Journ. of Physiol., 1897-8, XXII., 264.
9. *Beobachtungen an einem Affen mit verstümmeltem Grosshirn.* F. GOLTZ. Arch. f. d. ges. Physiol., 1899, LXXVI., 411.

<sup>1</sup> This article is intended to give a brief résumé and review of the principal articles upon the function of the cerebrum which have appeared since 1895.



10. *Beitrag zur experimentellen Analyse coordinirter Bewegungen.* H. E. HERING. Arch. f. d. ges. Physiol. 1898, LXX., 559.
11. *Ueber Hemmung der Contraction willkürlicher Muskel bei electrischer Reizung der Grosshirnrinde.* H. E. HERING und C. S. SHERRINGTON. Arch. f. d. ges. Physiol., 1897, LXVIII., 222.
12. *Les centres moteurs corticaux du cerveau humain déterminés d'après les effets de l'excitation faradique des hémisphères cérébraux de l'homme.* L. LAMARCQ. Arch. clin. de Bordeaux, 1897, 491, 568.
13. *Ueber die musikalischen Centren des Gehirns.* W. LARIONOW. Arch. f. d. ges. Physiol., 1899, LXXVI., 608.
14. *Cortical Localization in Ornithorhyncus.* C. J. MARTIN. Jour. of Physiol., 1898-9, XXIII, 383.
15. *Sur la physiologie du corps calleux et sur les moyens de recherche pour l'étude de la fonction des ganglions de la base.* D. LO MONACO. Arch. ital. de biol., 1897, XXVII., 296.
16. *Sur la physiologie des couches optiques.* D. LO MONACO. Arch. ital. de biol., 1898, XXX., 198.
17. *Recherches expérimentales sur la physiologie de la couche optique.* J. SELLIER et H. VERGER. Arch. de physiol., 1898, XXX., 706.
18. *Les hémianesthésies capsulaires expérimentales.* J. SELLIER et H. VERGER. Jour. de physiol. et de pathol. génér., 1899, I., 757.
19. *Le lobe occipital et la vision mentale.* J. SOURY. Revue philos., 1895, XL., 561; 1896, XLI., 145, 285.
20. *On the alleged Sensory Function of the Motor Cortex Cerebri.* E. A. SCHÄFER. Jour. of Physiol., 1899, XXIII., 310.
21. *Ueber Rindenreizung am freilaufenden Hunde nach J. R. Ewald.* G. A. TALBERT. Arch. f. Physiol., 1900, 195.
22. *Sur les mouvements des membres produits par l'excitation de l'hémisphère cérébral du côté correspondant.* E. WERTHEIMER et L. LEPAGE. Arch. de physiol., 1897, XXIX., 168.
23. *Ein Beitrag zur Lehre von den Beziehungen zwischen Lage und Function im Bereich der motorischen Region der Grosshirnrinde mit specieller Rücksicht auf das Rindenfeld des Orbicularis oculi.* TH. ZIEHEN. Arch. f. Physiol., 1899, 158.

Since the discovery, about 1870, that the cerebral cortex responds to electrical stimulation, knowledge of the function of the various parts of the brain has been rapidly accumulating. This method, which is of value only for the determination of the brain connection with the musculature, has been supplemented by the earlier extirpation methods and by pathological and embryological data. In the use of this method great care must be exercised that the stimulation produced does not spread to other areas which are not intended to be stimulated, and this great source of error limits the use of the method to the most skilled experimenters.

The extirpation method, a method in which the brain is exposed and a part is cut away or its physiological activity destroyed by cauterization, enables us to collect data regarding not only the motor function, but also the sensory. By means of this method we are enabled to keep an animal alive for many days or even months, to note its actions, and to experiment often for a determination of the character of its sensory, motor and associational processes. The clinical and pathological method, or rather evidence, is closely allied to the extirpation method. Patients may be observed for days or weeks, and after death the abnormal mental conditions are often found to have been concomitant with well-marked lesions in the brain. A histological examination often shows the course of the nerve fiber tracts concerned in these artificially produced and 'normal' pathological conditions.

To these methods Flechsig has added a fourth. He and other investigators have determined the time at which the different parts of the brain arrive at their full development, and the embryological data is further correlated with the development of various bodily and mental processes.

The use of these methods has shown (*a*) that the cortex may be divided into separate areas which control very definite muscular or sensory processes, (*b*) that the frontal, part of the parietal, and the occipito-temporal regions are probably concerned with the association of motor and sensory functions, and (*c*) that an animal may live with almost the entire cerebrum lacking.

The motor area in the brain is found to be in the region surrounding the fissure of Rolando. Towards the lower end of this fissure stimulation produces movements of the head and face, and extirpation causes loss of voluntary control in those muscles. Higher up on the fissure of Rolando is found the area governing the movements of the hand and arm, above which is the center for the leg and foot and towards the mesial aspect of the cerebrum is found the area for the trunk muscles.

Bary (2) has attempted to determine whether or not the motor area can function at birth. Stimulation of the region which in an older animal always produces movements of the limbs, he finds at times ineffective on newborn dogs, cats, rabbits and guinea pigs. On a newborn child Westphal found that stimulation of the brain did not produce muscular movements. If both investigations are accepted, the facts are significant to the psychologist. In the child we may have at first only reflex movements, and later the development of the cortex permits voluntary motions. In most of the animals upon which Bary experimented he found, on the other hand, that movements can be excited by cortical stimulation, the connection of the nerve cells with the periphery being fully established within twenty-four hours after birth.

The early experiments of Fritsch and Hitzig, of Ferrier, of Munk, of Goltz, of Schäfer, and Horsley, and of others showed the general position in the cortex for the movements of the various muscles. The areas for single muscles were not so well investigated, and the attention of the later experimenters has been directed mainly towards the finer localization of the muscular control.

Martin (14), experimenting on *Ornithorhynchus*, found that although the areas for the groups of muscles are well marked these areas could not be separated into areas for single muscles. Cunningham (8) found practically the same for the opossum. His results show that "cortical representation of the fore-limb, of the mouth and tongue, of the muscles of deglutition and of those causing movements of the ear seem to be well marked, although the foci for the individual movements executed in these regions of the body are not sharply differentiated." Precise local differentiation of individual movements does not exist in the opossum or in *Ornithorhynchus*.

In other animals and in man it has been found by Hering (10), by Wertheimer and Lepage (22), by Lamarck (12), and by Bechterew (3) that movements produced by cortical stimulation are coördinated movements, not movements of single muscles. Hering (10) asserts that each motor disturbance which is obtained after extirpation of any brain area is not a simple disturbance but a coördinated muscular disturbance.

The results obtained by Wertheimer and Lepage are interesting in the light of some results in cross education. They find that stimulation of any area, normally producing a movement in the opposite side of the body, if sufficiently strong will also produce a movement on the same side. The authors seem to think that after the nerve fiber has crossed to the opposite side of the cord it may give off a collateral



going to the corresponding muscles on the same side as the cell body. Were this true, 'cross education' would not be 'cross education,' but would be resolved into a direct education of the corresponding part.

The stimulation of the areas concerned in eye movements indicates better than most experiments that the areas for any given muscle may be distinct from others. Ziehen (23) and Du Bois-Reymond and Silex (6) obtained simple movements of the orbicularis oculi and of the recti and oblique eye muscles, but the conditions are not the same as for the movement of other parts of the body.

The results of Wissler and Richardson (this REVIEW, 1900, VII., 29) are in harmony with the idea of centers for coördinated movements, but in all these cases it is difficult to determine whether the coördinated movement is controlled by a distinct center or is due to a nervous discharge over the association tracts. Researches similar to those of Bary and of Westphal, if tried on monkeys of different ages, might determine this point, as well as the development of the formation of simple motor habits.

Most of our experiments on brain functions have been made on animals, and the troublesome question has always arisen, "How far can we apply these results to man?" The question has been answered to a certain extent by Bechterew (3), in his recent confirmation of the great physiological similarity between the brain of the monkey and that of man. On a man who had the brain exposed, Bechterew stimulated the motor region and found that the general order of the motor centers in man in gyri about the fissure of Rolando is analogous to that found in apes. The center for the lower extremity lies in the upper part of the gyrus centralis posterior, the center for the upper extremity in the middle part of the gyri anterior and posterior to the fissure of Rolando. Underneath the center for the upper limb is the center for the thumb and the fingers, and below near the fissure of Sylvius is the center for the face. The center for the side movements of the head and the eyes, as in apes, lies in the posterior part of the second gyrus and in its immediate neighborhood. The center for the thigh muscles lies in the upper part of the convolutions anterior to the Rolandic fissure, above the center for the upper limb. In man, as well as in apes, the center for the thumb and fingers lies near the center for the upper extremity.

After extirpation of any motor center in an animal, for example, a dog, at first there seems to be almost complete paralysis of the muscles connected with that area. If the area for one of the forepaws is excised, the dog is for a few days seemingly unable to move that limb.

This effect may disappear within the course of a few weeks and a certain amount of movement in that limb be reëstablished. In man, paralysis due to lesion of the motor cortex will not disappear. In addition it is often found in cases of paralysis in man that the motor disturbance is often accompanied by a sensory defect—in a loss of tactile or muscular sensations. Schäfer (20) and Bechterew (4) in their recent articles represent the two views held regarding these phenomena: (1) that the cortex about the fissure of Rolando is purely motor, and (2) that it is sensory-motor. Schäfer, holding to the first of these opinions, would probably say that the lesion in man is almost never localized in a small area and that the large area of disturbance may produce many symptoms in addition to the paralysis without there being one center for the different processes. Schäfer's conclusion seems based more upon animal experimentation and upon experiments which appear to the reviewer inconclusive. None of the published experiments upon animals is of crucial value and the view of Schiff and Munk, represented in Bechterew's article, seems at present most in accordance with the facts. Much of the clinical evidence supports the view upheld by Schäfer that the so-called motor cortex is 'motor.' Bechterew, however, reports three cases from his own clinic, which have led him to believe that this region of the brain is not purely motor, but has also certain sensory functions. His conclusion is that "these facts set aside all doubt that skin sensibility and muscular feeling in man have their center in a portion of the brain other than that usually assigned to motor function. This fact is understood when we consider that these two qualities of sensibility are of considerable use in voluntary movement." In conjunction with the sensory disturbances noticed after extirpation of a motor area, it should be noted that stimulation of a sensory area—such as the visual region in the occipital lobes—sometimes produces movement of the organs concerned in the perception. This may be due, it is explained by the holders of 'distinct-sensory-and-motor-areas' view, to the conduction of the impulse to the motor zone by means of the associational tracts. All the data may be explained from both points of view. What seems to be needed most is some crucial experiment, which will permit of only one interpretation. Such an experiment, it seems, should not be difficult to devise.

The sensory functions of the cortex have been determined in animals by extirpation and in man by the study of brain disease. Soury's admirable articles (19) in the *Revue philosophique* review the whole subject of the seat of visual sensations. There seems to be consid-

erable unanimity of belief in considering the occipital lobe as the seat of light perception. Closely connected with this region are found the corpora quadrigemina, the pulvinar and the geniculate body. Soury concludes that the anterior quadrigeminal bodies are not essential for visual perceptions. Lesion in this region does, however, produce a disturbance of eye movements and of pupillary innervation. Degeneration of the pulvinar produces no hemianopsia when the geniculate body is intact, and he thinks that the pulvinar and the quadrigemina are possible reflex optic centers. Sellier and Verger (17) find disturbances of vision after lesions of the optic lobes, some other sensory (tactile) disturbance, but no motor deficiencies.

Lo Monaco (15) noted no disturbances, either sensory or motor, following lesions of the corpus callosum, and electric stimulation was negative. Lo Monaco (15) further noted the effects following lesions of the optic thalami. The thalami have been considered at various times sensory (Luys) and motor (Magendie, Schiff). Injury was said to produce movements of *manège*, but the author did not notice such in animals operated upon by him. Visual defects existed for a month succeeding the operation, but these finally disappeared, as did also the tactile and pain disturbances noted by him immediately after the operation.

Andriani (1), from experiments upon dogs, concludes that the cortical zone in the region of the fissure of Sylvius and in the subadjacent white matter, and in the hippocampal gyrus, comprise the parts and centers for the tactile sense. The disorders noted after ablation disappear, however, in 40 to 50 days. After excision of the posterior Sylvian zone and of portions of the hippocampal gyrus there are found slight transitory disorders of smell but no disorders of taste. After unilateral excision of the limbic lobe and of the marginal gyrus there are bilateral disturbance of taste and transitory disorders of smell.

Lesions of the posterior part of the internal capsule produce, according to Sellier and Verger (18), in limbs of opposite side an incomplete motor paralysis with loss of idea of the position of the limbs, a hemianæsthesia to touch, and a lack of ability to locate pain. These disturbances are transitory like the cortical. They continue for three to six weeks, but later seem to leave no trace.

The main contributions to our knowledge regarding the function of the association areas have come from a study of clinical and pathological material. A recent study by Winkler<sup>1</sup> and the article by Clapham (7) indicate that something may be added from the study of

<sup>1</sup> Article not yet published. The details were reported to the present writer by one who has seen Winkler's work.



the external configuration of the brain. A comparison of a large number of brains of men and women shows a greater development in the second frontal convolution in women than in men, a greater development in emotional men than in non-emotional men, and a lesser development in non-emotional women than in the ordinary woman. These results would indicate that the frontal areas are concerned in emotional processes, as would also the clinical facts. (*a*) the change in character (*wirzelsucht*) and (*b*) the tendency to make puns and foolish jokes (*moria*), which often accompany disease and injury of the frontal lobes. Experiments made by the reviewer on cats lead to the conclusion that the frontal areas in these animals are not concerned in emotional states.

Clapham, considering the following facts, concludes that the occipital lobe is probably more concerned in the intellectual processes than is the frontal lobe—which is usually considered the seat of intelligence: (*a*) In the lower races the occipital lobes are small. (*b*) In vertebrates other than man the occipital lobes are small. (*c*) In the individual this portion of the brain develops late. (*d*) In the insane and in the lowest class of the mentally deficient there is found a very small development of the occipital lobes, while in idiots the frontal lobes are large in comparison with the remainder of the brain. This view is supported by Retzius, Carpenter, Bastian and Hughlings Jackson, and in addition Ireland and Flechsig seem to think that the most important part of the brain for great mental performance lies in the posterior region. The comparison of brains of eminent men with brains of non-intellectual people shows that in men like Gauss and Helmholtz the region between the motor zone and the visual area is more highly convoluted than the frontal areas. This great development of the parietal region is probably to be associated with the great development of association power.

In dogs Larianow (13) has found in the temporal region an area which seems to be concerned with perception of music and lesions of the corresponding region in man have produced *amusie*.

In conclusion it will be well to point out that although our knowledge of the brain has been steadily broadening, the progress during the past five years has been slow and in many ways unsatisfactory. For the present, the discovery of facts seems to be more important than discussion, and it is to be hoped that investigators will soon invade the almost unexplored field to be found in the three association areas, and in the cortical sensory zones.

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## EXPERIMENTAL.

*Rhythmus und Arbeit.* MARGARET KEIVER SMITH. Philosophische Studien, XVI. Bd. 2. Heft. 197-305.

This is a continuation (and conclusion) of the excellent work on the influence of rhythmical action upon the quality and amount of work done, which was reported in a previous number of this journal.

The earlier sections concerned activities predominantly physical in their character; here more strictly mental forms are approached. On the presupposition that the articulation of sounds in syllables would fall within the class of repeated movements and exhibit the results discovered in the earlier series, the writer undertook a set of investigations concerning the influence of the rhythm habit in learning and reproducing letters and nonsense syllables. These were put on an endless slip of paper attached to a uniformly revolving drum and presented in succession before a small aperture in a screen. The observer read these letters and syllables either silently or with enunciation until the series of twelve was memorized. A double test was used, in one form of which the subject recited the whole set of letters or syllables in order, and in the other was required, on the presentation of any element at random, to name its successor. In all experiments the comparison was between the simple rhythm of articulate speech and the complex rhythmic forms of verse meters, Iambic, Trochaic, Dactylic and Anapæstic.

The results are thus briefly summarized. In the reading and learning of such material thus presented there arises a rhythmical habit of repetition which is almost never absent, and which sooner or later falls into some form of meter. In the earlier stages of the process and while attention is strained upon the difficulties of articulation this rhythm is absent; it appears only when a certain degree of facility has been reached in the spoken reproduction of the material. Where these difficulties remain unsurmounted, as in the case of one to whom the tongue is unfamiliar, the rhythm may altogether fail to appear.

In all cases—within the limits of these experiments—the complex rhythm of verse meters was found more favorable to memorizing than simple rhythm, the reason assigned by the subjects being that it facilitated grouping of the material and by thus reducing the number of separate unities made the attention process more easy. The metric accents were found to afford a periodic impulse to the renewing of the attention.

As to the types of rhythmic combinations which gave the best re-

sults it can only be said that the triple rhythms appeared more adapted to the purpose than quadruple, since the Iambic and Trochaic measures were used by three persons each, while the Dactylic and Anapaestic were characteristically employed by only one each. In all cases the rhythm habit manifested a strongly motor character. Where the material was not reproduced in rhythm the subject followed it by inner rhythmic speech. All subjects accompanied the rhythm adopted in learning the syllables by some form of motor reaction which reflected it, by tapping in time with the hand or foot, by nodding the head, swaying the body lightly, and the like.

With one exception the appearance of the rhythm habit was accompanied by a feeling of pleasure, but this overtone was never aroused until the friction of adjustment had ceased and the rhythmic repetition had grown smooth and facile. To conclude, then, the rhythm habit is always present in such processes, it constantly tends to pass over from simple to complex forms, it influences the effect of the process favorably and it is accompanied by a characteristically pleasant feeling.

In her theoretical discussion of results the writer infers the existence of a primitive impulse toward a motor rhythmization of all repeated movements, an impulse which results in a successful coördination of such movements into rhythmic groups when they follow each other at nearly like intervals and within certain limits of rapidity. This impulse is compared to the subjective rhythmization of uniform sound impressions of a particular rate of succession. The reviewer's belief is that the writer might justly have gone a step farther and regarded these cases as not merely analogous but in truth identical, on the ground that the subjective rhythmization depends for its appearance upon the establishment of a rhythmical and metrical system of motor accompaniments. In these acts of learning, the writer concludes, the material of rhythmization is given in this spontaneous impulse to introduce periodicity, but the adoption of a particular complex grouping must be regarded as a secondary conceptual activity designed to increase the grasp of material and release the attention from the strain involved in regarding each element as a separate unity. A short bibliography is appended to the monograph.

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*L'Année psychologique*, 6e année (1899). By A. BINET and others. Paris, Schleicher Frères. 1900. Pp. 774.

The sixth volume of the *Année psychologique* contains a larger



proportion of original contributions than its predecessors. Some of the researches are quite extensive and partake of the nature of monographs. These will be noticed separately in these pages.

M. Ed. Claparède contributes an exhaustive general review on the subject of Agnosia; it includes an historical sketch of the more important advances and a systematic discussion of the literature on various disorders embraced under this generic name, together with a scheme of these forms and an outline of tests for discriminating between them and measuring them; while these tests are, for the most part, gleaned from well-known sources, their selection and arrangement are original with the author and constitute a valuable contribution to the subject. The article concludes with a bibliography of over 180 titles; neither the review nor the bibliography are confined to the year covered by the *Année*.

Professor Binet contributes a general review of literature for 1899-1900 on the relation of plethysmography to psychology. The increasing interest in this subject is shown by the fact that seven important articles are included in the summary, all but one belonging to the year 1899. The same remark applies to the general review, by Professor Zwaardemaker, of current literature on the sense of smell. This includes a *résumé* of eleven articles, published from 1898 to 1900, all of considerable importance.

Among the summaries of particular articles, those relating to vision not unnaturally occupy the most space. Here, as elsewhere, the experimental side predominates. The articles chosen for review are representative of the entire field of experimental psychology, at least, and in view of the impossibility of covering all the literature the selection is judicious. The general bibliography at the end of the book is, as in previous years, by mutual arrangement identical with the *Psychological Index*.

H. C. W.

*Nouvelles recherches sur la consommation du pain, dans ses rapports avec le travail intellectuel.* ALFRED BINET. L' *Année Psychologique*. 6e année. 1899. Pp. 1-73.

In the fourth volume of this annual, Professor Binet published the general results of an investigation, carried on by himself and others, as to the relative consumption of bread by the students of a male normal school in France. He then reached the conclusion that bread-consumption gradually decreased, with some irregularities, during the scholastic year, beginning with October and ending in July, indicating

an impairment of appetite due to prolonged mental effort. A criticism of this conclusion and its supporting method was made in the fifth volume of the *Année* by E. Blum, who found contradictory facts arising in an investigation of his own, and rejected Binet's interpretation that mental work bears any relation to bread consumption, the consumption of other food-stuffs being fairly constant.

The present article presents a study of only the first part of a more careful, exhaustive, and extended series of observations on this complex question of nutrition. It gives the full record of the daily consumption of bread and other foods at the Male Normal School at Auteuil for ten months, from January, 1899, to December, 1899, omitting August and September. This record also includes, imperfectly, the physical exercise and the mental work, and, perfectly, the daily temperature at meal time, the barometric pressure, and the general state of the weather. The consumers in this record are described as strong, robust young men, of a high intellectual caste. The average daily consumption of bread is estimated by the ratio of the number at meals to the total consumption for the day. A special record was made of the 'wasted' bread, which was 10.3 gr. a person, and increased with the increase in consumption. An attempt to find the rate of bread-consumption by the servants of the school during the two months of vacation was not successful.

The elaboration of the present study falls into three parts, marking the efforts to determine 'the influence of meteorological conditions,' 'the influence of conditions of nutrition (as menu, quality of bread),' and 'the influence of psycho-physiological conditions (physical labor, intellectual labor, emotions, etc.).' Extreme care is exercised in these efforts, and a judicious hesitation is displayed in assigning causes for the facts brought to light in the record. After an examination into the traceable influences cited in the first and second groups above, which are found to be very uncertain, M. Binet concludes with the greatest probability that the intellectual labor of preparation for examinations diminishes the amount of bread consumed. The whole outcome of this new study is to confirm the conclusion arrived at by the author in his preceding study.

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*L'audition colorée.* M. DAUBRESSE. *Revue Philosophique*,  
March, 1900. Pp. 300-305.

M. Daubresse offers a few critical remarks, somewhat conserva-

tive and skeptical in tone, on the phenomena of colored hearing, suggested by the fact that since the publication of the second edition of Mendoza's book on the subject, it seems in a fair way to become the property of the general public, especially of the musical public. The unscientific character of many of the reports on colored hearing is pointed out. When, for instance, a person is said to think of an historical period as colored red or green, what, exactly, are we to understand by the statement? When a subject tells us that Sunday is white, does he mean that all objects are white on Sunday or that the word is white? Many of the cases are explainable by assuming the most ordinary processes of association. The whole subject needs much more investigation, and M. Daubresse suggests that artists and musicians may give useful assistance in such a study.

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### FEELING AND ÆSTHETICS.

*Pity.* F. H. SAUNDERS and G. S. HALL. *American Journal of Psychology.* July, 1900.

For largeness of view, literary taste, and fine psychological discrimination this treatment of pity is characteristic of President Hall. In its way it is an exquisite protest against the narrowing tendency of explanation into which a mere experimentalist might easily fall. One reads the article with a growing sense of the largeness of the problem and of the necessity on the part of the investigator of delicate perception and wide experience.

The article is based largely on returns to questionnaires which, despite various objections, do furnish a wide inductive field not afforded by narrower methods. The objects stimulating pity are of course various: hunger, cold, lack of shelter and clothing, weakness, innocence and helplessness, sickness, deformity and death, poverty, vice and crime, old age and various experiences implying lack and want. It is a case of *descending incongruity*, but unfortunately for a certain Spencerian theory not resulting in laughter. The physical symptoms of pity as gathered by Mr. Saunders are important and suggestive, some of them being 'smarting and burning in the eyes that grew wet,' 'loss of control of the voice,' 'a stuffy feeling,' 'heart in the throat,' 'weakness of knees,' 'lips and throat parched,' 'heart seeming to stop,' 'a bursting feeling,' 'a weight in the chest,' 'flow of perspiration,' etc. The pedigree of pity, excessive and morbid pity, the pedagogy of



pity and the supreme rôle it plays in the spread of Christianity are the subjects discussed in the last part of the article. The following comments on the article may not be altogether out of place :

The transmission of acquired characteristics is adduced to explain the genesis of pity where social heredity would amply suffice. For example, the authors suggest that 'the custom, widespread among primitive people, of meals in common and punishment by exclusion from the tribal feast may have so affected our heredity as to give stronger color to our pity for hunger.'

The complicated instinctive reactions of sympathy so thoroughly demonstrated by Sutherland are readily associated with the new objects of the everchanging social environment. The objects in themselves are of course never pitiful, ridiculous or sublime; the reactions make them such, a case of transferred epithet. Hence, the objects of pity (laughter, anger, etc.) depend upon the native and acquired habits of reaction of each particular individual, the associations between stimuli and reactions being largely a matter of experience or education.

In this connection it may be well to point out the error of the doctrine of emotional congruity (James, 'Principles,' Vol. I., pp. 576 ff.) in the phenomena of association. Joyful ideas are said to call up congruous ideas, sad thoughts other sad thoughts, etc. Instead of ideas being joyful, sad, sublime, etc., in themselves, it can be said with much greater accuracy that the association of ideas proceeds in the regular line of habit, and that these ideas are clothed or endued with the emotional phase prevalent at the moment. In the alembic of, say, an angry mood objects usually associated with indifference or regard are suffused with the angry glow, and this process continues until some new stimulus changes the mood. The cortex is in this way the resounding board for all vaso-motor changes.

Of the two classes of emotion, mania and melancholia, pity undoubtedly belongs to the melancholiac or depressed class. The vaso-motor changes are centripetal in nature and only when the pity becomes a *Miteinanderwollen* does it become centrifugal, lose its character of pity and become active sympathy. Pity is clearly a passive form of the complicated instinctive reactions of sympathy and love. Its course tends to the abnormal the more passive the sympathy becomes. The more active it becomes, the more useful it is and the more it verges into the mania-courage-combative mood. The permanence of pity in the world may be regarded from this standpoint.

Pity is thus an outgrowth of the parental and especially of the

maternal instinct, the *Ewig-Weibliche*. This specific and instinctive aspect is also strengthened on the social-heredity side by the clannish tribal spirit of cohesiveness and solidarity. The pity or rather sympathy shown the defective, the insane, the deaf and blind receives its justification in the necessity for social solidarity. Christianity, appealing as it does to the maternal instinct, has accomplished much in the direction of social cohesiveness. Royalty with its armies and brass-band accompaniments appeals manifestly to the combative instinct.

With the increase of theoretical and applied science pity, like fear, will tend to disappear; fear will give place to faith characterized by mathematical precision and prevision, while pity will be superseded by a thoroughly organized *Miteinanderwollen* for social purposes.

Historical epochs in which the cult of pity has flourished have been characterized by depression, seclusion and inactivity so far as occupational work is concerned. The *spirituelle* element of the Middle Ages, crowded out by opposing agencies, exhibited the cult of pity at its maximum. The Crusades were to a large extent Crusades of pity induced, there is evidence to show it, by depressions and famines.<sup>1</sup> The Flagellant movements of history also arise out of plagues, famine, and depression (Hecker, 'The Black Death'). An historical psychology is needed as a corrective of statistics gathered on a small scale.

The paralysis of pity and the repentance of the sinner are alike signs of depression, divided motives and defective nutrition. Pity reaches its climax in the laughter of the soul at itself. We are all 'miserablists' in our fits of depression. As *Heimweh* is now known to be a disease, so pity may probably be shown to be malnutritional in its nature. As the fear of lightning and thunder are not 'ancestral reverberations,' but are due to changed atmospheric stimuli, so pity may always involve a 'bowels of compassion' theory for its explanation. The most suggestive paragraphs in the article deal with pity accompanying the depression of twilight and the decadence of autumn.

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*Zur psychologischen Analyse der ästhetischen Einfühlung.* STEPHAN WITASEK. Zeitschrift für Psychologie u. Physiol. d. Sinnesorgane. Bd. 25. Sonder-Abdruck.

The concept of *Einfühlung*, like the cognate ideas of *Scheingefühle*, *bewusste Selbst-täuschung*, is a necessary concession of psychol-

<sup>1</sup> Fritz Curschmann, 'Hungersnoete im Mittelalter.' Ein Beitrag zur deutschen Wirtschaftsgeschichte des 8-13 Jahrhunderts. Teubner, 1900.

ogy to the metaphor-making tendency inherent in æsthetic reflection. It is precisely because the problem of the subject-object relation presents itself at every step in æsthetic theory that it is impossible to dispense with terms which by their very metaphor express their function as limiting terms in psychological analysis. Says Lipps: "Der ästhetische Genuss [for him based upon *Einfühlung*] liegt begründet in dem Einklang des Eigenen und des Fremden, in der durch die Einwirkung von aussen geweckten und durch die Einstimmigkeit mit ihr gesteigerten und in sich selbst frei gemachten Bethätigung meines eigenen Wesens, in dieser eigenen Art in einem objectiv bedingten Erleben mich selbst frei auszuleben." In the meantime this term *Einfühlung*, for which it is difficult to find an English equivalent, although it describes a phenomenon familiar enough, contains many psychological difficulties, and the vigorous study by means of which Witasek has sought to reduce metaphór to analysis gives a hope that it may now have more definite psychological meaning.

*Einfühlung*, reading one's own affective content into other persons or into things, whether ethical or æsthetic, involves, first of all, the transference of feeling (*Gefühlsübertragung*), the objectification of feeling in the sense that it becomes part of the objective content of presentation. Can a feeling, as such, be thus objectified and become part of presented content? Witasek answers the question in this somewhat paradoxical way: Only when it becomes idea. Only by becoming a presentation and subject to the laws of presentation and representation can it become objective content. Against the view of Lipps, which he calls the *Actualitätsansicht* (the sense of which is that its feelings carried over, read into the object, are actual affective reactions of the subject, conceived to be part of the object, because of a temporary identification of subject and object, through conscious or unconscious self-illusion), is developed the *Vorstellungsansicht*, which looks upon the feelings thus read in as ideal feelings, abstractions from real feelings and subject to the laws governing ideas and not to those of feeling.

The grounds for this position are to be found in an analysis of various æsthetic states, where he points out that neither the presentations themselves nor the ideas associated with them through imagination have the reality coefficient, the element of judgment necessary to make them the presuppositions of real feeling. The laws of feeling reactions require that the idea which calls it forth shall have a real and immediate relation to the organism. Neither is it psychologically conceivable that a fiction should take the place of reality, nor does an



analysis of æsthetic states themselves describe any such fictitious identification. The *Vorstellungsansicht* conceives the ideal feeling, therefore, as of such a nature 'dass in die complexe Vorstellung die den ästhetischen Gegenstand in seiner Gänze darstellt, auch die Vorstellung des Gefühls mit eingeht,' and together with the other presentations with which it is associated it becomes the presupposition of real feelings.

What is this ideal feeling? Witasek analyses the feeling-complex (emotion, in English terminology) in the accepted way, into the algedonic element, the ideal content and organic sensations. And last and most important is 'die Art des zeitlichen Ablaufes des Complexes und die dabei obwaltenden Intensitätsverhältnisse.' It is this latter characteristic of the individual feeling-complex which makes it capable of being intuitively presented. This aspect of a feeling-complex, though not capable of being presented independently of the content, is the characteristic element which makes the feeling capable of being presented with object content in a perceptual series, such as tones, relations of light and shade, gestures, etc., where it becomes the 'fundierter Inhalt' of the series (p. 43). Witasek then proceeds to show how such an 'ideal feeling' is capable of fulfilling all the laws of feeling-association, assimilation, abstraction—and of becoming itself a presupposition of feeling.

This theory of the presentation and representation of feelings is in accord, at many points, with the views advanced by the present writer in the article on the 'Problem of a Logic of Emotions,' in the May number of this REVIEW—views, it may be added, which, though developed independently of Witasek and before the article came into the hands of the writer, were yet influenced by the same thinkers, Meinong and Ehrenfels. Consequently our criticism shall concern itself with only one point of difference in the two views. This point is, however, fundamental. In the first place, Witasek conceives that in this 'Abbild' or ideal feeling, the affective, pleasure-pain element is contained in representation only (p. 9). Just what a representation of pleasure and pain may be, except, perhaps, a conceptual judgment that the state *has been* pleasurable and painful, it is difficult to see. Immediate pleasure and pain is obviously for Witasek, as for other psychologists, the criterion of the real feeling, as distinguished from the ideal. But memory or representation of a pleasure or pain, as such, is precisely the difficulty which proves insuperable to the critics of the theory of affective memory. It seems unnecessary, however, to make this absolute distinction between real and represented feeling. Rather is the

feeling in representation to be looked upon as an abstraction, in which the organic sensations with their hedonic tone still remain, but so dampened as to make the ideal relations of the content, the temporal and intensity relations, the 'dynamic constant' of the present writer, predominant and the part of importance to the attention, to be connected with other objective content. Thus the organic sensation and hedonic elements may be considered as present in the same way that, in abstract concepts, motor tendencies are still present though the processes of abstraction have inhibited them.

It is just at this point that Witasek has failed; he gives us no hint of the process by means of which feelings are represented to consciousness—how from being immediate and *real* feelings in his sense, they are projected into relations with ideal content. Here the doctrine of abstraction and generalization of emotions would be in place. And in reading the numerous illustrations of *Einfühlung* in the arts, with which Witasek has enriched his discussion, it was interesting to see how the feelings described were predominantly emotional abstracts in the sense here referred to. It is true that the too conventional gestures in acting do not bring up the true idea of feeling, but it is also certain that the effective emotional expression on the stage is the result of an abstraction of numerous particular emotional tendencies, an abstraction which has made clear and definite the 'dynamic constant' or system of time and intensity relations.

Witasek concludes with a general discussion of the relation of the æsthetic psychosis to *Einfühlung*. He opposes Lipps in his identification of the two. Not all *Einfühlung* is æsthetic, for it is present in the ethical and social relations of sympathy, as well. While *Einfühlung* in this case is not always pleasurable, the æsthetic always is. On the other hand, there may be æsthetic pleasure without *Einfühlung*, as in the enjoyment of saturated colors, of simple tones, geometrical figures, etc. An emotional theory of art might, however, properly question the right of this sensuous pleasure alone to call itself æsthetic.

WILBUR M. URBAN.

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*La Musique des Couleurs.* L. FAVRE. Bibliothèque de Méthodes dans les Beaux-Arts. Paris, Schleicher. 1900. Pp. xiv + 112.

The aim of this book is to show that a 'music of colors' analogous to the music of sounds is possible and practicable. It leads up to the problem through brief general discussions of 'vibration,' 'emo-

tion,' 'invention,' the 'nature of art,' etc. Painting makes use of fixed colors. M. Favre believes that we may have also an art of colors in motion—a regulated art, not merely the agreeable play of colors as seen in electric fountains, pyrotechnics, ballets, etc.

Such an art would develop in two directions: The play of colors in space and the play of colors in time. As in sound, we would have differences of pitch, of intensity, of duration, of timbre. We would have also a 'melody' and a harmony of colors.

He finds the determination of the scale and the discovery of consonant and dissonant notes a difficult matter. Our lack of training is, he believes, responsible for our inability to judge accurately in such cases. Much would have to be determined arbitrarily as in the history of musical scales.

In the development of the music of colors he would make use of the *Leit-motif*, after the analogy of the music of Wagner.

It is only fair to say that M. Favre recognizes the fact that analogies may be used to excess.

Most of the points he makes are to be found in the article on 'Color Music,' by William Schooling, in the *Nineteenth Century*, Vol. 38, p. 125. M. Favre had discussed the subject still earlier in his *La Vérité*, 1889. His present work is a suggestion of problems and not their solution.

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## MEMORY.

*Experimentelle Untersuchungen über die Gedächtnissentwicklung bei Schulkinder.* ALEXANDER NETSCHAJEFF. Zeitschr. für Psychologie u. Physiologie d. Sinnesorgane. Band 24, Heft 5. Pp. 321-351.

The investigation covered by this paper—an account of which Dr. Netschajeff gave at the recent meeting of the International Psychological Congress at Paris—was carried on during the past year in the schools of St. Petersburg. In all, 687 children—494 boys and 193 girls—ranging in age from 9 to 18 years, were tested. Six different schools were represented in the test: an elementary school for boys, an elementary school for girls, a Realschule for boys, a Gymnasium for girls, and a private institution for boys and one for girls. With one exception (a school of the older boys), all the tests were made in the early morning session, that the element of fatigue might be eliminated. Preliminary to the investigation, the eyes of the children were tested that they might be seated to the best advantage during the experiments.



Eight separate tests were made: In the first, twelve objects (newspaper, handkerchief, lantern, etc.) were shown the children. Each object was exposed for five seconds, and two seconds elapsed between the different objects exposed. At the conclusion of the test, the children were required to recall the names of the objects they had seen. In the second test, twelve sounds were produced (the tearing of a piece of silk, clapping of hands, note sounded on a trumpet, etc.). In the third test, twelve numbers were pronounced (27, 54, 76, 11, 69, 23, 71, 37, 83, 24, 95, and 48). In the fourth test twelve common words of three syllables with marked visual imagery were pronounced (koláska, wagon; ósero, ocean; etc.). The fifth, similar to the fourth, with twelve words of auditory imagery. The sixth test contained twelve three-syllable words with pronounced taste, temperature and pressure imagery. In the seventh, the words contained marked emotional associations, as sabóta (sorrow) and ssomnénje (doubt). In the eighth test the words were all of an abstract character, as déjstvie (activity) and prosstranstivo (space). The tests were all conducted in the manner of the first.

Dr. Netschajeff's conclusions, briefly summarized, are as follows: (1) Memory follows pretty closely age lines in its development. In general, there is increase in power to recall with advance in years, excepting, of course, the slight retardation which comes at the puberal period. (2) The character of the word has very much to do with the ability to recall. Boys, for example, have stronger memories for actual experiences, and girls the stronger memories for words and numbers. (3) There is marked parallelism in the memory for figures and abstract words. (4) Individual differences were most pronounced in the tests with words of a concrete and emotional character, and least with numbers. (5) The girls, with slight exceptions, exceed the boys in their ability to recall, the most marked sex-differences falling between the years from 11 to 14. (6) In subsequent tests with the same children Dr. Netschajeff found a rather high average of memories of the visual and motor types, and a rather low average for memories of the auditory type.

WILL S. MONROE.

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*Memory.* By F. W. COLEGROVE. With an introduction by G. S. HALL. New York, Henry Holt & Co. Pp. 367.

The statement of President Hall in the introduction to the book is confirmed by further perusal: "Despite the many special labora-

tory, clinical and other studies of memory and countless theoretical exploitations of the subject, a plain, simple, scientific handbook was still lacking, so that from the practical point of view, as well as from the service always rendered by juxtaposing and comparing different aspects of great subjects, such a book as this is very opportune."

Recognizing the aim of the author, to make a study of memory or memories by the inductive method and to state the facts in accordance with the latest scientific research, yet in language so simple as to be easily understood, we must say he has accomplished his purposes fairly well. The book is not meant to be an exhaustive treatment of the subject, and hence many critical points need not be mentioned. The historical orientation is very short and sketchy. Collins and Bastian as well as many others are not noted in the chapter on Aphasia. The so-called strange feeling of familiarity in recognition is not noted nor is the problem of recognition in general treated. I fail also to see any thorough distinction between imagination and memory. Kennedy's thorough *résumé* of the experimental work on memory is not mentioned.

ARTHUR ALLIN.

UNIVERSITY OF COLORADO.

*Le Problème de la Mémoire; Essai de Psycho-Mécanique.* P. SOLLIER. Paris, Alcan. 1900. Pp. 218.

The work is composed of five chapters; the first reviews the varying opinions of different scholars concerning the mechanism of memory. Questions concerning the seat of memory, partial memories, reproduction, recognition and aphasia are discussed. In the next three chapters the act of memory is analyzed into three stages, conservation, reproduction and recognition. In the fifth chapter the author attempts a theory of memory. M. Sollier seeks to explain memory more fully by the application of the laws of physics to the psychic. He attempts to establish *analogies* between the phenomena of memory and purely physical phenomena. He comes to the conclusion that the seat of memory, as he calls it, is to be found in the region of the 'centres préfrontaux,' and that here the 'évocation des souvenirs,' the 'conservation de l'état dynamique correspondant aux diverses impressions' takes place.

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UNIVERSITY OF COLORADO.

## MOTOR PHENOMENA.

*Creeping and Walking.* AUGUST W. TRETTIEN. American Journal of Psychology, XII. 1900. Pp. 1-57.

A questionnaire was sent out calling for information regarding the early locomotion of the child. The first mode of locomotion is most often some species of creeping (60 per cent. of the 150 children reported on), but sometimes hitching (30 per cent.), rolling (7 per cent.), or even squirming (3 per cent.). After the child has learned to creep, and before he learns to walk, he often shows a strong inclination to climb, and develops remarkable agility at it. This is made possible by the early development of the arms as compared with the legs.

The most important question discussed is as to the way in which the child comes to walk. The author is able to contribute several fresh instances in which the child apparently did not *learn* to walk, but—at some stimulus—suddenly started off and walked, often so absorbed in some object as to seem quite unconscious of his movements. But other cases were reported in which imitation and instruction seemed to help the child. The author does not agree with Bain that walking is a pure instinct, independent of consciousness. Nor would he call the first steps voluntary. He conceives the first act of walking as an *ideo-motor* process. The child's attention is attracted by some object; the sensations from the object start a motor discharge, which follows paths provided in the nervous system and gives rise to the appropriate reaction. Consciousness is absorbed by the object, by the sensations; if it turns to the movements, coördination is apt to be lost, and the child falls.

After the first few steps are made, the child tends to walk a great deal. A bad fall or fright may inhibit this tendency and produce a temporary reversion to creeping. Except in preventing such accidents, or in helping along cases of 'post-maturity of the instinct,' the intervention of adults is of doubtful value, and sometimes of injury.

*On the Correlation of Mental and Motor Ability in School Children.*

WILLIAM CHANDLER BAGLEY. American Journal of Psychology, XII. 1901. Pp. 193-205.

About 200 children were tested in strength, speed, accuracy and steadiness of movement, and in various compound reaction times. Their school standing (excluding motor studies such as writing and drawing) was also obtained. Correlations between these different



kinds of measurements were then studied. The general result, as stated by the author, is an inverse correlation between mental and motor ability. This conclusion should, however, be accepted with considerable caution. The author himself advises caution, on account of the relatively small number of cases. Besides, the inverse correlation is by no means strongly marked. It is seen in but three of the six motor tests, the other three giving an indifferent result. This shows incidentally that there cannot be a high degree of correlation between the different measures of motor ability, that term really covering a large number of different abilities, not proportionately developed in different persons, and not correlated alike with mental ability. And the latter term, too, covers a number of separate kinds of ability, as is seen in the author's own result that there is no correlation between school standing and mental reaction times. For these reasons there is little use in attempting to give each person an average rating in mental ability and in motor ability, and then correlating the two.

Motor ability increases with age. School standing, in the author's tables at least, shows an opposite tendency. It is easily suggested that this factor of age may so affect the statistical result as to produce the slight inverse correlation observed. The author has met this objection by a certain arrangement of the material. But as to what the result is, the reader cannot satisfy himself, on account of typographical or arithmetical errors in the table.

*Beiträge zur Lehre von der Lage- und Bewegungsempfindungen.*

F. KRAMER and G. MOSKIEWICZ. *Zeitschrift für Psychologie*, XXV. 1901. Pp. 101-125.

This paper has a peculiar sort of interest for American psychologists, because it is written in apparent ignorance of several papers by American writers who have treated the same subjects much more fully and in most cases with the same results. The one point of disagreement concerns the application of Weber's law to the perception of movement. The authors find the error in reproducing or judging a movement to be very nearly proportional to the extent of the movement. The error does indeed increase a little more slowly than the whole extent, but only a little. This work cannot, however, be compared in thoroughness to that of Fullerton and Cattell, who found the error to increase much more slowly than the whole movement. This new material is comparatively slight in amount, and much less exact in procedure and treatment. On repeating the authors' experiments, the undersigned found that in his own case the

error increased much more slowly than the whole movement, and also that the procedure contained a probable source of error. The reproduced movement, instead of starting from the same point as the 'normal,' started at the end of the normal, of which it formed a prolongation. Any such change of position makes the reproduction less accurate, as the authors themselves recognize in another connection. Now the longer the normal, the more the reproduction will be displaced, and the greater will be the difficulty of comparing the two. (This statement can be tested by making two *short* movements some distance apart. It is more difficult to keep them equal than if they are close together.) The author's procedure thus tends to exaggerate the errors of the long movements.

Another part of the article is concerned with the constant errors that appear when the movement is reproduced in a different position from the normal. Most of the facts mentioned in this connection have been previously published by Delabarre or by the present reviewer.

On the subject of the perception of position some new matter is brought forward. Even here, indeed, the form of experiment is the same as was used by Bowditch and Southard in studying another side of the subject: the finger is placed at some point in front, taken away, and replaced as nearly as possible—all without using the eyes. The results were: (1) Practice effected a great improvement, which reappeared in nearly full force after a year of no practice. (2) The accuracy was much greater when the subject made both movements actively than when his hand was guided first to the point by another person. In the former case, the sensations of movement assist those of position. (3) The sensations on which we depend for the perception of position cannot be those of muscular tension, since the accuracy is not diminished by requiring the second movement to be made against the resistance of a small weight or rubber band. (The reviewer is, however, unable to find this true in his own case.) (4) The perception of position is much more accurate within the familiar space in which the hand is customarily held in writing than in space further forward or further to the side.

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## COMPARATIVE.

*The Mental Life of the Monkeys; An Experimental Study.*

EDWARD L. THORNDIKE. Monograph Supplement No. 15 to the PSYCHOLOGICAL REVIEW. New York, The Macmillan Co. May, 1901. Pp. 57.

This monograph recounts the results of extended observations and experiments with three *Cebus* monkeys. The chief aim of the research was to answer these questions: (1) How do the monkeys vary from the other mammals in the general mental functions revealed by their methods of learning? (2) How do they, on the other hand, vary from adult civilized human beings? Incidentally, the report presents some facts concerning their instinctive tendencies and numerous, exact records of what things they learned, and how, and how fast.

The methods used were the same in principle as those of the 'Animal Intelligence; An Experimental Study,' but were extended and varied in several ways. The monkeys were tested with a variety of mechanical contrivances, the operation of which brought them food, and also with a variety of pairs of signals, successful discrimination of which was rewarded by food.

They were shown to possess the typical animal form of learning by the selection of certain impulses to act from amongst a number and their association with certain situations by reason of the success they bring. With the monkeys, however, the association is both more rapid and more permanent, and the approach to suddenness and definiteness in their learning simulates that of human beings. This rapidity and suddenness is even more notable in the cases of learning to react differently to two slightly different signals. The records in the latter experiments are the nearest to evidence of the presence of percepts and images in the animal mind of any of the verifiable facts of animal behavior. The existence of any *general* stock of percepts and images of use in determining the monkeys' reactions is denied by their failure to succeed by force of imitation or passive experience of the movements where they had failed by the typical animal method of learning by the selection of unforeseen successes. Common anecdotes of imitation receive no support, though equally no disproof, from the histories of the three animals tested.

Regarding the comparative position of the monkeys in a mental phylum I may quote from the monograph:

"The monkeys represent progress in mental development from the generalized mammalian type toward man:

"1. In their sensory equipment, in the presence of focalized vision.



"2. In their motor equipment, in the coördinated movements of the hand and the eye.

"3. In their instincts or inherited nervous connections, in their general physical and mental activity.

"4. In their method of learning or associative processes; in

"a. Quicker formation of associations,

"b. Greater number of associations,

"c. Greater delicacy of associations,

"d. Greater complexity of associations,

"e. Greater permanence of associations.

"The fact of (1) is well known to comparative anatomists. Its importance in mental development is perhaps not realized, but appears constantly to a systematic student.

"(2) is what accounts for much of the specious appearance of human ways of thinking in the monkeys and becomes in its human extension the handy tool for much of our intellectual life. It is in great measure the prerequisite of (4) (c).

"(3) accounts for the rest of such specious appearances, is at the basis of much of (4) (b), presages the similar, though extended instincts of the human being, which I believe are the leading efficient causes of human mental capacity, and is thus the great mental bond which would justify the inclusion of monkeys and man in a common group if we were to classify animals on the basis of mental characteristics.

"(4) In their method of learning, the monkeys do not advance far beyond the generalized mammalian type, but in their proficiency in that method they do. They seem at least to form associations very much faster and they form very many more. They also seem superior in the delicacy and in the complexity of the associations formed and the connections seem to be more permanent.

"This progress may seem, and doubtless will to the thinker who looks upon the human intellect as a collection of functions of which ideation, judgment and reasoning are chief, to be slight. To my mind it is not so in reality. For it seems to me highly probable that the so-called 'higher' intellectual processes of human beings are but secondary results of the general function of having free ideas, and that this general function is the result of the formation after the fashion of the animals of a very great number of associations. I should therefore say, 'Let us not wonder at the comparative absence of free ideas in the monkeys, much less at the absence of inferences or concepts. Let us not wonder that the only demonstrable intellectual advance of

the monkeys over the mammals in general is the change from a few, narrowly confined, practical associations to a multitude of all sorts, for that may turn out to be at the bottom the only *demonstrable advance of man*, an advance which in connection with a brain acting with increased delicacy and irritability, brings in its train the functions which mark off human mental faculty from that of all other animals.'"

THE AUTHOR.

## ANTHROPOLOGY.

*On the Relation of the Cephalic Index to Height, Weight, Strength, and Mental Ability.* D. A. SARGENT. Jour. Boston Soc. Med. Sci., IV., 6. 20 Feb., 1900. Pp. 135-143.

Doctor Sargent of the Hemenway Gymnasium of Harvard University, by the research of which this article is a report, has apparently added probability to the long-standing supposition that long-headed, dolicocephalic, people differ as a rule in certain significant ways from persons who are short-headed, brachycephalic. It is interesting to recall in this connection an expression once common in New England speech, "He's a long-headed chap!", meaning always the possession of cleverness, of *savoir faire*. It is a vindication of this wisdom of a character-reading race that is offered here, a corroboration also of the results obtained in Europe by Ammon and by Beddoe.

The statistics in this case, none too numerous, come from eleven hundred male Harvard students who had recently been physically examined in the routine way of the Gymnasium. The records were divided into groups according to cephalic index, those with an index of 75 or less being classed as dolicocephalic, while those with an index of 83.33 or over were called brachycephalic.

The mean height of the dolicocephalics was found to be 175.5 cm.; and of the brachycephalics 171.5 cm. The mean weight of the former was 63 kilos and of the latter 61.5 kilos. The mean strength of the dolicocephalics was 575 points, and of the brachycephalics 550 points. (The strength-test applied was that in ordinary use, *viz.*: The number of kilos lifted with back and legs straight, plus the number lifted with the legs bent, plus the grip of both hands, plus the expiratory power, plus one-tenth the body weight in kilos, multiplied by the number of times the individual can raise his weight by dipping between parallel bars and pulling his weight up to his chin on the horizontal bar—this number is the number of points made.) "Over 5% of the dolico-

cephalics surpassed a strength test of 800 points, while only 5% of the brachycephalics surpassed 700 points." Here certainly is good evidence that at Harvard the long-headed students are better physical men than the short-headed students, a conclusion which is borne out further by other statistics as to health, etc., which need not be quoted in detail here.

As regards intellectual status, the results, it seems to Dr. Sargent, are in a like direction: he considers the dolicocephalic type on the whole superior, though not in academic scholarship. The reason for this exception suggested by the investigator is that the long-headed students devote less time to their studies than do those who are short-headed. He finds that of the dolicocephals, 6% received an average rank of A, 30% of B, 54% of C, and 9% of D, 'and some had dropped out of college' (*how many* is not stated). Of the brachycephals, 10% had received an average rank of A, 37% of B, 44% of C, 8% of D and 1% of E.

Dr. Sargent concludes thus: "How far the results of this investigation have tended to confirm or refute the conclusions of the European anthropologists as to the general superiority of the dolicocephalic over the brachycephalic types, I must leave my readers to determine. The facts brought out are certainly very suggestive to my mind, and I regret that the brief time that can be allotted to this paper will not admit of further elaboration. If we have in our midst a type of man who is superior in height, weight, strength, and energy to all other racial types, it is an exceedingly interesting fact. It is also both interesting and instructive to note that the qualities that make the dolicocephalic type superior have endured the shock and strain of hundreds of generations and are still so persistent as even to defy, in some instances, the disadvantages of bad nurture and poor environment. It is also encouraging to note that, if we include the sub-dolicocephals, this type probably predominates in all our higher institutions of learning, though it would be hardly safe to base this conclusion upon the investigations at Harvard alone."

That in a certain class of young men the long-headed persons have more physical power and vigor seems to be almost demonstrated by this and similar work, but to claim that from such statistics anything valid can be made out as regards intellectual values seems at present an unwarranted presumption. A man's mind cannot be graded in any such arithmetic way.

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## NEW BOOKS.

*Experimental Psychology: A Manual of Laboratory Practice.*  
EDWARD BRADFORD TITCHENER. Vol. I.: Qualitative Experiments. Part II.: Instructor's Manual. New York, The Macmillan Co. 1901. Pp. xxxiii + 456.

*Malebranche.* HENRI JOLY. (Grand philosophes series.) Paris, Alcan. 1901. Pp. xii + 296.

*The Mediterranean Race: A Study of the Origin of European Peoples.* G. SERGI. London, Walter Scott. (Imported by Charles Scribner's Sons, New York.) 1901. Pp. xii + 320.

*Seventeenth Annual Report of the Bureau of American Ethnology,* 1895-96. J. W. POWELL. Washington, Government Printing Office. 1898. Part I. Pp. xciii + 468.

*Eighteenth Annual Report of the Bureau of American Ethnology,* 1896-97. J. W. POWELL. Washington, Government Printing Office. 1899. Part I. Pp. lvii + 518.

*The Limits of Evolution, and Other Essays Illustrating the Metaphysical Theory of Personal Idealism.* G. H. HOWISON. New York, The Macmillan Co. 1901. Pp. xxxvi + 396.

*The Human Nature Club: An Introduction to the Study of Mental Life.* EDWARD THORNDIKE. New York, London and Bombay, Longmans, Green & Co. 1901. Pp. viii + 235.

*Atlas and Epitome of the Nervous System and its Diseases.* (Saunders' Medical Hand-atlases.) Professor Dr. CHR. JAKOB. Trans. from the second revised German edition. Edited by EDWARD D. FISHER, M. D. Philadelphia and New York, W. B. Saunders & Co. 1901. 83 plates and 218 pages text.

*Maine de Biran; Ein Beitrag zur Geschichte der Metaphysik und der Psychologie des Willens.* A. KÜHTMANN. Bremen, Max Nössler, 1901. Pp. viii + 196.

*Observations psychologiques sur le spiritisme.* TH. FLOURNOY. (Communication au IVe Congrès International de Psychologie.) Reprint. Paris, Alcan. 1901. Pp. 11.

*The Limits of Reliable Memory.* JOSIAH PHILLIPS QUINCY. (Reprint from Proc. of the Mass. Historical Soc.) Cambridge, John Wilson & Son. 1901. Pp. 10.

*Ethics.* Vol. III. *The Principles of Morality and the Departments of the Moral Life.* WILHELM WUNDT. Trans. by MARGARET FLOY WASHBURN. London, Sonnenschein; New York, The Macmillan Co. 1901. Pp. xii + 308.

## NOTES.

DR. ROBERT MACDOUGALL, of Harvard University, has been appointed to the chair of experimental psychology in New York University.

C. N. McALLISTER, assistant in the Yale psychological laboratory, has been made lecturer in experimental pedagogy at Yale.

DR. EDWARD L. THORNDIKE, of Teachers College, Columbia University, has been appointed adjunct professor of genetic psychology in that institution.

PROFESSOR F. C. FRENCH has resigned the chair of philosophy at Vassar College and Dr. H. Heath Bawden has been appointed associate professor of philosophy in the institution.

FREDERICK E. BOLTON, PH.D. (Clark), has been promoted to the head of the department of pedagogy in the State University of Iowa, succeeding Professor J. J. McConnell, who has resigned to accept the superintendency of schools at Cedar Rapids, Iowa.

SHEPHERD IVORY FRANZ, PH.D. (Columbia), assistant in physiology in the Harvard Medical School, has been appointed instructor in physiology at Dartmouth College. He will give courses both in the Academic Department and in the Medical College.

M. TH. RIBOT, professor of experimental and comparative psychology at the Collège de France and editor of the *Revue Philosophique*, will retire from his chair on a pension at the end of the present academic year.

WE learn that a department of Experimental Psychology has been established in connection with the Pathological Laboratory of the London County Council Asylums at Claybury. Dr. W. G. Smith, lately of Smith College, has been placed in charge.

THE death is reported, on May 22d, of Dr. William D. Thomas, professor of psychology in Richmond College, Va.

THE American Branch of the Society for Psychical Research has recently issued a questionnaire with a view to ascertaining human sentiment with regard to a future life. In a preliminary note the special purpose of the inquiry is stated as follows:

There is a widespread literary tradition that men naturally desire a future life. From this assumed fact it has been variously argued that (1) such a universal desire cannot be destined to disappointment, and (2) it must vitiate convictions and engender illusory evidence in its own support.

But there is some reason to suppose, both from the ordinary conduct of men and from sporadic declarations of individuals, that this tradition is very far from accurately representing the facts, and that these are actually more various and complicated. Moreover, it should not be forgotten that in India the literary tradition seems to be exactly reversed, and it is assumed that men naturally crave for extinction or absorption in the Absolute.

It becomes a question, therefore, what the actual sentiments of men are, and what, consequently, is the actual bias with which they are likely to receive the doctrines and the evidence that bear on the subject.

Both these questions are capable of being determined with sufficient precision by instituting a statistical inquiry over a wide field—collecting answers until it becomes evident that the percentages of the various types of answer have become constant.

Copies of the questionnaire may be obtained by addressing the secretary, Richard Hodgson, 5 Boylston Place, Boston, Mass.



# THE PSYCHOLOGICAL REVIEW.

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## CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

### FURTHER OBSERVATIONS ON THE MONAURAL LOCALIZATION OF SOUND.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The present paper is a sequel to one published in the May number of the REVIEW for the current year. In the latter we reported an extended series of experiments upon a man totally deaf in one ear, showing that he possessed a somewhat surprising ability to locate certain kinds of sounds, *i. e.*, complex sounds of medium tonal range, although entirely unable to locate pure tones. Without attempting to restate in detail the conclusions we felt warranted in drawing from our observations, it may suffice to say that we connected the subject's localizing capacity with the modifications of timbre which complex sounds undergo when they come to the ear from different directions. These modifications are due to the damping or reinforcing of their constituent partials by the pinna, the external meatus, the bones of the head, etc. The present report is offered as a supplement confirming the main thesis of the previous paper by investigations upon other subjects. It brings out with special clearness the variations in monaural localization which are connected with the duration of deafness. The cases afford from this point of view a somewhat striking explanation of the contradictory reports emanating from aurists with reference to the effects of unilateral deafness upon sound localization.

Like case *A* of the earlier paper, these new cases are all

instances of internal ear deafness. We shall designate them as *C*, *D*, *E* and *F*.<sup>1</sup> They display in a striking manner the differences between the localizing capacity of persons who have recently lost their hearing in one ear and those who have been deaf long enough to accommodate themselves to the abnormal conditions. It does not follow from this that all persons deaf in one ear necessarily develop their latent capacities of monaural localization, and it seems probable that this is less likely to occur when deafness has come on in mature life than when it exists from early childhood. All our subjects, however, have shown ability to improve somewhat rapidly under the training afforded by the experimentation, although they have, of course, been kept in entire ignorance of the correctness or incorrectness of their judgments during the course of the tests.<sup>2</sup> *C* is a man of thirty, who has been deaf from early childhood. The precise time and conditions of the onset of deafness cannot be verified. The subject is under the impression that the trouble followed a

<sup>1</sup>We regret that Dr. Politzer's paper on 'Paracusis Loci' (*Archiv für Ohrenheilkunde*, Bd. XI., p. 231), which is the only account of extended observations on monaural localization to which we have had access, does not enter into more of detail concerning the exact methods he employed and the clinical features of the cases. These were in the main apparently instances of middle ear deafness with various degrees of defect in the hearing of one or both ears. It seems improbable that in such cases habituation to the abnormal condition, in contrast to the usual binaural capacities, should have become thoroughly established, as our tests show it may after a considerable period of unilateral total deafness. Dr. Politzer appears to have used as sounds principally the ticking of a watch and whispering. Our tests show that both of these sounds can be localized with considerable accuracy monaurally, the whispering better than the ticking. But they are less easily located than many other sounds, owing to the peculiarities of their physical composition. These peculiarities of various kinds of acoustic stimuli are more fully discussed in our first paper. This statement will, however, explain our inability to compare our results more explicitly with Dr. Politzer's.

<sup>2</sup>The apparatus employed is described in the first paper. In the experiments recorded here the subject had the head placed at the center of the spherical cage and not the intact ear. Our observations upon *A* seemed to indicate that localization occurred with reference to the whole body and not with reference primarily to the ear. The variations due to alteration of the position in this particular are small and we have not felt warranted in taking the additional time necessary to test the matter conclusively, although the gentlemen who have served as subjects for us have been most generous in their sacrifice of time and convenience.

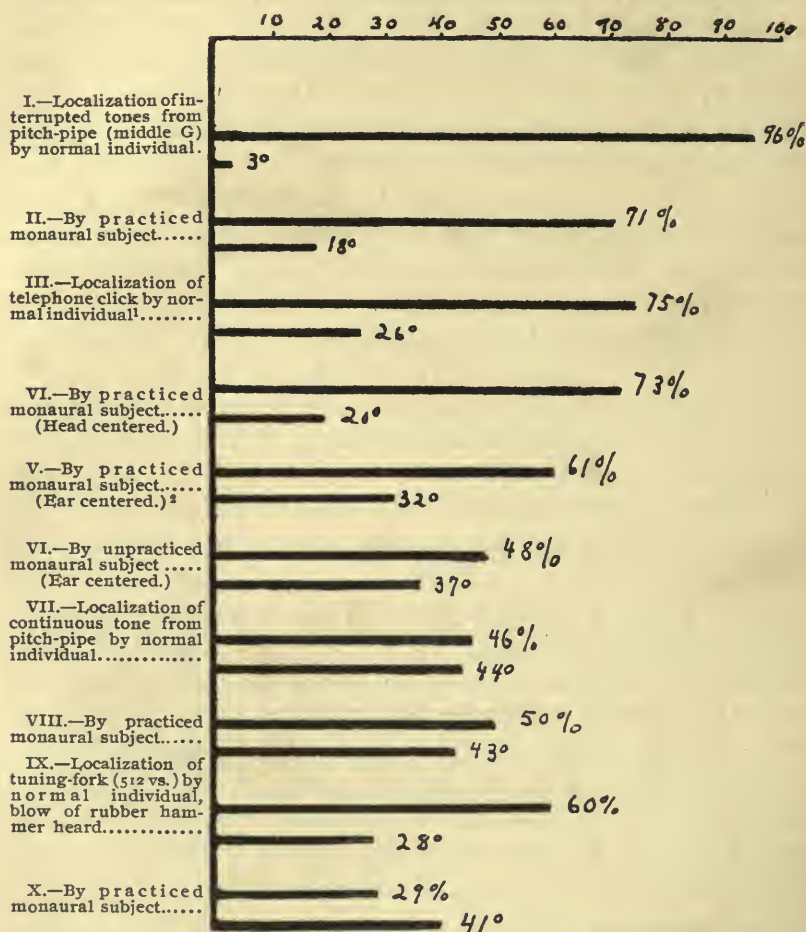
febrile disease. In later childhood there were transient attacks of severe vertigo. *D* is a man of twenty-four, who lost the use of one ear when ten years old, in connection with an acute attack of osteo-myelitis. *E* is twenty years old. He lost the hearing of one ear six years ago as a result of hemorrhage in the labyrinth following a severe fall. *F* is sixty years old. He was operated upon a year ago for acute suppuration of the mastoid accompanied with labyrinthine symptoms. At the time of the experiments the wound was still open. Taken in connection with *A*'s case, already reported, this gives us observations upon two persons deaf at least twenty-six years, one ten years, one six years, and one deaf for a single year.

When compared with one another the results show increased capacity of localization with increasing periods of deafness. The new cases show precisely the same relative accuracy with the different forms of auditory stimuli employed as did *A*. The more nearly a pure tone is approximated as a stimulus, the more inaccurate the localization. An improvement in the method of giving tuning-fork tones, which was also employed with *A*, resulted in securing a practically pure tone without any of the noise elements incident to setting the fork in vibration. Under these conditions all the subjects failed uniformly to localize. Occasionally such tones would seem opposite the intact ear, but quite as often no location could be assigned, and it seems quite safe to say that in monaural hearing really pure tones are unlocalizable.<sup>1</sup> Very high-pitched tones like those of the Galton whistle are subject to rather large errors of localiza-

<sup>1</sup>The earlier tests on *A*, employing tuning-forks, had been made with as perfect suppression of the composite sounds accompanying the starting of the vibrations as the means at our disposal seemed to permit. We were aware, however, that the result was not wholly satisfactory, and that we did not always secure the purity of tone desiderated. The results of the tests confirmed this in a demonstrable inability to localize most of the tones, combined with an anomalous certainty and accuracy in the localization of occasional sounds of this type. The method finally employed involves having the subject stop the intact ear with the finger until after the fork has been struck and all the overtones thus produced have died away. He is then touched with a long rod by the operator in such a manner as to convey no indication of the direction of the operator or the fork. Upon this signal he opens the ear and attempts to localize the sound. Under these conditions the localization appears impossible, regardless of the intensity of the tone.



TABLE I.



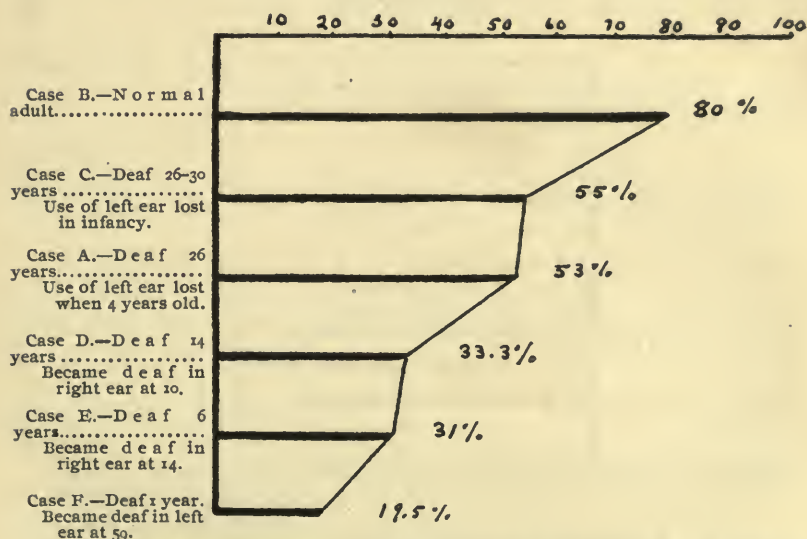
Showing comparative accuracy in auditory localization of a normal person and a practiced monaural subject. Entry VI. introduces for additional comparison an unpracticed monaural subject. Of each pair of lines the upper one represents the percentage of correct judgments, the lower one showing the average error in degrees. The tests upon which the table is based involve the localization of sixteen points (eight on the equatorial circle, four above and four below), as explained in the May number of this REVIEW (Vol. VIII., p. 232). 1,449 tests are incorporated in the table, of which approximately one-fourth were made upon normal individuals.

<sup>1</sup> Confusions between front and back (180°), give the average error in this case a somewhat misleading magnitude. Apart from this confusion, the errors are generally small.

<sup>2</sup> In this case the number showing the average error (32°), is approximate and not exact. A wayward record sheet prevents accurate verification. The possible error is small.

tion for reasons canvassed in our first paper. Complex noises like the rapid clickings of the telephone plate are localized with considerable accuracy. Similarly, complex tones near the middle of the musical scale are well localized. An ordinary reed pitch-pipe (middle G) was employed for the purpose.<sup>1</sup> In the case of this complex tone it proved, as in binaural hearing, considerably more difficult to effect a confident localization when the tone was continuous than when it was interrupted. Not to mention other possible reasons for this, it is certain that the identification of the tonal modifications, which are in monaural

TABLE II.



Showing by the percentage of correct judgments the variation in the capacity of monaural localization of the telephone click (eight points on the equatorial circle), as connected with the duration of deafness.

hearing the indices of direction, is much assisted by rather rapid repetitions of the tone.

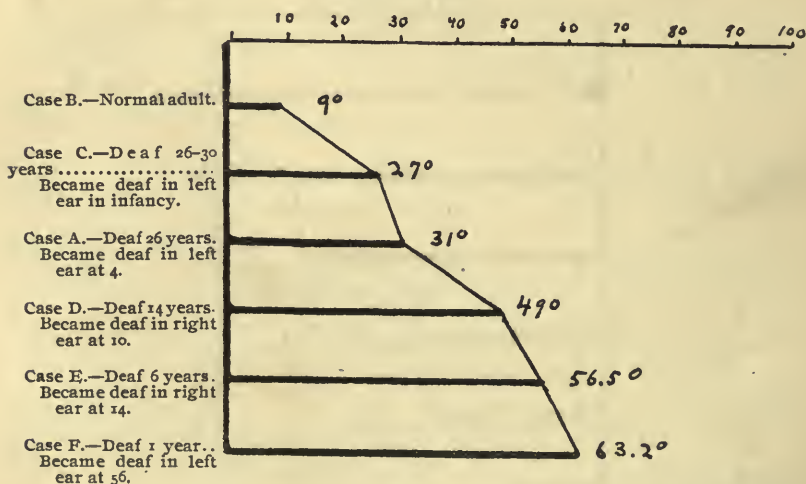
It does not seem essential to present a complete tabular statement, such as was given in the report of *A*'s case, for each form of test with the four new subjects. The differences are purely matters of degree, which can be exhibited satisfactorily in a briefer way. We have come upon no new principle, nor have

<sup>1</sup> This series, in the case of *A*, was not contained in the original report, but his results with this stimulus fall in perfectly with our general hypothesis.

we failed in any case to verify the hypothesis we have offered. We may first represent graphically the differences in the capacity of localization between a well-trained monaural subject like *A* and a normal person, when sounds of varying complexity are used as stimuli. Table I. exhibits these relations.

Table II. presents the variations among persons deaf in one ear, as connected with the duration of their deafness and the period at which they became deaf. The percentage of correct judgments is used as the basis of the table, the same sound, a telephone click, being used throughout the tests.

TABLE III.



Showing in degrees the average amount of error made by the several subjects in attempting to localize eight points upon the equatorial circle. Telephone click used as stimulus.

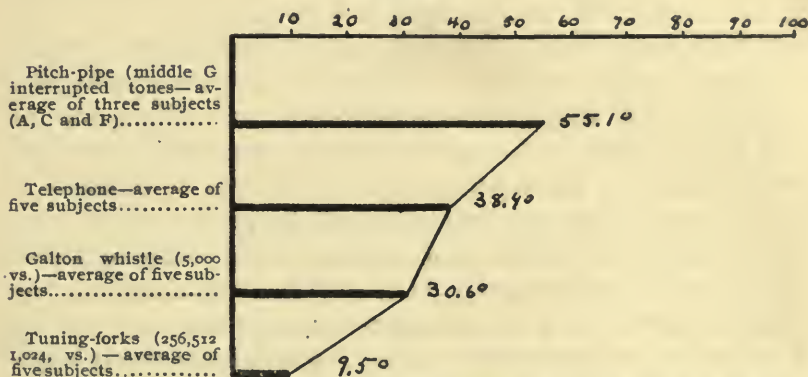
Table III. shows the same general relations, but the average error in degrees is made the basis of the comparison, instead of the percentage of correct localizations. Table III. is probably more truly indicative of the differences between monaural and binaural hearing, and of the variations among monaural individuals themselves, than Table II.

In Table IV. we have shown graphically, on the basis of results from all the monaural cases we have examined, the variation in accuracy of localization, as compared with the noticeable



complexity in the sound. The fact should be emphasized, that all the subjects agree in their judgment of the relative ease of localization of the general kinds of stimuli, that, furthermore, their subjective impressions are verified by their objective accuracy, and that the diagram is, therefore, representative of an average, which does no violence to the relations in any individual case, as so often occurs in averages based on statistical evidence. The tuning-fork tests are included for tabular completeness only. It is perfectly certain, we think, that pure tones are unlocalizable in monaural hearing. Mere chance might be expected to produce a larger percentage of correct judgments than is here presented. But neither the percentage test nor the

TABLE IV.



Showing by percentage of correct judgments for various forms of auditory stimuli the connection between accuracy of localization and discriminable complexity of sounds (eight points on equatorial circle).

average error test is to be weighed beside the overwhelming deliverance of consciousness itself, which in the case of pure tones is absolutely different from that in the case of complex sounds.

So many of the aurists' tests are made with a watch and by means of whispering (this is true in Politzer's tests already referred to), that in view of the frequently asserted inability of monaural subjects to localize those sounds, it seems desirable to refer briefly to our own tests, which show clearly enough that such sounds, although more difficult to localize than some others, are still capable of decidedly accurate localization. The

occasional inability to localize is, therefore, probably to be connected with conditions of aural irritation, or with the lack of adaptation to the new monaural conditions.

Our tests were made in a manner already suggested in the description of the procedure in the experiments with tuning-forks. The operator, having removed his shoes, took his position at any desired distance from the subject (as a matter of fact we used various distances) and in any desired direction from him. The subject during this period kept the intact ear tightly closed. On being touched with the signal rod, he opened the ear and attempted to localize the sound. As the operator moved noiselessly, it was also possible to make tests without the stoppage of the ear. No suggestion was obtained in any case of the direction from which the sound might be expected to come, and no information was given during the series regarding the success or failure of the localization. We may quote the results of a single series of tests with each kind of sound. We quote the first tests of each kind, which represent, therefore, a minimum of familiarity with the sound and a maximum of difficulty in localization. Of twenty-two tests made by gently whispering a monosyllable like 'K' at eight different points on the equatorial circle (average distance four feet), only four mistakes occurred. They involved in no case an error of more than  $45^{\circ}$  and all four of the confusions were between points immediately opposite the deaf ear and points slightly back of that. If the confusion between front and back, which is so frequent in binaural hearing, be taken into account, this result is quite as accurate as most normal individuals would attain on a first trial. It certainly is difficult to reconcile it with any radical inability to localize.

Similarly, of the first series of tests with a watch (distance two feet) ten out of sixteen positions were correctly judged, the same points being used as in the previous test. No error was larger than  $45^{\circ}$ , and an error of this amount is frequently met with in first tests upon normal individuals, as our records show. Decidedly the watch must be used sparingly, if accurate localization is desired with either binaural or monaural persons.

In series XIII. of our earlier paper we attempted under the

limitations of indoor work to decide whether the changes in the character of complex sounds, furnishing the basis of determining their direction, were functions of changes of distance also. The indoor tests resulted negatively, the subject showing highly accurate capacity to distinguish changes of distance from those of direction, thereby demonstrating that, within the limits of variation possible under the laboratory conditions, the two series of changes need not lead to serious confusions. It seems judicious to report at this time a more drastic test made out of doors. We may anticipate the result at once, however, in saying that the test failed to show any great difficulty in distinguishing distance changes from changes of direction.

The tests were made at night, with the greatest attainable quiet, in a large open field some eight or ten acres in area. Precautions similar to those adopted in the whispering test were introduced to prevent any suggestion of the direction from which the sound might be expected. We used the same eight directions employed all through our tests, varying in entirely irregular order the distances from five to twenty-five and fifty yards. The sounds might thus come from points on any one of three concentric circles, and the subject was required to judge both the distance and the direction. As stimuli we employed the pitch-pipe previously used, and an automatic counter, resembling a pedometer in its mechanism, and producing when operated rapidly a clear, metallic, clicking sound. Indoor tests had shown that both sounds could be localized very accurately. Seventy-two tests were made, in which the direction was correctly judged 52 times, or 72 %. The average error was  $14.3^{\circ}$ , and only two errors were made in excess of  $45^{\circ}$ . These both involved the point immediately opposite the deaf ear. Of the 20 erroneous judgments, 4 also involved errors of distance. In addition to this there occurred 5 errors of distance without errors of direction. All the errors of distance were caused by confusions of sounds 25 yards distant with those 50 yards distant. When it is remembered that at the greater distances the sounds were only with difficulty audible on the side of the deaf ear, this confusion is not remarkable. On the whole, therefore, the test seems to confirm the indoor experiments in its indication that with com-



plex sounds there are relatively few confusions of distance and direction, the modifications which the sounds undergo by changes of the two kinds being differentiable in the main by the single ear. Sounds near the threshold of intensity are, as is well known, very difficult to localize in normal hearing, as well as in monaural hearing. We should expect to meet with a much larger percentage of confusions, therefore, if we employed distances so great that the sounds could barely be heard in any direction. A similar result would accrue from intrinsically feeble sounds near at hand. This is true with the watch tick, for example.

The experiments suggest very pointedly the possibility and desirability of a little training for persons who lose their hearing in one ear. If the disaster occurs in infancy or early childhood, it seems probable from our observations that experience itself serves to build up the new localizing processes. But when the injury occurs later on in life, it appears problematic how far the adaptation is carried. In any case it is well recognized that many patients suffer considerable distress through their lack of confidence in their capacity to localize sounds, and it seems probable that this distress can be largely removed by a little formal drill, by means of which the patient may have opportunity to notice carefully the changes in sounds arising from changes in their position. The most defective case we have examined (*F*) showed an increase after one hour's practice from 12.5% of correct localizations to 25%, and a decrease of his average error in degrees from  $70^{\circ}$  to  $53^{\circ}$ . This improvement occurred, too, without the subject's knowing whether his judgments were correct or incorrect. The result is attributable, therefore, merely to systematic direction of attention to the peculiarities of sounds coming from different directions. If to this were added the knowledge from visual sources of the position of the stimulus, there is every reason to anticipate a very great and rapid improvement with considerable, if not complete, relief from the distress already mentioned. The gaining of self-confidence in the matter is a large part of the victory. The discipline suggested is distinctly comparable with the ocular gymnastics prescribed to persons suffering from defects of the oculomotor mechanism, and is herewith respectfully submitted to the attention of aurists who may not employ such methods.

# CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

## NEW APPARATUS.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The following description is offered of several pieces of apparatus, which have been found to be of practical value in the psychological laboratory of the University of Chicago.

I. A multiple contact key. This is shown in Plate II. It is intended to eliminate the difficulty of adjustment and the possible source of error found in the use of the common type of bar-key. In the keys used in the Chicago laboratory it was found that the force required to ensure double contact was liable to loosen the screws by which the adjustment for simultaneity was made, also that the bar was liable to bend slightly, giving contacts which were not exactly simultaneous. The error was occasionally found to amount to 10  $\sigma$  or more. No doubt these errors can be overcome by accurate workmanship. But even granting this, the extreme accuracy and the ease of adjustment of the key herewith described lead us to regard it as preferable to the bar form of key.

The key presented here consists of a cylinder of wood (length 23.5 cm., diameter 15 cm.) mounted upon steel supports, which rest upon a wooden base. Upon the surface of the cylinder are fastened six plates of brass, 25 mm. in width, and in length equal to one-half the circumference of the sphere. The thickness is 3 mm., beveled, however, to an edge at each end. They are arranged alternately along the length of the cylinder at intervals of 12 mm. where their edges rest against a straight line drawn parallel to the axis of the cylinder. Resting firmly against the cylinder are six pairs of strips of phosphor-bronze (spring-metal) which constitute the electrodes of the six

electric circuits. Each strip is 2 mm. thick, 9 mm. wide, about 8 cm. high from the platform, and curved slightly at the top, so that only the edge rests against the cylinder. The members of each pair are 6 mm. apart. Each strip is set into a circular bar of brass 25 mm. long and 9 mm. in diameter, through which there is bored a hole with a binding-screw for making connections; this bar is then continued as a screw, which passes through the projecting edge of the base and is secured to the platform by nuts above and below. This arrangement allows the strips to be raised and lowered at will. Each circuit is made when the strips rest against the brass plates of the cylinder, and broken when they rest against the wood. The key thus allows three circuits to be made and three to be broken at the same moment. Simultaneity is secured by adjusting the electrodes so that the lower edge of the pair whose circuit is to be made, rests against the upper edge of the brass plate on the line where the circuits are made and broken, and so that the upper edge of the pair whose circuit is to be broken, rests against the lower edge of its corresponding brass plate on the same line. If the electrodes were filed to a sharp edge the edges might all rest upon the same line, but this was thought to be inadvisable owing to the danger of wear and the consequent disturbance of adjustment. The width of the edge renders it also necessary to confine the adjustment to one of the two lines where the brass plates end, but this is all that the use of the key requires. The simultaneous making and breaking of the several circuits is effected by turning the cylinder, in the direction of a clock, by a rapid wrist-motion, through a semicircle; when the reaction has been made the key is turned, still in the clock-direction, back to its original position.

The key was tested on a drum, after being easily adjusted by the eye, and no error could be found. Whatever the error, it was certainly under .0001 of a second; and whatever error there may be in the adjustment, is minimized by the rapid motion of the surface of the cylinder, which, again, is due to its size. The only improvement we have to suggest is that, where the electrodes are passed through the platform, they be cut square instead of round, so as not to permit of rotation during





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adjustment. Since the accuracy of the key after adjustment depends upon the stability of form of the parts, great care must be taken, in the construction of the cylinder and the base, to secure well-seasoned wood.

II. A platform for tuning-fork and marker. This is shown in Plate II., attached to a Scripture drum. It is intended to facilitate the adjustment of the several forks and markers to the drum, particularly to enable them all to be withdrawn or applied in one movement. The fork and marker rest upon a steel platform (25 cm. x 7.5 cm.) down the middle line of which is cut a groove, 20 mm. wide, with projecting flanges. The fork is secured by two screwbolts, the heads of which slide in the groove, with thumbscrews on the upper end, as shown in the photograph plate. The marker is secured to a rod 10 mm. in diameter. The rod ends below in a bolt-head which slides in the groove, and is clamped to the platform by a thumbscrew. The fork and marker may thus be located anywhere along the length of the platform. This upper platform is screwed to a steel plate (100 x 80 x 5 mm.) which moves in a track to and from the drum upon a lower platform. The latter ends on the right in a socket, which fits over an upright rod (diameter 19 mm.) and thus fixes the whole apparatus to the platform of the drum. When the fork and marker have been once adjusted to the drum their relative position remains fixed. By means of the screw which appears at the front of the platform, they may, with the upper platform, be withdrawn from the drum and re-applied without any alteration of relative adjustment. The fork can thus be easily set in vibration before coming into contact with the drum. The head of the adjusting screw can easily be supplied with a circular vernier, if extreme delicacy and accuracy of adjustment are found necessary.

III. An apparatus for determining the impact limen is shown on the lower portion of Plate III. In the use of cork weights great difficulty is always experienced in placing the stimulus gently and steadily upon the skin. The instrument shown in the plate employs the principle of the balance to secure this result. It is simply a very delicate balance with three knife-edge



bearings, weighing accurately down to a milligram, mounted on a heavy steel platform raised on levelling-screws and provided with tracks at right angles to each other, upon which it travels by means of screws. In this way a considerable area of the skin can be explored without disturbance to the subject or the instrument. The mode of operation is this: The long arm at the left of the instrument carries a flat cork point, which can be detached by a screw and replaced by another of any given shape or dimensions. The cork point is attached to a flat bar sliding up into a slot in the arm. This permits a vernier arrangement, as shown on the plate. The sliding bar carrying the point is clamped firmly at any desired point by a friction-clutch, which may be seen projecting toward the body of the instrument. When the arm, supposing this to be the portion of the skin to be explored, is properly placed, the cork point is lowered, until it is just in contact with the skin, when the pointer of the balance is opposite to zero on the dial. The point is then pushed up any desired amount, say two millimeters. A weight of any desired size (for example, 10 milligrams) is placed in the pan over the arm. At the base of the support for the balance-arms are seen two levers. One of these releases the balance and permits the cork point to move gently down upon the skin. The oscillations are very slow and as the point comes into contact with the skin the balance-arm moves up again and the second lever is then pressed down, bringing the pointer to zero once more. The height of the fall and the weight in the pan can either or both be varied, and the physical expression for any given stimulus is easily computed. The instrument is noiseless and capable of rapid manipulation.<sup>1</sup> It must be constructed with great delicacy to secure constant action in the oscillations of the arms, and these oscillations must be slow to secure the best results. In two years and more of use it has proved very reliable. It can readily be converted into an instrument for giving passive pressure stimulations instead of impact. The principal dimensions are as follows: base plates 25 cm. square, balance arm 30 cm. long, supporting pillar and contact arm, when pushed in, each 15 cm. long.

<sup>1</sup> Cf. Griffing's lucid presentation of relevant considerations in his monograph, 'Sensations from Pressure and Impact,' 1896.

IV. A standard for adjusting a common form of the registering tambour to the surface of the drum in plethysmographic work is shown on the upper portion of Plate III. It consists of a heavy metal base carrying an adjustable upright, permitting the raising or lowering of the carriage above by means of the screw-clamp upon which, in the plate, rests the glass tube. The carriage itself is adjustable around a horizontal axis by means of a thumbscrew. It travels forward and backward by means of a track and an endless screw of fine thread. The clamp mounted upon the carriage to receive the rod of the tambour is adjustable around a vertical axis. For delicate work some such device is almost indispensable and this is as convenient as any form we have seen for tambours of this type. We give no dimensions, as these must necessarily be dependent upon the particular kymograph and tambours to be employed.

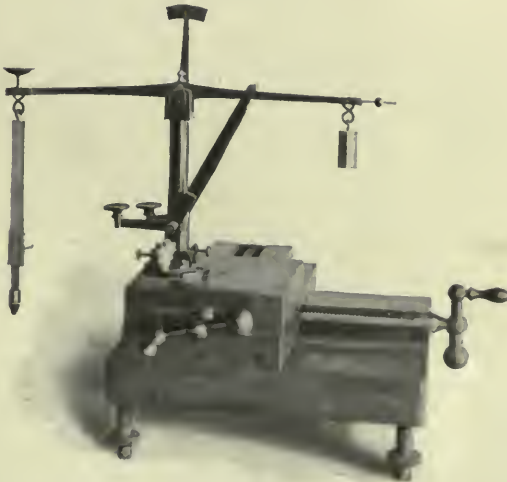
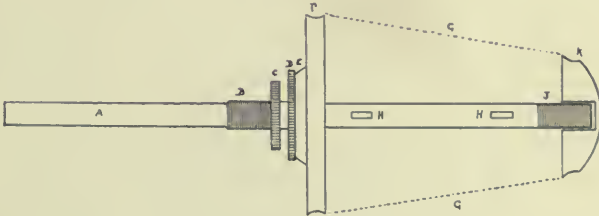
V. An adjustable form of the Hallion and Comte plethysmograph. This is shown in the sketch in the middle section of Plate III. *A* is a thin metal tube (length 15 cm.) carrying the metal disk *F* (diameter 6 cm. by 5 mm. in thickness). *A* is perforated with slits at *H* and terminates in the metal disk *K* (diameter 3 cm., thickness 5 mm.). *E* is a leather washer, 5 mm. thick, compressed and held in place by the two metal washers *C* and *D*. *G* shows the position of the rubber covering, which is tied and cemented to the grooved edges of *F* and *K*. The screw-threads at *B* and *J* on *A* permit, by changing the distance between *F* and *K*, an adjustment of the tension of the rubber covering *G*, after the joints of the rims of *F* and *K* have been made air-tight. It is thus possible to secure the maximum efficiency from the instrument without dependence upon the skill with which the rubber chances to be stretched at first. We have found the conical form of the instrument preferable to the cylindrical shape commonly employed. It permits a more perfect contact between the surface of the fingers and the rubber.

VI. Thermal apparatus (shown on Plate IV.). This consists of two parts; the first is for the purpose of maintaining a

constant temperature; the second for the purpose of locating points on the skin.

*a.* The first appears in the lower part of the plate, resting on the larger table. The larger and smaller vessels are for hot and cold water respectively, which passes in a continuous stream to the temperature-point on the smaller table. The problem of keeping the hot water at a constant temperature without the aid of an assistant, is solved by employing a constant flame, a constant quantity of water and a constant supply of fresh water at an approximately constant temperature. The first can usually be secured by a group of Bunsen burners; to secure the last two we used a tank of galvanized iron (diameter 35 cm., height 50 cm.), tight at the top with the exception of a hole (diameter 4 cm.) into which as a gauge we fitted a glass tube in a cork. The tank is always kept full, its fulness being indicated by the presence of water in the glass tube, which is open at the top. In order to prevent the tube from overflowing, also to avoid undue pressure within the tank, the water which comes through the feed-tube to the tank is not drawn directly from the faucet, but from a bottle with an opening near the bottom (an aspirator), which is set in a pan upon a shelf above the feed-tube, but not above the top of the tube projecting from the tank. (This part of the apparatus does not appear upon the plate.) The bottle is fed from a faucet through a tube; it is kept constantly full and the overflow is caught by the pan, from an opening at the base of which it is carried off through a rubber tube to a sink. In this manner the pressure of the water flowing through the tank is kept constant. For some purposes it is desirable to have only a small amount of water in the tank. On such occasions the pressure is of course only approximately constant. The flow of water through the apparatus as a whole is regulated by the lower glass stopcock fastened to the standard at the further end of the large table. The water flows from the temperature-point, through this stopcock, to the glass tube fixed to the standard at the rear end of the large table, thence into the funnel and out through the waste-tube to the sink. By allowing the water to fall into the funnel, instead of running out through a continuous circuit of tubing, it is possible to estimate, by the





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size of the stream outlined against the black cardboard, the approximate rate of flow. The stopcock thus governs the rate of inflow as well as of outflow; if the flow outward is completely checked, the result is that all the water from the faucet overflows the aspirator into the pan and thence returns to the sink, the quantity of water in the tank remaining always the same; in this manner it is possible easily to regulate the amount of cold water coming in to cool the hot water in the tank. By adjusting the supply of gas and water any given temperature from  $34^{\circ}$  C. to  $60^{\circ}$  C. can be secured in the contact point and indefinitely retained. Much higher temperatures can readily be obtained, if necessary. The thermostatic devices employed to render such apparatus automatic can be applied, if desired. But the expense is thereby considerably increased.

By opening and closing the proper tubes with clamps the cold-water tank is brought into the system and the hot water separated from it. The water flowing through the cold-water tank flows not directly into the tank but through a coil of lead-pipe 37 feet in length and thence out to the temperature-point. The tank itself is filled with cracked ice and salt, which cools the water during its passage through the lead-coil. Since the temperature to which the water is reduced depends upon the rate of flow, it can be regulated within certain limits by the use of the stopcock. By varying the amount of ice in the tank and, if necessary, opening the circuit from the hot-water tank, it is easily possible to secure and retain temperatures in the point ranging from  $+34^{\circ}$  C. to  $-12^{\circ}$  C. In working with temperatures below the freezing point, alcohol should be mixed with the water supplied to the coil. By substituting a small amount of ether and solid carbon dioxide for the cracked ice and salt, temperatures as low as  $-100^{\circ}$  C. can be obtained. The temperature at the copper point varies widely in this case with the length of tube, the wrapping, etc., between the point and tank.

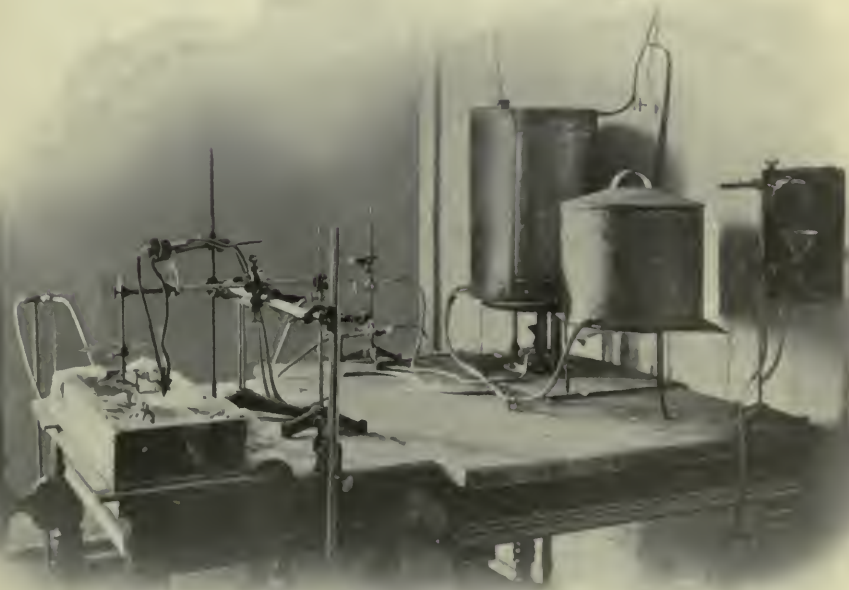
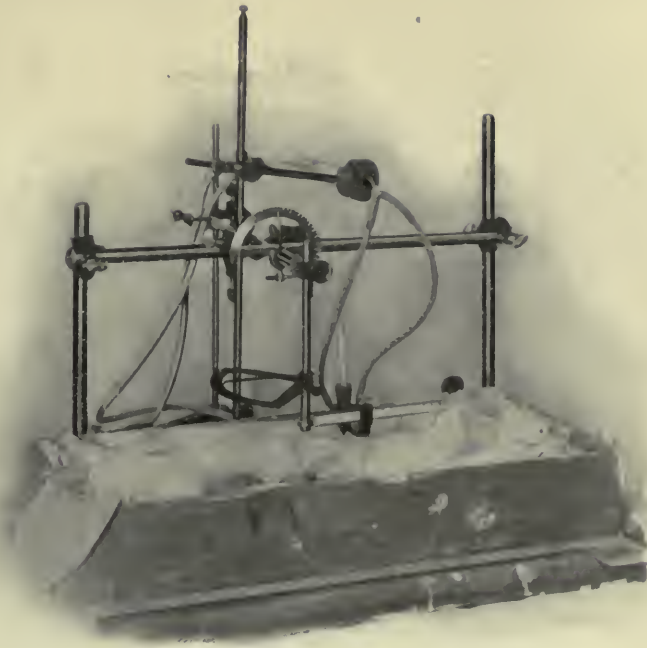
Both tanks are heavily jacketed with asbestos, which is not shown in the plate. The cut fails also to show a stopcock for drawing off the melted ice from the cold-water tank. Double tubing is used to connect the point with the cold-water tank. This has not been found necessary with the hot-water system.



In work with the extreme temperatures we cover all but the tip of the copper contact point with felt. This practically eliminates disturbing radial effects from the lateral surface of the cone. Thermally opaque screens with apertures of any desired size can of course be used, where a more complete elimination of this factor is desired.

b. The upper part of the plate shows in detail the temperature-point and its adjustments.<sup>1</sup> The point consists of a pyramidal vessel of thin copper, 35 mm. in height, 35 mm. in width across the top. Soldered to the top on opposite sides are two tubes (internal diameter 2 mm.); one of these extends through the top to the lower point of the vessel, which ensures the passage to that point of all the water entering the vessel. Into an opening in the top of the vessel is fixed a rubber cork, holding a thermometer, the bulb of which extends to the bottom of the vessel. For this purpose special thermometers were constructed with the particular ranges of temperature desired. The gradations show a degree to the  $\frac{1}{4}$  inch. The temperature-point is fixed to the clamp which slides on the graduated rod, but is extended from it 5 cm. by means of two thin strips of spring-brass, which ensure the return of the point to its original position after being pressed against the skin. The position of the point may be varied in four ways: by rotation in a vertical plane, measured on the upper of the two circular scales; by rotation in a horizontal plane, measured on the lower circular scale; by a vertical movement of the rod holding the lower scale, regulated by the rack-and-pinion movement at the top of the rod, which acts also as a vernier; and by sliding the clamp along the lower horizontal rod. The arm upon which, in the arrangement shown by the cut, it is proposed to locate the temperature-spots, is held in a plaster cast in the large box beneath the temperature-point. The whole apparatus is clamped firmly to the table, so that, by noting the registration on the several scales, it is possible to adjust the apparatus for any desired point. If the arm remains absolutely fixed, this means an absolute location of any point on the arm.

<sup>1</sup> This is similar in many ways to that described by Miss Washburn in the *American Journal of Psychology*, VI, 423.



Angell and Fite on New Apparatus.





So far as we are aware, this apparatus can be employed in connection with any of the supplementary devices for securing accuracy in the localization and identification of points on the skin. It pretends to no special novelty, being merely a combination of arrangements already familiar in apparatus designed for these purposes. But it has proved very convenient and it is quite inexpensive, when compared with the more elaborate instruments, such as the kinesimeter of Hall and Donaldson.

We shall be glad to furnish further information about the cost or construction of any of the pieces of apparatus reported in this paper.

## FURTHER NOTES ON THE RELATION OF STIMULUS TO SENSATION IN VISUAL IMPRESSIONS.

BY PROFESSOR C. LLOYD MORGAN, F.R.S.  
*University College, Bristol, England.*

In a previous paper (PSYCHOL. REV., May, 1900) I gave in some detail the results so far reached in an experimental investigation on the relation of stimulus to sensation in visual impressions. I attempted to show that to obtain equal increments of sensation there are required increments of stimulus in geometrical progression. This law so stated was based on experimental data, and is a generalization directly founded on them.

In the *American Journal of Psychology* for July, 1900, Mr. Max Meyer<sup>1</sup> pointed out that if a constant be added to each of the terms of my stimulus-series, the stimulus-sums are in geometrical progression. This is so. And a logarithmic curve is thus obtained which gives the results of experiment plus an assumption as to what may hold good beyond the limits within which the observations were conducted. I sought to avoid any such assumption and to give a generalized statement of facts actually observed.

My investigation being experimental, I stated the results in each case in arbitrary scales, assigning 0% of stimulus and sensation to my 'black,' though fully aware that it was not absolutely black, and assigning 100% of stimulus and sensation to my 'white,' 'red,' and 'dark blue,' though it is obvious that their luminosities differed widely. In a description of experimental work this appeared to me the simplest and most direct method of statement. As in practical measurements of temperature an arbitrary zero is taken as the starting point, so did I make my 'black' the arbitrary zero of my scale. I was not

<sup>1</sup> For a reply to his mathematical criticisms see F. R. Barrell, *American Journal of Psychology* for October, 1900, p. 135.

dealing with an absolute zero nor attempting to obtain an absolute scale; though the possibility of translating the arbitrary into the absolute, as the result of more extended experimental work, not unnaturally presented itself to my mind.

Mr. Max Meyer in his criticism further shows that my curves for 'red on black' and 'dark blue on black' may be represented as portions of my curve for 'white on black.' Obviously they can be so represented, since they are curves of the same family, constructed on the same principle, and more limited in extent. I had myself thought of presenting them in this way, but preferred to give the results on the percentage scale by means of which they had been practically determined. In the brief notes which follow, however, I shall take advantage of Mr. Meyer's suggestions as to method of presentation.

1. Realizing that the black surface-paper I used was only a very dark gray, I first addressed myself to obtaining something more nearly approaching an ideal black. I had found the black velvet which Professor Kirschmann recommends difficult to work with. The deepest black I have yet obtained is produced by coating the surface-black with 'black enamel' paint. This gives a reflecting surface. But by so arranging matters that it receives the reflection from black cloth or velvet, good results are obtained. I now work with this new arbitrary zero and am glad to find that my results more nearly accord with Delbœuf's scale.

2. I now allow for an optical illusion which perhaps slightly vitiated my previous determination. It is well known that a circular area of, say, 3 inches radius on a black disc of, say, 6 inches radius, looks bigger than a black area of 3 inches radius on a white disc. When one is shading from inner white to outer black, the position of the mid-gray does not, for similar reasons, appear at quite the same distance from the center of the disc as when one is shading from inner black to outer white.

3. Instead of viewing the reflection of the whole disc, I now use a mirror six inches long by one inch wide. This isolates a strip of the reflected disc-surface. The accuracy of shading can be better judged in such a strip; and by marks on the edge of the mirror the position in which the mid-gray should fall can be indicated.



4. I find I was misled in a way which opens up an interesting point. I took much pains to get a mid-gray ring between an inner circle of full white and an outer circle of black. Unless I am mistaken, however, the value of such a ring in terms of luminosity is *not* the same as the value of mid-gray for continuous shading. I also endeavored to check continuous shading by obtaining a series of graded rings. But I have good reasons for believing that the curve through the points determined by means of such rings is *not* the same as the curve for continuous shading, and that the method of work I used as a check led me into error. The stimulus increments in the graded disc require a larger factor than in that for continuous shading. The difference between the curves seems to be due to contrast, the effects of which have been shown by Lehmann and others to be important. I drew attention to these contrast effects in the ring-discs, and they are clearly seen in the figure I gave. But I did not realize that they had any observable influence on the sweep of the curve. I now believe that they modify it materially, shifting the stimulus value of the mid-gray through at least 5%, probably more. In ring grading higher percentages of white are required than for continuous shading. I am not, however, prepared to discuss the question further at present.<sup>1</sup> More experimental work is required; and for this I shall have little time till the summer. But it opens up the possibility of obtaining quantitative estimates of the effects of contrast.

5. The net result of these corrections is that I now make 12% of white stimulus as the value of mid-gray for continuous shading with my amended black.

6. The constant to be added to each term of the series of sum-values for stimulus on this curve, so as to convert it into a logarithmic curve, is 1.895. When this constant is added the stimuli are in geometrical progression. This suggests an absolute scale obeying the logarithmic law. But we are still quite ignorant (from the experimental standpoint) of the relations of stimulus to sensation beneath the threshold of our arbitrary zero.

7. If the illumination of either a disc for continuous shading, or one with graded rings, be materially reduced, the shad-

<sup>1</sup> Cp. *Croonian Lecture*, Proc. R. S., Sept., 1901.

ing or grading, as the case may be, is no longer satisfactory. This fact was noted for ring discs by Delbœuf; but his hypothesis of 'tension' (*Examen critique de la loi psychophysique*, p. 148) to explain the fact is unnecessary. The occurrence of the phenomenon is a necessary consequence from the nature of the curve. Since reduced illumination lessens the luminosity of the white but leaves the black relatively unaffected, the length of the curve is diminished at its upper end. But from the character of the curve a reduction of the stimulus-luminosity of full white by, say, 12% is accompanied by a reduction of its sensation value by rather less than 3%; and the new mid-point for sensation requires relatively more white stimulus than the old mid-point required.

A particular example will serve to make this point clearer. In the well-illuminated disc the mid-point for shading from black into white requires 12% of white stimulus. Let us suppose that the illumination be reduced by one half. Instead of 100% stimulus for full white on the disc we have only 50%. Now reference to the curve (Fig. 1) shows that the sensation value of 50% stimulus is 83% on the scale of sensation. The mid-point for sensation between this and black will be  $\frac{8.3}{2}$  or 41.5%, for which the required stimulus is seen from the graph to be 8%. So that whereas the proportional amount of stimulation for mid-gray with full illumination is  $\frac{12}{100}$  that for the new mid-gray with halved illumination is  $\frac{8}{50}$ . In other words, the amount of white in the mid-gray has to be increased by one-third, or in the proportion of 12 to 16. This necessarily follows, in principle, from the fact that diminished illumination involves a greater proportional reduction of stimulus than of sensation. Fortunately, however, for the requirements of practical observation, a reduction of the illumination by 15% only alters the stimulus value of the mid-gray in the proportion of 12 to 12.87.

8. I have carried further the experimental work on color-shading. Making use of Sir William Abney's method of measuring the luminosity of any colored surface by matching it with a gray, I have thus determined the luminosity in terms of stimulus of five colored papers. Their positions on the white-black curve are given in the accompanying diagram (Fig. 1). If the

curve be taken as approximately correct, this gives not only their stimulus values, but their corresponding *sensation values* on the arbitrary scale. Thus is afforded a means of calculating what should be the mid-point for the shading either of the color into black, or of one color into another. In the shading of red into blue, for example, we must deal with that portion of the curve

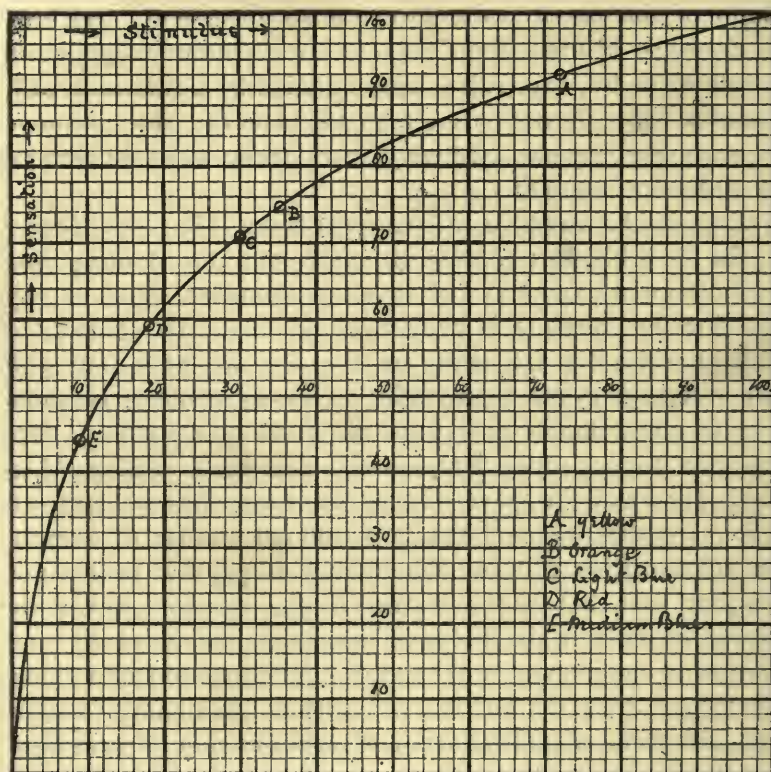


FIG. I.

which lies within the limits assigned by the luminosity of red and that of blue respectively. In the following table the percentage mid-point is given, first as determined by experiments based on the method of shading (column I.), secondly as calculated from the curve from the luminosity as determined on Sir William Abney's method (column II.).



	I.	II.
Yellow on Black.	13.5%	13.8%
Orange on Black.	18	18.6
Light Blue on Black.	19	19.7
Red on Black.	23	23.6
Medium Blue on Black.	28	29.5
White on Medium Blue.	25	24.7
White on Red.	30	30.6
Yellow on Light Blue.	40	39.1
Red on Medium Blue.	44	43.
Orange on Medium Blue.	36	35.4

It appears from the above that the relative effect of stimulation by any colored surface is proportional to its luminosity.

9. I have redetermined the minimum visible of white on my new black as .1%, and that of such black on white as 1.1%, and have made some progress in determining the minima visibilia on intermediate stages of gray. The results so far obtained seem to show that the least perceivable increments are in geometrical progression; but I am not prepared to discuss them without further experimental work. The curve obtained from them is certainly not the same as that for continuous shading.

10. I find that the minimum visible of any color on black requires an amount inversely proportional to the luminosity of the color as compared with white. The luminosity of my red paper being 18% (*i. e.*, equivalent to a gray with 18% of white stimulus), the minimum visible is by calculation  $.1 \times \frac{100}{18} = .5$ ; the amount determined by experiment being also .5%.

## DEVELOPMENT OF VOLUNTARY CONTROL.<sup>1</sup>

BY J. H. BAIR.

### I. INTRODUCTION.

How we acquire voluntary control over a muscle is a problem of vast interest and importance, because of the light its solution would throw upon the nature of the will; and the intense interest manifested in its solution is shown by the solicitude with which the actions of children are observed, the various experimental investigations, both direct and indirect, which are being carried on, and the many attempts to formulate theories which shall reckon and be consistent with all the facts.

This article is concerned with an investigation of the conditions and processes involved in getting voluntary control over a muscle or group of muscles. So far as we are aware, comparatively little or no work has been done by the experimental method (that here employed) toward the solution of this problem. The method usually employed is the genetic, which is at a disadvantage in two respects: (1) It is impossible to determine when a movement becomes voluntary, since the child begins its life with a series of movements. There is no uniformity of development, and any conclusions that may be drawn are necessarily too general for a definite theory of the will. (2) This method does not afford the advantage of introspection.

We have approached the problem in a definite way, selecting a muscle over which we had, as yet, no control, and by developing this control and keeping accurate records of the progress of the development and careful introspections of the accompanying mental states, discovered a number of facts, which, together with others found in the literature on the subject, justify us in drawing certain conclusions.

The muscle selected to work with was the *retrahens* of the

<sup>1</sup> No. IV. of the *Studies from the Psychological Laboratory of the University of Michigan*.

ear (see Fig. 2, p. 480). This muscle was chosen: (1) Because of its complete isolation from other muscles; (2) because of the complete inability of most persons to contract this muscle; (3) because of the comparative ease with which it can be worked, and the definite movement of the ear attending its contraction. The difficulties connected with the selection of this muscle were: (1) That of getting a record of the movement for comparison; (2) that of getting subjects unable to move their ears, and at the same time prepared to give good introspection. There were other difficulties of a more general character which will be discussed when we describe our apparatus and method of work (Section II). The above difficulties were met in a satisfactory way, as we succeeded in devising practical apparatus for recording the contractions, and also in securing desirable subjects to work with. Of the fourteen subjects who rendered us their services, only two could move their ears at the beginning of the experiment and these only when vigorously raising their brow. The remaining twelve had no idea of the movement and could not produce it, however hard they tried.

## II. APPARATUS AND METHOD.

The difficulties of devising a practical apparatus for registering the ear movement on the kymograph have already been referred to. The scheme thought of and employed throughout the experiment was to have two Marey tambours connected by a rubber tube so that the lever of one tambour would respond to the movement of the other. The great difficulty was in attaching the receiving tambour to the ear. Our first method was entirely unsatisfactory and was used only until a new apparatus could be prepared. The head was placed in a kind of stocks and fastened. The receiving tambour was fastened to a standard which was brought up to the head, and the tambour was so adjusted to the ear that the lever, which had a notch in it to fit over the ear, rested on the top of the ear and communicated its movement to the kymograph. The contraction of the muscle corresponded to the downward stroke of the recording lever. The stocks were unsatisfactory for two reasons. (1) If the clamps were fastened so tightly about the head as to eliminate



all head-movements it was unendurably painful. (2) If they were not thus tightly fastened errors would come in and it was impossible to determine what part of the record represented head, and what part ear movements.

By the second apparatus these difficulties were overcome. The ear movements alone were registered. The apparatus was comfortable, and the head could be moved freely in any direction without modifying the record. Also, a tambour could be (and was) attached to each ear and a record taken of each ear at the same time without interfering with the other.

This new contrivance (helmet), a cut of which is shown below, Fig. 1, is made up of the following pieces: The first

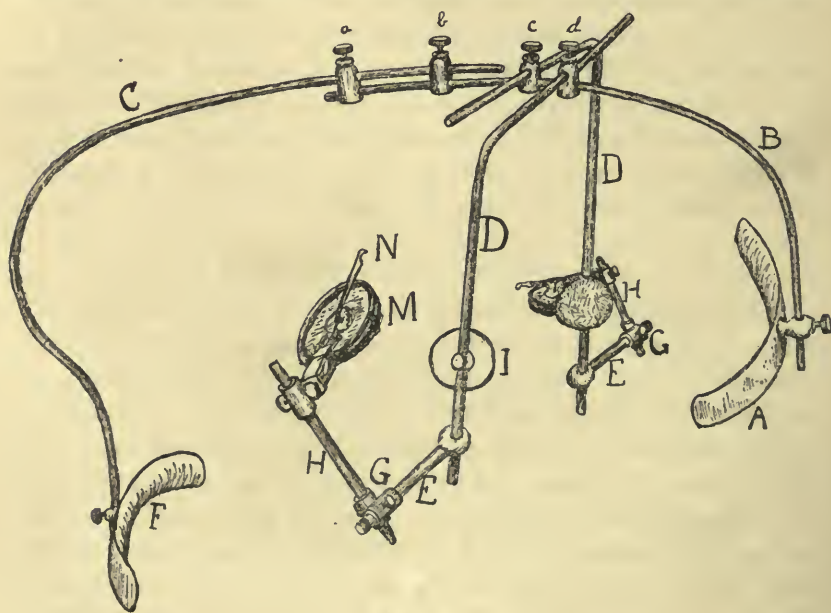


FIG. 1.

piece, *A*, is a padded plate bent to fit the forehead. It is attached by means of a post, fitting and thumbscrew to rod *B*. Rod *B* is 6 mm. thick and 25 cm. long, gently curved to fit over the median line of the head, and 6 cm. from its forward end is a sharp bend of almost 90°. At 9 cm. from the other end are two binding posts with right-angle fittings. The lower fittings are fixed to the rod *B*, the upper, through which the rod *D*

passes, are connected with thumbscrews *c* and *d*. Rods *D* are 30 cm. long and bent at right angles at their middle point. To these rods are attached the temple pads, *I*, in the same manner as *A* is fastened to *B*, and *F* to *C*. These pads, when they are properly adjusted, fit snugly to the temples. *a* and *b* are also binding posts attached to *B*, *a* at the extreme posterior end and *b* 4 cm. from it. The lower fittings are parallel with the upper, through which *C* passes. This rod *C* is 26 cm. long and is bent to fit the median line of the occipital part of the head. To its lower and posterior end is attached plate *F*, which fits the nape of the neck, is similar to *A*, and is similarly attached. The apparatus is adjusted by loosening the thumbscrews *a*, *b*, *c* and *d*. *C* and *B* are pushed together until *A* and *F* fit closely to the head, when *a* and *b* are tightened. Then *D* and *D* are pushed together until the pads *I* fit closely to the temples, when *c* and *d* are tightened. *E* is a hollow rod 9 mm. thick and 10 cm. long, with a loop at one end which fits over rod *D*. A set screw with a head the same size as the rod extends the whole length to the loop and holds *E* in the desired position. The rod *H*, of the same length and thickness as *E*, to which the tambour *M* is attached, is fastened to *E* by a right-angle clamp with set screws. *N* is the lever of the tambour and is easily adjusted to the ear so as to communicate to the drum the motion of the ear.

The other apparatus used in this experiment was that for giving the subject the idea of the movement by means of artificial contraction of the retrahens. It consisted of the following: An induction coil (alternating current), key, ammeter and electrodes. The current was kept constant by means of the ammeter and the gauge on the secondary coil. One of the wires from the secondary coil terminated in a large sponge moistened with saline solution. This was held in the hand of the subject. The other wire, also terminating in a small sponge, was periodically applied by the attendant to the retrahens of the subject; but this method was soon abandoned because there was a lack of harmony, as the attendant could not always apply the electrode at the instant the subject tried to move the ear. The electrode was now permanently adjusted to the ear and the current

was applied by the subject himself by means of a key. The electrode was held in place over the retrahens by means of a wire 28 cm. long, which was bent to fit over the top of the head. The lower end was bent downward, outward, forward, upward and inward, and held the electrode attached to it exactly in place over the retrahens.

In the second series of experiments, *i. e.*, in learning to alternate the ears, an electrode was attached to each ear in the manner just described, and the key was so arranged that the subject could stimulate first one ear and then the other.

A third series of experiments was made on a new set of subjects. Here the apparatus was the same as in the other series, except that the current was entirely dispensed with. The object of this series was to see whether the movement could be acquired without the idea of the movement first being given by means of artificial contraction.

The method followed in these experiments was uniform throughout. A record of all the experiments was kept. These were dated, numbered, and the subject's name countersigned. On this record the unusual things were noted and the probable reason stated. In a notebook were also kept the introspections of each experiment, with the date, number, and name of the subject appended.

The order generally followed in the experiment was: (1) The muscle of the ear was contracted by the current, applied about once every second, the subject at the same time remaining passive. (2) The subject tried to help the current to contract the muscle. (3) He tried to prevent the current from contracting the muscle. (4) The current was withdrawn and the subject tried to move of his own accord. The number of contractions in each of these series varied from ten to thirty. Sometimes the series were interchanged to see what difference it would make on the height of the contraction.

As to the method of the introspection, the subject was allowed to state what he had noticed in the experiment, and then usually several questions were asked, care being taken not to suggest the answer expected. Frequently the subject was requested to direct his attention in a certain way, or to notice particular



things in the course of the experiment. Each subject, before being finally discharged, was requested to state briefly the process by which he learned the voluntary control of his ears. All things were considered in making out a summary of results and in drawing conclusions.

The height of the contractions, in each series, of all the curves, was measured, and each series was averaged. The accompanying tables (I., II. and III., pp. 492, 493, 494) show the varying relations of these averages as the subjects got better voluntary control over the muscle. The curves were very easily read by means of a very thin sheet of transparent celluloid, upon which millimeter lines were cut and filled with white lead. This sheet, when placed over the record, affords a rapid and accurate means of reading it.

### III. RESULTS OF THE EXPERIMENT.

Before entering upon a discussion of the results of our experiment and the processes of voluntary development, we desire to call attention to Fig. 2. This sketch of the anatomy of the parts with which the experiment deals is presented in order to give the reader, who may not be familiar with them, a better idea of the arrangement of these muscles and their motor nervous connections, and also to facilitate the explanation of our facts and of the physiological processes by which a voluntary movement is acquired. The figure shows that the three ear muscles are well isolated from each other and from the other muscles of the head and face, and that the connections between them are mainly nervous. In our sketch are shown only the motor connections. The sensory nerves supplying these parts are similarly distributed and for that reason are not presented in the figure.

The *retrahens* (1), the muscle worked with in the experiment, notwithstanding the fact that most persons have no control over it, is adequately supplied with both motor and sensory nerves, and there is, therefore, every reason to believe that control over it is acquired in exactly the same way as control over any other muscle, and that the processes involved are identical. All of our subjects acquired control over this muscle

in the same way, and the introspections were also fairly uniform, so that the results, in general, of each subject corroborate those of the others.

There were three stages involved in learning the voluntary movement of the ear. Each of these three stages will be discussed separately and a typical curve submitted to illustrate it.



FIG. 2.

Fig. 2 shows the muscles of the head and their motor nerve connections: 1. Retrahens. 2. Attollens. 3. Attrahens. The heavy lines are branches of the 7th (facial) nerve, which is the motor nerve of the head and face. The sensory distribution is not shown in this cut.

These curves, together with the tables showing the relations of these stages in heights of contractions, are given at the end of this section.

Before proceeding to a discussion of these stages it will be necessary to explain these curves. The curves presented are

all uniform in order of series. Each curve contains four groups of contractions. These are as follows: The group marked (1) is produced by the periodic application of the current at intervals of one second. In (2) the current is similarly applied, and in addition the subject tries to help the current. In (3) he tries to prevent it from contracting the muscle, and in (4) the current is withdrawn and the subject tries to contract by voluntary effort alone. The crests and the lines between the groups of each curve represent the ear at rest. The downward and the upward lines represent respectively the contraction and relaxation of the ear.

*Stage I.*—This stage is represented by curve I., and is the stage before voluntary power is acquired over the muscle. As will be seen, (1), (2) and (3) are practically of equal height, except that (2) is somewhat smaller, due probably to fatigue. A definite kinæsthetic idea of the movement was given from the very start by the artificial contraction, and in every case the subject felt his ear move. All the men worked with stated that where they assisted the current (2) in contracting, their subjective experience was that the height of the contraction was increased (due to their effort), when the record showed that (2) was no higher than (1). In (3), where they tried to hold the muscle against the contracting influence of the current, none felt any power at first, but after a few experiments several felt a strong power to resist, while the others retained their sense of impotency until they really got the voluntary control. As soon as this control was acquired all held the ear contracted, as shown in (3), curves IV. and V., while at the same time they fancied they were holding the muscle relaxed in spite of the contracting current. There was some reason for believing that the subject had some power of inhibiting the contracting effect of the current even before he had acquired the power of making a voluntary contraction, as is shown by (3), curves II. and III. Curve II. is the most marked case in all our curves and was for that reason selected.

In looking over Table I., at the end of this section, *i. e.*, the table of results before the voluntary movement was acquired, it will be seen that (3) is on an average smaller than (1). This is



a fact we cannot account for. It cannot be due entirely to fatigue, for it appears when (3) is given before (1). Waller<sup>1</sup> made a series of experiments in which he showed the inhibition of voluntary and of electrically excited muscular contraction by peripheral excitation. His explanation, which is in terms of antagonistic muscles, or of conflicting molecules set free by the two kinds of excitation, voluntary and electrical, does not seem to satisfy our case, because there were no antagonistic muscles, and there could not yet have been any molecules set free in the muscle due to volition, as there was not yet any voluntary control. This phenomenon disappeared as soon as voluntary control was acquired, for then, as just stated, the subject unconsciously held his ear contracted.

It will be seen in (4), curve I., that although the idea of the movement was given again and again, by the contracting current, the movement could not be reproduced when the current was withdrawn. Here the idea of the movement was definitely given; but the idea of the movement does not seem to be sufficient to enable us to reproduce the movement, as it is believed to be by several prominent psychologists.<sup>2</sup>

<sup>1</sup> *Brain*, Vol. XV., p. 35.

<sup>2</sup> In a recent experiment on 'The Mental Life of the Monkeys,' by E. L. Thorndike, published as a monograph (*PSYCHOLOGICAL REVIEW*, May, 1901), the last in a series of similar experiments made on cats, dogs, chicks, etc., reported in a monograph, entitled 'Animal Intelligence' (*PSYCHOLOGICAL REVIEW*, 1898), it was found that monkeys, as well as cats, dogs, chicks, etc., were unable to do things from being put through them. It was seen from these experiments that the animal has not the ability to form associations except such as contain some actual motor impulse. All of Thorndike's animals failed to form such associations between the sense impressions and ideas of movements as would lead them to make the movements without having themselves beforehand in those situations given the motor impulses to the movements. These experiments show that animals do not have imitative impulses, that they do not demonstrably learn to do things from seeing or feeling themselves make the movements, much less from seeing others making them.

The question now comes, is it different with man? Can man form such an association between the sense impression and the idea of the movement that he can reproduce the movement at will? Thorndike took it for granted that he can, and this is the opinion common among psychologists. Stout has written his chapter on imitation ('Manual of Psychology') on this assumption. Baldwin has worked it out into a theory ('Child and Race'). Our experiment convinces us that man is not unlike the animal in this respect. To be put through the movement does not enable him to reproduce the movement vol-

*Stage II.*—This stage, represented by curves II. and III. and by Table II., is that where voluntary movement just begins to make its appearance. In this stage the retrahens is always contracted by biting the jaws together or vigorously raising the brow. The ear was thus first reached by innervating a group of muscles over which one has already control. It was reached by making it one in the group. This group is supplied by the same motor nerve (facial). One of its branches supplies the ear, as may be seen by Fig. 2. It may be questioned whether the ear movement thus effected was not due to the pulling of the skin upon it caused by the contraction of the other muscles. We are obliged to admit that part of this movement was due to these external connections. But this is not the way the voluntary control accomplished in stage III. (where the movement could be made independently of the innervation or contraction of any other muscle) was acquired, for the following reasons: (1) As soon as the brow was vigorously raised the retrahens could be seen to swell, and when the finger was placed over it the thickening could be felt every time the brow was innervated; (2) the idea of the movement had already been given by the contracting current, and all attempts to copy the movement thus produced were futile; why should not the movement caused by the pulling of the skin likewise fail? Another explanation must be sought. This will be attempted later on (section V.), after our facts have been stated and after some of the facts bearing on this problem have been reviewed.

In (4) of this stage a decided progress of voluntary control in curve III. over curve II. is shown. The innervation of the retrahens with practice and attention has become much greater and a larger movement is effected. This stage ended with a maximum contraction of the ear accompanied by a maximum contraction of all the muscles with which it was associated. There are several inferences that may be drawn from this stage of development:

untarily. However much may be said in favor of man's superior mental qualities, 'free ideas,' etc., he is nevertheless conditioned by the same laws as the animal, and cannot learn a movement apart from its chance function in a motor impulse.

1. Learning to contract a new muscle is a matter of association with another muscle, or group, whose voluntary movement is already known.

2. There is no sense of innervation until a movement is effected. All the subjects who did not at once succeed in making a movement stated that it seemed to them like trying to do something when they had no idea of how to accomplish it. No effort was felt until the brow was raised.

3. Learning to make a voluntary movement is largely a matter of learning to relax. Relaxation is first learned by withdrawing the attention from the movement just effected. A voluntary attempt to relax it will merely tighten its tension.

4. In learning a movement the power to contract varies with the fluctuation of attention. The fluctuations in the height of contraction at regular intervals are traceable throughout the experiment. Several long series of (3) and (4) of thirty minutes each were made; in (3), especially when fatigue began to set in, the ear was regularly relaxed at periods corresponding with the length of the attention wave, and in (4) the power to contract, when greatly fatigued, also markedly varied with the attention.

*Stage III.*—In this stage two things are accomplished. In the first place the maximum of contraction is reached, *i. e.*, the voluntary contraction (4) becomes larger than the contraction of the current (1), as may be seen in curve IV. This maximum was reached by concentrating, as far as possible, the motor energies upon the ear. Secondly, this maximum of ear contraction could finally be effected without raising the brow or innervating any other muscle than the one which produces the movement desired. The brow movement with which the ear was associated is gradually relaxed, until finally it is entirely eliminated and the ear can be moved independently of it. This gradual brow relaxation we are not able to show by our curves, but we can show what amounts to the same thing in curves VII., VIII., IX. and X., which are attempts at alternation of the ears, and the principle involved is the same. These curves, VII. to X., show how one ear gradually relaxes until it does not respond at all when an attempt is made to move the other.

This is the important stage in the development of voluntary



control. It begins where the ear movement has become a part of the general movement, and cannot as yet be produced except as it is accompanied by the whole group of contractions of which it is now a part. This is the stage where it is separated out and becomes independent of the group to which it had been tied. The child of a few months is practically in this stage. It has acquired a few general movements. It is now the problem of a lifetime to learn independent movements. There is no limit to the amount of analysis, of separation, of dissociation of muscles from their group, and of learning to innervate and control each individual muscle. In the general movement, so early seen in the child, all the muscles are innervated together, and all that learning to do things implies is learning to direct the impulse, at will, into this or that channel in such a way as to accomplish the end. It implies that gradually all the superfluous movements will be dropped out, and only those essential to accomplish the end will be retained. Learning to do things implies segregation, elimination, coördination, adaptation. The motor impulse, which was at first diffused throughout the group, is concentrated upon one part of the group, until that part is moved independently of the group of which it was originally a part.

Anyone who has observed a young child will have noticed how, *e. g.*, the two arms are tied together and for a long time respond together symmetrically.<sup>1</sup> What we have accomplished in stage II., *i. e.*, moving the ear in the group of muscles innervated by the same motor nerve, is largely given to the child phylogenetically, that is, the child is born with general movements which have been of great use to the race. These general movements Bain calls spontaneous, Preyer impulsive, and Wundt automatic. They are also often referred to by psychologists as random movements. These form the basis of voluntary movements. The child separates out of this general movement a definite movement which will help it to secure its

<sup>1</sup> Preyer, 'Infant Mind,' Chap. VI.; Moore, 'Mental Development of Child,' Mon. Sup., PSYCHOLOGICAL REVIEW, Part I., Sec. 2; Baldwin, 'Mental Development,' Vol. I.; Spencer, 'Psychology'; Schofield, 'Unconscious Mind of Child'; Shinn, 'Observations on Childhood,' California Studies; Preyer, 'The Senses and the Will'; Bain, 'Emotions and Will.'

freedom. First both hands respond together, then one gradually leads and the other follows, and finally, the reach is made with either hand independently of the other.<sup>1</sup> There is a continual breaking up of groups. One arm is moved independently of the other, then one hand, then one finger, etc. How this is done we have discovered in stage III. of our experiment, where we have the most simple possible case.

In our experiment we have had the same thing over several times, first in learning to move the ears independently of the brow, then in learning to alternate the ears, then again in learning to raise the brow without innervating the ears, and finally in raising one eye-brow independently of the other.<sup>2</sup>

We have already noted that prior to any attempt to alternate the ears the stimulation was applied to the left ear only. In stage I., before any voluntary ability was acquired, the curve of the right ear was a straight line. In stage II., where there was a rise of volition by association with the brow, the right ear accompanied the left except in (1) where the current alone was applied, as will be seen in the curves. This is an interesting fact and shows that the motor discharge is symmetrical, as is the case with the child with its arm movements. It took considerable practice to break up this symmetrical movement, and to move each ear independently of the other. It was much more difficult to learn than the first part of this stage, *i. e.*, learning to move the ears independently of the brow.

The introspection of this stage gave the clue to the process involved in breaking up a movement, and when fully comprehended the alternation was learned almost at once. The introspection first disclosed the way in which an independent movement is accomplished. We found that so long as we attempted to move one ear and at the same time inhibit the other, we did not succeed in alternating, but in spite of our efforts both ears responded to the same degree. But just as soon as one ear was

<sup>1</sup> Baldwin, 'Mental Development,' I., p. 64.

<sup>2</sup> It is interesting to note that before the association was made between brow and ears the brow could be raised without moving the ears. But now, ever since the ears can be moved independently of the brow, the brow cannot be raised without moving the ears. One of the subjects has since learned to raise the brow independently again.

attended to and the other was for the time being forgotten, the one attended to responded to a greater degree than the other, as is shown by curves VII. to IX. And after a little practice continuing this process the one ear could be moved to its maximum while the other remained almost entirely relaxed, as is shown by curve X.

In trying to facilitate the alternation of the ears the current was alternately applied to the two retrahens as described on page 478, and the subject tried to innervate in the rhythm of the stimulation, but here again the other ear persistently accompanied the one wished to be moved. Here the trouble was that the attention of the subject was partly directed on the inhibition of the ear not to be moved, and the victory was won only when the attention was unified. Our general conclusions for stages I., II. and III. are :

1. Before voluntary control over a muscle is acquired, it takes more than the idea of the movement of that muscle, in order to be able to reproduce the movement. The facts of stages II. and III. corroborate the conclusions of stage I.

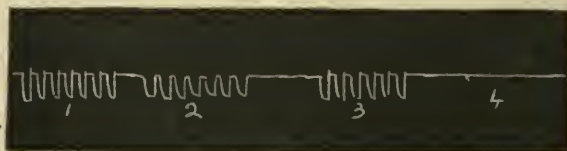
2. We first get control over a muscle in a group, and then only can we single it out and get independent control over it. The physiological reason for this fact will be given later on.

3. The more closely the attention can be directed to a movement to be made and the more nearly the part of the movement desired not to be made can, for the time being, be forgotten, the more likely is the desired movement to be accomplished.

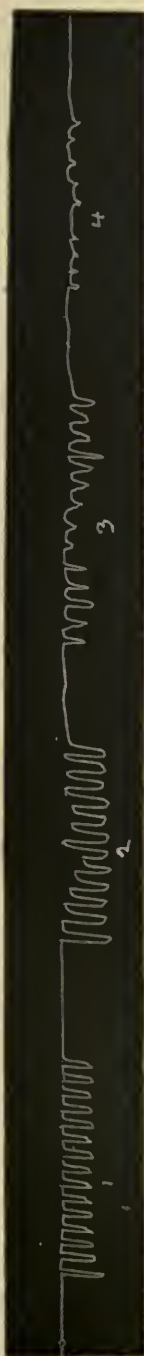
In order to make good our conclusions a new set of subjects were engaged and the experiments were repeated, except that the current was not employed at all. These experiments were only lately begun and all of the men have not yet completed the processes. All are in the third stage. One who has been directed learned very rapidly, and can now contract his ears to the maximum without raising his brow, but has not yet succeeded in alternating. All of the rest of this set of men were left to acquire the movement in their own way. They generally were more slow to learn than the men in the first experiment. This slowness in learning may be due to the fact that they have not had the advantage of the artificial movement.



It is difficult to know what was the advantage of the artificial contraction in learning the voluntary movement. It does not seem to have opened the motor tract, over which evidently an impulse had never passed. For if it had opened the motor tract there is no reason why the voluntary movement could not be made at once. Furthermore, if the muscle had been contracted by a sensori-motor impulse, *i. e.*, if the muscle upon stimulation by the current, had been partly or wholly contracted through the sensori-motor nerve connections, only the ear stimulated would have responded when voluntary control was acquired. But this was not the case; both ears responded alike to a voluntary effort, as may be seen from curve V., notwithstanding the fact that one ear had never been stimulated. In our opinion, the sensation attending the artificial contraction furnished to consciousness the location of the part from which the sensation came with reference to other sensations contiguous to it which could be voluntarily produced. In trying to reproduce this sensation, the contiguous sensations would naturally be reproduced. And if the given sensation by an extraordinary effort should be reproduced it would at once be cognized, whereas, if it had not been given, when produced by voluntary effort it could not so easily be singled out from the group in which it is given and made the object of the attention. When a sensation has been given separately it is much more easily singled out from a group, and this would explain why the first group of men to whom the idea was given learned the voluntary movement more readily than the second group to whom it was not given. We have no conclusion from these later experiments except those which corroborate those already given.



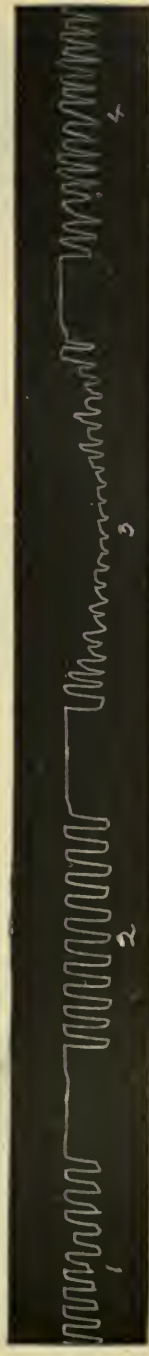
*Curve I.* 1 shows contraction due merely to the application of current. 2 shows contraction due to the application of the current plus voluntary effort. 3 is an attempt to inhibit the contraction produced by the application of current. 4 shows an attempt to move ear voluntarily without the help of current.



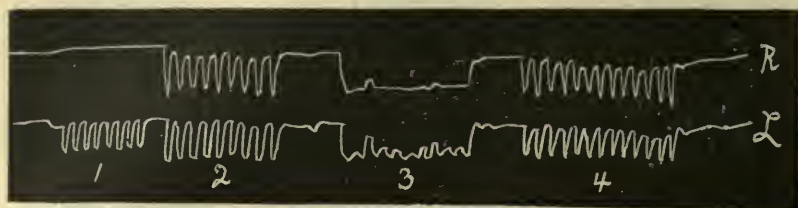
*Curve II.* Same as curve I. This curve shows a rise of voluntary contraction, and 2 shows summation effect of current and voluntary effort.



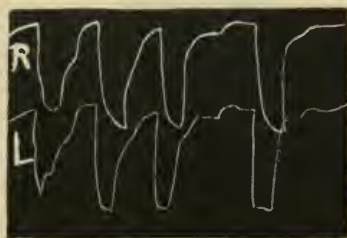
*Curve III.* Also same as I. and II. This curve shows a greater freedom of movement of ear and also more effective voluntary control.



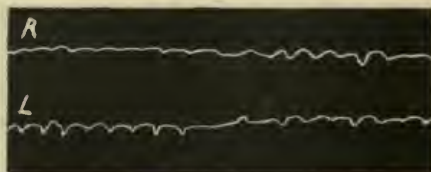
*Curve IV.* Same as preceding curves; shows complete voluntary control. It also shows power (3) of holding ear contracted.



*Curve V.* This curve shows a simultaneous behavior of the two ears, the upper of the right and the lower of the left ear. The electrode had never been applied to the right ear, nor had there ever been an attempt to move the right ear, the current being applied only to left ear. 1 shows the response to stimulation only. The right ear in this case remains at rest. 2 shows voluntary movement plus stimulation, and it will be seen that the right ear moves as far as the left. 3 shows an attempt to hold the left ear against the movement and the right ear responds also. 4 shows voluntary contraction of the left ear and the right ear responds to the same degree.

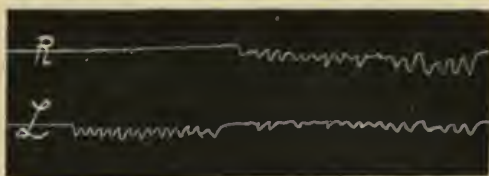


*Curve VI.* Shows the movement of the two ears of a subject who had no idea of ear movement when the experiment was begun, nor was the current ever applied to either ear.

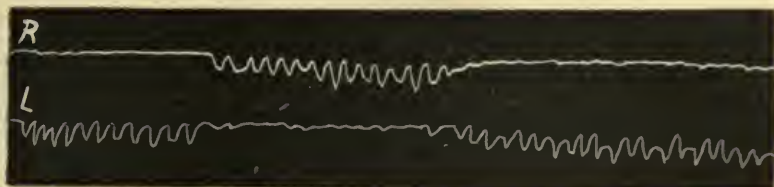


*Curve VII.* Shows first rise of power to hold one ear while the other is being moved voluntarily. The upper curve being the movement of right ear and lower the simultaneous behavior of left.

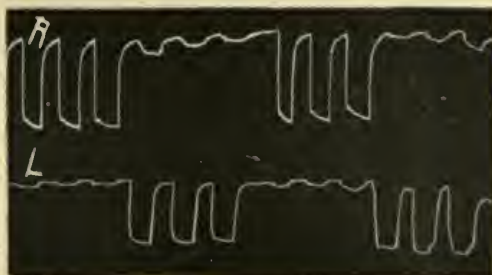




*Curve VIII.* This curve shows a more complete power to hold one ear while the other is moved voluntarily.



*Curve IX.* This curve shows a decided progress over curve VIII. in holding one ear relaxed while the other is contracted.



*Curve X.* This shows the power to contract one ear to the maximum while the other is almost entirely relaxed.

In the following tables, I., II., and III., arranged to correspond to the three stages of development of voluntary control, columns (a) show the average height of contraction, measured in millimeters, of each of the series (1), (2), (3) and (4) of each curve; columns (b) show the average of the lack of power to relax in each of the groups (2), (3), and (4), likewise measured in millimeters.

By comparing the averages of these four series throughout the tables, some idea may be formed at what rate the voluntary contraction is acquired, and also at what rate the power to relax was acquired.

Column (b) in (3) shows the average at which the ear was held contracted in the attempt to hold it against the current.

TABLE I.

Name of Subject.	No. of Experiment.	Date of Experiment.	(1) Contraction caused by stimulation of muscles only.		(2) Contraction caused by the stimulation plus voluntary effort to move.		(3) Contraction caused by the stimulation plus voluntary effort to inhibit its effect.		(4) Voluntary effort to move without any stimulation.	
			(a)	(b)	* (a)	(b)	(a)	(b)	(a)	(b)
Ps.	1	Nov. 2	1.8		2.1		2.0		No Contractions.	
St.	1	" 7	1.0		.75		.75			
"	2	" 12	1.6		1.25		1.1			
"	3	" 12	1.9		1.9		2.0			
"	4	" 14	1.3		1.2		1.2			
Br.	1	" 7	1.0		.9		.95			
Cn.	1	" 8	.7		.7		.7			
"	2	" 12	1.6		.95		.8			
"	3	" 15	1.2		1.3		1.2			
Ba.	1	" 1	3.0		2.8		3.0			
"	2	" 3	1.1		2.8	.4	2.0			
"	3	" 4	2.5		2.5		2.4			
"	4	" 5	2.4		2.3		2.1			
Nr.	1	" 7	.6		1.1		.7			
"	2	" 8	1.1		1.2		1.0			
"	3	" 12	1.1		1.8		2.3			
Kn.	1	" 6	1.1		1.1		1.0			
"	2	" 9	1.0		1.5		1.1			
"	3	" 19	1.4		1.8	.3	2.6			
"	4	Dec. 7	1.8		1.8		.9			
Mt.	1	Nov. 2	1.0		.8	.5	.9			

TABLE II.

Name of Subject.	No. of Experiment.	Date of Experiment.	(1) Contraction caused by stimulation of muscles only.		(2) Contraction caused by the stimulation plus voluntary effort to move.		(3) Contraction caused by the stimulation plus voluntary effort to inhibit its effect.		(4) Voluntary effort to move without any stimulation.	
			(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Ps.	2	Nov. 7	1.9		2.6	.5	2.5		1.5	
"	3	" 9	1.5		1.9	.6	2.0		1.0	.4
"	4	" 12	1.6		3.0	1.4	1.9		1.5	.3
St.	5	Dec. 4	2.9		3.9	2.5	4.0	2.4	2.3	1.8
"	6	" 7	3.0		3.7	2.1	2.7	1.2	1.6	1.2
"	7	" 10	3.2		2.9	2.0	2.7	1.8	1.0	.8
"	8	" 12	2.1		2.2	1.8	2.0	1.0	.3	
"	9	" 14	3.9		4.0	2.5	3.5	2.0	.9	.5
Br.	2	Nov. 15	.9		1.0	.4	.9		.4	
"	3	" 20	2.0		2.1	.9	1.7		.5	
"	4	" 22	1.6		2.2	.5	2.0		1.5	
"	5	Dec. 4	1.4		1.4	1.2	1.1		.2	
Cn.	4	Nov. 22	2.2		2.8		2.5	1.0	.4	
"	5	Dec. 12	2.6		3.4	1.1	3.5	1.9	1.4	.5
Ba.	5	Nov. 5	3.5		5.7	1.3	2.6		.9	
"	6	" 6	2.7		3.0	.2	2.6		2.2	.5
"	7	" 8	3.4		3.6		3.3		.5	
"	8	" 9	2.5		2.9	.5	2.0		3.0	
"	9	" 9	1.9		3.1		1.5	.2	2.2	.4
"	10	" 15	2.9		3.7		2.4		1.6	
"	11	" 19	3.5		3.9	1.2	3.6	.5	1.4	
"	12	" 22	2.9		3.0		3.3	1.2	2.5	.7
Nr.	4	Dec. 11	1.2		2.4	.7	2.3	.4	.3	
"	5	" 12	2.8		2.9	1.0	2.		1.1	.6
Kn.	5	" 14	2.8		3.6	1.0	4.3	2.9	.7	
"	6	" 15	2.9		3.4	.4	2.2	1.0	1.2	.2
Ns.	1	Nov. 2	2.5		3.1	.4	1.7		2.9	
"	2	" 9	2.6		2.6	1.8	2.1	.2	2.1	
Hn.	1	" 2	2.4		2.7	.2	2.6		4.0	1.2
"	2	" 4	2.0		2.5	1.1	2.0		1.0	
"	3	" 5	1.3		3.5		1.0		1.2	
"	4	" 6	3.0		3.2	.5	2.6		2.6	.7
"	5	" 9	1.5		3.5	.4	1.8	.4	1.1	
"	6	" 9	3.2		3.2	.9	3.0		2.3	
Mt.	2	" 19	1.8		2.7	.8	1.5		1.7	
"	3	Dec. 20	{ L. 1.5 R. 0		1.1	.5	.9		1.2	
					1.3	.4	.4		1.5	



TABLE III.

Name of Subject.	No. of Experiment.	Date of Experiment.	(1) Contraction caused by stimulation of muscles only.		(2) Contraction caused by the stimulation plus voluntary effort to move.		(3) Contraction caused by the stimulation plus voluntary effort to inhibit its effect.		(4) Voluntary effort to move without any stimulation.	
			(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Ps.	6	Nov. 20	3.0		3.7	.3	2.7	1.2	1.6	
"	7	Dec. 3	2.3		2.6		2.2	.8	1.4	
"	8	" 10	3.8		4.1	.5	1.9	.7	1.8	.7
"	9	" 14	4.0		5.0		3.4	.2	3.7	1.1
"	10	" 14	3.5		4.5		2.5	1.3	2.3	
St.	10	" 13	2.6		2.1		2.6	2.0	.6	
"	11	" 14	2.0		3.3	1.8	1.6	.9	3.2	2.8
"	12	" 21	2.2		2.6		2.0	1.3	1.8	.6
Br.	6	" 4	2.1		2.1	.4	2.4	1.0	2.0	.5
"	7	" 6	2.8		3.0	.6	2.5	.7	1.5	
"	8	" 11	2.2		1.9		2.1		.8	
"	9	" 18	2.1		1.1	.2	2.0	.4	1.2	.3
Ba.	13	" 5	3.8		3.9		2.9	1.2	3.0	
"	14	" 6	3.0		3.8		2.7	1.5	3.0	
"	15	" 7	3.7		3.5		3.4	1.0	2.5	.4
"	16	" 11	3.6		3.8		2.5	1.4	2.2	
"	17	" 12	2.0		2.7		2.1	.8	1.7	
"	18	" 18	2.7		4.0	.5	2.5	1.1	4.0	
"	19	" 18	2.0		2.9		2.7	1.9	2.8	
"	20	" 18	2.5		4.2		2.5	2.1	4.4	
"	21	" 19	2.7		4.3		2.8	2.0	4.1	
"	22	" 20	4.7		5.9		5.0	2.1	5.0	
"	23	" 20	L 3.0 R 0		3.1 1.9		1.6 1.2	.7 1.1	2.7 1.6	
"	24	" 20	L 2.5 R 0		3.5 1.5	.3 .5	1.0 ?	? ?	3.8 2.0	
"	25	" 21	2.7		4.0		2.6	1.5	3.7	
"	26	" 24	2.3		3.4		3.1	1.6	3.2	
"	27	" 26	5.0		6.1		4.1	.7	2.5	
"	28	" 26	L 3.0 R 0		2.7 2.3		2.7 2.5	1.8 1.5	3.0 3.3	
"	29	Jan. 8	L 2.5 R 0		2.7 1.2		2.3 1.5	1.6 .9	2.3 1.4	
"	30	" 22	4.7		5.9		5.0	3.1	5.0	
"	31	" 22	L 2.0 R 0		2.7 2.6		2.0 ?	1.4 ?	2.5 2.0	
"	32	" 23	L 2.4 R 0		3.0 2.5		2.0 ?	1.4 ?	1.8 .9	
"	33	" 28	L 3.0 R 0		4.5 4.8	.9	4.1 4.0	3.6 3.6	3.5 4.1	
Hn.	7	Nov. 22	3.5		3.5		3.0	.6	1.8	
"	8	Dec. 5	1.7		2.7		1.4	.9	2.9	
"	9	" 9	1.8		2.5		1.6	1.0	2.2	
"	10	" 11	2.0		2.3		1.1	.3	2.6	
"	11	" 17	2.0		4.3		1.7	.4	3.0	
"	12	" 19	2.5		4.5		2.3	.8	3.7	
"	13	" 19	2.7		4.5		2.2	1.2	4.0	
"	13	" 12	2.1		4.2		2.0	.5	4.1	

## IV. THEORIES OF THE WILL.

There are, roughly classifying, three general theories<sup>1</sup> of the will. The first one holds that voluntary movements are entirely acquired by the individual. The second maintains that the power is given by inheritance, as in the case of many animals. But in the case of man there is an after-birth evolution, and only as the organs become perfected or at least far enough developed to take up their function, do they do so spontaneously. These two theories are not consistent with the facts and need not, therefore, be further discussed. The third theory, that held by most of the prominent psychologists, physiologists and evolutionists of to-day, is a compromise between the other two. It is a conscientious attempt to be consistent with all the facts and it reckons with the data from all sources of investigation. This theory is formulated with many modifications by its different exponents. The theory was first formulated from a certain number of facts. Each time the theory was stated new facts, which were the result of observation and investigation, had to be taken into consideration, so that the theory itself underwent a gradual evolution. The theory in its latest formulation is so modified as to embrace generally the facts of evolution, child development, experiment, pathology, etc.

On account of its close relation to our facts, this theory, or rather the individual formulations of it by its several representatives, will be briefly stated. This will greatly facilitate the explanation of our facts and their bearing on these various theories.

Famous among the first modern theories that attempt to co-ordinate all the observed facts with the theory of evolution, and a theory which still has a rightful claim in modern psychology, is that of Bain.<sup>2</sup> He holds that a primordial element called spontaneous energy or surplus activity of the body, a predisposition of the moving organs to come into operation of their own accord, previous to and apart from the stimulation of the senses or of feeling, is the fundamental basis of the development of voluntary movement, that the profuse activity seen in infancy and childhood springs in a very great degree from an inherent

<sup>1</sup> Kirkpatrick, 'Development of Voluntary Movement,' *PSY. REV.*, Vol. VI.

<sup>2</sup> Bain, 'Emotions and Will,' pp. 304 ff.

active power, with no purpose at first except to expand itself, and that such activity gradually comes under the guidance of the feelings and purposes of the child. It is surplus energy discharging itself without waiting for the promptings of sensation, and during the course of experience and education these discharges are so linked with feelings as to be an instrument of our well-being in prompting pleasures and removing pains. Some of these random movements happen by chance to bring the organism into some happy adjustments which are pleasurable. There is an association between the pleasure and the movement which brought it about, and the memory of the pleasure, in order to prolong the gratification, incites a repetition which serves to *fix* the adjustment and make the movement more and more automatic.

Spencer's theory<sup>1</sup> was in many respects novel. He also starts with random movements. These are caused by a diffused discharge of molecular energy throughout the motor mechanism. There are by heredity certain paths through which the greater portion of the discharge tends to pass. But there is always a leaking from these special paths into other channels, so that every specific muscular excitement is also accompanied by some general muscular excitement. This results in diffused movements, and an adaptive movement is thus made by chance. This happy movement is 'clinched' in the following way: A successful movement is accompanied by pleasure, and consequently a large draught of nervous energy is concentrated toward the organ from whose movement the pleasure came. The previous discharge which happened to send an impulse through the line which caused the pleasurable movement has opened a channel of escape and consequently has become a line through which sufficient molecular motion is drawn to repeat the action. As the discharge is repeated the path becomes more permeable and of relatively less resistance, so that the discharge is more easily directed along this line each time, until finally the channel becomes so well defined that the discharge takes place through it without much conscious effort of the will and tends to become automatic.

<sup>1</sup> Spencer, 'Psychology,' I., pp. 496 ff.



Sully<sup>1</sup> is practically a repetition of Bain and Spencer. In brief, he holds that the child is born with certain random, reflex, and instinctive movements, which are essential to the voluntary-motor process because they bring to the child the experience of certain results of movements, *i. e.*, are pleasurable or profitable, and the association of these results with particular varieties of movement. These original involuntary movements bring about a favorable change in the child's condition, whether to lessen discomfort or to introduce a positive element of pleasure, and the child's attention is called to them. The association of the pleasure with the movement which caused it is the necessary antecedent of the conscious pursuit of an end.

James<sup>2</sup> also recognizes the importance of random movements as necessary antecedents of voluntary movements. He says that when a movement has once occurred in a random, reflex, or involuntary way, there remains in the memory an image of the movement which can be desired, proposed, and deliberately willed. But sensations and movements must be given before we can have a voluntary act.

Külpe<sup>3</sup> also holds that voluntary action is derived from accidental movements and their consequences. He accepts as far as they go the theories of Herbart, Lotze, and Bain, in their explanation of the origin of voluntary actions through the mechanical connection between the idea of the result and the movement, but if this were the only condition there could be no self-determination and man would be an automaton. Here he accepts the explanation of Wundt,<sup>4</sup> who gets over the difficulty by distinguishing between associative and apperceptive connections. The former are the results of given relations between ideas, while the latter imply a comparative and selective activity of the subject, and will and apperception for Wundt are but phases of the same process.

Dewey<sup>5</sup> likewise recognizes in the young child original motor impulses, which have no definite adjustment, but are spent

<sup>1</sup> Sully, 'The Human Mind,' vol. II., pp. 189 ff.

<sup>2</sup> James, 'Psychology,' vol. II., pp. 487 ff.

<sup>3</sup> Külpe, 'Outlines of Psychology,' pp. 449 f.

<sup>4</sup> Wundt, 'Grundzüge der physiol. Psychologie,' 3d ed., vol. II., pp. 469 ff.

<sup>5</sup> Dewey, 'Psychology,' pp. 359 ff.

through the whole system and give place to random movements. These original impulses are discharged through the channels of least resistance. These channels of least resistance are phylogenetically determined. The tract which has been of most use to the race is physiologically the most open, and is, therefore, the one through which the greater part of the discharge is made. Every movement is accompanied by a sensation which becomes the symbol of the movement. These muscular sensations are constantly reported to consciousness and by association we learn what act they stand for, and the movement becomes localized to the degree in which the idea of the act becomes definite. A child on first learning to do something moves the whole body, but gradually the bodily movement is eliminated, the motor impulse is more definitely directed, until finally only the part willed to be moved moves.

Preyer's theory<sup>1</sup> is also very similar to those already stated. It is, in short, as follows: Many reflexes of the infant at birth are already strongly marked. These have a great phylogenetic significance, because, through their frequent repetition, the harmonious co-working of many muscles as a means of warding off what might be unpleasant or injurious is soon perfected and the development of the will is made possible through these co-ordinations.

Baldwin<sup>2</sup> in a general way accepts the Spencer-Bain theory. He does not think, however, that the association which causes the repetition of any movement is between the pleasure of a happy chance adaptation and the movement which brought the pleasure. He asserts that the association is rather between the pleasure and the pleasure-giving thing, which is not necessarily contained in the fact of one movement rather than another. The pleasure is not in the movement but in what it gets for the organism. Another point at which he digresses is in his theory of imitation. Spencer and Bain hold that all our voluntary movements are *copies* primarily given in the random, reflex and instinctive movements, and that the individual, again to use Spencer's term, *clinches* the movements by associating them

<sup>1</sup> Preyer, 'The Senses and the Will,' pp. 326 ff.

<sup>2</sup> Baldwin, 'Mental Development, I., pp. 367 ff.

with the sensations accompanying them. Baldwin, on the other hand, insists that the adaptive movement is not necessarily such a copy, but that the new movement may be an imitation of a movement, the idea of which is given through some other than the kinæsthetic sense. Through the *try-try-again* disposition, especially of children, 'external copies,' *i. e.*, those given through sight or audition, are persistently imitated; and, to quote his own words, "The accommodation of an organism to a new stimulation is secured, apart from happy accidents, by the continued or repeated action of that stimulation, and its 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 excess stimulation of the organs most nearly fitted by former habit to get that stimulation again."<sup>1</sup>

With this brief statement of the general theories as to the rise of voluntary control of the muscles and the factors involved in a volitional act, we will proceed to the interpretation of our results and their bearing on these theories.

## V. INTERPRETATION OF FACTS.

In another section (III.) we have already attempted to describe the facts of this experiment. It now remains for us to interpret these facts and state their bearing on the theories above mentioned.

One of the aims of the experiment was to determine whether the definite idea of a movement is sufficient to enable one to reproduce it at will. Our results justify us in saying *No*. As has already been explained in the preceding discussion, a definite kinæsthetic idea of the movement of the undeveloped ear muscle was given by the artificial contraction of that muscle. The idea of the movement thus produced was a memory image of the sensation which was the concomitant of the movement, plus also a visual memory image of the movement. Yet the contraction could not be made voluntarily, not even after it was repeated a sufficient number of times thoroughly to impress the

<sup>1</sup> Baldwin, *op. cit.*, p. 179.



sensation and definitely to fix the association between the muscular sensation and the visual impression of it. With all this preparation the subject failed to reproduce the movement voluntarily and there was no sense of innervation. To refer again to the introspection, the subjects could feel no output of nervous energy, or, to quote the words of one of the subjects, "It seemed like trying to do something when you have no idea how it is done. It seemed like willing to have the door open or some other thing to happen which is beyond your control."

All that was done by this preliminary work, *i. e.*, producing the contraction artificially, was to give a vague idea of the direction the motor impulse would have to take to reach the ear. And in the great effort to reach the ear a great deal of motor energy was discharged into muscles lying approximate. The brow was raised, the teeth clinched, and this proximity was perceived and a more strenuous effort was made to get closer, until finally the ear was budged. As soon as the ear was reached the sensation was cognized and it became the fixation point of the attention. In other words, the attention was narrowed down from the general sensation of the innervation of the adjacent muscles to the sensation of the specific movement sought for. The first time the ear movement was effected by voluntary effort the sensation was but faintly recognized, on account of the feeble discharge going to this muscle as compared with that going to the other muscles of the group. Here the subjects who had had the benefit of the artificial contraction had a positive advantage over those who had not, in that they had already a definite memory image of the movement, and as soon as it was reproduced, it would be recognized, whereas it could not be recognized, except in a very vague way, in the second case. It is a well-known fact that any particular sensation or element of a complex can be recognized or singled out more easily in the complex if it has been presented previously by itself, as, *e. g.*, in the case of a compound clang, any note in it can easily be singled out by the attention if previously given separately. And this is as true of the other senses as of the auditory.

Although the sensation attending the ear movement was

identified as soon as the least response was effected, yet at the same time the subject was perfectly aware that only a small part of the total sensation came from the ear; and in repeating the innervation he would try to have a greater part of this complex sensation experienced from the ear. In thus trying harder to move the ear farther the brow would also be raised higher, the jaws set more tightly, etc., so that relatively the sensation experienced from the ear may not have been greater than before, but absolutely it was very much greater. The process continued until the ear muscle could be contracted to its maximum. The ear was definitely located with reference to the other muscles which were innervated with it. An association was made between the sensation from the ear and the sensations coming from the whole group.

As soon as the sensation arising from the movement of the ear was associated with the concomitant sensations of muscles close to it, over which there was already definite voluntary control, there was a basis for learning the voluntary control of the ear. To put this into a general statement would be to say that in order to acquire voluntary control of a new muscle it must be associated with the movement of some muscle we already know. This principle, if it can be shown to be true, which, it seems to me, our facts would make indisputable, is entirely consistent with a well-known pedagogical principle of association on the intellectual side, namely, that we can only get hold of a new thing in terms of what is already known. If we cannot associate it in any way with our past experience it can never have any significance for us.

We now come to the second part of the process (stage III. previously described). We have gotten a hold on the ear by association with muscles known. The next thing to do, now, since this association is made and the ear can be made to respond to an effort of the will, is to break up this association and move the ear independently. In the accomplishment of this the attention is the all-important thing. This sensation which satisfies the effort of the will, *i. e.*, the one attending the contraction of the retrahens, becomes the thing upon which the attention is focused. The associated muscles are gradually

relaxed. They respond less with each trial until finally only the ear responds. The rapidity with which this is accomplished is entirely proportional to the concentration of the attention. The attention directed to one activity tends to inhibit the other activities which may be in progress at the time. This is a conclusion which we could not help but draw from our facts and introspection. It was the experience of all that the more completely the attention could be concentrated on the movement to be made the greater was the attending success. When all the attention was put upon the movement while the brow was forgotten the movement could be made without any of the concomitant brow movement, whereas when part of the attention was directed on inhibiting the brow movement while the other part was directed to the ear movement both ear and brow responded together.

Our conclusion as to the nature and importance of attention is corroborated by many observers. Mrs. Moore<sup>1</sup> has noticed the same fact with reference to children, and we will quote what she says: "Inhibition was first induced by a sense stimulus, which in drawing attention into another channel caused a movement already in progress to cease." Preyer<sup>2</sup> says that inhibitions are positive willings in another direction, and he lays it down as a pedagogical principle not to have any 'Don'ts.' Inhibition, Ribot<sup>3</sup> furthermore holds, is connected with the attention process, and when we attend to one idea or impression there is a momentary inhibition of the other ideas or impressions. Exner<sup>4</sup> also gave it as his opinion that inhibition (*Hemmung*) is not by a positive effort of the will shutting out a movement in process, but it is rather taking the attention away from the movement and directing it into another channel. James is certainly not far wrong when he says that one process inhibits another by appropriating its molecular energy. The familiar facts of everyday experience are sufficient to show the pertinency of the above

<sup>1</sup>Kathleen C. Moore, 'Mental Development of Child,' *PSYCHOL. REVIEW*, Monograph Sup., Part I., Section 3.

<sup>2</sup>Preyer, 'Infant Mind,' p. 64.

<sup>3</sup>Ribot, 'Psychology of Attention.'

<sup>4</sup>Exner, 'Psychologische Erklärung der psychischen Erscheinungen,' p. 72.



conclusion, *e. g.*, trying to banish unpleasant thoughts only makes them more vivid. We banish evil thoughts by substituting others for them. It implies a moving away from them of the attention. If we try not to do a thing and keep our attention on the 'not doing,' we are most likely, in spite of ourselves, to do it. A bicyclist in keeping his attention on a stone he wishes to avoid is most likely to strike it.

Another, an involuntary consequence of attention and attentive concentration, is the arrest of the bodily movement or limbs. When we fall into a deep study while out walking, *e. g.*, we ease our pace and may even come to a standstill. "If an idea suddenly seizes upon us with full force, we interrupt what movement we may be making, quite automatically."<sup>1</sup> Concentration of the attention has, accordingly, from all these facts here enumerated and from the facts of our experiment, an inhibitory effect on the motor innervation of muscles whose activity is not implied in the attentive act.

The importance of attention in learning voluntary movements, as well as in learning anything else, is everywhere emphasized. Recent experiments made by Bryan and Noble<sup>2</sup> on the learning of the telegraphic language serve as a good illustration. "It is intense effort that educates. Each step in advance costs as much, and indeed more, than the former. Men do not as a rule become experts, because they will not make the painful effort necessary." Having now stated the facts, and the processes and factors involved in the learning of voluntary movements, from the psychological side, we will proceed to explain the same facts from the physiological side, for it is only when explained from their physiological basis that these facts can have any real significance.

The retrahens, notwithstanding the fact that most persons have no voluntary control over it, is adequately supplied with motor and sensory nerves (see Fig. 2). It is probably because ear movement has become of so little importance to man, that the motor tracts supplying the ear have become sealed, so that none of the molecular energy (to borrow Spencer's terminology),

<sup>1</sup> Külpe, 'Psychology,' pp. 433 ff.

<sup>2</sup> 'Telegraphic Language,' *PSYCHOL. REV.*, Vol. IV., pp. 50 f.

which is diffused throughout the organism of the infant in proportion to the physiological openness of the different motor tracts (this openness being phylogenetically determined), is discharged through them. Many who believe in the transmission of characters hold that it is largely a matter of nervous susceptibility—that the resistance a nerve offers to this original discharge throughout the nervous mechanism is in inverse ratio to the amount of use it has been to the race. This hypothesis, if it be true, would explain why the random movements of the infant, *e. g.*, those general movements of the hands towards the mouth, etc., so closely resemble those necessary to survival in the struggle for existence. The child, we might say, is born with general motor tendencies, which from the direction of these motor discharges reflect in a large degree the history of the race.

The ear muscles, on account of their long uselessness, are among the few muscles in the human body that have not, at some time or other, been innervated. No motor impulse has ever passed over them on account of the relatively great resistance they offer, and therefore the first thing to be done is to force an opening (*Bahn machen*). The first movement of the ear was effected by an intense innervation of the muscles (*i. e.*, those already under the control of the will) physiologically connected with it by means of the same motor nerve. Referring again to our illustration of the anatomy of these parts (Fig. 2., p. 480), it will be seen that the same motor nerve (facial) which supplies the head and face supplies also the ear. By vigorously raising the brow the ear also made a slight response to the impulse. This may be explained as follows: We have a vague idea of the location of the ear through sensation in general; we concentrate our energies in that direction, which results in a motor discharge in the same general direction, but the discharge follows the paths of least resistance, and all the muscles supplied by this nerve and its branches will be contracted in proportion to the ease with which the impulse is diffused through the different branches. The ear may not be reached at first. Up to a certain point the additional discharge of motor energy finds a way of escape more easily through the old channels than through the new, but beyond this point a part of the impulse is forced

through the new tract, and each time the process is repeated more of the impulse is discharged through this line until it is so well opened that the muscle can be contracted to its maximum. As the energies are now more definitely centered on the reproduction of the specific sensation of the movement originally sought for, more and more of the motor impulse is directed through the channel which produces this desired sensation, and continually less leaks out through the old channels, until eventually the whole impulse is so directed as to produce specifically the movement desired, and this is *voluntary* control. The concentration of the energy upon one channel of discharge, or one course of action, seems to have one of two effects or both. It either lessens the resistance along the line of concentration, or it increases the resistance of the other possible paths, and it is not improbable that it does both at the same time. Wissler,<sup>1</sup> in a series of experiments on the diffusion of the motor impulse, found that, *e. g.*, when a motor discharge is directed to the extreme accessory muscle of the arm, the law of diffusion is, primarily, to the muscle directly innervated, and secondarily to the adjacent related muscles in the order of their distance anatomically from the muscle innervated. These facts seem in direct support of the view above advanced, namely, the importance of the attention; the resistance is much less along the line in which the discharge is directed, *i. e.*, toward the fixation point of the attention, and the farther away from this point any process is, the greater is the inhibition of the motor tracts supplying it.

We have no reason to believe that the physiological processes involved in the learning of the ear movement are not exactly the same as those in the acquirement of any other movement, as, *e. g.*, that of the hand. As we have already so strongly emphasized, the learning of the movement is largely a matter of the attention, and, in general, it is just in proportion as the learning of a movement is a vital thing that the bodily energies are concentrated upon it. A child, *i. e.*, brings together all its force and directs all its attention to learning a movement which is essential to its freedom. All who have ob-

<sup>1</sup> PSYCHOLOGICAL REVIEW, Vol. VII., Jan., 1900.



served children (and this is a fact emphasized by all genetic psychologists) have noticed how persistent they are in learning a movement. In learning to walk a child is usually so much absorbed in the attainment of the end that the incidental bumps and falls, however severe or painful, are hardly noticed. When once a successful movement is made, the attention is so engrossed on a repetition that everything else is for the time forgotten. In acquiring voluntary control over the ear this fact has particularly impressed itself upon our mind, and in all probability voluntary control over each and every movement of the body must be acquired in the same manner. And while, in the one case, since the acquiring of this control is a matter of salvation to the organism, the attention would naturally be drawn upon it; in the other it would be merely a matter of accomplishment. The attention required to learn would be arbitrary and for that reason could not be so close and consequently the movement could not so readily be acquired, provided, *i. e.*, that the attention is the important factor in learning, which it seems, in view of the facts, none can doubt.

We will now look at our facts from a more general standpoint and show their bearing on the genetic theories above stated (Sect. IV.). We cannot do better than accept the general hypothesis that the child begins its existence with a profusion of random movements which have no definite adjustment and no fixed relation to the stimulus, but which serve to bring into play the voluntary motor mechanism and supply consciousness with experience and thus constitute the psychical initiative to voluntary movements. It is generally argued that all movements, before they can be voluntarily produced, must first be given in a random or reflex way. Our experiment shows that at least one movement, namely, that of the ear, can be acquired without first having been given in this way. But some movements, however, as we have seen, must be given in this random and reflex way in order to form a basis for voluntary movements, because the unknown muscle (in our case, the retrahens), if we are to get control over it, must be associated with muscles already known, *i. e.*, with those into which the motor impulse can be discharged voluntarily. This association is made by

overcharging the old channels through the vigorous effort to reach the new muscle, and thus forcing part of the impulse into new channels, among which is that supplying the muscle for which the impulse is intended.

It is universally accepted that the child, in addition to random movements, has certain reflex movements which are due to a certain definite connection between the motor and sensory paths, *i. e.*, the physiological circuit<sup>1</sup> is already completed so that the organism will respond to a specific stimulus in a certain definite way.

It is generally admitted that the random movements are due to a diffused molecular discharge throughout the motor nerves, in which the greater part of the impulse goes over the tract which has been of most use to the race. It must be assumed that the movement which has best served the race will likewise be useful to the individual. And, as Jastrow<sup>2</sup> says, "The existence of habits implies an environment sufficiently constant to repeatedly present to the organism the same or closely similar conditions." Habit implies a definite connection between certain motor and sensory tracts. It also further implies that certain tracts are so well defined that the motor discharges along them take place almost automatically. Under an unchanging environment, *i. e.*, the movements which have served the race best and consequently which receive the largest part of the motor discharge in infancy are soon found to be the means by which the child must realize itself, and are, therefore, repeated on account of the accompanying gratification. If, on the other hand, there is a constantly, but gradually, changing environment, the diffused discharge will procure the adaptive movement. Any part of this diffused discharge (in a modified environment), or of the movement produced by the discharge, which effects the gratification will immediately attract to it the attention and consequently as the process is repeated will gradually appropriate more and more of the general discharge, until only the part which secures the gratification is moved, and that channel becomes relatively the course of least resistance.

<sup>1</sup> Dewey, 'Reflex Arc Concept,' *PSYCHOL. REV.*, Vol. II.

<sup>2</sup> *Popular Science Monthly*, Nov., 1892.

But no movement which brings gratification to the organism or which is of any considerable use to it can be due to the single contraction of any individual muscle or group. An act in an organism so complicated as that of the human being implies the working together of a number of muscles, each performing its part at the different stages of the act. How this is done is not difficult to understand from our point of view of the attention. In all our acts the movement follows the attention. The attention does the switching. This is especially true while learning a movement. A good illustration is learning to swing clubs. When learning to alternate, if the attention is directed to the one hand the other either stops or goes in the direction to keep the body balanced, or there may be a symmetrical discharge to the two hands. In the last case the attention is divided between the two hands, and the impulse goes out in two directions. In learning and coördinating new movements, we do not shift our attention rapidly enough, and in consequence the other movements which are necessary to complete the adaptive act are inhibited. Each part of a complex act is repeated until the sensori-motor circuit is well defined, and the movement as a whole can only be accomplished when each element, once put into operation, tends to act out its part automatically or with very little attention to each part to be operated. Ward<sup>1</sup> says: "The common factor, in all voluntary action alike, seems to be a change in the distribution of the attention under the influence of feeling." And while control is being acquired the power to relax a muscle is the power to shift the attention away from it either to some other part of the activity or to the antagonistic muscle.

In closing this discussion we cannot do better than to interpret our facts in the light of the theories above stated. Assuming, therefore, that the original motor discharge hypothesis and the hedonic concept are correct, and also assuming that the race is, and has been for many generations, exposed to a constant, or at least only gradually changing, environment, the facts of our experiment could only be interpreted in the following way: The motor nerve elements evolve from a central system, from funda-

<sup>1</sup> *Mind*, Vol. XII., p. 64.



mental to accessory. The 'primordial impulse' is discharged along the paths of least resistance, which paths are phylogenetically determined. This discharge along the race-defined tracts causes movements which are beneficial to the organism and hence are attended by pleasurable sensations. These sensations and the movements which cause them are associated, *i. e.*, physiologically speaking, the sensori-motor circuits are completed, so that the process can be reversed, *i. e.*, when the sensation or the idea of the sensation of any one of these movements is given to consciousness there will be the coördinated motor response. But only those movements can be made by the idea of the movement which have previously been made by a motor impulse, *i. e.*, a stimulus will not cause a motor response except where the circuit has already been completed and the central association made, and this association could only be secured (except in the case of certain reflexes where the circuit is already completed at birth) when the movement was first caused by a motor impulse. It is quite certain that motor nerve elements evolve from the central system, from fundamental to accessory.

In the case of a gradually changing environment, which must be the case with man, the adjustment is not so simple as in the above case of fixed environment. In this case the discharge along the path of least resistance would not effect the happy movement and the gratifying sensation, but some part of the general movement caused by the diffused discharge would contain the adaptive element. This would be cognized as pleasurable, and the energies concentrated upon repeating it and prolonging the gratification, until a definite association were made and the circuit were completed and become well defined. This is the way movements are specialized, adapted, and coördinated—most of the bodily movements were originally given through this diffused discharge, and the general sensori-motor paths are thus completed. Nearly the whole process from the impulsive child to the manually skilled adult is one of adaptation through analysis, as above described.

But we have seen that not all the movements possible to come under voluntary control were primarily given in this general

movement, or movements, caused by the diffused motor discharge. How control is acquired over such a muscle we have already described in detail, and we need here only summarize it in a general statement. In this case only a vague, indefinite idea of the location of the movement to be acquired is given through general sensation (visual, auditory, cutaneous). An impulse is sent out in the general direction, which results in a general innervation. In the response there may be an element of the movement sought for. As soon as the movement is effected, however feebly, the sensori-motor circuit is beginning to be made. The sensation will also at the same time be cognized and the effort made to increase the sensation. When an impulse has once actually reached the muscle the process from that point onward is one of segregation, specialization, and analysis, as described in the case of the adaptive movement.

## PSYCHOLOGICAL LITERATURE.

*Ethics.* Volume III., *The Principles of Morality and The Departments of the Moral Life.* By WILHELM WUNDT. Translated by MARGARET FLOY WASHBURN, Ph.D. New York, The Macmillan Co. Pp. 304. \$2.00.

While the two earlier volumes of Professor Wundt's 'Ethics' do not fall within the province of this review, their more important bearings on Volume III. will be considered, in order to make explicit the logical framework of the author's system. Two methods are recognized by Professor Wundt, the speculative and the empirical. The former looks upon fundamental moral laws as original and perfect possessions of the mind, to be discovered by insight and brought to clear consciousness. The latter undertakes, but seldom carries out, a full investigation of the field of morality, as a basis for the discovery of moral laws. Neither method is satisfactory when used alone. Every speculative system overlooks a multitude of true and important facts. And empirical systems cannot discover fundamental ethical principles, but only pave the way for their discovery; "the actual discovery is the task of speculation" (Vol. I., p. 17). Both methods must accordingly be combined, the empirical being used first, to 'furnish the materials with which speculation erects its structure.' The empirical method reaches to "direct abstractions and inductions from experience. Speculation begins, on the other hand, whenever hypothetical elements enter into the formation of our ideas—elements not derived from experience, but introduced into it under the influence of the logical requirement of unity of thought" (Vol. I., p. 18).

In making thorough and conjoint use of the two methods in the order indicated, Professor Wundt has made a noteworthy advance. By his thorough survey of the facts of morality he has done much towards establishing Ethics on a scientific basis, and making it possible to apply critical tests to his groundwork and conclusions. For, as long as Ethics is based on the analysis of facts selected by mere insight, instead of by some explicit process of reason that can be subjected to objective tests, no conclusion of proper scientific authenticity can be attained. Such results of insight are little more than judgments of experts, for on such judgments they are founded, and their weight



depends upon the wisdom of their authors. As Kant and, indeed, Socrates taught, scientific conclusions are the result of explicit reasoning processes that are open to critical testing. Nor is there any doubt that the speculative process, as defined by Professor Wundt, is a necessary supplement. The gathering and testing of facts, abstraction, and generalization, are aimless and inept processes, unless unified and systematized by hypotheses.

In carrying out the method decided on, Professor Wundt devotes Part I. (Vol. I. of the English translation) to 'the moral consciousness of man, as it finds objective expression in the universal perceptions of right and wrong, and, further, in religious ideas and in custom' (Vol. I., p. 19). Here he discusses the light thrown by language on ethical ideas, and the relations of religion, custom, and law to morality. The volume is interesting and valuable, and has already been reviewed in these pages (Sept., 1898). In Part II. (Vol. II. of the translation) occidental ethical systems are considered historically; first those of Greece and Rome, then the Christian systems of the patristic and scholastic periods, and finally modern, chiefly continental, systems of ethics. The volume closes with a thirty-page classification and criticism of ethical systems that is thorough, considering the space it occupies, and valuable. These few pages suggest the errors that Professor Wundt plans to avoid in his own system. Parts III. and IV. deal respectively with 'the *principles* on which all judgments of moral value rest,' and with 'the *applications* of these principles to the great provinces of the moral life' (Vol. I., pp. 19-20). In Parts I. and II. the empirical method is used to reach facts and generalizations therefrom; in Parts III. and IV. the speculative method is used to discover fundamental moral principles and rules of application.

And here it must be confessed that, promising as Professor Wundt's plan of treatment is, the broader features of his execution are disappointing. Considered together, the four parts can only be described as disjointed. Parts I. and II. are not closely articulated to Part III., nor is Part III. to Part IV., as will appear when the latter is taken up. After a study of the facts of the moral life covering some three hundred pages, the author's conclusions are, that "morality expresses itself in *antitheses*" (Vol. I., p. 326), *e. g.*, approval and disapproval; that "certain goods are regarded as morally desirable whose enjoyment promises an *enduring* satisfaction" (Vol. I., p. 327); that 'there are two fundamental *psychological* motives' (Vol. I., p. 328), reverence on a religious, and affection on a social basis; that the principle of the heterogony of ends is a matter of universal experience; and a few

others of lesser significance. Other results there are of a less general character, but I have been able to find none that throw light on the nature of morality as a whole, or that give, or are thought by Professor Wundt to give, decisive evidence for or against any principle reputed moral. And the insulation of Part II. is almost as complete. The few concluding pages of criticism of ethical systems do not rest explicitly upon any portions of Part I., nor upon any principles discovered earlier in Part II. They are Professor Wundt's independently reasoned opinions of the failings of ethical systems.

This does not mean that Parts I. and II. have been of no assistance to Professor Wundt in writing Part III. These investigations must greatly have broadened and deepened his ethical views. Consciously and subconsciously they must have changed his opinions in matters of vital import. Regrettable is Professor Wundt's failure to observe the nature of the processes that led from the facts earlier studied to the later conclusions, or, if he observed them, his failure to set them forth, so that their validity might be tested. It is well to saturate the mind with pertinent facts, and then speak, or otherwise act, without further ado; in practical affairs that is often the best plan. But science insists on scrutinizing every link of the chain that stretches from facts to conclusions. No chain is stronger than its weakest link. But the strength of Professor Wundt's chain of inferences cannot be estimated, as many important links are submerged and out of view.

Part III., which occupies nearly two-thirds of the volume before us, is divided into four chapters, entitled respectively, *The Moral Will*, *Moral Ends*, *Moral Motives*, and *The Moral Norms*, the author's designation for the fundamental rules of right action. The first chapter furnishes the psychological foundation for the later ethical structure, and centers about a very interesting discussion and characterization of the 'social will.' This puzzling and contentious conception plays an important rôle in the author's system, which cannot be understood without it. And, first, Professor Wundt does not hold that there is one consciousness enveloping the component individual wills that, in their unity, make up the social will. The social will is a totality of individual wills made one by unity of purpose. Yet, in a sense, the social will has self-consciousness. In some cases, at least, there is a consciousness, common to the component individual wills, of purposes shared by them in common; whether or not individual wills ever actually coöperate for common purposes without consciousness of unity of purpose, and whether or not such a totality of wills would constitute a social will, Professor Wundt does not say.

In general it may be said that the author is most anxious to emphasize the solidarity, in fact, the conscious solidarity of the social will; many times, for instance, he insists that the individual will knows 'itself to be an element in a total will which supports it in its motives and ends,' as he expresses it in a striking passage (Vol. III., p. 27). I have noticed no mention of the organization of the social will; certainly there is no discussion on this point.

Professor Wundt also shows that social wills differ in scope, *e. g.*, the family will, the wills of associations, the wills of the state and of humanity; and holds that the state is a person, because of 'multiplicity of ends and autonomy of will' (Vol. III., p. 284).

The last two sections of the first chapter discuss freedom and conscience. The former discussion is based upon Professor Wundt's distinction between mechanical and psychical causality, the latter alone being shown to reign over the will; this epistemological discussion taps some of our deepest problems. The gist of the author's opinion on the main question is packed into his pregnant opening sentence, "Freedom is the capacity of any being to be determined in its action by a reflective choice between different motives" (Vol. III., p. 37).

In the author's words, gathered from several contexts, moral conscience may be said to be made up of ideas, immediately associated with the emotions of approbation and disapprobation, that as a whole constitute imperative motives, and pass judgments upon the agent's motives and character. Four imperative motives are distinguished, the two motives of external and internal constraint, and two free imperative motives, permanent satisfaction and the moral ideal.

Professor Wundt considers three main orders of moral ends, the individual, the social, and the humanitarian, which form a hierarchy of ascending worth. But individual ends are never 'true objects of morality.' The preservation, the satisfaction, even the perfection, of self is a true moral end only when sought in the interest of social or universal advantage. And here begins a dialectical process that rules the entire discussion. If the agent's well-being is not a proper moral end, the well-being of other individuals bears the same taint. Thus "there remain two social ends as the true objects of the moral will,—public welfare and universal progress" (Vol. III., p. 80). Nor will the dialectic permit these two ends to be interpreted in individualistic terms. Public welfare is not 'the sum of the welfare of all individuals,' it is not 'a maximum of happiness,' any more than universal progress is 'the progress of as many individuals as possible' (Vol. III., p. 81). "The individual, however perfect and happy, is but a drop



in the sea of life. What can his happiness and pain signify to the world?" The end is not individual but social, and, pushing it to the limit, not this or that society, but the supreme society embracing humanity. The well-being and progress of the universal social will is the genuine and supreme moral end. So I interpret Professor Wundt; his meaning here is very difficult to interpret accurately. Individual well-being and progress cannot be summed, so long as the individuals are isolated atoms, but organize them into social wills and consciousnesses (as earlier described), and there supervene unitary objects of endeavor. "The universal psychic products [*e. g.*, the state, science, art, universal culture] of human society" (Vol. III., p. 86) are the objects of our supreme moral concern.

Happiness enters negatively rather than positively into the end, as conceived by Professor Wundt. If his pursuit of moral ends did not bring in happiness, man could not maintain it. But happiness is not a proper object of moral endeavor. The dialectic shows it; collective happiness is a nonentity, since happiness cannot be summed; besides, teleological activity is man's essential trait, and the only proper object of his concern. The second reason is strong, but the other two will not, I venture to believe, bear scrutiny. As to the last, teleological activity is no doubt the *essential* aspect of man's nature, but there are other aspects, *e. g.*, feeling, and they are surely not matters of indifference, even to the social will.

Profound is Professor Wundt's insight that the *ultimate* end is an ideal of the reason that cannot be defined. The progress in perfection of objective psychic products, *i. e.*, forms of social will, is only our proximate end; to what perfection progressive mankind should ultimately attain, it will forever be impossible to determine. Every improvement of the social will leads, because of the nature of will, to the opening of new possibilities.

In order for an act or a disposition to be moral, the motive must be moral as well as the end, and with moral motives the third chapter deals. The earlier half distinguishes motives of perception, motives of the understanding, and motives of reason, and points out that the ultimate spring of immorality is egoism. The second half deals with immoral motives. Especially good is the discussion of the conditions of immoral volition. Immorality cannot be explained, Professor Wundt shows, either by extreme individualists, who assert reality of the individual but deny it of the social will, or by extreme universalists, who deny reality of the individual and assert it of the social will. If either of these alternatives were true, immorality would become a

matter of quantity, or else a mere appearance, because of the non-entirety of the individual. But both are equally real, though the individual is of lesser moral worth, and, "as man's whole psychical existence is bound up in the society to which he belongs," it is plain that, "the will is moral, \* \* \* as regards its character or disposition, so long as the motives that determine it coincide with the ends of the social will" (Vol. III., p. 110). To be sure, social wills, as they are brought forth in historical development, are fallible, but actual wills are only relative, and the absolute social will is "an idea of the reason, by which we must always suffer ourselves to be guided" (Vol. III., p. 111).

The final chapter of Part III., which covers fully seventy-five pages, opens with the statement that a norm "designates which one of various possible kinds of action ought to be performed" (Vol. III., p. 138). But the discussion is concerned only with fundamental norms, which cannot be derived from any more general principles. Like ends, they are classified as individual, social, and humanitarian, each of which has a subjective aspect, prescribing items of character, and an objective aspect, prescribing kinds of action. Professor Wundt's formulations follow: individual norms, "Think and act in such a way as never to lose respect for thyself," and "Fulfil the duties to thyself and others, which thou hast undertaken"; social norms, "Respect thy neighbor as thyself," and "Serve the community to which thou belongest"; humanitarian norms, "Feel thyself to be an instrument in the service of the moral end," and "Sacrifice thyself for that end which thou hast recognized to be thine ideal task." Inspiring and nobly conceived are these norms, but no reasons are given for regarding them as exhaustive or accurate.

In view of the multiplicity of norms, their occasional conflict is inevitable, and the author finds it necessary to lay down a rule of preference. The hierarchy of moral ends provides the rule required: "When norms of different orders contradict each other, that one is to be preferred which serves the larger end" (Vol. III., p. 140). Professor Wundt sees that this rule may be misconceived, but undertakes to show that, properly understood, it is unconditionally decisive. In this endeavor his success is doubtful. Quite literally, at every moment there are conflicts of ends and of motives, and only by the organization of life on some less mechanical principle can such problems be solved; our familiarity with social organization blinds us to the complexity of the problem.

The chapter, and Part III., close with a thirty-page discussion of legal norms, which well deserves consideration, did space but permit.

Part IV. contains Professor Wundt's views on the practical problems that arise in the different departments of the moral life, under the captions, The Individual Personality, Society, The State, and Humanity. Very little, if any, explicit reference to the principles of morality earlier discovered is made. But the moral significance of such possessions and institutions as property, intellectual cultivation, associations, the state considered as a financial, a legal entity, etc., and international law, are interestingly discussed on the broad basis of the author's ethical outlook. Some of his more striking views may be briefly suggested. It is the duty of the state to see that private labor has the freedom requisite to a share in intellectual interests. In natural science, political science, and history all should be educated; in philosophy, philology, and the history of civilization only the higher grades of mankind. Woman is intellectually capable of certain higher kinds of employment, but she is out of place in politics, and science has been shown not to be her true province.

Criticism, which may well be at fault, is the most fitting tribute to a scientist of Professor Wundt's standing. Appreciation is a matter of course. Two contributions, if no others, would, I venture to believe, place his work among the most important of its kind and century; first, the modern formulation of the conception of the social will, the insistence, as against narrow individualists, on its reality, and the happy employment of this profound truth to solve several knotty problems; secondly, the insight and demonstration that the moral task of mankind is ultimately indescribable, an inspiring ideal of the reason. The English translation of such a work is to be warmly welcomed, because of its difference from our other authorities and because of its intrinsic excellence.

The translator of Volume III. has done her work surprisingly well. An unusually idiomatic and involved German text has been rendered into equally idiomatic and decidedly readable English. I have noticed only three blemishes, an unfortunate use of 'when' and 'where' on pages 91 and 118 respectively, and the mistranslation 'self-regarding' towards the bottom of page 100. The volume has an index, which, however, might well be fuller.

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*Beiträge zur Philosophie des Gefühls.* Von F. RITTER VON FELDEGG. Leipzig, Verlag von Johann Ambrosius Barth. 1900. Pp. vi+122.

Conceptions of the world as force and will, as idea, thought and reason, have been entertained in every age; but the author, in this



confessedly dogmatic and polemical treatise, develops a view of reality in which feeling is fundamental, and he understands feeling in a somewhat unusual sense of the word. "The aim of modern philosophy is to achieve a synthesis of the world as matter and the world as consciousness; but until recently philosophy has only tried to realize this synthesis *in abstracto*; and yet, exactly this synthesis is more concretely given, as an actual feeling within my consciousness, than any other in the world. Empirically to trace out, in this feeling, the dual basis of real and ideal, of objective and subjective being, is the chief problem of every psychology which seeks to go beyond the achievements of the past; and, as such a psychology necessarily stands in close relation to philosophy, it must also be one of her greatest needs" (27). Philosophy is here repeatedly arraigned for trying to realize this synthesis by a definition and elaboration of abstract conceptions, instead of seeking for it among the given facts of experience. The abstract dualism of thinking and extended substances apprehended by the 'understanding,' with which Spinoza struggled, was certain to fail. Spinoza's 'understanding' is a mere  $x$  which cannot be further deduced, because it appears at the end, instead of the beginning, of his system, and cannot be derived from the notion of 'thinking substance.' Essentially the same difficulty shadows every philosophy which does not find in consciousness a concrete synthesis of subject and object, of thought and thing. The demonstration of an in itself monistic and self-identical subject-object alone can overcome this difficulty (28). Fichte's logical 'ego' and Hegel's 'Spirit' are not centers of consciousness in the concrete, but its outermost abstract periphery; Schopenhauer's 'will' stands to consciousness in a relation of exclusion; Schelling's 'unconscious spirit,' which is in the main synonymous with von Hartmann's 'unconscious,' is absolutely useless as the foundation principle of a philosophy of consciousness. The mistake of all these philosophers (except perhaps Schopenhauer) lies first in their beginning with an abstract conception instead of a concrete self-conscious experience, and secondly in their placing self-consciousness, as an ideal product, at the end of a biological development, instead of at its beginning. These fundamental errors rest at bottom upon a confusion of self-consciousness and self-knowledge. The latter comes late in the process of growth; the former is the germ of all growth. The former is a feeling, the latter an ideal product. "Self-consciousness is accordingly the original state, the root of all mental life; self-knowledge, its flower" (p. 30 f.).

There exists an important difference between a philosophy which starts with abstract conceptions and proceeds analytically, and a philosophy based upon actual experience: even professedly experimental philosophies sometimes start with an abstract definition of experience which vitiates their contents; but the time for such philosophies is, in the writer's judgment, once for always passed. "From now on one will have to seek for the basis of the world-order no longer in airy abstractions, but rather wherever the world-fact (*Weltthatsache*), in its most concrete form, is to be met with; and I do not conceal the opinion that this place is our feeling" (47).

In the preface, the author refers to the change which has taken place within recent years in the methods and aims of German philosophy, and expresses the hope that in the near future the Germans will again look up to the clouds for that which exists in the clouds. "The German to-day sees the smallest pebble along the way, but he no longer looks off to the horizon toward which he nevertheless journeys." He must seek a monistic view of the world; and this differs from pantheism in that the latter is always anthropomorphic and simply pushes the great problem of human nature one step further away, while monism seeks in nature as well as in man for the one explanatory principle of things. The mistake of all existing monistic philosophies lies in their starting with a dualism of abstract conceptions, matter and consciousness, whose opposition they can only resolve by some illogical *tour de force* which leaves us unsatisfied. The philosophy of feeling regards matter not merely as an abstract conception but as given in our feeling; and here it exists, not separate from our subjective being, but rather in identity with it. The essence of feeling consists in the identity of a subjective with an objective, and feeling is 'the original actuality of ourselves.' The philosophy of feeling teaches these two things, that in metaphysics feeling alone, out of the totality of consciousness, must be considered; and that in feeling these two fundamental data, matter and consciousness, are not simply given, but united in perfect identity.

The *Kritik der physiologischen Psychologie* deals chiefly with the views of Ziehen, but regards these as illustrating the general position of this new branch of science. He objects to its notion that consciousness begins with sensation—to the neglect of reflex and automatic processes. "In fact," he writes, "consciousness is to this psychology ('without a psyche,' to quote Lange) merely a sort of physiological cadence, the tip of a handkerchief, which protrudes from behind out of the physiological professor's coat-pocket, and of

which he would be ashamed if he did not find it so absolutely necessary." The tendency of the physiologists to despiritualize the ego is evident here. They first rob the ego, bit by bit, of its being, until nothing is left but an internally barren cell-heap: then proceed with infinite pains, bit by bit, to refill the empty cells. Secondly, he objects to Ziehen's account of the origin of sensation out of its stimulus. This view is contradicted by Ziehen's own doctrine of a psychophysical parallelism, according to which a sensation is a double thing—a physiological process with a psychic correlate. Likewise, physiological psychology reduces the attention to an association process dependent upon outer stimuli, and we become thereby our own spectators without will and without initiative. Virtues are simply ideas with positive feeling-tone; vices, ideas with negative; if there be an absolute good or evil, it must be the function of metaphysics and religion to show it. For Ziehen the problem of the freedom of the will sinks to a mere farce. Finally, the ego which physiology seems determined to drive out of psychology entirely, is still retained in altered form. My body moves itself in response to stimuli, and I become aware of those movements for myself; but what am I? A center of self-consciousness which cannot be identified with the body. "Thus physiology, against her will, has reached a spiritualistic hypostasis which, while it suspends the fundamental relation of identity between body and soul, contradicts all that, up to this point, she has taught." The physiological conception of the psyche is untenable.

In the essay on the Origin of the Psyche, the positions are taken that the individual psyche has no beginning in time; that it is originally a feeling, and that this feeling is ultimate reality. In the views of Du Prel as to the nature of the psyche, the author finds much that agrees with his own. Münsterberg and Otto Schneider are quoted in support of his views as to the nature of the ego and its relation to the content of consciousness. The Self of which we are conscious and the content of consciousness cannot be separated. The former without the latter is a ghostly abstraction; the latter without the former, a contradiction.

As to the limits of the Objective and the Subjective in the Knowledge Process, the author holds that every phenomenon is entirely objective or entirely subjective, according to the point of view we take, the two points of view being grounded in the unity of subject and object—which self-consciousness always involves. The two phenomena, the object and subject, do not completely correspond in the



individual finite consciousness, as the fact of a threshold of sensation, for example, shows; but there is no reason for saying that the stimulus, even in this case, is not psychic. On the contrary, were it not so, no transition from a non-psychic stimulus to a psychic process would be conceivable; even the spatial separation of the two exists only for consciousness. The boundary separating subject and object should not be conceived as a surface standing at right angles to the line of perception, but as within the process of knowledge running parallel with it. "Upon a purely empirical basis we have to regard the intelligible and physical, the objective and subjective causal series, as two lines running parallel with each other and by no manner of means constituting, as physiology asserts, their own prolongation" (74).

The next essay deals with *The Ethical Movements of the Present*, which, while preaching morality heroically, fails to ground it philosophically. The ethical movement says, "Seek to realize your highest duty, because it is the crown and glory of humanity"; but when asked why humanity should be made an end, this movement replies: "Because humanity is morally good." It grounds morality in humanity and humanity in morality! Moreover, the law of motivation is here underestimated, or rather completely misconceived. Morality is conceived to be a matter of practice and education; man must be trained to goodness. But in this they forget that goodness can be realized only where profound conviction of the necessity of goodness exists, and it is never the result of mere practice. "No stone falls without a cause and no man acts without a motive" (87). The presupposition of all morality is belief in a supersensuous order, belief that morality is no mere social agreement or law of habit, and that, on the contrary, it has a superhuman as well as human validity (90).

So far we follow the author's discussion with great interest; but in the next essay, on *Palingenesis from a Psychological Standpoint*, he enters a field of speculation in which, as it seems to the reviewer, the weakness of this 'philosophy of feeling' becomes apparent. Each psyche is a synthesis of an empirical and a transcendental element. "Every part of the soul known to us is a self-identical being which is nevertheless joined to a second, unknown part whose identity we, from a psychological standpoint, can neither affirm nor deny"; and this simply opens a free field for the doctrine of the reincarnation of the soul upon earth. The transcendental, unknown half of the soul is the bearer of a transcendental individuality which runs through a number

of empirical reincarnations, because it combines each time with a new empirical half-soul. No knowledge is vouchsafed us, however, as to the nature of the bond which realizes their union, and no bridge of memory enables us to pass from one stage of existence back into previous stages.

The last essay of the book deals with the relation of this doctrine to the fundamental principles of morality. To be true to one's self, in the highest sense, means altruism, because this highest self is transcendental and universal; and yet, such altruism is but the highest form of egoism; and thus, a synthetic basis for morality is reached, enabling us to combine the ethics of hedonism and the ethics of reason with the ethics of perfection or eudemonism.

This is not the place for a careful discussion of this theory of reality; but we may be allowed to say something as to the impression it has made. That philosophy should start with the concrete fact of self-consciousness, rather than with some abstract logical construct, many are ready to assert and have asserted; but why conceive self-consciousness as pure feeling? Were there no element of discrimination in it, the rise of knowledge would be inconceivable. All the author's criticisms of the type of monism represented by the physiologists can be turned against this view by a simple substitution of terms. Moreover, why regard self-consciousness as pure feeling rather than as self-knowledge? The author will answer, because it is a feeling *before* it is a knowledge; but in doing so, he first assumes that primitive self-consciousness is pure feeling, and then attempts to base philosophy upon the lowest rather than upon the highest stage of evolutionary process. This demands that a missing and very careful account be given of the relations of the latter to the former. The deduction of knowledge and volition from pure feeling is as impossible as their deduction from the logical concept of matter. In other words, feeling as the author uses the term seems to us as much an abstraction as matter or thought. My self-feeling exists at the primitive stages of evolution no more than my self-knowledge or self-realizing volition. Many of the author's criticisms of current philosophical, psychological and ethical movements are well stated, though not always new; but whether they are wholly justified by the ancient mysticism into which his doctrine leads us is doubtful. It is an intensely interesting discussion, however, vigorous and earnest in style, and one to which the present writer feels much indebted.

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## VISION.

*Effect on the Persistence of Vision of Exposing the Eye to Light of Various Wave-Lengths.* FRANK ALLEN. *Physical Review*, XI. (5), 1900.

The work that has been done from time to time at Cornell University, under the direction of Professor Edward L. Nichols, on the persistence of vision is of extreme interest. By utilizing the different means for attaining accuracy that are available in a well-equipped physical laboratory at the present time, results were gotten already in 1884 by Professor Nichols himself, and again by Ferry in 1892, which much exceed apparently in smallness of probable error anything that the psychologist has been in the habit of looking for. The method employed was essentially that of flicker photometry, comparison being made in this instance not between two different lights but between alternate light and darkness as regards the physical conditions, or, between alternate light-effect and its persistence as regards the physiological conditions; that is to say, spectral light was alternately let on and cut off by means of the sector disc first employed by Aubert (and named by him the episkotister), and the absence of any flicker was taken as indicating that no perceptible falling off had taken place in the volume of the excitation produced in the visual apparatus during the interval when light was cut off by the filled-in portion of the disc. (It will be seen, therefore, that it is not persistence of vision, but *undiminished* persistence of vision that is in question.) This method is in frequent use, of course, in laboratories of real research; the source of the improvement as it is carried out at Cornell seems to be the greater accuracy with which the velocity of the rotating disc is measured. The disc was made to rotate by means of an electric motor, the speed of which was recorded electrically on paper placed on the cylinder of a chronograph; "so constant were the different adjustments of the apparatus, the intensity of the light, and the sensitiveness of the eye, that observations taken at different times, and even on different days, differed from the mean value by less than two per cent."

There is much that invites discussion in this article, but as further work is still going on in the subject it will be better to wait until fuller results have been obtained. Certain points may be mentioned, however, with a view to suggesting some possible changes in method. (1) Instead of observing a narrow band of color, it would probably be better to adopt König's plan of substituting a simple diaphragm for



the whole eye-piece of the spectrometer; by this means a large surface, instead of a narrow band, of homogeneous light (*viz.*, the face of the last prism) is presented to the eye, and the sources of error that lie in an extreme upsetting of the state of equilibrium of the retina are avoided. (2) Still more is it desirable in fatiguing the eye with certain colors that a larger surface should be affected; if only a narrow band—two millimetres wide—of color is looked at, it must be a very difficult feat to keep the light that impinges next exactly upon the portion of the retina already affected. (3) The argument by which the writer would show that blue, for instance, is not a simple color is plainly not rigorous. After fatigue by blue, it is not blue, he finds, but the adjacent colors, violet and green, that have their period of undiminished persistence lengthened (that have, that is to say, become of diminished brightness); and he infers from this that blue is not a simple sensation, but a compound of violet and green. But there are several objections to the thesis that the effect of fatiguing by a mixed color is to fatigue the element of the mixture disjunctively and to leave the mixture itself unaffected. In the first place, why was not this curious proposition tested directly? Nothing would have been easier, if it be true, than to show that in the case of fatiguing by orange and purple, or yellow-green, or blue-green (colors which are accepted by all theories as mixtures) the orange or the purple or the yellow-green or the blue-green showed, itself, no trace of fatigue, but was only a bright spot between the two exhausted elements of which, in the given case, it was composed. But again, the effect of white light on the retina shows conclusively that the author is amiss in the explanation which he has hit upon of his phenomenon. He gives the table in which are summed up his results concerning the percentage of increase in the (undiminished) duration of color impressions when the eye has first been fatigued by exposure to various colors:

Region Affected.	Color of the Fatigue Stimulus.					
	Red.	Green.	Violet.	Blue.	Yellow.	White.
Red . . . . .	8%	0%	0%	0%	10%	9%
Yellow. . . . .	0	0	0	0	0	5
Green . . . . .	0	12	0	10	12	13
Blue. . . . .	0	0	0	0	0	13
Violet . . . . .	0	0	27	16	0	12

It certainly looks like a curious fact that while fatigue by red, green, or violet produces an effect upon each of those colors, respectively, and upon no other color, fatigue by blue produces no effect upon

blue, but only upon green and violet. But if this is because blue is not fundamental but a mixture, and if when a mixture is the cause of the fatigue the mixture itself does not suffer from the fatigue, how shall we explain what takes place when the source of the fatigue is white light? White is, physiologically, upon the present hypothesis, nothing but red and green and violet. We have just been told that green and violet when combined into blue have no effect whatever in the way of prolonging the duration of the blue light sensation (but only that of the green and the violet); what then is the cause of the fact that, when *white* light is the fatiguing agent, blue is affected just as much as either green or violet? It cannot be the green or the violet that does this (for neither of them affects anything but violet or green); it cannot be the red (for that affects only red); there is, in fact, no way of explaining it, and it can only be that the whole conception is wrong, and that, whatever may be the cause of the results described, it is not this (which would be anyway very improbable)—that green and violet, while they operate together to give the sensation blue, do not operate together to cause fatigue in blue.

There are other points in this paper which arouse consideration (to make use of a useful German phrase). The normal curve of persistence is reprinted, for the sake of comparison, with each figure of a fatigue curve; but it appears in two different forms—usually it shows a minimum persistence of .0120 seconds (and this is what is given in the table of values for the normal curve, p. 264), but in the diagrams exhibiting fatigue for violet and for blue this whole curve is transposed, and shows a minimum of about .0105 sec. There is doubtless some simple explanation for this, but none is given; and with a method which is said to show a variation of only two per cent. in observations taken on different days, to come suddenly upon a discrepancy of twelve per cent. is confusing. The difference makes, of course, a great change in the character of the blue fatigue curve, as exhibited in the diagrams.<sup>1</sup>

Again, the wave-length chosen for a light to cause blue-fatigue was  $\lambda = .430\mu$ , but this is already, according to the best observers, violet, and, in fact, a violet than which there is none redder in the spectrum;  $.430\mu$  is just the limit beyond which no color change is perceptible.

<sup>1</sup> Mr. Allen explains, in a note which I have received from him since this was written, that the discrepancy here referred to is due to an unavoidable change in the brilliancy of the acetylene light used. The sentence which I have quoted at the end of the first paragraph above would, therefore, seem to need a slight modification. And it still seems singular that the light should appear at just two different degrees of brilliancy, and no others.

Both Hering and König are agreed (an instance of good fortune) that blue is  $\lambda = .470 \mu$ . If they are right, then light of  $\lambda = .430 \mu$  ought to cause additional persistence, upon the assumption of this paper, in violet only, instead of in violet and green.

To conclude, this paper presents facts which (if they are confirmed by additional observers) will certainly prove to be of the very greatest interest; that there is any ground for the way in which they are here interpreted seems to be, however, questionable in the extreme.

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## PAIN-SENSE.

*Undersökningar öfver smärtsinnet.* (Investigations on the Pain-sense.) By SYDNEY ALRUTZ. Upsala universitets årsskrift, 1901. Upsala, Akademiska Bokhandeln. Pp. 136. 2 kr. 50 öre.

The work is historical, critical and experimental. It places Dr. Alrutz in line with Blix, Goldscheider and von Frey. The monograph would be welcomed in an English edition as the best recent statement on this subject. Since it is accessible to only a small number of readers in its present form we shall here give a brief résumé instead of attempting a critical review. We shall omit the purely historical matter and the details about technique, and mention only some criticisms, positions taken in disputed questions, and new facts observed, although it is difficult to do justice to the work by this method of gleaning.

*Chap. I. Pressure Spots.*—These are studied for the purpose of determining their relation to the pain spots. For stimuli he uses a graduated series of Thunberg's glass hairs. The experiments show that in the hair-covered skin the pressure spots always lie to the windward of the hairs; that in its remaining area between the hairs no pressure spots could be demonstrated; that in the hairless areas the pressure spots exist and are more numerous and difficult to differentiate, but the sensations elicited from them are of the same quality as in those elicited from the hairy areas; that the withdrawing of the pressing point may call forth the same kind of pressure sensation as the applying of the point calls forth; that pain sensations are obtained from the pressure spots only when the pressure is great enough to irritate adjacent pain spots; and that likewise heat and cold sensations cannot be obtained by stimulating a pressure spot only.

*Chap. II.-IV. Pain Spots: The Double Pain Sensation.*—The latter title refers to Goldscheider's 'secondary pressure.' The experi-



ments show that both immediate (primary) and delayed (secondary) pain sensations may be produced by a point stimulus; that both cannot be obtained from each and every point, and that the primary sensation has a pricking, point-like character, while the secondary is of an itching and irradiating nature. In different spots one may produce, by pricking, only secondary pain sensations, or only primary pain sensations, or both, or may fail to produce any pain sensation at all. It is impossible to count them exactly, but the author thinks that von Frey's estimate of the number of pain spots is somewhat too low. Both pressure spots and cold spots were found capable of giving the secondary pain sensation, but neither would give the primary. A topographical study of the skin shows that the secondary pain (Goldscheider's pressure) sensation can be obtained in certain areas, *e. g.*, on the arm and the abdomen, and that it differs in intensity for different areas, and that it cannot be obtained in others, *e. g.*, on the tip of the nose and in the middle of the cheek. In general, the possibility of obtaining the secondary sensation varies topographically about the same for thermal as for mechanical stimuli. The secondary pain sensations readily call forth reflexes.

"There exist spots in the skin which, upon stimulation with a point, give pain alone, or rather a pricking sensation. \* \* \* Neither the primary nor the secondary pain sensations have anything to do with pressure sensations. \* \* \* That which specially characterizes the secondary sensation is that it has, at least in most parts of the skin, the character of pure itches when caused by a weak mechanical stimulation with a point. \* \* \* The skin spots for the primary, pricking pain sensations do not coincide with the spots that most readily and clearly give the secondary or more or less itching sensations. \* \* \* I believe that there is a qualitative difference between the primary and the secondary pain sensations."

*Chap. V. Pain, Pain Nerves and Feeling Tones.*—The historical résumé in this chapter is most excellent. Only a few of the criticisms can be noted. He opposes Goldscheider's view that there is a resemblance between pressure and pain. The theory of Nichols in regard to specific end organs is rejected. Marshall's criticism of the sensation hypothesis can be valid only on the assumption that it is directed against the view that would make sensations of the feeling tones. The poverty of the English language in possessing only the single word 'pain' to denote widely separated facts of consciousness is the cause of endless confusion in reasoning. Strong's theory of the coördinated sensations is criticised. The author's view is that, "in

case of very strong pain stimuli, the pain sensations coming from the end organs of pain are so intense and unbearable that the pressure and temperature sensations that perchance occur concomitantly fall into the background of consciousness, or are even suppressed." Among several other views of von Frey, his classification of pain as both sensation and feeling is rejected. In the debate between Richet and Fredericq the author sides with the latter.

On analgesia and hyperæsthesia for different stimuli on the same cutaneous area, the author cites two cases of clinical observation which lead him to think that the conditions under which these phenomena occur are very complicated, and of course pathological, and that Strong's cases on this point need to be verified before we dare build any theory of specific energies upon them.

*Chap. VI. Sensations of Tickling and Itching.*—The historical statement is followed by some experiments. A single stimulation of a single point may bring about these sensations. The repetition of the contact may simply cause a summation of infra-liminal stimuli. There is no close resemblance between the tickling sensations and sensation of touch, and it is difficult to observe any resemblance between tickling sensations and sensations of pain. On the contrary the itching sensations both resemble the pain sensations and have transition forms to the pricking pain sensation. Evidences are given to support the theory that the itching sensations have a distinct quality, and that there are distinct end organs for the mediation of this quality. The difference between itching and tickling is simply a difference in degree.

*Chap. VII. Pain Qualities.*—The historic review is followed by an explanation of the 'paradoxical cold sensation.' The author demonstrated this phenomenon at the meeting of the Paris International Congress of Psychology. Some time after a cold stimulus has been applied and removed a secondary sensation of cold may arise and be more intense than the primary. It is caused by the warming from within. Experiments show that Rollett's conclusion that chloroform and ether cannot produce real temperature sensations is false. The heightened sensibility in places stimulated with menthol may be explained by the heightening of irritability caused by the action of the menthol on the end organs of temperature and pain. The pain sensations produced by heat are identical with those produced by cold. Hence if we distinguish between heat-pain and cold-pain we thereby denote the elements of complexes, *i. e.*, addition of temperature to the quality of pain. The pain produced by mechanical stimulation is of the same quality as that produced by cold. There is a difference

between dull and aching pains. The dull pain comes from the deeper-lying organs. The difference between pricking and aching pain is primarily a difference in the spatial attribute of the pain quality. As stated above, itching is a distinct quality of sensation, but between it and tickling there is only a difference of degree. The termini of the pain, cold, and heat nerves lie in the order mentioned inward from the surface. The cause of differences in the elementary quality of pain is not to be found in the difference in the kind of stimuli. The same means may produce different qualities and the same quality may be elicited by entirely different stimuli. Different qualities undoubtedly depend upon different end organs, both peripheral and central. A bibliography is given.

This monograph presents an extreme view, as is seen in the above. But, for determining the conditions of the end organs and the elementary sensations of the skin, this tendency is needed in the investigation in order to search out the facts. The opposing extreme simply slurs matters over by speaking of 'nerfs tactiles' and 'feeling pain.' The skin is yet a mysterious and unknown territory. The explorers tell us strange and conflicting stories. This is chiefly on account of the complexity of the phenomena.

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## EQUILIBRIUM AND VERTIGO.

*Le Vertige: Etude physiopathologique de la fonction d'orientation et d'équilibre.* DR. GRASSET. *Revue Philosophique*, Vol. LI., 225-251, 385-402, March and April, 1901.

The aim of this article being an examination of the physiological processes concerned in a certain abnormal condition, rather than a discussion of their bearing on the psychology of space, the author at the outset agrees to pass over all questions of space perception, the origin of the space notion, and the relative importance of the several senses in determining this notion. He proposes to discuss simply the particular nervous apparatus concerned in orientation and the maintenance of equilibrium, its physiology and pathology. As he points out, the former function (orientation) is primarily sensory, though its apparatus includes afferent nerve connections, while the latter (equilibrium) is primarily motor, its apparatus including, however, sensory connections which serve to determine the position of the parts of the body concerned in maintaining the equilibrium. The two act together as a single apparatus or organ by means of a series of centers, some of



which are automatic or reflex, while others are voluntary. Dr. Grasset, it should be noted, limits the term 'equilibrium' (*équilibre*) to the motor function exclusively, and uses the term 'equilibration' (*équibration*) to denote the combined sensori-motor function.

A large portion of the article is devoted to the normal workings of the function of equilibration. The discussion of the nervous apparatus concerned in this process is perhaps the most interesting and valuable part of the study; the psychologist, at least, will deem it so. We shall give a brief summary of the conclusions reached in this part, omitting the neurological minutiae.

The author first discusses the various centripetal courses, *i. e.*, those which contribute to the sensation-complex of orientation. These he limits (in man) to the kinæsthetic, tactile, labyrinthine, and optic courses. While distinguishing carefully between the general kinæsthetic sense and general tactile sensibility, he thinks the two should be classed together for purposes such as the present discussion, since they coöperate in furnishing data regarding the position of the body and its members, the state of contraction of various muscles, as well as the form, weight, distance, etc., of nearby objects. The kinæsthetic apparatus in the labyrinth of the ear is similarly distinguished from the auditory (sensory) apparatus adjacent to it, and is regarded by the author as prior to the latter in origin. Each, however, contributes data for the orientation of the head. The orientation function of vision is likewise dual: by vision proper we determine the form, size, and distance of objects; but the motor apparatus of accommodation, eye movement, etc., aid considerably in these determinations. The centripetal courses of orientation are thus seen to belong to two main categories: (1) Extrinsic courses: those which transmit impressions obtained from outer stimuli—notably touch, hearing, and vision. (2) Intrinsic courses, which transmit impressions obtained from inner stimuli; these include the general kinæsthetic apparatus, the special kinæsthetic apparatus in the ear, and the special kinæsthetic apparatus connected with the eye.

The centrifugal courses concerned in the equilibrium function (in the narrower sense) comprise all nerves which lead to the muscles, either for contraction or for relaxation, whether voluntary or automatic. No special classification of these is given.

The centers and central courses are discussed at considerable length. The author regards it as proved that the cerebellum has all the elements necessary to constitute it an independent center for the function of equilibration; it may therefore be considered a reflex cen-

ter of that function. But in the cortex and basal ganglia several centers are found which exert control over the function, viz., the centers of the centripetal courses already mentioned; by means of these it becomes a voluntary function. These latter centers supplement one another in the work of control, and in case of functional disturbance in any one of them the others may take its place. The bearing of this on the pathological state of vertigo is apparent; thus, in sea-sickness, where the disturbance is partly visual, closing the eyes is beneficial; in gastric vertigo, on the contrary, opening the eyes affords some relief. The author proceeds to discuss at some length the relation of these higher centers to one another, and to the subcortical centers of equilibration.

Turning now to the problem of vertigo, we find Dr. Grasset insisting on its essentially sensory character. This is set forth in the very beginning of the article (from which portion we quote) and reiterated later.

"Vertigo," he says, "is a *sensation*, that is to say, it is a *subjective phenomenon of consciousness*. It may have objective causes and consequences; but essentially it is subjective and conscious. \* \* \* In the second place, it is a *deceptive* (fausse) sensation. \* \* \* [It may be] either hallucination or illusion. \* \* \* This deceptive sensation, which constitutes the primary element of vertigo, is a sensation of *displacement* (rotation) of the objects about us, or of our own displacement ('*entraînement*') in relation to the surrounding objects. \* \* \* There is [however] in vertigo another constituent element besides that which we have already analyzed. \* \* \* [This second element is] *the sensation of the loss of equilibrium*." (Pp. 226-233; the italics are in the original.) These are the leading points in Dr. Grasset's statement, which is supported by many citations of evidence and interspersed with arguments against other current views—notably that of M. Pierre Bonnier, who includes the sensation of *reeling*, as well as the correlate motor phenomena, under the head of vertigo. The first impulse of the present reviewer was to exclaim against Dr. Grasset's disregard of these motor elements, which obviously stand in close relation to vertigo; his second thought was that this, being largely a question of nomenclature, might well be left to convenience or analogy for settlement. Thus, the special sense organs have motor connections, but the sensation side is so prominent that they are classed as sensory apparatus; *per contra*, in pathological phases of these same functions sensory and motor elements may be equally prominent, as in the case of strabism. In the case of vertigo, the motor disturbance may be re-

garded as a stimulus of the specific sensation. The present tendency in psychology is to trace out the sensori-motor connection where possible, and correlate the two sides in the discussion of any particular function. The real criticism that we have to make on Dr. Grasset's position, therefore, is that while the sensation under discussion is an important element in disorientation and disequilibrium, still the total sensori-motor disorder includes something more than this, and all elements should be included in any general treatment. This is perhaps chiefly a criticism of the heading of the article; the subject matter, the reviewer thinks, would justify a broader title to indicate its actual scope.

As for the general question of terminology, it seems important to distinguish between the sensation and the sensori-motor disorder. In English we have the word *nausea* to indicate one prominent element in the sensation-complex; we have, further, the two words *vertigo* and *dizziness*, one of which might be used to denote the sensori-motor disorder of which this sensation-complex is the conscious manifestation. A complete differentiation of these three words would tend to clear the analysis of the phenomena in point.

In concluding the article, Dr. Grasset gives a summary of disorders of orientation and equilibrium, and discusses the several types which he finds. His scheme may be indicated briefly as follows: *I. Subjective symptoms*: (1) Anæsthesia; diminution or loss of sensations which give rise to orientation (kinanæsthesia, etc.). (2) Hyperæsthesia; abnormal increase of these sensations (abnormal fatigue, cramps, etc.). (3) Paræsthesia of orientation alone (disorientation). (4) Paræsthesia of orientation and equilibrium together (vertigo). *II. Objective symptoms*: (5) Akinesia (paralytic astasia-abasia). (6) Hyperkinesia (movements of 'entraînement' and propulsion). (7) Parakinesia with irregular contractions, in walking or other movements (ataxia and incoördination). (8) The same, in state of rest on the part of the subject (chorea, etc.). (9) Parakinesia with tremors, in walking, etc. (multiple sclerosis). (10) The same, in state of rest (paralysis agitans).

H. C. W.

#### EXPERIMENTAL.

1. *Les méthodes de l'esthétique expérimentale. Formes et couleurs.* J. LARGUIER DES BANCELS. L'Année Psychologique, 6<sup>e</sup> Année, 1899, 145-190.
2. *Recherches anthropométriques sur 223 garçons anormaux âgés de 8 à 23 ans.* TH. SIMON. Ibid., 191-247.



3. *Attention et adaptation.* A. BINET. Ibid., 248-404.
4. *Recherches sur la sensibilité tactile pendant l'état de distraction.* A. BINET. Ibid., 405-440.
5. *Expériences de suggestion sur des débiles.* TH. SIMON. Ibid., 441-484.
6. *Formation des voyelles.* DR. MARAGE. Ibid., 485-492.

1. This article reviews the various efforts made to solve the chief problems of æsthetics, dwelling principally upon an analysis of the labors of Fechner, with the aim of pronouncing upon the validity of the various methods which have been employed in more recent experimentation. The author concludes that experimental methods can be applied to only a small number of æsthetical problems, and that the field must continue to remain largely in the hands of 'general psychology.' The observation of the relatively simple phenomena of an æsthetical quality is regarded as the smaller task of æsthetics.

2. This memoir records original measurements made upon degenerates, defectives, and idiots comprising the colony at Vaucluse. The object of this study is to find, as nearly as possible, the correlation there may be between physical development and intellectual capacity, which is affirmed to be directly traceable. The facts of the study were derived from the following measurements: The height, the chest, breadth of shoulders, maximum circumference of the head, the weight, and the stretch of the arms. The special studies, worked out with great detail, include correlation of different measurements, a comparison, by ages, of the measurements of these children with the results of studies of normal children made by some twenty observers, from Schmidt to Hrdlicka, and also a comparison of the extreme grades of intellectual capacity displayed by members of the colony. As to this last point, it is found that "it is less an alteration of the proportions of the various parts of the body which distinguishes the idiot or the imbecile from the weak-minded, than the absence of his development considered in its totality."

3. This is the most interesting and most important memoir in the *Année*. In aiming to organize methods for estimating or measuring the force of voluntary attention, Binet discovers a complement to the processes of attention which is indicated by the second term in the title; thus he offers a new point of view from which to study voluntary attention. B. selected his subjects from a class of 32 children in a public primary school in Paris, arranging them in two groups, one including the five 'most intelligent,' and the other the six 'least

intelligent' of the whole class. 'Natural intelligence,' rather than diligence or industry in school, as judged by several teachers, was taken as the basis of the selection of these children.

B. aimed to demonstrate the differentiation of the two groups by the application of well-known experimental tests of the following types: Tactile sensibility (back of the hand), simple and choice reaction time, counting dots, perception of increase and decrease in stimulation, counting rhythmic sounds, copying texts, series of numbers, phrases, nonsense texts, and designs, memory of letters, numbers, etc., accuracy in proof-reading and marking letters, simultaneous additions, and speed in reading figures, etc., etc. The tests comprised difficult and unattractive intellectual work not beyond the range of the children, whose ages varied from nine and a half to thirteen years.

B.'s criteria for selecting the tests in his preliminaries were as follows: Those tests which showed rather equal averages from the two groups were regarded as bad; those tests whose results demonstrated the intelligent group to be the better, were regarded as good. The degree of attention is measured by the number of errors committed by the subjects in any given test. B. is aware that the mental functions involved in each test are numerous and complex, but he does not attempt to analyze them. In this study he takes account only of the results. The errors above mentioned are regarded as the involuntary defects of attention, as dependent solely upon the mental constitution of the subject (p. 395).

The tests for perception of changes in the rapidity of the strokes of a metronome, rapid reading (by shutter exposure), and reaction-time constitute a group which does not present results corresponding to the grade of intelligence. Quickness, also, is found to be independent of intelligence. The remaining tests comprise a group whose results differentiate the two classes of children. The following particular features of the tests may be mentioned. The intelligent group had a much finer tactile sensibility at the first trial, but their improvement in later trials was less than that of the unintelligent group. This test was good for a classification of the subjects. Counting dots revealed no great differences, and does not seem to be a good test for a classification. The first trial at counting rhythmic sounds is good for classification, the intelligent children being much better, but the differences diminish after the first trial. Copying numbers is a good test for showing individual differentiation, while memory of numbers, analysis of a design, and simultaneous addition are excellent tests to exhibit group differentia-

tion. Correction of proofs differentiates the groups and classifies the individuals at the same time. B. did not attempt to correlate these tests in the present study.

B. views attention as a process of mental adaptation to a state that is new to us. In repeating tests he noticed less inequality between the two groups of children, and this was the most disturbing factor appearing in his problem. The reduction of group-differentiation he found to be due to the relative quickness of the adaptation of the pupils. Adaptation in the intelligent children is different from that in the unintelligent. The intelligent pupil adapts himself in the first trial better and quicker; the unintelligent pupil is slower the first time, but improves in adaptation in successive repetitions, thus reducing the group-difference. B.'s most important suggestion is that we should not lose sight of this process of adaptation, as it might vitiate results considered to be well established. This memoir is an important contribution to both individual and pedagogical psychology.

4. In making various efforts to find the difference in tactile sensibility between 'intelligent' and 'unintelligent' subjects, B. was led to the study here reported. The subjects were two girls and a young man. Compass contacts on the back of the left hand were used. Distraction of attention was brought about by giving tasks in addition of numbers, the subject repeating each progressive step aloud, the contacts being made during the search for the sums. B. finds that distraction cannot be produced uniformly, the question of the individual being of prime importance. A state of distraction was manifested in some cases by a verbal automatism which consisted in generalizing the answers. The percentage of answers, 'one point' and 'two points,' does not express a special state of sensitiveness under these conditions, nor does it serve to determine the threshold of sensitivity. The subject whose mind is distracted does not remember the order in which the contacts took place; and this uncertainty of memory, compared with the memory left by a séance when the subject paid attention, proves that distraction could diminish his consciousness.

5. Simon here reports the results of tests conducted by himself on some twenty-seven of the children studied in No. 2, at Vaucluse. He employed the methods of Binet in the latter's studies on the suggestibility of normal school children (see B.'s volume, *Suggestibilité*). There were five groups of tests; estimating the length of lines, the weight of cubes, length of lines exposed on a revolving disk, remembering groups of objects exposed on a card, and performing rhythmical movements. In the first two and the last tests, the suggestion was induced



by objective increase in stimulation, while the experimenter sought to produce suggestion verbally in the remaining tests. This study is admittedly meagre and imperfect, owing to the lack of subjects (varying from twenty-four to eight) and the vagueness of the results, which cannot be classified in any satisfactory manner. Automatism seemed less noticeable in the perception of weights and the production of movement. S. arranges these subjects into five types: The wholly imbecile, which is beyond suggestibility; those of firm judgment which prevents suggestion; those of medium suggestibility, who are most numerous; the automatic; and the bizarre type, which is very irregular and indefinite. In comparing his results with those derived by Binet from normal school children, S. finds these types analogous to those of B.'s excepting that the bizarre is not noted, while the automatic type is found more frequently among the normal children.

Weak-minded and normal children seem to behave about the same; though the former seem less susceptible to suggestion, which is, in part, ascribed to the emotional character of the reactions of the latter.

6. This account of a study from the physiological laboratory of the Sorbonne, after reviewing the three theories of Helmholtz, Hermann, and Guillemin, which are set aside, offers the following explanation of vowel-formation: "Vowels are due to an intermittent aerolaryngean vibration, reinforced by the buccal cavity and producing *ou*, *o*, *a*, *é*, *i*, when the latter is in unison with the sum of the vibrations; transformed by it (the buccal cavity) and forming the other vowels when the unison does not exist; the number of intermissions gives the fundamental note on which the vowel is uttered. A whispered vowel is produced when the buccal cavity functions alone; the vowel is sung when the larynx functions alone; when the buccal cavity and the larynx function simultaneously, the vowel is spoken."

EDWARD FRANKLIN BUCHNER.

*Avons-nous des sensations spécifiques de position des membres?* ED.

CLAPARÈDE. *L'Année Psychologique*, 7<sup>e</sup> Année, 1900. Pp. 249-263.

Claparède here vigorously reaffirms his negative answer to this title-question, by reviewing briefly the logical and psychological arguments in favor of the two possible answers; by referring to a few results which he has derived from tests on movements; and especially by a critical discussion and final rejection of the views of M. P. Bonnier, published in the latter's *l'Orientation* (1900), which maintain an affirmative answer to the question. Position cannot be felt immedi-

ately. It is a complex phenomenon, involving the intermediation of an association of images, and appearing as the result of 'inference,' the term with which C. replaces the word 'judgment,' employed in his earlier expositions. He thus indicates that our ideas of position as such are not due to mere sensations, nor to an active, conscious operation or process. Position is only a state of relation between diverse data presented by the pressure and tensions experienced in the portions of an arm, *e. g.*, which has moved. The determination of position is only a particular case of that psychical operation known as the localization of parts of the body, which in turn is regarded as totally dependent upon the fact that the various portions of the body offer surfaces possessing different degrees of sensitivity, whence the 'local color' which is a quality in tactile and muscular sensations. Thus position is an affair of perception, rather than the content of sensations locally derived; and it usually involves an escort of visual images which are called forth by muscular-tactile impressions. C. insists that introspective and experimental data can be interpreted only to the conclusion that 'sensations' of movement and of position must be regarded as qualitatively different.

One half of the article is devoted to an examination of the materialistic views of Bonnier, who regards the alleged 'sens des attitudes segmentaires' as the foundation of all our mental life. C.'s rather caustic strictures are properly placed, saving us, as they would if widely known, from a perennial pseudo-psychology which says that "tactile orientation results immediately from the topographical distribution of images in our [cortical] centers"!

EDWARD FRANKLIN BUCHNER.

*The Synthetic Experiment.* I. M. BENTLEY. Amer. Jour. of Psychol., XII., 2. Pp. 405-425.

A synthetic experiment is one where a given complex conscious state is made to result from the simultaneous production of simpler states. Dr. Bentley's article contains, first, a discussion of the theoretical significance of such experiments, and second, an account of methods used in the experimental synthesis of the tactual perception of liquidity. In the first part, he indicates the value of synthesis as a test of analysis, and notes that to be sure of the validity of a synthesis we must know (1) that no extraneous suggestion has produced the experience and (2) that the experience is produced only when all its components are present. He points out the difference between a synthesized perception and an illusion, a distinction that rests largely on

extrinsic circumstances, such as the way in which the experience is produced, and its congruity with other experiences. The special conditions and difficulties of the synthetic experiment in various departments of mental life are referred to, as for instance, the difficulty of synthesis in the case of feeling because of the absence of voluntary control over organic sensations, and the import of artificial conditions in the reaction experiment as a synthesis of action. The most significant portion of this theoretical discussion is the author's careful statement that successful synthesis cannot after all prove the exhaustiveness of the corresponding analysis; it shows only that if

u put certain simpler processes together the required complex process will result, not that the complex process is merely the sum of the simpler ones. It does not prove, in other words, that the complex perception may not contain something which cannot be isolated as a component, but results from the togetherness of the other components.

The author's experiments in the synthesis of liquidity are highly suggestive in the matter of method. He preceded them by requiring his subjects to analyze the perception of various liquids: it would seem better, by the way, in order to avoid every possibility of extraneous suggestion, not to have this preliminary analysis made by the subjects who are to serve later in the synthetic work. The subjects had their eyes blindfolded and nostrils stopped, and the introspection was carried out for perception of simple contact with the tip of the finger, dipping the finger in, and pulling it out. The results for three typical liquids, mercury, benzine and water, indicate that pressure and temperature are the chief components, and that wetness is not itself perceptible. The synthetic part of the experiment endeavored to produce the requisite conditions of temperature and pressure. Dipping the finger into a thin sheet of rubber stretched over a beaker failed to give the perception of liquidity; flour in a funnel, with the finger inserted into it through a small brass ring to simulate the liquid boundary, was almost completely successful; while the full perception of liquidity resulted from inserting the finger into a rubber sheath formed by putting a small weight on the middle of a thin rubber sheet stretched over a beaker of cold water, the rubber being thus pulled down into a dry pocket closely surrounded and pressed upon by the water. Here the temperature and pressure conditions were exactly right, but wetness was quite ruled out, thus confirming the evidence of analysis that it is not a component part of the perception.

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*On some Minor Psychological Interferences; a Study of Misspelling and related Mistakes.* T. LE MARCHANT DOUSE. *Mind*, IX., 85-93. January, 1900.

This is a brief study of *lapsus calami* observed in the written 'answers of candidates at a certain university examination.' They are classified under five heads: (1) Prolēpsis, or 'Assimilation from ahead' (*e. g.*, introduce, mordern), (2) Metapēdēsis, or 'Overleaping' (*e. g.*, superstion for superstition), (3) Metallagē, or 'Cross Compensation' (*e. g.*, Padoga for Pagoda), (4) Opisthomimēsis, or 'Assimilation from the rear' (*e. g.*, Househould, Evidendence), (5) 'Contamination,' in the philologist's sense (*e. g.*, 'Tuetonic' was written for 'Teutonic' through the unconscious influence of the word 'Tuesday').

This classification may be compared with that of the present writer in his 'Study of Lapses,' in which (1) is treated under 'Anticipatory Substitution' or 'Insertion,' (2) under 'Anticipatory Elision,' (3) under 'Exchange and Inversion,' (4) under 'Persistence,' and (5) under 'Conflict and Coalescence.' As was there pointed out (and this is confirmed by the article here reviewed), the greatest psychological interest centers about the phenomena of conflict and coalescence or 'Contamination,' since in such errors we see the actual process of disintegration and reconstruction going on.

Attention is called to the fact that the mind tends to throw the material presented to it, no matter how inchoate, into some form which will 'carry a meaning.' This is seen in the 'Popular Etymology' which, for example, transforms 'asparagus' into 'sparrow-grass.' The author also calls attention to the occurrence of *lapsus linguæ* conforming to his five types, but he does not develop this side of the subject; he contents himself with a few sporadic instances by way of illustration. This is an interesting subject and one could wish that the author had expanded his treatment on the psychological side.

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## MENTAL TESTS.

*The Correlation of Mental and Physical Tests.* CLARK WISSLER. Monograph Supplement No. 16 to the *PSYCHOLOGICAL REVIEW*. New York, The Macmillan Co. June, 1901. Pp. 62.

This monograph concerns itself with a statement of the methods and conclusions pertaining to an attempt at correlating a series of psychological, academic and physical tests. The data had been in process of accumulation for several years by the Department of Psychology at

Columbia University. The following tests were made yearly upon sixty to seventy freshmen of Columbia College and repeated upon those who remained to the end of the senior year: Length and breadth of head, strength of hands, fatigue, eyesight, color vision, hearing, perception of pitch, perception of weight, color preference, reaction-time, rate of perception, naming colors (rate of reading), rate of movement, accuracy of movement, perception of time, association time, imagery, auditory memory, visual memory, logical memory and retrospective memory. Records of stature, weight, lung capacity, etc., together with data concerning parentage, personal habits and health of the men tested were taken by the Director of the College Gymnasium. In the examination of the data obtained from the above the author employed the method of correlation developed by Karl Pearson, from which the relative degrees of functional relations holding between the various pairs of tests can be estimated with considerable precision. The data for some 250 freshmen were treated under three main divisions: Psychological tests, academic tests and physical tests. The former were chiefly tests of quickness and accuracy. Correlating the results for each test of this group with the others showed the relative rank of the individual in the group to be little more than a mere chance relation, or, in other terms, there is no evidence of important functional relations between the activities employed. This leads the author to say that these tests must be measures of special abilities. The result was the same in correlating the memory tests. The college standing furnished the data for the academic tests, and correlation within this group revealed an agreement of rank between the different subjects of study to a degree equal to that holding between the height and weight of the same students. The physical tests were found to correlate with each other to a considerable degree, as stature with weight, strength of hand with lung capacity, etc.

When the author attempted to correlate the psychological tests with the academic and then in turn with the physical, the relation was little more than that of chance. This was also true of the relation between the academic and the physical tests. The general conclusions of the research are: (1) That the laboratory mental tests show little inter-correlation in the case of college students; (2) that the physical tests show a general tendency to correlate among themselves, but not with the mental tests; (3) that the markings of students in college classes correlate with themselves to a considerable degree, but not with the tests made in the laboratory.

THE AUTHOR.

## PSYCHOLOGICAL CONGRESS.

IV\* *Congrès International de Psychologie, Paris, 1900; Compte Rendu des Stances et Texte des Mémoires.* DR. PIERRE JANET, Secr. gen'l du Congrès. Paris, Felix Alcan. 1901.

Some of the papers are printed in extenso, others in epitome. It is possible to give here only terse amplifications of the titles. The following embraces the papers that might be grouped under the head of Philosophical Psychology.<sup>1</sup>

A. 4<sup>e</sup> SÉANCE GÉNÉRAL.—ÉTUDES PHILOSOPHIQUES SUR LA PSYCHOLOGIE.

*Sieben Rätzel der Psyche.* Dr. K. B.-R. AARS, Christiana.—The following are simple and fundamental facts—problems inaccessible to analysis: psychic elements, simultaneous comparison, successive comparison, the state of expectation (futurity, causation), the process of identification, objective projection, and the existence of psychic beings other than self.

*Note sur la conscience de l'effort intellectuel.* Professor HENRI BERGSON, Paris.—(1) Every intellectual work consists in passing from a design or scheme to an image. (2) In every intellectual effort there is a strife on the meeting of multiple and analogous images which try to thrust themselves into one and the same design, some not filling it up entirely, others passing by until at last the coincidence of the image with the design is attained. This movement of all particular images gives us an impression *sui generis*, which ought to enter largely into the consciousness that we have of intellectual effort.

*Psychologische Atomistik.* Professor HUGO MÜNSTERBERG, Cambridge, Mass.—A hierarchy must develop in psychology, like that found among the natural sciences. This will begin with the purely theoretical, indivisible element; hence, psychological atomism. This will have nothing to do with the physical atom. Nor is sensation this ultimate element. We obtain the first suggestion of this *Urelement* in the principle of similarity. So long as sensations are similar they cannot be absolutely simple. The psychic *Urelemente* must be completely dissimilar. In physics the aim is to reach elements that shall be absolutely similar and devoid of quality. Psychology demands atomistic elements that differ only in quality, and are qualitatively incomparable. Herewith the distinction between the different senses

<sup>1</sup>Other papers will be noticed in these pages. See also the general report of the meeting in November, 1900, number of the REVIEW.—ED.



falls. The aim is now to build a system out of this chaos of postulated elements by arranging them in series which are not similar. The principle of assimilation will here be helpful. The speaker illustrated fully in the various sense departments how the process of assimilation makes it possible to construct a system of psychic elements. The aim was merely to show the possibility.

*La douleur.* Professor VLADIMIR TSCHISCH, Dorpat, Russia.—The speaker took exception to Richet's theory that pain is produced by strong stimuli and by all abnormal states. Chemical substances that transform living tissue into dead tissue produce pain. The same law may be adapted to chemical, electrical, and thermal stimuli. Such excitations as cannot kill, *e. g.*, a brilliant light or a disagreeable perfume, do not produce pain. Such stimuli as can kill, *e. g.*, venomous substances, mechanical and thermal stimuli, do produce pain—however, not all, *e. g.*, morphine, cocaine. All stimuli that are harmful to the individual produce disagreeable feelings; such stimuli as kill the individual produce disagreeable feelings; stimuli that kill living tissue, transforming it into dead tissue, produce pain. Pain may be regarded as a danger signal.

*Terminologie psychologique.* ED. CLAPARÈDE, Geneva.—There is urgent need of a rigorous scientific terminology in psychology. The speaker requested that a committee of one for each of the four languages represented at this congress be appointed to formulate some elementary definitions and report same for discussion at the next meeting of the congress. Favorable action was taken on this by the appropriate committee.

#### B. SECTION II.—PSYCHOLOGIE INTROSPECTIVE DANS SES RAP- PORTS AVEC LA PHILOSOPHIE. (5 Séances.)

*La psychologie des sports.* FRANÇOIS DA COSTA GUIMARAENS, Paris.—The attraction in sport lies in the excitement that it produces. This pleasurable excitement is simply the result of an increased activity of the vital functions. This increase of activity is due simply to a more active oxygenation. Therefore the attractiveness of sport is simply the excitation produced by oxygen. This paper led to considerable discussion on the topic.

*Sur les théories herbartiennes et physiologiques du plaisir.* PIERRE TISSERAND, Bourges.—The Herbartians admit two kinds of pleasure, physical (sensational) and moral (affective). To these they attribute different causes: to the first physiological and to the second psychological. This is wrong. We cannot separate moral pleasure

and physical pleasure; and every feeling has a physiological condition. The Herbartians make a false separation between soul and body and between sensation and feeling. Pleasure does not belong to the domain of mechanical necessity, nor to thought as a pure act; it is inseparable from life and spontaneity.

*Sur les sentiments et les sensations et leurs différences fondamentales.* Mme. MARIE DE MANACEINE, St. Petersburg.—Sensation and feeling have always been confused. Sensations may be divided into objective, *e. g.*, sight and hearing, and subjective, *e. g.*, pain, hunger, and thirst. The elements of time, space, and causality are realized in every sensation. Feelings obey only the principle of causality. Joy fills the body to the finger tips. A man under the domination of a feeling swears eternal faith or vengeance, as if the feeling were permanent. Sensations bring the body into the foreground of consciousness and tend to obscure the mental self. Thus cutaneous pain accentuates the consciousness of spatial relations of that part. Feelings tend to make us oblivious to the body, *e. g.*, in martyrdom. Excessive sensations may run into the quality of painfulness, but the affective states do not change quality with increasing intensity, though strong enough to kill. Sensations are aroused by physical stimuli: feelings are not. From this point of view sexual attraction lies primarily in a sensation. A classification of the feelings follows.

*Péripatétisme et psychologie expérimentale.* E. PEILLAUBE, Paris.—Modern experimental psychology is coming to adopt the peripatetic method. This is the objective method. The first chapter in psychology should be a discussion of life in all its aspects. This is the Aristotelian procedure.—In the discussion Professor Séailles held that the agreement between the peripatetic and the modern methods is only superficial. M. Peillaube made an elaborate defense.

*Die verschiedenen Richtungen der Weltanschauung.* EUGEN VON SCHMIDT, Freiburg in B.—Materialism is not philosophical. Spiritism is not scientific. Rationalism (spiritualism) is both scientific and philosophical. What is then life according to monistic rationalism? "It begins with the life-force becoming individual as Soul, which then strives gradually to build its body according to immanent laws, and to develop itself until the organism ends, *i. e.*, dies." The soul arises from the souls of the parents. It is not a gradual formation through the development of the sexual elements, for that would be a process in time. It comes into individual existence at the *point* of the first contact of the two sexual germs. The plant kingdom

represents the lowest stage of life—unconscious sensibility and striving; man the highest, and animals an intermediate stage. The modern doctrine of organic evolution is considered unreasonable. "With man a new kingdom is come to the earth, the kingdom of Spirit to which the ape does not belong . . . Spirit is reasoning power through which the reason becomes self-conscious or personal."—In reply to the question of Dr. Pavicić about the speaker's conception of the nature of spirit dissociated from body, the speaker replied: "The soul is a psychic force, an individual life-force, which, like every 'Naturkraft,' is indestructible but loses its individuality in death and may return to the general life-force. It [the soul in which the world-reason has become personal] leaves all earthly behind and goes, as an immaterial point, . . . into a higher world, but retains the last result of its earthly development, which I have called memory's traces of the life on earth. . . . The eternal World-reason reaches personality in man, but it cannot satisfy itself with a mere earthly, human personality in the endless 'Weltall,' but develops through innumerable single worlds into innumerable personalities, each on a higher spiritual stage than the preceding."

*The Psychological Impossibility of Scepticism as Shown in the History of Pyrrhonism.* MARY MILLS PATRICK, Ph.D., Constantinople.—Pyrrhonism furnishes the best historical illustration of an attempt at absolute scepticism. Those men in the Pyrrhonic school who succeeded most nearly to attain to *ἐποχή*, or suspension of judgment, so completely failed in doing so as to prove the psychological impossibility of scepticism. They ran into a veiled dogmatism. For example, with Pyrrho, equilibrium of soul which may exist because of the impossibility of knowledge becomes a positive theory for the attainment of happiness. His life revealed this dogmatism. Scepticism pure and simple is not a philosophy that one could live. Aenesidemus and Sextus Empiricus are brought forward as further illustrations of the same fact.

*Identité et continuité du moi.* PAUL CARUS, Chicago.—We distinguish three elements which together form in our consciousness an organic whole: consciousness of our body as a unit, the continuity of our personal history, and the identification of our being with our aspirations and ideals. The consciousness of bodily unity is imposed by the necessity for its acting as a unit. It is not the substance but the form that persists. The true identity is experienced in the identity of our ideals. Individuality is the life of the body in time and space. Personality is the form of life, of thought, of feeling. Individuality



and personality are two aspects of the same reality. The individuality perishes; the personality persists.—In the discussion Professor Chatterjii, of Benares, said that this is no new idea. It is the fundamental doctrine of ancient Buddhism. Professor Buisson, of Paris, asked, What is form without matter?

*La classification des états de conscience proposée par le Prof. Fr. Brentano et ses applications à l'analyse psychologique des jugements.* Professor V. VAILATI, Italy.—The speaker developed an elaborate and artificial classification of judgments based largely on Brentano's theories.

*The Psychology of Tickling.* Professor JAMES SULLY, London.—Professor Sully's paper is printed in extenso in the report before us. The precise nature of tickling sensations is not yet fully understood. These sensations have a tendency to irradiate. They are allied to the organic sensations. All parts of the body are not equally susceptible to titillation. Unknown sense qualities may be involved. The feeling tone is complex: sometimes pleasant and sometimes predominately painful. Tickling elicits two distinct tendencies in movements: defensive movements and movements expressive of pleasure. The former generally occur in response to disagreeable sensations, though we must not forget the complexity of the process. The power to call forth the laughing reflex is not limited to the stimulation of end organs at certain depths or in any special area. Laughter is not merely the expression of the feeling tone of the sensory process. Laughter and the defensive movements may occur together. Self-titillation does not provoke laughter. The laughter is due partly to the assignment of meaning to the sensation. To this apperceptive process the following mental conditions are favorable: Indefinite expectancy, uncertainty, uneasiness, apprehensiveness, and above all a good disposition and playful attitude. Animals are ticklish. In infants it appears first near the end of the second month, and may, therefore, have psychic antecedents. It is an inherited reflex. Possibly it may have originated in the pleasurable experience of the lower animals in having insects picked from their hair. It may also be a trace of imitation of play at war or fighting. Hence its utility. The volume of the laughter may be accounted for by the swift recovery from momentary, half-developed fears. Laughter is the best way of announcing the friendly, playful mood.

*Psychologie et métaphysique.* J. P. DURAND.—As the new psychology tends to condemn metaphysics, positivistic materialism proposes to annihilate psychology. Both are wrong. There is a perma-

ment field for psychology, which is not even touched by other sciences, but psychology must admit its dependence upon metaphysics just as every other science does. The leaders in all sciences have employed metaphysics. A lengthy illustration of a metaphysical conception of the soul is given.

*Perception et conception.* HENRI ABIT.—There is a pure perception which is like the primitive tufa. The pure concept precedes this. This is the concept of space. The spatial idea is not the result of the perceptive process, but it is at the very bottom of it. The idea of space cannot be accounted for through perception, by making perception an active process.

*De l'universalité du jugement esthétique.* Professor VICTOR BASCH, Rennes.—Do universal æsthetic judgments exist? This brings up Kant's celebrated antinomy of taste. Every æsthetic judgment may be reduced to three principal factors: the directly sensible, *i. e.*, the sensual pleasure, which reduces itself upon final analysis to the consciousness of a maximum of stimulation with a minimum of fatigue; the formal factors, or the intellectual pleasure caused by unity in multiplicity; and the associative factors, which are essentially feelings couched in symbolic sympathy through which we may not only share intimately the life of beings separated from us by time, space, and unreality but may even confer our life upon inanimate objects. The directly sensible factor results from a sympathy between our nervous system and external stimuli; the formal factors result from a sympathy between the primordial law of our being and the forms of beings and things; and the associative factors arise from a sympathetic relation between past events in our life and the actual objects of our contemplation. The first two factors are relatively stable; the last is variable. The first two may be universally shared. Associative feelings, on the other hand, are essentially unstable, and their communicability depends upon an act of reconstructive sympathy which demands knowledge and a plasticity of imagination, of which half the world is incapable, and which is limited even among the élite to particular epochs and forms of art.

*Psychologie de la timidité.* Dr. J. P. HARTENBERG, Paris.—Bashfulness consists of fear and shame. It appears only in the presence of human beings, or with the idea of such presence. A description is given. Bashfulness may become morbid, and therapeutic measures are then justifiable.

*Ueber die Aennlichkeit.* Professor ANTON MARTY, Prague.—The speaker discussed the logical and the epistemological aspects of the principle of similarity.

*Meine Auffassung der Willensfreiheit.* WILHELM STERN, M.D. Berlin.—The speaker outlined the theory of freedom which is the groundwork of his treatise on positivistic ethics (Berlin, 1897). The doctrine is a subjective determinism, based upon empirical psychology.

*The Value of Hypotheses in Psychology.* Professor E. F. BUCHNER, New York.—Recent psychology has been engaged in gathering data—facts in and about consciousness. There is now a tendency to begin a generous discussion of the concepts derived from them. This praiseworthy attitude will lead to a careful analysis of the explanatory hypotheses by the scientists themselves, rather than by the meta-physician solely. The assumption of mental processes, of faculties *vs.* dynamic development, of psychical dispositions, of a simple relation between cerebral processes and conscious products—these hypotheses were discussed from a psychological point of view with reference to their adaptation to the phenomena of the science.

*L'illusion du libre arbitre.* SULLY PRUDHOMME, Paris.—“Whether free acts exist or do not exist, man has the illusion, at least, if not the real assurance, that, according to the witness of his consciousness, such acts do exist in the exercise of his will. This is an entirely empirical statement on my part, and I am entirely astonished at it; for is it not surprising, if everything in the universe is necessitated, that a mental state should be found from which to represent non-necessity, even if it is an illusion? From what combination of necessary factors can come such an image, whether true or false, of something which implies absolutely nothing of necessity and even represents its opposite? There is here certainly a problem to be solved. I can only call attention to it.”

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## IMITATION.

*The Theory of Imitation in Social Psychology.* CHARLES A. ELLWOOD. American Journal of Sociology, VI. (6). Pp. 721-741.

This paper, which was read before the meeting of the Western Philosophical Association last January, is a criticism of the position that imitation is the sole method of individual development and social organization. The author advances four arguments against such a position. First, the selection of models for imitation, the fact that we do not imitate all the copies presented, implies the existence of other



instinctive factors beside imitation. Second, the theory makes too great a gulf between sub-human societies, organized on the basis of other instincts, and human society, which is supposed to be based on a single instinct, that of imitation. Third, it does not sufficiently recognize the importance of natural selection, in its psychical aspect, as a factor in human progress. And fourth, the theory 'rests upon no sufficient basis of ascertained facts.' It is, in short, too abstract, too far removed from the facts of life. "Both as M. Tarde and as Professor Baldwin conceive it, the social process is a process which might very well go on in a company of disembodied spirits—in a vacuum!"

There is no doubt that the author's general position is a safe one. One cannot read Tarde without becoming aware of the danger of supposing that the fact of repetition in society always indicates the process of imitation as its cause. But this pushing of the principle of imitation to its extreme limits is really an instance of the method that must be pursued in dealing with a new or a neglected factor. In order to find out what it will explain, we must proceed on the hypothesis that it will explain everything. It is well when the exploiter of a principle can also be its critic, but sometimes the constructive part of his work gains in enthusiasm and thoroughness if he leaves the limiting and paring-down process to others.

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## PSYCHICAL RESEARCH.

*Proceedings of the Society for Psychical Research.* Parts 36-40; Vol. XV. Feb., 1900, to Feb., 1901. Pp. 522.

Of these five parts, three (Nos. 37, 39 and 40) contain reports upon the meetings of the Society, Mr. Myers' presidential address and an address in memory of Professor Sidgwick. The articles calling for more extended notice are found chiefly in Parts 36 and 38, with one in 40.

Part 36 contains four leading articles. 'The Fire Walk' and 'Reflections on Mrs. Piper and Telepathy,' by Andrew Lang; 'Discussion of the Trance Phenomena of Mrs. Piper,' by Mrs. Sidgwick; 'On some Philosophic Assumptions in the Investigation of the Problem of a Future Life,' by F. C. S. Schiller.

Mr. Lang reports three recent, and some earlier, descriptions of the 'Fire Walk.' In the first of the three, Col. Gudgeon, British

resident at Raratonga, describes how he and three other Europeans walked barefoot across a circular area of red-hot stones 12 ft. in diameter, on Jan. 20, 1899. "I got across unscathed," says Col. Gudgeon, "and only one of the party was badly burned; and he, it is said, was spoken to, but, like Lot's wife, looked behind him—a thing against all rules. I can hardly give you my sensations, but I can say this—that I knew quite well I was walking on red-hot stones and could feel the heat, yet I was not burned. I felt something resembling slight electric shocks, both at the time and afterwards, but that is all." In the second case, Dr. T. M. Hocken, F.L.S., describes how at Mbenga, one of the Fiji Islands, he saw seven or eight natives walk across and around an oven of white-hot stones 25 or 30 ft. in diameter, leaving the circle at the point of entrance. Some of those who 'passed through the fire' were carefully examined both before and after their descent into the oven, but no clue to the cause of their immunity was found. The third case is reported by Col. Andrew Haggard, who witnessed the 'Fire Walk' at Tokio, April 9, 1899. Col. Haggard examined the feet of the performers afterwards, 'they were quite soft and not a trace of fire upon them.' Mr. Lang thinks that "every known physical or conjectural psychical condition of immunity fails to meet the case, and we are left wholly without an ascertained, or a good conjectural, reason why for the phenomena."

Both Mrs. Sidgwick and Mr. Lang attack the possession theory in the Piper case. Mrs. Sidgwick grants 'for the sake of argument that the evidence proves,' in this case, 'that knowledge is in some way derived from those who are dead,' but holds it does not prove that the communicating intelligence 'is any other than Mrs. Piper herself,' *i. e.*, a secondary personality of Mrs. P.'s, analogous to those studied by Janet and others. Mr. Lang will not go beyond the notion of 'telepathy *à trois*' in seeking an explanation for the phenomena, and would not be surprised 'if some normal explanation \* \* \* were to be found' for them all.

Mr. Schiller's article is directed in the main against the assumption that between this life and that of the future world there can be no psychical continuity, an assumption which leads to the rejection of some evidence for the future life on the ground that it is 'not supernatural enough.' Mr. Schiller holds that the method of science requires the contrary assumption; the unknown must be construed in terms of the known until new experience gives a new 'known' where with to correct the initial assumption. In conclusion, he points out that from the position of an 'idealistic experientialism we may con-

ceive ourselves as passing through any number of worlds, separated from each other by (partial) discontinuities in our experience, each of which would be perfectly real while it lasted, and yet would have to be declared unreal from a higher and clearer point of view.'

Of the minor articles Professor Harlow Gale's 'Study in Spiritistic Hallucinations' is especially worthy of note. Professor Gale draws an instructive parallel between the case which he there reports and some of the Moses' phenomena.

The greater portion of Part 38 is devoted to Book II. of Professor W. F. Barrett's exhaustive monograph 'On the So-called Divining Rod,' of which Book I. appeared in Part 32, Vol. XIII. This second book alone covers 253 pages, of which about 100 are devoted to reports of new cases and experiments. In the remaining 150 odd pages are found much statistical and historical information and a study of the probable causes of the rod's motion. The whole study is illustrated by reproductions of photographs showing the scenes of the experiments, drawings showing some of the geological formations in which water has been found by dowzers, etc. Professor Barrett sums up his conclusions in pages 313-314. In brief, they are: The dowser meets with a degree of success which can not be due to chance and calls for some explanation. Whatever the source of his knowledge he does not consciously get it through the usual avenues of sense. It is usually presented to him as to others through the automatic motion of the rod; sometimes through certain sensations. Both of these are to be regarded as arising 'from a subconscious and involuntary suggestion impressed upon the mind of the dowser.' After making all possible allowance for suggestions unconsciously received through the ordinary channels, we are driven to the assumption that in some dowzers there exists 'some kind of transcendental perceptive power.'

Part 40 contains a brief preliminary report by Dr. Morton Prince upon 'The Development and Genealogy of the Misses Beauchamp,' a case of multiple personality. As Dr. Prince promises a more minute study of this most interesting case a detailed review of it is scarcely called for at present.

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## NEW BOOKS.

*Das sittliche Leben.* H. SCHWARZ. Berlin, Reuther & Reichard. 1901. Pp. xi + 417.

*L'année philosophique:* 11e année, 1900. F. PILLON. Paris, F. Alcan. 1901. Pp. 316.

*Psychologie de l'idiot et de l'imbécile.* 2e éd., revue. PAUL SOL-LIER. Paris, F. Alcan. 1901. Pp. iv + 236.

*Pascal.* AD. HATZFELD. (Grand philosophes series.) Paris, F. Alcan. 1901. Pp. xii + 291.

*L'évolutionnisme en morale; Etude sur la philosophie de Herbert Spencer.* JEAN HALLEUX. Paris, F. Alcan. 1901. Pp. 228.

*Études de psychologie.* J. J. VAN BIERVLIET. Ghent, A. Siffer; Paris, F. Alcan. 1901. Pp. 201.

*Les timides et la timidité.* DR. PAUL HARTENBERG. Paris, F. Alcan. 1901. Pp. xv + 264.

*L'opinion et la foule.* G. TARDE. Paris, F. Alcan. 1901. Pp. vii + 226.

Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie. H. Schiller und Th. Ziehen. Berlin, Reuther & Reichard. The following:

*Die neueste Wendung im preussischen Schulstreite und das Gymnasium.* I. F. HORNEMANN. IV, 2. 1901. Pp. 68.

*Die Sprachstörungen geistig zurückgebliebener Kinder.* ALB. LIEBMANN. IV, 3. 1901. Pp. 78.

*Die Entwicklung der Pflanzenkenntnis beim Kinde und bei Völkern.* WILHELM AMENT. IV, 4. 1901. Pp. 60.

*The Circulation of the Nervous System.* HERMAN GASSER, M.D. Plattville (Wis.), Journal Publishing Co. 1901. Pp. 156.

*Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung.* A. THUMB und K. MARBE. Leipzig, W. Engelmann. 1901. Pp. 87.

*IVe Congrès international de Psychologie; Compte rendu des séances et texte des mémoires.* (P. JANET, Secrétaire.) Paris, F. Alcan. 1901. Pp. 814.

*Histoire et solution des problèmes métaphysiques.* CHARLES RENOUVIER. Paris, Felix Alcan. 1901. Pp. 447.

*Gustav Theodor Fechner.* Rede zur Feier seines hundertjährigen Geburtstages. WILHELM WUNDT. Leipzig, W. Engelmann. 1901. Pp. 92.

*The Play of Man.* KARL GROOS. Trans. by ELIZABETH L. BALDWIN, with preface by J. MARK BALDWIN. New York, Appleton. 1901. Pp. ix + 412.

*Notes on Child Study.* EDWARD LEE THORNDIKE. (Columbia Univ. Contributions to Philosophy, Psychology and Education, Vol. 8, Nos. 3-4.) New York, Macmillan Co. 1901. Pp. 157.

*Theologie und Metaphysik; Das Verhältniss der Theologie zur modernen Erkenntnistheorie und Psychologie.* GEORG WOBBERMIN. Berlin, A. Duncker. 1901. Pp. xii + 289.

## NOTES.

PROFESSOR KARL GROOS, of the University of Basel, has accepted a call to the University of Giessen as full professor.

PROFESSOR TH. RIBOT has retired from the chair of philosophy at the Collège de France (Paris) which he has occupied for many years.

AMONG the degrees bestowed at the recent anniversary of the University of Glasgow, psychology received recognition in the persons of Professor R. M. Wenley (Michigan) and Professor J. Mark Baldwin (Princeton); the degree of LL.D. was conferred upon each.

PROFESSOR CHARLES H. JUDD, recently of New York University, has been appointed professor of psychology and pedagogy at the University of Cincinnati. Professor Judd has been in charge of these departments at the summer school of the University this year.

WE note also the following appointments: Dr. M. F. Libby to the chair of philosophy at the University of Colorado, made vacant by the death of Professor Francis Kennedy. Dr. S. S. Colvin, Ph.D. (Strassburg), as assistant professor of psychology at the University of Illinois. Dr. Margaret K. Smith as professor of psychology at the State Normal School, New Paltz, New York.

MS. intended for publication in the PSYCHOLOGICAL REVIEW should be sent after October 1st to J. Mark Baldwin, Princeton, N. J.

# THE PSYCHOLOGICAL REVIEW.

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## THE INFLUENCE OF IMPROVEMENT IN ONE MENTAL FUNCTION UPON THE EFFI- CIENCY OF OTHER FUNCTIONS.<sup>1</sup>

### III. FUNCTIONS INVOLVING ATTENTION, OBSERVATION AND DISCRIMINATION.

BY PROFESSOR E. L. THORNDIKE,  
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AND DR. R. S. WOODWORTH,  
*New York University Medical College.*

The functions trained in these experiments were those of observing accurately some detailed features of the words on printed pages. The subject would, for instance, practice marking every word containing the two letters *e* and *s* until he attained a considerable improvement in speed and possibly in accuracy as well. Before and after this training he would be tested in marking words containing other combinations of letters, misspelled words, a certain letter printed promiscuously amongst others, words of a certain length, different parts of speech, etc.

The quickness of the work was measured by the time taken to do a given amount, or in some cases by the amount done in a given time. The accuracy was measured in two ways, (1) by the number of omissions, and (2) by the proportion of the words

<sup>1</sup> *Corrigenda.*—In Table IV. of the previous article of this series (page 385 of the July number of the REVIEW) there are three errors in computation for which I am responsible. In the 5th column 82.3 should be 92.3; in the 6th, 123.3 should be 123.2; and in the 8th, 40.3 should be 41.3. In the next to the last line of the table 60 becomes 67, and 63, 62.

EDWARD L. THORNDIKE.



marked to those which should have been marked. A comparison of tests before and after training can thus be made. It is wise to use two measures of accuracy, since a change from say 4 to 2 omissions does not mean so much improvement as 50 per cent. On the other hand, a change from marking 96 to marking 98 per cent. of the words that should be, means an improvement of more than 2 per cent., for the more accurate one is at the start the harder it is to gain further accuracy.

The early and late tests were rarely with perfectly equal tasks. If you use the same pages after as before training, you get a probability of practice effect from the first test itself. If you use different pages, they are of course of slightly varying difficulty. Individual records, then, must not be taken too seriously. General tendencies are all that we pretend to demonstrate.

The experiments seem to be fairly good ones, for they concern processes comparable to the training in school life which pretends to improve our general habits and powers of attention, discrimination and accurate work, and are still easily administered and calculated.

We may first examine a sample test in some detail and then recount briefly the results of the others.

Five subjects practiced marking the words containing *e* and *i* in a book containing matter of about uniform style and difficulty and character of topics. Before they began and after they had attained considerable improvement they were tested with marking words containing *i* and *t*, *s* and *p*, *c* and *a*, *e* and *r*, on similar pages (different pages being used before and after training), with marking words containing *a* and *n*, *l* and *o*, and *e* and *r*, on pages different from those used in the training series in length of lines, size of type and style of matter (the same pages being used before and after training), with marking the misspelled words on a page containing a hundred such, with marking the letter *A* on a sheet containing 500 capital letters in a random arrangement.

Their records are given in Tables I., II. and III. Tables I. and II. give the improvement in the training series. There was equality in the length of the first and last pages marked, but

unluckily the first page had an unusually large number of words containing *e* and *s*, making it a harder page. The second page on the other hand had fewer than those which happened to be final pages for the different subjects. Under A we have given the records for time, percentage marked and omissions in the case of the second page, and under B in the case of the first page. Under C we have given the records for the last page in the case of each subject, and under D the records for the average of the last four pages.

The comparison from A to C seems the best to go by. This comparison makes the training seem a little more than it perhaps was, in that the errors of the last trials are more below the average of the last four trials than they should be, but this is offset by the facts that the tests under C had 12 per cent. more *s* and *e* words than did those under A and that the latter were second tests.

TABLE I.  
IMPROVEMENT IN TRAINING SERIES.

	Time.				Percentage Marked.				Omissions.			
	First Training.		Last Training.		First Training.		Last Training.		First Training.		Last Training.	
	A.	B.	C.	D.	A.	B.	C.	D.	A.	B.	C.	D.
Ber.	185	235	150	151	78	92	96	93	16	8	4	6
Br.	245	290	120	118	86	100	90	93	10	0	8	5
Be.	240	262	150	155	83	76	81	75	12	28	13	20
Wh.	185	225	105	101	81	90	98	90	13	12	2	7.5
E. M. T.	165	190	105	111	84	83	95	95	11	20	4	4
Totals.	1020	1202	630	636					63	68	31	42.5

Table II. gives the ratio between the end and beginning of the training series in time and accuracy according to the

TABLE II.  
RATIOS OF END OF TRAINING SERIES TO BEGINNING.

	Time.	Percentage Marked.	Omissions.
Ber.	81	123	25
Br.	49	105	80
Be.	62.5	98	108
Wh.	56	121	75
E. M. T.	64	113	36
Totals.	62	115	50

A-C comparison. In these and all following tables figures referring to time are given in seconds unless otherwise stated.

TABLE III.  
IMPROVEMENT IN TEST SERIES IN TIME.  
THE FIRST COLUMN UNDER EACH HEADING REFERS TO A TEST BEFORE TRAINING, THE SECOND COLUMN TO ONE AFTER.

	<i>i-t</i>	<i>s-p</i>	<i>c-a</i>	<i>e-r</i>	<i>a-n</i>	<i>l-o</i>	<i>e-r</i>	Mispelled Words.	<i>A's.</i>	All Tests Together.										
Ber.	200	150	165	160	170	163	210	172	62	68	55	40	65	68	175	160	75	79	1177	1060
Br.	176	146	170	95	140	110	225	203	68	45	64	35	85	63	120	108	110	85	988 <sup>2</sup>	795
Be.	232	180	184	170	186	170	306	225	101	71	78	54	108	73	190	150	—	—	1385	1093
Wh.	190	175	170	120	140	140	175	180	80	55	65	45	70	60	135	120	78	75	1083	970
E. M. T.	170	115	146	105	165	120	183	165	85	48	65	46	68	50	140	103	97	85	1119	837
Totals.	968	766	835	650	781	703	1099	945	396	287	327	220	396	314	760	641	360	324	5752	4755
Ratio in training series.																				
Ber.	75	97	96	82	110	73	105	91	105								105		90	
Br.	81	56 <sup>1</sup>	78	90	66	55	74	74	77								77		81	
Be.	62.5	92	91	73	71	70	68	68	79								79		79	
Wh.	56	70	117	102	69	70	86	86	90								96		90	
E. M. T.	64	72	73	90	57	71	74	74	70								88		75	
Totals.	62	78	90	86	72	67 <sup>1</sup>	79	84	90								90		83	

<sup>1</sup> Possibly 155 above and so .90 here and a higher average below.

<sup>2</sup> Br. *s-p* test thrown out.



Table III. gives the improvement in time in the various tests. Part *A* gives the absolute quantities and Part *B* the ratios of the tests after training to those before.

TABLE IV.  
IMPROVEMENT IN TEST SERIES IN ERRORS.

	<i>i-t</i>	<i>s-p</i>	<i>c-a</i>	<i>e-r</i>	<i>a-n</i>	<i>l-o</i>	<i>e-r</i>	Misspelled Words.	<i>A's.</i>	All Tests Together.
Ber.	17 23	5 2	5 5	38 35	9 13	3 5	3 2	10 9	5 3	95 97
Br.	19 27	4 6	8 11	15 17	14 14	0 4	2 2	6 1	2 1	70 83
Be.	30 16	8 2	12 6	24 22	13 15	1 1	3 5	1 7	17 5	109 89
Wh.	10 5	5 1	10 3	34 17	8 10	2 0	0 0	11 5	1 0	81 41
E. M. T.	38 18	6 2	8 7	49 22	6 12	2 2	3 3	7 10	0 0	119 76
Totals.	114 89	28 13	43 32	160 113	50 64	8 12	11 12	35 32	25 9	474 386
Ratio in training series.										
Ber.	135	40	100	92	144	167	67	90	60	102
Br.	142	150	137	113	100	∞	100	16	50	119
Be.	53	25	50	92	115	100	167	700	45	89
Wh.	50	20	30	50	125	0	100	45	0	51
E. M. T.	47	33	88	45	200	100	100	143	100	64
Totals.	78	46	74	70	128	150	109	91	36	81

Table IV. gives in a similar manner the improvement as measured by the number of errors.

Table V. gives in a similar manner the improvement as measured by the percentage of words marked.

TABLE V.  
IMPROVEMENT IN TEST SERIES; PERCENTAGE OF  
WORDS MARKED.

	Training Series.	<i>i-t</i>	<i>s-p</i>	<i>c-a</i>	<i>e-r</i>	<i>a-n</i>	<i>l-o</i>	<i>e-r</i>	Misspelled Words.	<i>A's.</i>
Ber.	123	92	119	100	104	76	66	104	101	102
Br.	105	87	88	91	98	100	55	100	105	101
Be.	98	127	146	120	102	85	100	91	96	112
Wh.	121	107	125	122	124	88	129	100	107	101
E. M. T.	113	145	127	103	149	70	100	100	97	100
Totals.	115	108	119	107	113	82	89	99	101	103

By testing eight other individuals with three of the tests in the same manner as the five of the tables, save that the former had no training whatever between the tests, we gained an approximate measure of the improvement to be expected on the after-training tests because of their being a second trial. The ratios of their speeds were 95, 91, and 86, but the sum of the omissions of the eight rose from 14 to 20, 8 to 10, and from 36 to 48. So no improvement can be demonstrated in their records.

It is clear from the tables that the improvement in the function of observing and marking words containing *s* and *e* is not equivalent to improvement in the group function of observing the make-up of words. Neither the speed nor the accuracy acquired in the training is a general power equally applicable to other data. And although the functions operative in the tests were so similar to that trained the loss of efficiency with them is considerable:

Speed is more likely to be improved than accuracy. This may mean that certain habits of eye movements and stops are formed that are identical elements in both functions trained and tested. The most notable improvements in accuracy occur with *s-p* and *e-r* on the same style of page as the training series (ratio of percentages of words marked 119 and 113). But here again there are identical elements, observing the letter *s* in the first,

observing the letter *e* in the second, and the width of column, similar sort of distractions, etc., in both. Of the three tests with a different sort of page from that used in the training, *e-r* shows again the most improvement (here the least deterioration) in accuracy.

We cannot ascertain just which of the functions tested improved most and so cannot discuss the way in which greater alteration of the data alters the efficiency of the group function. The reason for this is that we cannot equate time saved with accuracy lost or *vice versa*. As was stated on page 554, the variability of any single test makes any minute examination of the tables unprofitable.

We shall present the experiments of which this is a type in a still more condensed summary.

(a) Subject T.

Training: 90 minutes' practice marking verbs; time for first 10 pages 417, errors 10; time in last 10 pages 341, errors 1; percentages 82.8 and 10.

Tests: (1) Other parts of speech; before training 412, 15; after training 398, 4; percentages 96.6, 26.3. (2) Marking words containing *s-p*, *i-t*, etc., before training 638, 8; after training 610, 9; percentage 96, 112.

(b) Subject T.

Training: 123 minutes' practice marking prepositions. Time at start 907; errors,—mistakes 16, omissions 10; time at end 756; errors,—mistakes 2, omissions 4; percentages 83.3, 12.5 and 40.

Tests: Other parts of speech; before training 308, 4; after training 314, 3; percentages 102, 75.

(c) Subject T.

Training: 90 minutes' with verbs and 30 minutes with prepositions.

Tests: Marking words over 7 letters and words over 5 letters; before training 233, 2; after training 188, 2; percentages 81 and 100.

(d) Subject T.

Training: 72 minutes' practice marking adjectives; time at start 620, errors 19 or 30; time at end 488, errors 5; percentages 79 and 26 or 17.



Tests: Conjunctions and pronouns; before training 142, 6; after 127, 2; percentages 90 and 33.

(e) Subject T.

Training with prepositions and adjectives as described.

Tests: (1) Marking words containing *s-p*, *i-t*, etc.; before training 700, 9; after training 619, 6; percentages 88 and 67; (2) marking words of 5 and of 7 letters; before training 188, 2; after training 203, 0; percentages 108, 0.

(f) Subject T.

Training with words containing *e* for 80 minutes. Time at start 543, errors 5; at end 468 and 5; percentages 86 and 100. Also some 20 minutes training in marking words containing *a* and *n*.

Tests: Words containing *s-p*, *i-t*, etc. Before training 441, 4; after 367, 9; percentages 83 and 225.

(g) Subject T.

Training with words containing *e* as above and with words containing *a* and *n* for 188 minutes. Time at start 583, errors 43; at end 436 and 13; percentages 75 and 30.

Tests: (1) Conjunctions and pronouns; before training 136, 3; after 149, 4; percentages 110 and 133. (2) Words of 7 and 5 letters; before training 203, 0; after 185, 6; percentages 91 and —. (3) Words containing *s-p*, *i-t*, etc.; before training 441, 4; after 376, 7; percentages 85 and 175.

(h) Subject T.

Training: All the training so far described plus a vast amount of work in correcting all the work recorded so far.

Tests: Marking Latin verbs, prepositions, adverbs, and conjunctions, and French and German verbs; before training 840, 18; after training 764, 11; percentages 91 and 61.

(i) Subject W.

Training: About 250 minutes' practice in marking English verbs. Time at start 617, errors 22; time at end 431, errors 11; percentages 70, 50.

Tests: (1) Marking various other parts of speech; before training 42 minutes 36 seconds, errors 15; after training 40 min-

utes 55 seconds, errors 50; percentages 97, 333; (2) marking parts of speech in French texts: before training, verbs 272, 3, adjectives 407, 3, adverbs 440, 10; after training 336, 5; 402, 4; and 394, 8, respectively. Totals before, 1,219, 16; 1,132, 17; percentages 93, 107.

(j) Subject W.

Training: That described, plus training in marking words containing *e* and *t*. In the latter the records at start and finish were 981, 20; 569, 23; percentages 58 and 115.

Tests: (1) Words containing certain other combinations of letters. The results were:

	Before Training.		After Training.		Percentages.		Percentages of totals.	
<i>s-p</i>	439	9	503	7	90	82	88.5	105
<i>i-t</i>	827	13	643	11	78	85		
<i>r-e</i>	432	10	271	6	63	60		
<i>l-o</i>	271	2	230	3	85	133		
<i>a-n</i>	412	11	438	20	106	182		

(2) Words of over 6 and of over 7 letters.

Over 7 letters	386	7;	364	2 }	94	40
" 6 "	392	8;	364	4 }		

(3) Marking logarithms containing certain pairs of numbers. The results were:

In Marking.	Before Training.		In Marking.	After Training.		Percentages.	
	Time.	Errors.		Time.	Errors.		
4 and 8	118	—	3 and 7	125	—		
0 and 5	130	—	4 and 1	133	—		
	248	2		258	6	104	300

(4) Before training, marking words containing *oa*, *on*, *ti*, *es*, the two letters being in each case together in the order given; after training words containing *ea*, *io*, *ei*, and *ed*, also in each case together and in order. The results were:

	Before.			After.		
	Time.	Errors		Time.	Errors.	Percentage of Totals.
<i>oa</i>	387	0	<i>ea</i>	434	8	89 69
<i>on</i>	343	23	<i>io</i>	338	1	
<i>ti</i>	580	10	<i>ei</i>	358	5	
<i>es</i>	565	2	<i>ed</i>	535	10	
	1875	35		1665	24	

231 cases of such combinations should have been marked in the early tests, 167 in the late. Thus, the percentage marked of those that should have been marked was 85 in the early tests, 86 in the late.

(k) Subject W.

Training for 27 minutes (all at one sitting) in marking words containing *c* and *t*. Time at start 261, errors 1; at end 134, 11; percentages 51, 1100.

The functions tested before and after this training were the marking of words containing certain other combinations of letters. The total time before training was 655 seconds, and errors 12; after training 482 and 28; percentages 74 and 233.

(l) Subject E. T.

Training: About 90 minutes' practice in marking French verbs. Time at start 390, errors 16; at end 353 and 5; percentages 90.5 and 31.

Tests: (1) Words containing *s-p*, etc.; before training 740, 18; after training 666, 14; percentages 90, 80. (2) English verbs, adjectives and conjunctions; before training 557, 23.5; after training 525, 19; percentages 94, 80.

(m) Subject E. T.

Training: About 110 minutes' practice in marking words containing *a* and *t*. Time at start 720, errors 15; at end 586 and 8; percentages 81 and 60.

Tests: Words containing *s-p*, etc. Before training 664, 14; after training 577, 21; percentages 87, 150.

(n) Subject E. T.

Training: That of both (l) and (m).

Tests: (1) Words containing *i-t*, etc. Before training 778, 7; after training 516, 33; percentages 68, 471. (2) Words of over 6 and of over 8 letters; before training 282, 4; after training 249, 7; percentages 87, 175.

(o) Subject M. T.

Training: About 75 minutes' practice marking words containing *a* and *t*. Time at start 610, errors 127; at end 575, 95; percentages 94, 75. In the early pages there were 218 words



containing *a* and *t*, in the last pages there were 199. The proportions of words marked were thus 42 and 57, the final record being 136 per cent. of the record at the start.

Tests: Marking words containing *i-t*, *r-e*, *l-o*, *a-n*, *g-m*, *a*, *d*, *c*, *h*, six or more letters, eight or more. Before training 1290, 435; after training 1365 or 1305, 456; percentages 106 or 101, 105.

That the tests used in all these experiments were not unfair by reason of being tests of functions that could not be easily improved in any way is shown by the fact that in training series verbs, adjectives, prepositions, words containing *e*, *e* and *s*, *a* and *n*, *a* and *t*, *e* and *t*, all showed ready improvement when special practice was made a factor. W. added to this evidence by taking short training series with words containing *a* and *d*, *n* and *t*.

In about 22 minutes' practice with the former the time remained constant but the errors decreased from 17, 9, 10 and 15 in the first four pages to 4, 2, 6 and 1 in the last. Percentage 25.

In about 17 minutes' practice with *n* and *t* the times decreased from 66, 53, 46 and 64, to 39, 39, 48 and 34; the errors from 4, 4, 2 and 3, to 1, 2, 1 and 3 (*i. e.*, from 13 to 7). Percentages 70 and 54.

In the three articles of which this is the last we have endeavored to present as succinctly as possible the results of our experiments. It has not seemed worth while to subject them to a minute analysis, for the reliability of any individual determination is not sufficient to warrant special conclusions from it. The general attitude which comes from the examination of all the facts we have demonstrated, not a set of precisely formulated judgments, is what we have aimed to produce.

The next steps in the study of the interdependence of mental functions would seem to be the exact analysis of the influence of one on the other where such is present and the discovery of its amount and nature in cases of practical importance, for instance, in the case of the training given in school subjects, in occupations, in games, etc. Under the first head we should

hope to see experiments carefully devised, as these rough ones of ours were not, to detect the exact elements of any function that were changed by training, to measure such changes and to find which of such changes brought about increased efficiency in other functions and how. Under the second head we should put determinations of the exact improvement in the efficiency of various functions by commonly practiced educational disciplines and measures of the influence of the training of certain mental functions by school subjects on the efficiency of other functions.

## A GENETIC VIEW OF SPACE PERCEPTION.

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In the last half century the theory and general principles of evolution have modified almost every question in philosophy and science. As yet, however, few writers on space perception have given evolutionary principles much prominence in their discussion. In order to obtain a true view of the genesis of space perception it is necessary to ask, "What is the biological value of space perception?" or "What are the biological reasons for the existence of such perception?" This is the question which biologists are now asking regarding organs and instincts and one which psychologists must in the future ask regarding every mental power. Weismann's theory of the non-inheritance of acquired characteristics makes it necessary in answering such questions to show that the organ, act or power does or has helped in the survival of the species possessing it as well as that it might have been developed by use which is all that Spencer seeks to show regarding space perceptions.

Little argument is needed, however, to show that the ability to perceive space relations gives animals possessing it an advantage over those without it in the struggle for existence, for it is evident that any organism that is to survive must have a tendency to so move in response to stimuli as to secure those that are advantageous and avoid those that are destructive. This means that an object in one position shall be discriminated and reacted to differently from the same object in another position; otherwise food would as often be run away from as approached. To react favorably, therefore, there must be a peculiarity in the effects of a stimulus according to the position from which it comes that calls forth an appropriate motor response.

As soon as we take this attitude and look upon space reactions and space ideas as a means of survival we see at once that cer-



tain prominent theories of space perception may be thrown out on the ground of theoretical improbability. For example, James holds that every sensation has the characteristic of voluminousness, but as different responses for different voluminousness of sensation, even if such were experienced, would have almost no biological value as compared with variations in response for different directions and distances the theory may at once be thrown out as improbable.

On the other hand the evolutionary view gives a rational explanation to wide groups of facts, such as that the most mobile portions of all animals, including man, give the finest space discriminations. Evidently sensitiveness to the position of an object can be of no value to an animal except as it calls forth a useful response. Naturally the movable parts, therefore, have the greatest sensitiveness to locality. Other parts are sensitive spatially only because sometimes advantageous response may be made by moving the whole body or sometimes by carrying a movable part to the immovable portion that is being stimulated.

This view of space does not at once justify the hasty conclusions that the muscular sense, and the muscular sense only, is the spatial sense, for movements in any or all directions are of no value to an organism in themselves but only as they help in securing favorable and avoiding unfavorable stimuli. On the other hand the spatial data given by another sense as to the position of a stimulus would be of no value in itself, for it would give no means of appropriate response to the stimulus. Distance and direction have no significance or meaning except as distance and direction from something; hence, tactile spatial data would be of no value except as related to data depending on position of the body. Every spatial reaction that is biologically useful therefore involves the correct relation of (1) a stimulus, (2) the position of the body or one or more of its parts, (3) a movement of the body or a part of it. These are of no value separately but only as related in the reactions. It follows, therefore, that there is no such thing as a single space sense, or sensation for space perception depends upon the relation of at least three groups of sensations or their images.

Given the organic and equilibrium group of sensations and the movement sensations, then a stimulus to any special sense may call forth a response that produces increase or decrease of the stimulus. Such a response would be spatial, and would furnish data to consciousness, if the organism possesses any, for the perception of space. Smell, for example, is in this way to the fox a spatial sense, for he discriminates which of his movements increase and which decrease the odor that he has sniffed, and this guides him towards or away from the odorous object. The primitive spatial data are probably nothing more than the peculiarities of reactions that secure increase of favorable and decrease of unfavorable stimuli when the organic and equilibrium sensations are of a certain kind. Taste, smell and hearing, as well as sight and touch, are spatial to man in this way. Touch and sight are spatial in a much more definite and exact way, not primarily because of the intellectual advantage they give for the formation of abstract ideas of space such as are usually enumerated by psychologists, but because the good of the organism demands that a different response shall be made for each different part of those sense-organs that is stimulated. The only valuable response to smell, taste and sound stimulations is that which leads directly or indirectly to increase or decrease of the sensation, and, in general, only a very few kinds of movement are needed to accomplish these results. In the case of touch and sight, however, each portion of the sense-organs stimulated requires a different motor response. Several different appropriate responses to each stimulus are also possible, such as withdrawing the body or pushing the stimulus away with one of the limbs or bringing it to a position where it may more effectively stimulate another sense, as the eye or the tongue, and thus indirectly secure favorable results. The eye and the skin have therefore developed local characteristics which are associated with correspondingly different movements, and thus sight and touch, in connection with movement, furnish consciousness with most of the vast number of local signs that make definite space perception possible.

That spatial perception is primarily and for all practical purposes (except that of psychologists) merely incidental to use-

ful modes of dealing with objects is clearly indicated by common observation and experimental data. Experiments in moving the hand as directed by the eye show a remarkable degree of accuracy, but, on the other hand, where the attempt is made to *consciously* compare visual and motor perception of distance the errors are very large. The same is true in comparing visual with tactual in one region, as on the tongue, with visual or tactual in another, as on the finger or the back. Even the supposedly non-spatial sense, hearing, is a marvelously accurate spatial guide in practical activities, such as moving the vocal organs or the bow of a violin.

It is clear that, whatever the conscious states of an animal may be, he must, from the first, in order to survive, be a spatially reacting organism. We find, therefore, spatial relations indicated in the structure of all but the very lowest animals, for they have at least an upper and lower surface, and usually a front and hind end and a right and left side. In the lower animals, such as worms, each portion of the body is provided with a ganglion and a sensory and motor nerve. In the higher animals the sensory and motor fibers of the same nerves go in general to the corresponding portions of the body. Parts physiologically connected but not located near each other are connected by means of fibers in the central nervous system, as the front leg of a dog with the opposite hind leg. Human anatomy and psychology clearly show that man also is physiologically a space-reacting organism.

Turning now to the facts observed by students of children we shall find abundant evidence that man is a space-reacting organism *before* he is a space-perceiving intelligence. Four or five months before birth the child begins to move. Much of this motion is, so far as can be determined, spontaneous, being called forth by the internal growth changes rather than by external stimulus. External stimuli, however, such as pressure or temperature stimulations applied to the abdomen of the mother, or gravity and pressure stimulation resulting from changes in the mother's position, do produce movements. Such movements seem to be convulsive-like movements of the whole body rather than localized reflexes, though no very positive statement can be made on this point.



Immediately after birth localized reflexes in response to skin stimulations are numerous. A touch on the inside of the hand causes the clasping movement of the hand, and the toes clasp in a similar way when touched. If the back of the hand is tapped the hand is removed and withdrawal of feet may also be produced by stimulating them. A touch on the lips produces the sucking reflex and a touch on the cheek (especially if there is hunger) a blind groping with the head. A touch on the eyelid, the lashes, or the cornea, and often on the nose or forehead causes the eyes to close.

*Active touch*, in which sensations are intensified by motion and mingled with muscular sensations, begins in the touching of things with lips and tongue. Almost from the first any object, if not sucked, is mumbled. The fist is most often the object that is brought into contact with the lips and tongue, probably because of an instinctive tendency toward that position. The tendency to clasp with the fingers develops in two or three months into a tendency to scratch or rub the fingers over whatever is touched. The tendency to feel of objects with the hand is clearly evident at the close of the first quarter though for a time less strong than the tendency to feel of them with the lips. In the second and third quarters various parts of the body and objects in various positions are examined by the hand or sometimes other parts of the body, as the feet, though often for a time by the forefinger only, in connection with sight. Movement of hands to parts of the body touched are occasional in the first half year, even at the beginning of the second quarter, and common and accurate at two years. It is evident, therefore, that there is a reflex physiological space relation of stimuli on certain parts of the skin to certain local movements. In some instances these reflex relations are fully developed at birth while in others experience is necessary to their development.

*Equilibrium* movements toward balancing the head are also evidently reflex and instinctive almost from the first, while movements toward straightening the body when tipped may appear as early as one or two months. In the case of the *eyes* there is from the first a reflex tendency to turn the point of clearest vision on an object seen. This is shown in the tend-

ency to turn the eyes towards a light, and sometimes towards an object at one side, and to keep the eyes directed toward any object in direct vision even when that object moves slowly. This reflex tendency develops into sure and accurate turning toward and following of not too rapidly moving objects in the first quarter.

Closing the lids at the *visual* stimulus of a threatened blow is an undeveloped reflex, for it does not take place regularly until two or three months, though the visual stimulus helps to insure the closing of the lid at a tactual stimulus near the eye much earlier.

The visual stimulus of objects at a distance exercises much the same influence upon the hand movements as tactile stimulation on different parts of the body, for from the first attempts at reaching for objects in the second quarter of the first year children habitually reach in the right direction and for near objects only. The latter statement is contrary to common belief and the statements of psychologists, but I know no reliable evidence to the contrary. The supposed instances of children reaching for distant objects are, I believe, the products of imperfect observation and interpretation of children's movements, for the instinctive tendency to lean and reach toward any object of interest may be easily mistaken for an attempt to grasp the object. Baldwin emphasizes the dynamogenic effect of near over far objects as a physiological fact of great importance and all his records of reaching for colored papers show great accuracy in judgment of distances by his children, yet he indorses, without giving a fact in support of it, the old idea that a child will reach for the moon, meaning by the moon an object at any distance. I find no facts recorded by observers of children that, properly interpreted, support this old-time and almost universal opinion. Preyer mentions two instances, neither of which, properly understood, counts against the view of natively differing dynamogenic effects of objects at different distances. In one case a child holds an object out of a second story window for an adult on the ground to take. This merely shows the child's faith in the reaching ability of the adult and is no evidence of a misjudgment as to his *own* powers of reach-

ing, for he did not attempt to *hand* the object to the one below. It shows lack of knowledge of general spatial relations but not of space as related to his own practical activities. In the other instance a boy reached for a light in a car a number of times, laughing hilariously in the meanwhile. Clearly he was not engaged in disappointing and unsuccessful attempts or he would not have laughed with such glee. He was merely playing and enjoying the movement and perhaps the make-believe attempts at reaching. Apparently it was the father, the psychologist, rather than the child, the actor, who was deceived.

In the case of my little girl close observation for several months after reaching began revealed no instance of her attempting to grasp an object more than six inches beyond her reach. In the case of my little boy the expressive movement of holding out the hand toward an interesting object was not so easily distinguished from attempts at grasping, but there was clearly no grasping at objects beyond two feet. If an object was more than a foot away he leaned toward it as well as reached, while if it was six inches he reached with his hands only. In both children I observed instances in which they did not seem to know whether they had reached too far or not far enough when the error amounted to an inch or two. This shows a judgment of distance only a little less accurate than the judgment of direction, for objects were sometimes missed, especially when they were above or at one side of the face. I am convinced that the so-called judgment of direction and distance is not primarily conscious but physiological, the objects in the various positions constituting a different stimulus and calling forth a correspondingly different movement. The case is similar to that of the young chicken, which, from the first, picks at objects with considerable accuracy. The physiological tendency is the same, though not developed so perfectly at birth in the child as in the chicken.

Reactions to *taste* stimulations are from the first spatial, in that disagreeable tastes produce movements of getting the substance out of the mouth while agreeable ones cause movements toward increase of sensation and swallowing. Mucus is frequently ejected, and after hunger is fully satisfied the nipple



is often forcibly ejected by the tongue. Movements of withdrawal in response to disagreeable odors may be called forth quite early, but sniffing to inhale an agreeable odor does not appear until in the second year.

As to hearing, the instinctive tendency to move so as to increase favorable stimuli is specialized to some extent into a reflex tendency to keep the head turned directly toward an interesting sound which, at the close of the first quarter, develops into a tendency to turn toward any sound with accuracy, though the effect of experience is marked, for familiar sounds are more readily located than new ones.

Our study thus far shows that the child is at birth a spatially reacting organism, and since most of the movements during the first few months are made in sleep as readily as when awake and are made by children born without brains as well as by normal children we conclude that consciousness has at first nothing to do with the making of these movements. They are therefore purely physiological. The more complex instinctive and voluntary movements that appear a little later are the result of the development of semi-native reflexes and instinctive movements and of various combinations of them, the tendency to which is also native; hence they also are more physiological than psychical. Consciousness at first can merely cognize such movements. It cannot possibly represent and control a movement until after the movement has been made one or more times. Even in the case of adults movements often repeated and practically directed are not necessarily consciously known. For example, a large proportion of bicycle riders cannot and never could tell which way they turn the handle bars when a bicycle tips to the right. (My own students were divided almost equally on this question.) One part of the body may also have learned to adapt its movement to a visual stimulus when another has not, as has been proven by Reddingius. He found that when one hand has been trained to accurately touch a point seen through a deflecting prism the other hand still made an error proportional to the apparent displacement, while when the prism was removed the trained hand made the opposite error.

The spatial data given consciousness is from the first com-

plex and relational and always dependent upon position of body, direction and distance of stimulating object and movements of body, head, eyes, or limbs. The result of a reaction is the important thing biologically and the interesting thing psychologically rather than the sensations experienced while making the movement; hence little attention is given to the spatial data. Since adults who have not given special study to the subject know almost nothing about the elementary spatial data given, and since children have an almost infinitely less power and tendency to subjective analysis than adults, it is utterly preposterous to suppose that they are at first conscious of the elementary data of space perception as is often imagined by philosophic students of space perception.

The analysis of spatial data by the child probably begins in the practical need of distinguishing between its own body and other things, though the instinct of curiosity has some influence. The marked difference in the sensation complex which the child feels when he treats his own members as he treats other objects leads him to notice the distinction between his own body and surrounding things. At three or four months a child often shows that it has made this distinction by manifesting disappointment and impatience when it brings an object toward its mouth and gets its own finger instead of the object in contact with the lips. This early discrimination of the body from other objects is very crude and only along lines of familiar experience, for a child of a year or more may intentionally strike his hand or foot as he has been striking something else, to his immediate surprise and grief. Even in the third year a child may ask to have his fingers taken off with no more thought of the result than when he asks to have his shoes removed.

Biologically the most important portion of space is evidently in the region of the mouth and there is every reason to believe from a study of children that the first consciousness of space is of relation to the mouth. The first movements are to increase and secure gustatory and tactile sensations, and the tongue and lips are for some time the chief organs of active touch. Auditory, visual and olfactory perception become clearest when the mouth turns toward and approaches the stimulating object.

The hands are from the first often in the mouth, they move a great deal in that region and they continually go out from it to bring objects to that center of the child universe. Voluntary control is first established and always remains most perfect in front of the mouth, while movements high up, far down, or to one side are much more difficult to make with accuracy. It is not too much to say, therefore, that spatial perception first appears in the perception of the relation of other parts of the body and of objects to the mouth. In general this region also remains the most favorable one for conscious judging amounts of space as well as for making movements.

As to the further problem of space perception involved in the discrimination of distance, size and form, the principles involved may best be studied in connection with visual perception. In considering this problem we must take pains to avoid the common fallacy of supposing that a perception of a whole means the perception of the parts and their relations or that the perception of the meaning of complex sensory data involves a knowledge and consciousness of the elements of such a complex. We must also remember that a child has far less tendency to note and analyze his subjective sensations than adults. The psychologist is very much interested in all the spatial data given in the child's sensations, but the child himself cares nothing about them, his consciousness being absorbed in trying to do something with objects in various positions. Not space, but the recognition and manipulation of objects, whatever their position, are the interests directing his movements and thoughts.

As regards distance we have already seen that the dynamogenic effect of an object at a distance and one near by is such that, from the first attempt at grasping, the latter is reached for while the former is not. Judgments of size have been supposed to depend upon the amount of retinal surface covered by the image of an object; hence it is inferred that objects at a distance look small to children. Experiment, however, shows that for adults artificial increase or decrease in size of image on the retina may affect the judgment of distance instead of size and that change in ocular adjustment may affect judgment of size instead of distance. A slight change in attention or the introduction of other



objects in the field of view readily reverses the judgment of size and distance. Usually the individual does not know the basis of his judgment or why he changes it. The judgments of size and distance, therefore, depend upon the same complex data and are relative to each other, so that as soon as one judgment is passed according to the ideas suggested by the complex of sensations and images, the other follows automatically.

The child cares nothing about size and distance as such, but wishes merely to identify and manipulate objects. He knows nothing about size and distance apart from the other, to him, as yet unanalyzed characteristics of objects as wholes; hence he does not think as some have supposed that objects grow small at a distance, though he does often have difficulty in recognizing objects seen at unusual distances, just as an adult would probably fail to recognize near at hand a familiar figure that had been seen only on a church steeple. He is able to compare and identify objects at varying distances just as we can judge of the equality or difference in size of rectangles of various shapes without knowing either of their dimensions. Such comparison is easy for him just in proportion as the clearest and most familiar appearance is suggested by the appearance at the distance from which it is being viewed.

At first, and for some time with most objects, the child probably does not think of objects as being large or small, distant or near, but merely identifies interesting and often-perceived objects, whether near or far, and recognizes that he can often perceive an object better by moving toward it. As long as an object varies constantly in appearance as it or the child moves nearer or farther, there is no occasion to analyze out from the complex whole the characteristics of size and distance. When, however, an object familiar within certain distances is seen *much* nearer or farther away than ever before, it is either not recognized or attention is directed to the change in appearance and this may lead to the analysis of the spatial characteristics of objects from their more unchanging characteristics. This analysis is, however, chiefly brought about and made exact by comparing objects when they can be perceived most plainly, *i. e.*, when near by and directly in front, and where touch sensations also

aid in the perception. The spatial appearance of a familiar object when most clearly seen, therefore, comes to be accepted as its true appearance or size, while variations from this standard are connected with movements and interpreted as distance.

As to form and solidity it is perfectly true that every line, angle, surface and solid, except the sphere, affects the retina differently for every different direction from which it is seen, but untrained adults do not know this and sometimes cannot be made to believe that a square always looks like an oblong or a rhombus, when not seen at right angles to the line of sight. It is utterly improbable, therefore, that the child, who has so much less power of introspective analysis and who, to a greater extent than the adult, is interested in discriminating and identifying objects, rather than in noting their varying appearance, is conscious of the presence or absence of solidity or of apparent variations in form. Just as we can judge the relative size of objects of various shapes he can recognize solids as the same in spite of their changing appearances. The differing appearances are enough like the one familiar to him to suggest it and thus enable him to recognize the object.

Within a half year the child is able to identify a number of objects from any position and rarely mistakes a surface for a solid. At two or three years he discriminates hundreds of forms in nature and may distinguish the principal geometrical forms and some letters.

The discrimination of form from other characteristics and its analysis into lines, direction, surfaces and solidity is brought about by the fact that the mode of dealing with objects necessarily varies with their form, and by the comparison of one object with another, especially when they are beside each other in a favorable position for seeing and touching them. The appearance deemed the true one is that of the object when most clearly perceived (rather than any other or a complex of several such distinct views). Several perceptions, as solids are seen and touched when directly in front and turned in various ways, are fused into an idea of the object as a whole. For this reason a child in his early drawings shows invisible as well as visible portions of a solid and often fails to distinguish between a square

and a cube, or a circle and a sphere, both having to him the same essential elements. The following summary indicates some points in the study of the genesis of space ideas that have not in my judgment been sufficiently recognized and emphasized by writers on space perception.

1. Space perception in the child and the race is the result of, and an aid in, useful reactions to surroundings.

2. There is no such thing as a space sense or a sense of space, for space perception is the cognition of the *relations* of sensation groups to each other as reactions are made in the attainment of practical ends.

3. The most primitive space reactions are those that result in increasing favorable or decreasing unfavorable stimuli, and distance is not at first distinguished from other causes of varying clearness of perception.

4. At and probably before birth the child is a space-reacting organism.

5. The chief center of location and reference is in the region of the mouth.

6. Consciousness of space relations is the result of space reactions, not the cause.

7. The child has no interest in spatial data as such, but learns to recognize and manipulate objects in spite of their varying appearance with changes in direction and distance.

8. The clearest and most common appearance of an object gives him his idea of it and other appearances simply suggest this idea.

9. Ideas of direction, size, distance and shape are gradually formed as the result of the manipulation of objects and the comparison of one with another, while the concept of space as usually discussed is an abstraction from numerous experiences and more or less conscious analysis and synthesis.



## A CASE OF PSYCHICAL CAUSATION?

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The aim of this paper is to show that there is possibly such a thing as purely psychical causation. We shall begin by trying to apprehend the arguments—which are at once so widely accepted and so weighty—on the other side. Psychical causation is denied by present orthodox psychology for several reasons. (1) Historically, the first to be urged was that there is no necessary connection to be observed between any two states. Hume<sup>1</sup> used this argument to disprove causation in outer and inner worlds alike, but more recent thinkers (following Kant) have tried to restore it in the outer world. It is generally agreed, however, that even there it is only the way of viewing facts (uniform sequences) which our categorizing minds adopt—a subjective construction.<sup>2</sup> Now, while the character of the outer world is such as to allow this subjective construction, that of the inner, we are told, will not permit it. Psychical states lack two attributes which are requisites of causal connection; (2) they are not susceptible of measurement, and (3) they are not continuous with one another. A mental state is not a whole summed of parts, but, being a *consciousness* of something, is indissoluble and unitary, even though its object be divisible.<sup>3</sup> Nor could one conscious state ever merge continuously into another any more than one act of will can merge into another; each awareness is eternally ‘shut in its own skin, windowless.’<sup>4</sup> It comes into existence, and it vanishes and is gone forever.<sup>5</sup> So, on the one hand, we cannot find a quantitative equivalence

<sup>1</sup> Treatise, Part III., Secs. II., III., XIV.

<sup>2</sup> Typical of this attitude is Mr. Pearson's ‘Grammar of Science.’

<sup>3</sup> James, ‘Psychology,’ Vol. I, pp. 158-162, and Münsterberg, *Grundzüge der Psychologie*, Bd. I., pp. 263-270.

<sup>4</sup> James, *ibid.*, p. 160.

<sup>5</sup> Cf. Münsterberg, ‘Psychology and Life,’ p. 59.

between antecedent and consequent psychical states, and, on the other hand, we cannot have that continuous change which is a property of physical causation. Hence, there can be no psychical causal laws. And the argument is clinched by the principle of parsimony. (4) Since causation and necessary connection are the figment of our minds, and not the result of empirical observation, we must not apply them in regions where explanation is simpler without them. And, as the order and character of psychical states can be well explained on a physiological basis,<sup>1</sup> it must be useless to assert psychical causality, even if psychical states were both continuous and quantitative.

Such a weight of reasons indeed appears overwhelming. We are not allowed to regard causation (or even necessary connection) as a property of events themselves (whether physical or psychical), and we are not allowed even to *interpret* causally in the psychical world. But since these reasons are for the most part deliverances not of empirical observation, but rather of a certain amount of analysis, we may at least be permitted a little argument. Take reason (2) above, the first attribute in which the inner world was found wanting. Suppose we admit that psychical states cannot *ever* be measured; what then? Well, we may be obliged to forego the kind of causal laws that we find in the quantitative physical world, but not on that account necessary connection. There is, *a priori*, no need that whatever is necessary be quantitative—even though the converse be true. It is at least conceivable that some event might necessitate some other without their being measurable. Here one need only point to projective geometry and the algebra of logic—both regions of strict necessity yet non-quantitative.<sup>2</sup> Now as to the second defect of mental states—reason (3) above—the argument against their continuity surely confuses psychical states in general with consciousness. These two are always found together, I suppose; but for all that one is not the other. I may be conscious of a toothache, but the psychical state called pain is not the same as my con-

<sup>1</sup> See James, 'Psychology,' ch. on 'Habit' and on 'Association.'

<sup>2</sup> And Wundt himself advocates such a psychical causality (though only as a conceptual description), 'Outlines' (tr. Judd), pp. 320, 321.

sciousness of the toothache—it is the *object* of my consciousness. And no doubt the argument against continuity between *consciousness* of this and *consciousness* of that psychical content is true; but still the psychical contents *themselves* may be continuous. And certainly this latter seems to be, very often, an empirical fact for introspection—one's perception of a moving object (sometimes at least) seems a continuous psychical performance. Indeed, it is one of the emphatic assertions of present psychology that mental states present a steady onward flow.<sup>1</sup>

Now, if this argument is sound, the situation (as far as *facts* go) is not fatally different from that in the outer world. We seem to find gradual change from one psychical content to another, and even though there is possible no equation expressing the identity of antecedent and consequent, still we do not as yet find anything absolutely hostile to non-quantitative causation. But here the law of parsimony, taking its stand on Hume's criticism, confronts us. We can at most only show that there is a regular sequence between psychical states, just as there is in the physical world. The causation, the necessity of the sequence, would be our own convenient formulation of the matter, and as such must be subject to the law of parsimony. We must still forbid psychical causation because we can formulate the facts without it. Even though the idea of two plus two were always followed by the idea of four, we should not say the former caused the latter, but explain it by the physiological basis of association. The only possible ground for our granting this doctrine a place would be that the necessity is not our concept merely, but a real fact external to our categorizing mind. It must be shown that one mental state does of itself, on account of some quality which is part of its own content, necessitate a certain other. It must not merely be followed by that other, for then the sequence *could* be explained on physiological grounds. It must *produce* the other, in such a way that we can *see* the necessity of the connection between them. Could such be discovered as a characteristic of the mental

<sup>1</sup> Cf. Dr. Ward, Art. 'Psychology,' in 'Encycl. Britannica,' and James, 'Psychology,' Vol. I., pp. 237, 271.



states *in themselves*, then the law of parsimony would have no sway over us here, because the causality would be *not our mode of viewing* psychical events—that is, not our ideal or conceptual construction—but the *existing connection* between the facts *themselves*. So it is incumbent here to demonstrate an objective (*i. e.*, not constructed by us) necessary connection between some of our mental states, such as Hume could not find. In other words, Mr. Karl Pearson's doctrine of the subjectivity of causation must be refuted by the pointing out of a case in which necessary connection is an objective fact, a characteristic belonging to the psychical contents of some of our states of mind. To do this, we must first frame a definition of what objective necessary connection would be, and second, inquire if that definition is ever, in the psychical world, actually fulfilled. Thus in proving psychical causation one is practically compelled to prove that there may exist an *objective* necessary connection.

The thing to be shown is, therefore, that there is some one mental state which, if entertained, brings up other mental states, and brings them up in such a way that a real necessity is seen for their appearance. Now, the mental state of which I propose to illustrate this attribute, is the idea of *more* or *increasing* (defined as I shall soon indicate), and the mental states which follow it are the ideas used in elementary arithmetic. These latter must be proved to have such a psychological nature that their presence and their character can be seen to be derived from, necessarily connected with, the original state. The method will be to examine the contents of our mind when we entertain the idea of *more* and to show that they would naturally lead us, of themselves, to entertain certain other ideas; that even though this process seldom goes very far in us, it is because interests interfere, associations distract, or outer stimuli withdraw our attention. Then we shall ask, Does this succession of ideas exhibit that necessary connection which is requisite for true causation? Is the series any more than a uniform sequence, based, say, on habit, on early training? To answer that, we must see what conditions ought to be fulfilled if there is to be necessary connection between original and later ideas. We must designate the marks of necessary con-

nection and inquire if this case has those marks. This part of the task will be a purely logical one. We shall find that the idea of *more* is unique among ideas, in that it has a very remarkable psychological property enabling it to satisfy these logical conditions. And further, we shall find that the ideas succeeding have the same property of producing other ideas, that *more* had. The remainder of the paper will then fall naturally into three divisions. (1) Statement of the psychological contents of the idea of *more* as we shall use it, and evidence that the idea, if entertained, actually induces us, however feebly, to entertain certain farther ideas; (2) statement of the conditions required for necessary connection between antecedent and consequent, and proof that these conditions are fulfilled; (3) detailed account of the series of ideas resulting in the process (the elementary arithmetical ideas) as evincing continually the same productiveness found in the first idea. The interest of this third part lies in this, that we are shown the genesis and meaning of ideas of Number and Quantity, and that arithmetical necessity is proved to be, at bottom, a case of psychical causation.

1. First, then, what is the content of the idea as we intend to use it? That is, what is actually present to consciousness when this idea is entertained? The content is twofold, and consists of the idea of size and movement or change. (Inasmuch as change often means decrease while movement suggests, a little more strongly, an addition of something new, a going on to something new, the latter term will be used.) To see an increasing thing is to see a *size* that grows into a bigger size. Size is here not defined further than by some such equivocal word as bigness. It is what is in the focus of consciousness when we are thinking of the quality *size*—and that is about all we can say. It has no detailed meaning or definition as here used, any more than 'blue' has in "The sky is blue." It does not mean size of a certain amount, and does not imply measurement. And movement is here to be understood as belonging to, or a quality of, the size. These are not two ideas placed in juxtaposition, but organically united—as in a thing of qualities. The thing is the size and its characteristic is movement, taking

on of something new. Its complete description is given, as always with *things*, by one word, *moreness*. This may or may not be a correct description of what is always in our minds when we use the word; but it is an idea which we sometimes entertain, or which we might entertain easily enough, and the names *more* and *increase* come as near to it as any other names; therefore, let us agree to use these words.

Now we have to show that a state of mind, such as this, would naturally lead us to entertain certain further ideas. Let us see what would happen when we saw something that gave us the idea of increase. Suppose I am sitting at the car-window in a railroad station and the train starts and moves out past the switches, signals and bridges. Then, if I happen to reflect on what I see, I say, "The distance from the station *increases*," and there are in my mind the two elements mentioned above. At the first switch I am conscious that we are out of the station, at the second that we are farther out—the distance out (marked by the train's position) is moving on. But let us see what has happened in my mind while I was thinking of the element of movement. I could not have perceived the movement had not the perception of movement been present to my mind *successively*. As long as anything moves, there is a continuance of its motion. If a ball flies a hundred yards through the air, the motion it had during the first ten yards was kept up, was repeated for another ten yards, then another, and so on. And the same repetition is found in our *perception* of motion. If I see a fly on the window-pane, in a certain spot, then suddenly find that he has crawled to another spot without my seeing any intervening changes—without my having a *series of perceptions*—I do not say I *saw* him move; I can, at most, only *infer* a motion. In order actually to see him moving, I must have seen enough of the intervening changes to make the whole process seem a continuous one. I must have seen the process of change *repeatedly*. So with the perception of increasing distance from the car-window. If I am aware of the movement from the station out past a couple of switches to the first bridge, I must have been aware of the movement (however dimly) in each stage of that advance. As



we went from the first switch to the second, it was present; as we went from the second switch under a bridge, this idea of movement came again into my mind. And if at these various stages I had not been aware of the movement, I should not have been able to see the *continued* change, which is a necessary attribute of perceived motion.

This, of course, does not so far show that I have been aware of the repetition<sup>1</sup> as such. The idea of movement does not necessarily induce us to become aware of its own repetitive nature. The idea of *more*, however, *does* induce this awareness, for *more* and *increase* are relative words. Whereas, the movement of the train past several bridges need not force upon me the thought, "Going past bridges is a process often repeated just now," yet the idea of increase in distance tends to suggest the thought of the present distance as contrasted with the former distance. And if movement is a continuous process, certainly increase (as we have defined the word) is equally continuous. Therefore we have (whether aware of our own state of mind or not) the idea of increase present to us repeatedly. But if, each time the idea is entertained, it tends to bring up the contrast between the present distance and the former, there is certainly a *tendency* to get before the mind the idea of a series of increments. It is because the idea of *more* includes *both* the element of movement and that of relation between present and past, that it not only has repeated itself several times, but tends to make us aware of that repetition. To have the idea of something that increases, then, is to tend toward consciousness of a series of increments.

To be sure, this can hardly be proved by introspection—99 times out of 100 the passing bridges or signals lead to no thought of a series of increments in distance. But I think it would be generally agreed that, in these 99 cases, the direction of our train of ideas could be adequately explained by association, interest, or outer stimulus. The difficulty is not to explain why deviations from the above marked-out line occur,

<sup>1</sup>In using 'repetition' here and later, I do not mean to imply that the same individual idea can recur. It is simply (as explained later) another idea with the same character.

but why there should be a tendency along that line. And since we are dealing here with tendencies—and tendencies which, confronted by so many and so strong practical interests in our busy life, must be for the greater part submerged and thrust below the threshold of consciousness, we can hardly expect any proof further than general analogies and probable reasoning. I can only point out that, if our state of mind is such and such, it *would probably* urge our thoughts thus and so. But even this kind of argument is not without some weight. When we consider, for example, that a long-repeated series of repetitions—of rapidly passing telegraph poles—often forces itself on our notice by its very painfulness, and makes us clearly aware of a series of increments—it seems as though the same tendency must be present, even when the time is very short—though below the threshold of consciousness.

The next point to be shown is this: When the idea of an increment in distance tends to suggest the idea of a series of increments, each more than the last, we have a state of mind which itself evinces (if we analyze it) a tendency to widen and bring up still further ideas. This will certainly not be plain to introspection. We cannot immediately see that the thought of a series of *mores* would tend of itself to suggest anything further. But a little analysis can inform us of what is beyond sight. What, then, have we good reason to suppose is really going on in our mental machinery? Here are two (or more) increments, and we are aware that they are related (consciousness of *more* tells us this, because *more* is a relative word). Now what is the nature of this relation—I mean, what is its real psychological nature, irrespective of our being *aware* of that nature? First, it is such that it presents us with a series of terms—that is, a consciousness of terms arranged side by side; in other words, the terms are so arrayed before the mind that the relations existing between them (whatever they may be) could most easily be detected—provided we should happen to look for them. When we want to find the character of the relations existing between any two things, the first requisite is to get them arrayed together before the mind. So here, the materials are ready for the discovery of the relation's character. This, then, is the

psychological character of our consciousness of *more*—that the terms of the relation are offered in contrast, side by side before us. But to effect the discovery of the relation's character, another factor is needed. Unless we were *urged to look* for some kind of relationship, the being arrayed side by side before the mind would have no influence upon our minds. One can very well have the idea of a series of pendulum-beats and yet not tend to ask himself what is the relation between those beats. But if he has the idea *that they are more and more* (as in our case is presupposed) he has a suggestion of a relationship between them. This suggested relationship, now, amounts to a consciousness *that a relationship exists*. But, he does not know the *character* of this relation. For, the character it has (as we have seen) is that it presents the terms arrayed side by side for his inspection of what the nature of their relation is. The character is that the character is waiting to be discovered. The situation, then, is just this: We are made aware, by the word *more*, that there is a relation, but we are not told what sort of a relation it is, though the materials for the discovery are presented. Now, in such a case, what would be the natural tendency of our minds? Why, to try to discover the character of the relation. This tendency is an instance of a universal human attribute, as common in the child's as in the savage's mind—the primitive instinct of curiosity. It is this which makes us turn the eyes in the direction of a light that suddenly appears in the dark, or look around when a blow is dealt us from behind. If a man stands peering into the darkness, and some one says to him, "There is something over there," he will strain eye and ear alike to discover what that something is. An event occurs that acts on our mind as a pointer; the discovery of an *existence* urges the discovery of some *character* in that existence. So when we learn that a relation exists between the elements in front of our minds we naturally look to see what that relation is. Thus we tend to ask, what is the character of the relation between these increments?

We must get this process clearly in mind, for two reasons. The first is that upon it the whole advance depends. Every time we get a new idea, as we shall pass from stage to stage, it will be in this way. We shall be aware of a relation (sug-



gested by *more*) whose character we cannot yet tell, and this will induce us to get a description for it. This description or characterization will constitute a new idea. Each new idea, however, will be found to be an insufficient characterization of the original *more* (for reasons which will appear), and thus we shall find ourselves continually left with an existing, yet still to be characterized relation.

And, secondly, it is indispensable to understand this process, because (I hope later to prove) *such a one alone* displays that necessary connection demanded by causation between antecedent and consequent ideas. For even now, before we come to the definition of necessary connection, we can see that the psychological properties of this process are somewhat unusual. The original *more* led us to repeat itself—or rather (since we were actually repeating it all the time) gave us the idea of the self-same original over again, viewed as a relation. This relation, in asking us to characterize it, is to be restated in a new aspect—and so on. And if this restatement brings in no new materials from without it will be a *repetition*. The discovery of a relation which we characterize would thus lead to a process which is nothing more nor less than a process of *self-repetition*. There would then be something about the original idea that gives us new material, in a sense, and yet that new material, being a *repetition*, derives all its character and properties from its parent. Already it seems like a case of genesis, of actual creation and inheritance of family traits. But until we have seen more of its workings we cannot judge accurately of the process.

It is necessary, then, to go through two or three of the next stages before we can apply completely to the process the definition of necessary connection. So we take them in order:

1st Idea. *Sameness*.—Given the question: What is the character of the relation *more* between the increments? How describe moreness? What can we call it? And we are thereby urged to give it a *name*. And this name is to describe that positive relation (I say positive to signify that we are seeking a definite character) existing between the increments. We want a name for 'the relation between them, viewed as expressing

their nature relative to each other.' This is, whether we know it or not, equivalent to the relation between their contents. (For, the positive relation between any two things means the contrast or relation between their characters or contents.) We want, then, to give some name to the relation between the contents of the several increments. This is done by the word *sameness*. Sameness<sup>1</sup> here means only 'that relation which holds between increments in a series of *mores*.' And let it not be said that some new meaning, not originally present, has come in. For sameness is simply a short word for the cumbersome phrase 'that relation which . . . etc.,'—in other words the meaning of sameness is defined solely by reference to our original material. But even though no new material has appeared, we feel that we have really made a discovery, simply because we have coined a name. For it is a human failing, that we regard anything as of more weight when we have dignified it with a name of its own.

2d Idea. *Difference*.—Sameness does not satisfy the demand for description. For it is a psychological fact that when we are thinking "Here is the same thing again," we are (whether conscious thereof or not) tending to forget about the relation and to think only of the content. When we identify or recognize any object, it is necessary to attend to its qualities or contents—for in practice we distinguish things by their qualities. Thinking of the sameness thus leads to the thought of bare self-identity of the content (*i. e.*, as being a *size*) through the series, and here the relation disappears from the mind. So we have on the one hand the relation *more* before us, asking: What is my name? And on the other we have the idea *sameness* which, even while answering the question, leads the mind away from the relative nature of *more* to the characters of the individual members. Now, since we are seeking not so much for the individual characters in the series as for the relation between them, we are still urged to seek the nature of this relation. So we return to a situation much like our first one. Our question now is: What is the name for the *relation* between the

<sup>1</sup>Of course this use of the word is not the only one. The word simply seems the most convenient to designate what is described above.

members (not the contrast between characters, since that did not happen to be fruitful for the discovery of a relationship)? And this name is *difference*. *Difference* means simply the relation holding, not between the individual contents in the series, but between the members as members, *i. e.*, it means a relation in general between them. And no new meaning is here used, for this definition makes use of the original terms only. But again, having gotten a word for the above circumlocution, we think we have discovered a new idea.

So much for the process by which ideas are generated. Of course it is not meant that the idea of sameness, or of difference, first arose in this way. They may have arisen in any way we please: but it is claimed that, given the idea of more as above defined, *then* there is a tendency to call up ideas just like those we call by the names sameness and difference (that is, if they have been correctly understood). Nor is it claimed that this tendency is discoverable by introspection (though sometimes it may be). The argument asserts that, on general analogies, there would probably be a tendency to entertain further ideas, if we were aware of an existent something (in this case an existent relation), but were ignorant of its character. In other words, we have general psychical characteristics which give some warrant for the probability that there is a certain idea which tends to generate certain others. Though this is not saying very much, it is still an interesting fact—and it would be still more interesting if this apparent genesis were found to fulfil all the requirements for being an effect-necessitated-by-a-cause. We go now to that part of the inquiry.

2. We must first state the logical marks of necessary connection between events (whether physical or psychical). I mean connection between events themselves, and not merely ascribed to them by our categorizing mind. That is, we must state such logical marks in both antecedent and consequent as would show a real derivation of the latter from the former. Now, to have necessary connection between any two events,<sup>1</sup>

<sup>1</sup>I do not go into any detailed examination of the concept of causation, but discuss necessary connection only; first, for lack of space, second, because I think it would be generally admitted that where one psychical event led of necessity to another, the case would be for all practical purposes one of real causation.



three conditions must be satisfied. (a) The character of the second must in the last analysis be the *same* as that of the first. Otherwise there will be something in the consequent which cannot be shown to have sprung from the antecedent. There will be something different in kind in the consequent. This attribute of necessary connection is recognized when physical science equates the energy in cause and effect. But the fact that it cannot point out a complete *qualitative* identity between cause and effect—for instance, between the phenomenon of radiation of the sun's heat and that of evaporation, which appear to us as events of very different natures—has been a strong reason for many men denying objective causation. At least if such perfect identity could be traced from cause to effect—while yet there was some real change of events—one great objection to necessary connection would vanish. (b) This last parenthetic clause suggests the second condition: There must be a change of some kind. There can be no necessary connection without two events to be connected. Even though the consequent does not differ in character from the antecedent, it must somehow be a *distinct event*.

This could happen as follows: If I had an idea, no matter of what, and that idea tended to make me keep attending to it—that is, tended to *repeat itself*. Then the repetition would not differ in content at all (that is, in character) though it would involve a change in my state of mind—supposing that I was aware of the second case as other than the first. This second condition need not conflict with the first, then, if I have an idea that leads me to continue attending to it. A *self-repeating* idea therefore satisfies the first two conditions. But the phrase 'leads me to continue' was just used: now, how do we know that it *leads* me? Do we not assume the necessity that constrains me to continue attending?

This leads us to the third and crucial condition. (c) There must be a movement, or process of continuous flow, in the antecedent, such that it gradually becomes the consequent. Even while we entertain the antecedent, we must be conscious that it has a movement in it. This *connects* it with the consequent and enables us at the same time to account for the latter's char-

acter and existence. Its character is what it is, because it is simply a *later instance of* the antecedent (the antecedent moved on)—and it exists because *there was a movement* to begin with (*i. e.*, a change: the first instance changed into the second). Given the *character* of the antecedent and the *fact of its movement*, then we can see that the consequent had to appear, and, if no new elements are displayed in it, that its character is perfectly accounted for. Now, if we could ever find a pure case of this kind, we should be justified in saying that there was something about the antecedent that *necessitated* the consequent. Of course we could conceive the movement suddenly stopping, before the latter was well before the mind; but that would only be an *interruption* of the necessity, which if left alone would still have produced the results. The necessity itself appears in the fact of movement; for movement (presupposed to be continuous), where no foreign materials are brought in, implies so close a connection between its earlier and later stages that the later are offered as *growing out of* the former. And they *have* to grow out because there is begged the attribute of movement. And it does not matter whether there is real continuity or not, between the earlier and later cases—so long as there is apparent continuity; for we are trying to find a sequence which would, as it was present to our consciousness, fulfil the conditions of necessary connection. Our question now is: Can we ever find an event which has character and at the same time a process of change going on in this character, which leads to a series of repetitions of the original?

Such a case is, of course, the one we have been discussing, the idea of *more*. It was defined as involving a size (this was its character) that moved on (this was its property of increase); and this increase tended to make us aware of a repetition of the original. Then this repetition (or series) suggested a new name for itself (sameness as a new name for the original *more*), and after that the discrepancy between sameness and what was required by *more* suggested another name (difference): and throughout we saw this was a process of self-repetition. For sameness we defined wholly by the original ideas, no new idea having appeared; difference likewise was simply a new

name for the old mental content. The first peculiar quality in *more* is that it suggests (by its own *relative* nature) that it is a repetition of itself. And here is seen the special productiveness of the idea, beyond, for example, the idea of a series or of any complex combination where no moreness is thought of. For these latter do not have in themselves a tendency to stimulate thought, because they do not give us a consciousness *that there are relations* waiting to be described. In any series, it is true, there *are* such relations, but nothing tends to make us *aware* that there are. But *more*, being a *relative* word, is the same as a statement, "A relation exists here." And being also a word implying a content (a size) it also states, "The relation has a character, for it holds between terms with definite characters." Here is seen the second peculiar quality of the idea; that it is not merely relative, but dual, in that it has a content of its own to which it is relative.

Let it not be said that the whole procession of ideas above described can be fully accounted for on grounds of memory and instinct. It is true that without memory I could not recognize the original in the later elements of the series, and therefore could not get the idea of sameness. It is also true that unless I had been endowed with an instinct of curiosity, I should not desire to conjure up new names for the original more-series. This, however, does not prevent the train of ideas which arise from being a causal train. Memory is but a name for the way in which certain contents appear—a *classifying*, not an *explaining* name; curiosity, likewise, is but a name for many similar ways of feeling which we experience before the discovery of new materials. It matters not how they arose, nor whether they depend on physiological conditions for their possibility. The essential point of my argument is that when they do come into play in the manner described above they appear as names describing a causal process, comparing the way we feel (in case of curiosity) or the physiological conditions which make its action possible (in case of memory) with the way we often feel at other discoveries, or with the usual physiological conditions of a consciousness of a series.

3. It remains to state the rest of the process, showing thereby



that the original idea *more* leads naturally to ideas of number and quantity.

3d Idea.—*More* has now been defined as the relation between repeated lumps of size (increments of any kind) which are the same and yet different. But sameness + difference is insufficient. Sameness did not describe the relationality of *more*—it led the mind into the character of each increment—and now difference fails to tell us anything *about* moreness. When we are told that any two things differ, it is much the same as if we had been told that they are related somehow—difference is so very general a term. We at once ask, how do they differ? To describe *more* we need a third idea, telling the difference between the members. This idea is position in the series. Position is here defined as “That in which the repeated increments in a more-series differ.” But, again, no new content has appeared, for the definition is a circular one.

4th Idea.—*More* is now described as the same thing over again in different positions along the series. But this is unsatisfactory, because it is not a relation between the sizes, but only between their positions. It does not tell all there is to tell about *more*. *More* appeared to us at first as a relation between sizes, and though we have been trying to tell what this relation is, we have actually been describing only aspects of it. It will, therefore, continue to constrain us. So we ask, what is the relation between the sizes? This gives us a fourth idea, *sameness in size* (corresponding to difference in position). Size is here defined as “That in which the differently-placed increments in the more-series are the same.” It is not the same old idea we had at first (in the original definition of *more*), but means now: Size that may be compared with other sizes. This is not yet measurement, of course—which needs the idea of equality as well—but might be called, perhaps, the possibility of measurement.

5th Idea.—Still the relation *more* is not described enough. Sameness between sizes leaves out of account the relation between the later members of the series, *as later*, and the earlier, and thus tends to make us lose sight of the relation of increase. So that relation which keeps urging us to describe it will drive

us further to characterize the relational element, *i. e.*, to describe difference in position. What is the relation between positions? The answer is, later and earlier as corresponding to more and less. So we get as a fifth description: A series of same sizes which are *more* as they come later in the series. Here we have brought in the notions of *later* and *earlier* as describing *more* and as comparing it with its own part, that which is *less*.

6th Idea.—Since, again, later and earlier describe only positions, not sizes, we must ask for the relation the *sizes* bear to one another, *as sizes*—not as in this or that position. And here the answer is, sameness that is not moreness nor lessness; or, better, the equivalent word, *equality*. This has gotten before our minds the idea of a finite number-system. We have a series of equal sizes, which series increases as new members are added.

7th Idea.—But have we even yet described the relation which connects the more with the less—or one increment with the next? We want to find the bond of connection between them, that is, *more* says once again to us, “There is more about me than you have seen.” So now we will describe this new part, connecting any two increments, in our usual circular way, as ‘that about the relation *more* which is *more* than we have seen.’ In so doing we view the new part as a *more*, and it is related to the already present contents as more; therefore it suggests, just as the other one did, another element in between which again shall be more. Thus the relativity of *more* necessitates an explicit process of self-repetition. And as each one of these new “*mores*” has the same nature with the first, it follows that each one involves a ‘same size repeated,’ that is, the finite number series we had becomes an infinite number series.<sup>1</sup>

We have thus on our hands an infinite series of *mores*, which are all *more* than those preceding, and thus suggest a definition for each number as related to those preceding it and so on indefinitely. This investigation of the possible relations between numbers, however, gives rise to arithmetic, algebra, etc. The

<sup>1</sup>This infinite series is here seen to be an attempt to describe the infinitely numerous parts existing within the originally perceived and finite *more*. This explains why a quantity is a completed infinite.

necessity in these sciences will be due to the fact that number-relations are descriptions of an original material (something that increases) which bring in no new content, yet are the product of a natural movement found in the idea of more—a movement of infinitely complex self-repetition. And this movement is a psychical causation.



## STUDY OF EARLY MEMORIES.

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A study of the earliest memories of childhood cannot be otherwise than helpful in giving us fuller knowledge of the child-mind; and hence it has a direct interest for the student of pedagogy. Studies of earliest memories have been made by Victor and Catherine Henri, and the results of their investigations are published in the *Popular Science Monthly* (Vol. LIII.).

Of 123 persons, principally French and Russians, they found that 120 had memories reaching to years of infancy; 20 recalled several events of the same period of time. The ages varied between wide limits, the average being from 2-4 years. Most of the events remembered were out of the ordinary course of affairs and had affected the child strongly, though a few had recollections of common events recalled with striking detail.

Colgrove, in his book upon 'Memory,' devotes a chapter to 'Individual Memories,' studied with respect to the periods of life to which they belong. He finds that for the period from 1-5 years, males have the best memory for repeated occurrences, people and clothing; females, for novel experiences and single impressions. From his whole study he concludes that the facts remembered are those in harmony with the general psychical life of their period.

The present study is based upon the earliest memories of seventy-five of the students of Mount Holyoke College, and of twenty-five students at Yale. Each one was asked his or her earliest memory, and the age at which the event took place. There are some difficulties in making a study of this kind, which should be mentioned here, since the manner in which they are met makes a difference in the final results.

1. Is the memory given truly a memory, or has the person

relating it heard the event described so often as to come to believe he remembers it? In nearly every instance the person was asked if he had heard people talk about the occurrence—and if the answer was in the affirmative, note was made to that effect. Many whose remembrances were of events heard about later could describe details. For instance, one remembers that when she was one year and ten months old her father and mother took her on a journey with them. She cried on the cars and her nurse tried to quiet her. Of this she had been told, but she remembers in addition that the nurse had a blackbird perched on the brim of her hat, which frightened her whenever the nurse shook her head. Such a memory was not thrown out in the average of final results, while those with less circumstantial detail were. Many memories were of events which from their insignificance were not open to a doubt as to their reality. Such a one is: "I remember one evening in early spring standing by the table and having a piece of frosted cake. The doors were open and it was warm and pleasant."

2. Fixing the age at which the event took place. In many cases this can be done only approximately, and there is a large chance for error here. Some can be definitely fixed, as for instance the birth of a sister. Where the age given is very young, the memory was not counted in the final estimate unless definitely determined. This was done in all cases. The earliest age given was a little under one year and the memory was of the person's mother taking a little cloak from a drawer and putting it upon the child. Upon inquiry being made it was found that this cloak was worn to the wedding of the child's aunt, and thus the date of event was fixed.

3. Many people give a first memory and then think of another which may be earlier, or of several about the same time. In this case the one most distinctly remembered was chosen.

Besides the liabilities to error arising from these sources, it must be kept in mind that the questioner cannot always get at the salient points of the memory; or the circumstances under which the event occurred; indeed the subject himself often cannot give the latter; that the number of memories studied

is small, and that in the comparison between the earliest memories of men and women the number of men studied is considerably less than of women—in short, that the study could not be made under ideal conditions. However, the results are, I believe, approximately correct.

The results of the study were tabulated under seventeen heads. The memories were classified; first, with regard to the frequency of occurrence of the remembered event; second, the nature of the event, whether of itself likely to make a strong impression upon the child; third, the remembrance of very minor details; fourth, sense types; fifth, the relation of self to the event. Several other kinds of memories occurred so frequently that they were classified also.

Memory Type.	Total No. In- stances.	No. Women.	No. Men.	% Women.	% Men.
Single occurrences.....	62	55	7	73.3	28
Repeated occurrences.....	23	17	6	22.6	24
Repeated occurrences but first in- stance remembered.....	12	1	11	1.3	44
Extraordinary .....	32	24	8	32	32
Minor details.....	55	52	3	68	12
Visual.....	21	19	2	25	8
Auditory.....	1	0	1	0	4
Gustatory .....	5	5	0	6	0
Self principally involved.....	75	55	20	73.3	80
Self not principally involved.....	11	9	2	12	8
Pain .....	9	7	2	9.3	8
Persons .....	4	3	1	4	4
School .....	7	1	6	1.3	24
Dress.....	12	7	5	9.3	20
Birth of brother or sister.....	4	4	0	5.3	0
Learning to walk.....	5	4	1	5.3	4
Learning alphabet.....	2	0	2	0	9

Examples of memories classified as single occurrences are, 'sitting one afternoon in my little chair and pretending to read,' 'being carried home from church,' 'breaking sister's doll,' 'slapping baby sister'; of repeated occurrences, 'playing with my brother,' 'my pet horse and driving in the country.' 12 per cent. are a class which it was thought best not to group with either of these, since they have the nature of both. Such are, going to school for the first time, and the first kilts or trousers. Of course the first day in school happened but once, but the continued attendance at school may have



served to deepen the impression which otherwise would have faded away. Three memories could not be classified upon this basis, because of insufficient knowledge of the conditions.

Examining the table, we find that 73.3 per cent. of the women and 28 per cent. of the men have memories of the single occurrences. Of the repeated occurrences, 22.6 per cent. of the women and 24 per cent. of the men, while 1 per cent. of the women and 44 per cent. of the men have memories of the third class. These results agree with those of Dr. Colgrove mentioned above, that for this period the memory of men is better for repeated impressions, and of women for single impressions.

It was found that 68 per cent. of the women remembered very minor details and events of seemingly no importance, while but 12 per cent. of the men had memories of this character. For example, one gives for her earliest memory walking to church with her father, and taking hold of his hand to cross the street. One remembers when three years old trying to wipe dishes for her grandmother. When told that she was not doing it right, she turned and said, "Some day I'll be grandmother and you little girl. Then we'll see!" She remembers just where she stood and how everything in the kitchen looked. Another has just a glimpse of a sunny autumn day at her grandmother's. She remembers being under an apple tree beneath which were many mellow apples, and can recall the fragrance of the air and her feeling of happiness. Although she has not been there since she can describe the place perfectly. The little incident of getting into a carriage is the only memory one gives of a visit made when two and one-half years old. One, who remembers learning to walk, says that she wore a blue wrapper and came through a door. An interesting fact in connection with one memory is that the walk home after the first day in school and the red ribbon upon the broad brimmed hat worn then is distinctly remembered, while the school itself and the other events of that important day are wholly forgotten.

Often figures in curtains, or carpets, and the design upon a cup are described. One man gives as his earliest memory his

first kilts with gilt buttons. We cannot know, of course, how important the most trifling details and occurrences may seem to the child and so cannot estimate rightly.

Taine says: "The primitive impression was accompanied by an extraordinary degree of attention, either because it was horrible or delightful or entirely novel and surprising and out of relation with the habitual current of life," and the observations of Henri bear out this theory; but with the data from which this study is made this is not found true. Only 32 per cent. of the whole were memories of extraordinary events. Many events may have been of that nature to the child which do not appear so now, but an action such as carrying a puppy in a fold of the apron around the corner of the house could hardly be classified as such without knowing more of the circumstances, and the majority of these memories are of as trivial a nature.

Other examples of this kind are, 'lying in bed and seeing my little brother in mother's lap,' 'sweeping the front porch with a little broom,' 'sitting at mother's feet and seeing her do patchwork,' 'the door of a house at which I visited—a dog lay before it,' 'a green lounge in my grandmother's room,' 'seeing mother make pies,' 'peeping through a chink in the door at my brother,' and there are many others, some of which have been given above.

Among extraordinary events are grouped, 'birth of sister,' 'grandmother's funeral,' 'falling off a ladder,' 'having picture taken,' 'going on the cars,' 'being frightened by my great-aunt's pointing a knitting needle at me.' Such memories as these answer Taine's description of being out of the habitual current of life.

As to the sense type to which these memories belong, 21 per cent. were classified as visual. The impressions enter in nearly all cases partly through the sense of sight, but in this group were placed only those in which the *seeing* is made the principal thing. Such were, 'a bed of tulips in the square,' 'the green grass and trees seen when riding,' 'a glass mug on the window-sill when I had the measles.'

Auditory memories are rare, only one being given, the hear-

ing of the story of David and Goliath. This seems strange when we consider how many stories and lullabies children hear. Five mention things to eat, such as candy, cake, 'the griddle cakes with brown sugar at grandma's.' These were classified as gustatory, though that term here does not mean that the taste is actually recalled in the memory.

Motor impressions were not separately classified, but it is to be noticed that nearly all of the memories are of actions, and of events in which the child itself is principally involved. Only 11 were found in which the self was not intimately interested, 12 per cent. of the women and 8 per cent. of the men.

Nine remember feeling pain of sickness or accident. One remembers the smart resulting from plunging the hand into a cup of hot tea. She was one and one-half years old at the time, and has described the details accurately to her mother who did not believe she could remember it. None described feeling unusual pleasure, but most of the memories are of pleasant events.

Persons outside of the immediate family are seldom remembered, and these only as taking part in some action. Two examples out of the four are noteworthy. One remembers the face of a gentleman who stooped down to tell her, as she sat upon the floor, that she had brown eyes like some one he knew. The second had in her infancy been a favorite with a gentleman who went away from town when she was two years old. Several years later she saw him at a party and recognized him, although she did not know his name nor had she seen his picture.

Six men and one woman remember the first day at school. The small percentage of women giving this as their earliest impression may be due to the fact that the average age of the first memory for men is 4.4 years, while for women it is 3.01 years, which is younger than the school age. Five remember learning to walk; two, learning the alphabet; 9.3 per cent. of the women and 20 per cent. of the men remember articles of clothing. It is curious that only one from the seventy-five women gives a doll as her first memory.



## DISCUSSION AND REPORTS.

### THE RELATION OF EMOTION TO MATHEMATICAL BELIEF.

It has become quite the tendency to make a distinction between standing and current opinions and beliefs (Le Bon, 'Psychology of the Crowd'; Professor George Vincent, in University of Chicago lectures, 'Public Opinion'). The standing beliefs are those that endure unchanged during many generations and the current opinions are those that change with the days.

In connection with these distinctions it is suggested that the emotional concomitants of each of these are different, that while the emotional tinge of the current opinion is slight that of the standing belief is of the most intense nature. While this appears at first true, due reflection discovers that there is a strong exception. This exception is in the nature of mathematical beliefs, many of which have certainly persisted among civilized people a score of centuries. The common faith in the unchanging quality of numbers and the fundamental maxims of geometry are cases in point. It might be said that these opinions only apparently have no emotional coloring; that if you could convince anyone that two and two are not four the person would receive a terrible shock. This is perhaps largely true. But this only shifts the point of emphasis of the problem. The question is not, now, why do mathematical beliefs lack all emotional accompaniments, but, why are mathematical truths so far removed from the range of emotional shock? In the long run the two questions demand essentially the same consideration.

It is to be expected that an explanation of the origin and growth of the science of mathematics would cast some light on the relation of the two factors in question. If it is believed that the mathematical powers of the mind are a growth, then it is necessary to hold that mathematical beliefs are matters of social development as are the other beliefs. If, with Wallace, one should believe that the mathematical power of mind is a new creation, then he is free to hold that mathematical beliefs are not of social origin. The latter position is defective. Anthropologists have abundantly proved that mathematics,

especially geometry, grew up out of human experience, practical avocations. Axiomatic truths were first discovered and proved by practical methods. In its origin, at least, the science or the art of mathematics was a social affair. It is most important here, however, to see that the higher mathematics do not depend on social considerations. As Schubert has indicated (*The Monist*, Vol. IV., p. 294), mathematics as a science is entirely self-sufficient. Other sciences, even the most quantitative and exact, depend on sense-observation. Mathematics is independent of the direction civilization takes. It is deductive in method, as Des Cartes so clearly discerned.

I believe that the perception of the nature of mathematics, the independence of the science of the facts of other sciences, promises to furnish a clue to our query as to why the truths of mathematics are so little subject to emotional valuations. In order to develop this clearly it will be necessary to consider the relation of beliefs in general to the emotions.

When we attempt to classify beliefs we discover we have invaded the domains in which the master minds of modern times have contested with each other in their effort to establish a legitimate and accurate hierarchy of the sciences. For, the development of beliefs in the unfolding of society has given rise to the differentiation of the fund of knowledge into its separate and distinct spheres. Science is merely the authentication and defining of beliefs that have gone before and lighted the way. It would be of little importance to our consideration now whether or not Comte's classification of the sciences according to the principle of generality could be substantiated, or if his hierarchy expresses the historical order of those sciences. This paper is rather concerned with discovering the order which the great beliefs of society assume in accordance with another and different criterion. The problem here is to determine the gradation of beliefs when examined with reference to the amount of intensity of their concomitant emotions.

Of the possible schemes for the classification of opinions that of De Greef (*Introduction à la Sociologie*, Vol. I., p. 214) recommends itself to us most highly. De Greef erects seven categories of beliefs in the order of their depth of intensity. The classification is also intended to correspond in a degree with the order of their social genesis. His order, grading upward from the more to the less intense, is as follows: economic, genetic, esthetic, systematized beliefs (such as religious, scientific, etc.), moral, judicial and political. It is intended to include in each only the great fundamental opinions.

Some such order might be accepted, with modifications, by which to mark the grades of opinions as measured by emotional expression or reaction. Let emotional reaction under attack be the criterion, and in general the strongest reaction would denote the opinion about facts which stand closest to life processes or are so considered, those that seem most essential to the support of the grade of life which entertains the beliefs. Then of the general standing opinions, the deepest and most persistent would be precisely those that have lived longest in the race and are the most strongly imbedded.

It may seem indeed that from the simple fact that the beliefs are so old we have an explanation for the intensity of their emotional concomitants. But this would be a mistake. Upon the general basis of Baldwin's showing, it is clear that age has to do with it only in getting the ideas imbedded in the creeds, institutions, ceremonials, etc., so that it is certain to be impressed on the new individual of successive generations. The fact is, the old ideas have the advantage over new ideas only in having the social institutions and instruments of propagation behind them. They go on living in society by reason of the persistency with which they are dinned into the ears of children in the home, the school, the church. Then when you ask why they become established in the first place, I consider I have given the correct reply above, namely, because they stood close to the interests of life in some way.

This position is quite different from the one that makes the emotion in connection with such ideas the outgrowth of time. That view holds that simply from having believed the same thing for generations that thing becomes instinctive to the individual born into the race, as it were, and the emotion is of the instinctive order. I cannot believe that this is a true statement of the case. This would be only the reinstatement of the innate idea theory. We know of no hereditary ideas. Transport an infant of the most monotheistic and monogamic people on earth among polytheistic and polygamous races to be reared, and the parental ideas would not appear but the child will grow into the ideas of the strange people. In deaf and dumb children no inherited ideas have been discovered. In the changes of fashions which occur frequently there may be quite as great an emotional shock over the discovery by a society dame that her gown is out of style or order, perhaps a much greater one, than over the thought that her morals are askew. Ideas of fashion cannot be hereditary. Emotional discharges there follow common channels for a variety of stimuli, as in other cases.



If, then, it is true that ideas are handed down only by social process but not by heredity, it would at once appear that the emotion and idea cannot be connected in hereditary transmission. That is to say, the emotion has no special label before the fact in connection with it. The emotion is free to connect itself with any appropriate idea or situation which is presented. Theological beliefs, for instance, will awake strong feeling according as they arouse a situation of danger to the individual or as they are made to his mind to seem to be essential to his existence. Why should the average Christian feel shocked to hear the doctrine of the godship of Jesus attacked? Because he has been taught to think that Jesus as God stands between him and an awful torment hereafter. The whole position occurs as a terrible image upon entertaining a bare suggestion to the contrary. No one wants to lose his life. So with the other beliefs in order and proportion according as they have injurious situations connected with them to be rung up at order.

Instead of grading the great beliefs by rank according to age alone, therefore, I should classify them according to their ability to prove their clearness to men by their emotional coloring. The fundamental one is the belief in the right to live. The next in order is that of the right to means of subsistence. The third class is the genetic beliefs. Then come the religious, ethical, governmental, legal and scientific. This scheme might claim justification also on historical grounds. That is to say, the order has been that historically construed in a broad way. In some particular generation or locality or individual development, however, some other order may be the one.

From all that has gone before it may not seem strained to suggest that we now have a foundation for constructing an answer to our initial query, namely, why mathematical ideas, beliefs, are so remote from any liability to emotional coloring. When treating of mathematics I indicated the deductive nature of the science and how independent are the higher mathematics of ordinary affairs and of other sciences. Only the beginnings of the science were facts of sense-perception from which to make induction. Now this attitude of remoteness of mathematical truths, this aloofness from the vital affairs of society, precludes almost wholly the possibility of their bringing into mind the image, the situation of danger or otherwise. Let the truths be doubted or attacked ever so fiercely and there is no picture of a fiery furnace, or starvation, or a disrupted home or starving children arising from it. These truths rather stand outside the range of the fiery and burning interests of men. They seem to be mere instruments to use in connection with affairs,

like guns, or carriages, but there is nothing about them to fear or to laugh at, or languish over. As beliefs they never appear to be my personal beliefs.

This aloofness, this exclusiveness from the tug-of-war of heated, intense, vital matters explains, in my estimation, the lack of emotional accompaniment of mathematical truths. So, in the graded series of beliefs mentioned above, the mathematical beliefs should be placed after and above all the other beliefs when measured by emotional accompaniments. Thus they stand more distant from our warm personal interests, at least to the masses of the people, than do scientific tenets.

J. M. GILLETTE.

### REDREAMING DREAMS.

Several years ago I had a peculiar experience in dreams, in fact so peculiar that I made a note of it at the time.

More recently I had another, a similar experience, and I am now prompted to embody the whole in a short paper. I am led to do this partly because I see nothing like it reported in the literature within my reach, but chiefly on account of a concluding remark by Maudsley on 'Sleep and Dreaming' in his 'Pathology of the Mind'; he says, page 49: "Dreams have been a neglected study; nevertheless it is a study which is full of promise of abundant fruit when it shall be earnestly undertaken in a painstaking and methodical way by well-trained and competent observers. To physicians of all men it is likely that they will prove full of instruction."

Early in December, 1894, I became severely afflicted with an inflammation of the upper air passages, including pharyngitis and tonsillitis. There was considerable fever, my temperature rising to 103 degrees, a rapid pulse and loss of appetite. In the evening I became very restless and slept but little at night, being awakened at short intervals by, or with, disagreeable dreams. I occupied a warm and well-ventilated room at the time. The medical treatment during the day, as well as the day following, consisted in taking, with some food, ten to fifteen grains of salol every four to six hours, with enough codeine to allay the cough. I may add that I am six feet tall, strong and healthy, a little over thirty years old, and have at different times taken salol in similar doses but at longer intervals and always without experiencing any disagreeable effects. Moreover, I constantly dream while asleep, but a dream is seldom so vivid that I can recall it distinctly the next morning.

On the next, or second, day the local and constitutional symptoms were more marked, but in the evening I felt easier and lay comfortably dozing on the bed until 10 o'clock when I undressed and retired. Now my queer experience began: I had a short, vivid and coherent dream in which I was carried back to the days when I was an interne in a city hospital. I dreamed that the patients were not properly cared for in going to and coming from the general dining-room, there were too many decrepits who should have taken their meals in the wards, or even in bed, and who had great difficulty in following the procession up and down the stair-way. The scene and the characters stood out distinctly and vividly. The dream was very disagreeable; it excited and worried me, although it had no basis in facts, and besides, the feeding arrangements were matters with which I had nothing to do. I remained awake for some time thinking it over and the falsity of dreams in general, and then fell asleep again, but only to be awakened in a short time, disturbed and excited by the *same dream*. There was no variation in the scene nor in the faces. I was more excited and annoyed than before and tossed about until finally I was once more lost in sleep. In a few minutes I was again awakened by, or with, the same annoying dream. To make the story short: *I dreamed that same dream at least twenty times in succession*, and all in the course of about two hours and a half. It remains to say that every time I awoke the throat had to be cleared of the accumulated abnormal secretions and the gargle used. The secretions were certainly sufficient cause to account for my frequent awakenings independent of the dreams.

Shortly after midnight I got up to take a fifteen-grain dose of salol, with codeine. I was all out of humor and wondered if this sort of thing was going to continue all night—and a night never seemed longer. I remained awake, tossing about, for half an hour perhaps, and then became unconscious in sleep. In a short time, a few minutes apparently, I awakened: I had had a *new dream*. The scene was now shifted to the hospital where I was at the time (Long-cliff) and to a ward where the feeble and incapable are cared for. The subject was similar to that of the first dream, or rather, set of dreams: I saw that the patients were not properly fed and nursed. I was greatly distressed and excited, yet on thinking over the matter I saw how utterly false the dream was. Now, strange to say, I dreamed this also; I awoke at least twelve or fifteen times and was always annoyed by the same dream with only slight variations. Occasionally there was some incongruity in the scene, but the few charac-



ters always appeared distinctly. At 4 o'clock in the morning I took another ten grains of salol.

Although feeling very ill at the time, I was nevertheless much impressed by the peculiarity of this kind of dreaming and frequently consulted my watch to note the hours of the night, always wishing that daylight would come and put a stop to it. That the matter was not simply one grand dream, or illusion, if you wish to call it so, was evidenced by the fact of the gargling solution in the bottle being nearly consumed during the night. I must have gargled at least twenty times during the night judging by the amount of solution used. There still remained over two hours before dawn and I began to reason that if I must redream I might as well have a pleasant dream. I had frequently heard it said that a person could to some extent influence the character of his dreams by directing the thoughts to some subject previous to falling asleep, and I tried therefore to keep my thoughts on pleasant things, and finally dropped asleep. To be brief: *I had a third set of dreams.* This one was a pleasant one, or set, and I awoke six or eight times from it. After 7 o'clock I had a deep and refreshing, apparently dreamless, sleep lasting two hours. On getting up in the morning I was able to clearly recall the first and second sets of dreams, the sleepless intervals and the taking of the remedies, but the subject or nature of the third set had vanished; all that I could recall was that the dream was a pleasant one and did not worry me, and that there was some incoherency in the scene and characters. My temperature at this time had fallen to 100 degrees and I felt comparatively well, although rather weak. The following night was uneventful; the disease-symptoms had practically disappeared.

The above account is essentially as written at the time. I will now give my second experience, which occurred two years later, or, to be exact, on the early morning of February 3, 1896.

For about ten days previous to this date we had an epidemic of influenza; many of the patients and employees were coughing and sneezing and complaining in general. I too became affected and after trying various remedies without any decided good results, finally, on the evening of the 2d, took twenty grains of salol and one-fourth grain of codeine. I slept fairly well during the fore part of the night, but the nares being stopped up I was compelled to breathe through the mouth. Soon after midnight I had a very vivid dream. The subject was a puzzle, or game: A board measuring about 9 by 14 inches had two large holes in it lengthwise; above these was a row of about ten

or twelve small holes for pegs, arranged in the form of a crescent. In the lower left-hand corner were two additional peg holes. This part of my dream was very distinct. The *modus operandi*, as I remember it, was like this: A sentence, a quotation for instance, was to be thought of and the chief words, or the subject and predicate, were to be written on a slip of paper and placed back of the large openings. A peg to indicate the affirmative or negative of the sentence is inserted in one of the other lower corner holes, and a number of pegs equalling the remaining words placed in the crescentic row. The board thus prepared—fitted out with the two chief words, an affirmative or negative peg, and a peg for each other word—is held up to full view for a second or two, the company or guessers grasp as much as possible and then begin to construct the sentence. The board is then passed to the person giving the first correct answer.

The subject was a fascinating one and made a deep impression on me. A few days before this I had read of a man who 'got up' puzzles, or games, and who often received several hundred dollars for a new one. In my feverish condition visions of hundred dollar bills floated through my brain, and this, perhaps, had something to do with the redreaming. I dreamed it over and over and each time it had a charm. The next morning, however, it did not look so promising; the idea, though, was new. Soon the thought of having taken salol came to mind and I recalled my experience of two years before, as related above, and in the light of it I wrote out the present account.

I have briefly related the bare facts and shall not attempt to draw any conclusions. It seems to me, however, that the large and frequent doses of salol, over two drams in the first case, may have stood in a causative relation to the dreams, that is, to the redreaming.

Ordinarily we seldom give a second thought to a dream—"It is only a dream" we are apt to say. In the present case it is not the dreams themselves that are peculiar; the peculiarity consists in the *redreaming*.

ROBERT HESSLER, A.M., M.D.

## PSYCHOLOGICAL LITERATURE.

*La suggestibilité.* Par ALFRED BINET. (Bibliothèque de pédagogie et de psychologie, III.) Paris, Schleicher Frères. 1900. Crown 8vo. Pp. 394.

The aim of this book is to present the facts of normal suggestibility without reference to hypnotism or pathological conditions. The author has set himself these two problems: Is it possible to determine the individual suggestibility without having recourse to hypnotism? And, what are the relative merits of the tests proposed? The first eighty pages are devoted to an historical review of the subject, and the remainder of the book consists of original reports upon researches made personally by the author upon school children in Paris.

The author distinguishes the following classes of suggestion: (1) Obedience to the moral influence of another. This is the technical sense of the term. (2) The tendency to imitate. This may combine with the first but may also appear without it. (3) The influence of a preconceived idea which checks the critical sense. (4) Expectant attention, or unconscious errors of an ill-regulated imagination. (5) Subconscious phenomena produced during a state of distraction, or following an event which has created a division of consciousness, as in the case of automatic actions. He rejects the current loose uses of the term, such as to denote "the idea passing into action." In the narrow sense suggestion is a moral pressure which tends to produce an automatic state of consciousness. It may be effected through fear, love, charm, intimidation, respect, admiration, etc. This automatic state is analogous to the state of *râpport* in hypnosis.

Experiments are reported upon the following topics: the directing idea; moral influence (action); the question; imitation; and, subconscious movements. An individual study was made of each child by both objective and subjective methods. The children varied in age from seven to fourteen and the experiments were made in the office of the principal of the school with apparently no more disturbance than an examination in arithmetic or geography would have created. The report is dramatic and carries the reader through a lifelike experience face to face with the child. The tests may be mentioned *seriatim*.



*The Directing Idea.*—Here an attempt is made to eliminate the moral, personal influence and to develop autosuggestions. The tests are planned to throw light upon the trustworthiness of observation and memory under the influence of prejudice, *parti pris*, tradition, controversy, old fogysm, etc. Ordinarily the course of our daily life is determined by the presence of certain guiding ideas. Here concrete life is reduced to experimental conditions, and the origin, the course, and the efficiency of one idea is studied. There are several tests in the series. First, the child was required to mark off, on cross section paper, the length of lines presented by the experimenter. The lines were shown in the following order, the length being given in millimeters: 12, 24, 36, 48, 60, 60, 72, 72, 84, 84, 96, and 96. The order of the first five lines invokes the idea that there is a systematic order of increase in the series; after the fifth, the alternate lines are catch lines. The directing idea led first to the assigning of an increment to each of the catch lines, and consequently to a corresponding overestimation of the last lines in the series. On the basis of the records a 'coefficient of suggestibility' is computed for each child. These coefficients vary from 7 to 129.

These results suggested another test, namely, to what extent can the directing idea increase the apparent length of a line for each individual child? The children were asked to mark off the length of lines as before. This time thirty-six lines were shown. The first five were the same as before but the last thirty-one were all of the same length, 60 mm. This is a superior test. The order of the first five lines established the directing idea, and the manner of lengthening the following lines furnishes a measure of the suggestibility under these definite conditions. Eight out of the forty-one pupils continued to increase the length of the lines up to the end. One automaton made the last line 304 mm. long. The force of the directing idea is enormous. In general, there is a pretty close agreement between the individual coefficients in the two tests. By very ingenious questioning each child was led to give his introspective interpretation of the process. The majority were aware that they had made the lines too long, but when asked to mark the supposed errors, they corrected the overestimation only in small part. Many different, true reasons were given for the committing of the error. The introspection throws some light upon the mechanism of the directing idea, and the author is convinced that it is a legitimate and profitable method to employ in the study of children. He also thinks that tests of this kind are very helpful to the child, who thereby learns to form correct habits of observation and

action. This test was repeated upon twelve pupils of a higher grade in the school, and the results show that the older children were less suggestible than the younger.

A third test was made to determine whether the suggestibility found in one sense will be the same for another, *i. e.*, whether suggestibility is a general process of the mind. The test was made on discrimination for weight. The apparatus consisted of fifteen small boxes of uniform size. The first five weighed 20, 40, 60, 80, and 100 grams respectively, and the rest were all the same as the fifth. Three methods were employed in turn. First, the child lifted the boxes in order and stated whether the one lifted was heavier or lighter than the preceding, or equal to it in weight. Second, he lifted as before but verified for each step by repeated lifting of the two boxes compared. Third, the experimenter stated that the first box weighed 15 grams and the pupil then estimated the relative weight of the other fourteen, in grams. All three methods bring out a pronounced suggestibility. A few pupils are refractory; as many accept the suggestion and become complete automata; and the rest are distributed in various degrees between the two extremes. There is an approximate agreement between the individual coefficient of suggestibility for weight and that for the length of lines. This means that suggestibility in one sense experience implies suggestibility through the other senses as well. There are, however, differences in these records which indicate that an individual may be more suggestible through one sense than through an other. There are, therefore, types of suggestibility, just as there are distinct types of mental imagery. While we are furnished evidences of the mechanical action of the mind, the same phenomena reflect the almost infinite complexity and variability of the nexus which constitutes each child's characteristic reaction to the environment.

A very pretty illustration of law in illusion occurs in this test. The figures show that the sixth box is judged lighter than the foregoing more frequently than a similar judgment is given for any of the other boxes. Chance, one may think. No, this fact could have been foretold, because according to the law of negative illusion the greatest disappointment of expectation would occur at this point. Of course it must be taken into account that we have here a conflict between the negative and the positive suggestion.

The tests were repeated upon older pupils and it appeared, as in the case of the linear illusion, that the older children were less suggestible. The author remarks that suggestibility varies more truly with intelligence than with age. The reviewer has on hand data to demon-

strate that this variation with age and intelligence cannot be stated as a general law. There are certain classes of suggestion which diminish in force with increasing intelligence, as in the cases before us, but there are others which do not diminish, and there is a third class of suggestions which gain force by virtue of the intelligence. There is a grain of truth in Berillon's paradoxical thesis, that suggestibility is synonymous with educability.

Professor Binet employed the introspective method to excellent advantage in repeating this test upon his two daughters, twelve and thirteen years of age respectively. He is never content with mere figures. Throughout the work his aim is less the measurement of the extent of suggestibility than the more important study of its nature.

*Moral Influence.*—In the foregoing experiments the personal element was eliminated as far as possible; here it is the object of study. What this series of tests lacks in exactness and positive control, as compared with the foregoing, is here made up in lifelikeness and practical applicability. How do we influence each other with our words, our inflections, our gestures, our logic, our prestige, our authority? Two lines of experiment are pursued: The first is a test of docility as shown in the accepting or rejecting of an affirmation; the second is an experimental analysis of the personal influence through questions.

The affirmations are of two kinds, contradictory and directing. The former is taken up first. The experimenter asked the child to name each of the spectral colors shown to him. Then a series of nine colors, some pure and some mixed, were shown and as soon as the child named the color the experimenter contradicted him and said, No, it is so and so. The children were more refractory in this test than in any other. A much better test was made on the same point with affirmations about the length of a line. This is superior to the other test because the contradiction is not so offensive and it is more exact. A series of twenty-four lines, varying in length from 12 mm. to 104 mm. in an arithmetical ratio, was placed before the child. A standard line was then shown and the pupil selected the one that matched it in the series. Immediately the experimenter interfered with: Are you sure? Is it not this (pointing to the next line above)? Most of the children accepted the suggestion.

In the next series a directing suggestion was given before showing the line. The child was asked to mark off the length of a line 60 mm. long, shown to him. Presenting a second line of the same length, the experimenter said, "Here is a longer line." Then present-



ing a third line, he said, "And here is a shorter line." In this way he alternated the suggestion for a number of equal lines. Most of the children accepted the suggestion. The author does not take adequate account of the negative influence. The suggestion he gave was at once a motive for two opposing illusions. If the bright child actually believed that the third line should appear shorter than the second when seen, this third line ought by virtue of the definite expectancy to appear longer instead of shorter. It is not sufficient to say that those who do not accept the positive suggestion are refractory. The records represent the resultant of opposing influences and may be predominatingly positive or negative. When the two forces neutralize each other we have true perception, as far as these elements are concerned.

*The Question.*—How can a judge influence the witness by his questions? He may allow the witness to exercise complete spontaneity; he may force questions without biasing; he may make leading suggestions to the witness by the questions; and finally he may force extreme suggestions by questions. Here is a vast field for investigation. The author reduced these cases to experimental conditions for children and tried to work out the mechanism of the personal influence of the question upon a child.

A sou, a button, a stamp, a label, a photograph, and a small picture of a crowd were pasted upon a large card. The first experiment indicates the second attitude of the judge, as stated; it is the case of forced memory. The experimenter showed the object card twelve seconds and then, as the child reported what he had seen, he asked forty-one questions about actual details in the objects. The results show that a full and concise report may be made, and yet be false, *e. g.*, when a child sketches the cancelling letters R I S on a stamp that has not been cancelled at all. The great lesson from this excellent test on observation and memory is that definiteness and completeness of detail are not incompatible with falseness. Reliability is not correlated with ability to recall. There is a characteristic specialization and concreteness in the errors that is traceable to the questions. The errors are of two types: namely, logical errors, *e. g.*, having stated that the button is sewed on (it is pasted on), the logical error of assigning a color to the thread follows; and, errors of invention, *e. g.*, when some fancied object is described instead of the real. The pupils who were most suggestible in the former tests made the greatest errors.

The first case of the judge was imitated by repeating the same test

upon other children without asking any questions. The children simply wrote the answers as fully as they could. Their reports were less complete and numerous errors were made. The author concludes that if you desire faithful testimony from a child you must not ask him any questions, nor allow him to make oral report, but require him to write spontaneously what he knows. This is especially true about children because they are less capable than adults to distinguish between fact and fiction. But how many adults are not grown up children in this respect!

Following this, three tests were made by the use of written questionnaires. The same objects were shown as before. A number of children were divided into three groups and each group received a different set of questions. The first was intended to force memory, *e. g.*, "How is the button fastened?" The second gave a moderate suggestion about each object, *e. g.*, "Is the button fastened with a thread?" The third gave a strong suggestion, *e. g.*, "What is the color of the thread which passes through the holes of the button and fixes it to the card?" Thus, the last three cases of the judge are produced. Each set of questions brought out characteristic results. We are presented with a classification of these little witnesses with reference to the truthfulness of their testimony. The majority accepted the suggestions and wrote as if the memory images had been true and spontaneous. This feeling of spontaneity is an exceedingly important factor which was well brought out in the introspective reports. The tests were repeated upon some normal-school students. These adults made the same kind of errors as the children had committed, but not so gross. These students were undoubtedly more reliable observers than men ordinarily depended upon in courts.

*Imitation.*—The same object card was used as before. Children were called into the principal's office three at a time. One in each group was appointed president-secretary, whose duty it was to read the questions and write all the replies. A definite list of questions about the objects was furnished, and as soon as a question was read the three competed to see which one could answer first. The order in which the three answers came was recorded. Naturally there appeared a leader in each group, and the test consisted in determining to what extent the other two followed this leader. The results are astonishing and lend reality to some of the strange accounts of mob action. This introduces the psychology of contagion. The group is more suggestible than the individual. Each group, though formed by chance, has a group character or type. The imitation is very pro-

nounced. But the suggestion is more powerful than the tendency to imitate; many of those who succumbed to suggestion were not imitators of their comrades. On the other hand, a few imitate in resisting the suggestion. The children were regrouped and a similar test was made, with the result that each individual adapted himself to the new group and the laws determined in the other test were corroborated.

*Subconscious Movements.*—An 'autoscope,' to use Professor B. F. Barrett's term, was made by mounting a hammer so that it would swing up and down gently, supported by a spring. The child held the mallet head in the hand behind a screen and was required to look at a metronome and count the beats. The experimenter 'primed' the movement by causing the hammer to swing in time with the metronome, about ten times. The test consisted in determining to what extent the child would continue the movement automatically after having been thus started. A classification of the children in regard to the definiteness and duration of these automatic movements is given. A second test was made in which a pencil was used in place of the hammer. The experimenter communicated simple writing movements and these were often continued automatically. The children were generally conscious of the movement. These tests demonstrate the possibility of making quick and significant tests on automatism under normal conditions. They also show that automatism of action is not synonymous with automatism of judgment.

This bare description of the tests conveys no adequate idea of the author's ingenuity, erudition, tact and fairness in the manipulation of an experiment and the interpretation of its results. He is a worthy leader in this field of research. He has proved more than he set out to prove. Not only has he demonstrated a relation between normal suggestion and hypnosis, and between normal automatism and the spiritualistic phenomena of automatism by ready methods, but he has contributed much to our knowledge of the child mind and the theory of applied psychology. Instead of injuring the children he claims to have benefited them by explaining the errors to which they were subject and leading them to more accurate habits.

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*Psychologie des Willens, zur Grundlegung der Ethik.* Von HERMANN SCHWARTZ, Privatdozent an der Universität Halle. Leipzig, Wilhelm Engelmann. 1900.

The work before us is an interesting and in many ways original contribution to the theories of value which have grown up out of psy-



chological studies of recent years. Though in method somewhat akin to the Austrian school, it opposes to the empiricism and relativism of this school a doctrine of absolute norms to which the author himself gives the name of 'Voluntaristische Apriorismus.' By this title he would distinguish it on the one hand from the rationalism of Kant and the intuitionists, and, on the other, from the naturalistic and heteronomous theories of the origin and development of values which characterize the empirical point of view.

The phenomenology of values, as disclosed by the psychological analysis of Schwartz, may be summed up thus: Values fall into three distinct and ultimate classes.

(a) Values of organic condition (Zustand), mere feelings and, as such, objects of mere desire; (b) Values of the person, such as admirable qualities of the person or character, beauty, fame, valor, etc. (c) Impersonal values, social, artistic, scientific, in short all altruistic values.

These last two classes are distinguished from the first in that they are objects of outgoing will activity in the true sense, not of desire. Within each of these spheres of value, preference is determined merely by judgment of like and dislike. This is a purely analytical preference, determined by the degrees of satisfaction (saturation of the volitional tendencies). But the choice of one group rather than another follows a normative law of synthetical preference. There is in both individual and racial culture a tendency to development of motives from the lower to the higher classes, but while the origin of motives may be explained by principles of natural causality the choice of one class of motives over another is explicable in both individual and historical culture only by a synthetical principle inherent in the will. Moreover, the inherent necessity of reason to explain acts of will by sanctioning grounds found in the sphere of the representations which the volition brings in its train, shows this *a priori* principle of preference. In this connection the instinctive lies of consciousness, flattering by false reasons our higher sense of values, are instructive as showing the existence of the norms of personal and impersonal values. These phenomena of ultimate distinctions in values, of semi-logical development of motives in individual and social culture, of intuitive recognition of grades of value by the sanctioning consciousness, all point to synthetic *a priori* acts of will back of individualistic development.

Side by side with this positive programme, the psychological grounds for which constitute the larger portion of the work, Schwartz

carries on a running fire of criticism against all heteronomous conceptions of will-determination. With special energy he maintains that the doctrines that the will is determined by such principles as the 'Law of Relative Increase of Pleasure' (Ehrenfels), the 'Will to Life or to Power' (Schopenhauer and Nietzsche), or the logical principle of 'Universalization of Motive' (Kant), are dealing with mere abstractions, not real forces of the will. In accord with his conception of the primacy of will as the creator of values, which is the keynote to his position, the ground of will-determination in any real sense must be a 'Gesetzlichkeit der inneren Normen.'

The absolute dualism which his theory discloses between the 'Natur-zwang' of the merely natural laws of desire, as like and dislike, and the 'Norm-zwang' of synthetic preference of the will opens a way for a doctrine of freedom which rests ultimately upon the old Kantian principle of the indifference of norms and of nature to each other.

This is in briefest outline the obvious part of our author's system. Our first astonished question is: Whence these ultimate categories of value, whence our knowledge of this 'Gesetzlichkeit der inneren Normen,' which is active back of the naturalistic changes and development of motives? It would be easy to grow indignant over this bold and unmitigated dualism in method. Having shown such zeal against the dogmatism of Kant's intellectualistic ethics, of the naturalistic Trieblehre of Schopenhauer and Nietzsche, and of the pain-pleasure illusion and prejudice of the empiricists, has he presented us with the last consummate illustration of the assertatory method? We read on and on. Surely there is some logical deduction of these categories yet to come. Kant's ethical *a priori* may be but a pale spectre from the shadows of the pure reason, but at least we know its source. We know how to estimate its credentials. But no—a brilliant *argumentum ad hominem* tells us to go to our own will. Does it not choose personal over feeling value, impersonal over personal? And when, in its weakness, it fails thus to choose, does it not sophisticate itself with those lies of conscience which are the tributes paid by vice to virtue? We find nothing but a reconstruction of that table of values, attractive but powerless, which Martineau develops for us in his Idiopsychological Ethics.

Idiopsychological is Schwartz's position throughout, and by his own statement he has much in common with Martineau. And with such reflections and with this classification many readers will forthwith dismiss the book. Such a procedure would, however, not be wholly

just, for unpromising though the result may be there is yet back of it a psychological point of view which is worth notice for its own sake. In his preface we are assured by the author that an adequate psychology of the will is a prerequisite of a true theory of ethical values—a view which we can scarcely doubt, despite the strong reactionary movement in which we have been caught up. But immediately we are brought face to face with the distressing uncertainty prevalent as to true psychological method. Against the empirical method and its concept of the will as a complex of elements, we have in Schwartz the strongest insistence upon the will as a special function of the soul, with its own quasi-logical functions, independent of ideal content. Moral distinctions are neither of feeling nor of thought, but are idiopsychological and idiogenetic.

“Von dieser Antwort,” says Schwartz, “hängt für die sittliche Betrachtung unendlich viel ab.” Without doubt. If, as Schwartz’s analysis would have us believe, the function of desire is merely that of a preparatory process to furnish the material for the analytical and synthetical functions of the will, “reflexionsartige” (judgmental) processes, then indeed the real grounds of the will are not to be found in the empirical doctrine of motives, in representation and feeling, but in the will itself, and the entire basis for a causal method is gone. Naturally, such being the conception of will, the causal and empirical method of psychology is supplanted by a negative analysis, the purpose of which is to show the independence of will of motivation from without.

We find, therefore, a new doctrine of value and a new conception of volitional sufficiency. The so-called motives of the will, representations and feelings, are not of value because they are objects of desire or will, but *become of value* first by reason of the satisfaction (saturation) which they bring to the impulse of will which brings them into being. Even elemental will impulses—and this is the key to the situation—are conceived of as objectless. But these objectless ‘Willensregungen,’ by a law of concentration of ideas, bring into being a crowd of representations among which analytical and synthetical preference is exercised, on the basis of the satisfaction (saturation) of the impulse. ‘Nicht durch das Gefallen oder den Wunsch selber sind wir uns eines Zieles bewusst, sondern wir werden es durch eine Vorstellung die zu ihm hinzutritt und sich nachträglich bildet.’ The values or ends of volition may be interpreted or known in terms of representations, but the values consist not in objects of impulse or will, but in the satisfaction of the will that calls them into being.



This novel conception of satisfaction of the will, through saturation of the will impulse, by the more or less of content, the presence or absence of the same, in so-called analytical preferences, within the same sphere of values, and the reaffirmation of the doctrine of practical evidence in the sphere of synthetical preferences between different classes of values, is one, it is to be feared, with which scientific method can do nothing. From the point of view of science the values of the will can be known only in one of two ways: either by the ideal products of the will reconstructed logically or causally, or by the subjective states which are the functions of will activity in consciousness, these to be reconstructed either in terms of degrees of pleasure and pain or perhaps in semi-logical terms of extension and intension of sentiment—from the reviewer's point of view, the latter.

With the introduction of the conception of a purely practical evidence of the will, abstractly conceived, the distinction of Crusius between real grounds and knowledge grounds of the will is carried to its extremist conclusion. But how are we to know the real grounds of the will if they are not to be found in the phenomenal motives? If we do not reconstruct them out of the phenomenal motives, have we not to choose between a groundless will or an arbitrary metaphysical theory of the will? And if the values of the will are to be reconstructed out of phenomenal motives, how shall we escape the logical or causal methods?

Space will not permit of a detailed criticism of the errors of analysis involved in the statement that volition is not consciousness of ends, the most far-reaching of which is the failure to distinguish between different degrees of consciousness of ends. We must confine our notice to some of the logical consequences of the general position which make it so out of harmony with the prevalent theories of value. Having abandoned the basis of the 'continuity' theories of value which define value as that which is desired, and therefore all modifications of value as development of desire, Schwartz's classes of value lack all genetic, phenomenal relationship to each other.

To make an absolute distinction between Values of Condition, Personal Values, and Impersonal Values, he is compelled to use the conception of feeling, '*Gefühlsdisposition*,' in an arbitrarily narrow sense, to confine it to the grossly organic conditions of the organism with the strong consciousness of pleasure and pain. These are passive states, *desired*, while the personal values, such as power, fame, beauty, courage (the ethical sentiments in general), are objects of an active effort, *streben*, the value of which is determined by the act of will itself.

The author here betrays an unusually superficial acquaintance both with these sentiments themselves and with the psychology of the sentiments when he ignores entirely the prevalent and probable explanation of their relative independence of subjective conditions, by processes of abstraction, and by the objectification of the social consciousness. And here it may be remarked that a neglect of the social factor in the creation of values is characteristic of the work.

As a consequence of this absolute dualism, an entire group of sentiments, wonder, reverence, sympathy, which represent a more objective reference of consciousness, and yet do not fall into the personal and impersonal group, are described as neutral, and denied all further participation in the scheme of values, although manifestly occupying a transition stage between the lower feelings and the higher sentiments of personal and impersonal value. These values of consciousness are, once and for all, states of consciousness, and a psychological method which shall explain them must be genetic and empirical. It may not be possible to explain the degrees of value and the development of values by the law of relative increase of pleasure, which Schwartz insists is no 'real force,' nor even perhaps by the quasi-logical conception of greatest possible extent and intension of sentiments (Guyau, Krüger). Nevertheless, it is no help to our psychology of values to tread the abandoned paths of the old faculty psychology, to assume 'real forces' for every relative distinction of value we may make in the content of consciousness. The Kingdom of Science is not to be taken by storm.

As for the general methodological principles which underlie the entire work, we find them leading to a result which puts the science of ethics in the precarious position of choosing between the alternative of a groundless will, or at least one whose real grounds cannot be known, or of a transcendentalism, whose categories not only bring no deduction with them to certify their truth, but whose limits, even, are not clearly defined. There seems to be in the present state of our thinking two methods of value determination with which every thinker should set himself to rights. Both are monistic—and admit of no discontinuity of value. Genetic naturalism conceives of all higher values as expansions and modifications of the lower values of 'condition' determined by the laws of the social consciousness. The idealistic method conceives even the values of condition as well as personal and over-individual values as capable of systematization under the concept of an ideal self. In his absolute dualism between the lower and the higher values Schwartz has run counter to the dearest hopes of both schools of thought, and his work will probably be judged accordingly.

*Atlas of the Nervous System, including an Epitome of the Anatomy, Pathology and Treatment.* CHRISTFRIED JACOB. With a preface by Prof. Dr. AD. v. STRÜMPPELL. Authorized translation from the second revised German edition. Edited by EDWARD D. FISHER, M.D. With 112 colored lithographic figures and 139 other illustrations, many of them in colors. Philadelphia and London, W. B. Saunders & Co. 8vo. Pp. xix + 218.

This book is in small octavo form and appears as one of Saunders' Medical Hand Atlases, being the eleventh volume in that series. As will be gathered from the title, its scope is comprehensive and it is planned to give, from the standpoint of anatomy, pathology and treatment, a condensed presentation of the main features of the nervous system. Fully two thirds of the volume are given to the plates and their explanation, while the remaining third is text—which covers 202 pages. The text is divided into six parts, as follows: Part I., Morphology; Part II., Development and Structure; Part III., Anatomy and Physiology; Part IV., General Pathology and Treatment; Part V., Special Pathology and Treatment; Part VI., General Remarks.

The first three parts deal almost entirely with the gross normal appearances of the nervous system. All together, however, they occupy only 75 pages out of the total 202. On the other hand, they are accompanied by fifty-seven out of the eighty-four plates which the volume contains. This analysis serves to show the distribution of emphasis within the book.

We shall here examine only those parts which deal with the normal structure, since it is this side of the book which is nearest to the readers of this REVIEW. Beginning with the illustrations, we find that the plates which go with Part I.—on the morphology—are excellent, representing in the colors of the fresh state different aspects and sectional surfaces of the entire encephalon. For the most part the figures are somewhat less than natural size, but, nevertheless, they are both clear and artistic. On passing to the plates which go with Part II.—on the development and structure—it is first necessary to correct a fault of correlation. Among the plates, 15–26 inclusive are given to Part II., whereas in the text, Plates 15–53 are credited to this part. This contradiction is a matter of slight importance, except for purposes of reference. The following statements apply to Plates 15–26 only: These illustrations, which picture the developing nervous system and some histological preparations from the adult, are rather poor work and the coloring in many cases crude. Moreover, in some instances they are both inaccurate and misleading. Take for example Plate 19, showing



the cerebral cortex. By way of introduction it should be said that this is a new plate and one immeasurably better than that printed in the first German edition of this book. There is, however, no hint in the explanation that this figure is schematic in a high degree, although in it the bodies of the cortical nerve-cells are enlarged very much more than is the thickness of the entire cortex and hence they appear in the figure densely packed together.

If one can compare this figure with actual sections or with the plates in Hammarberg's 'Studien,' the misleading character of it will at once appear. That the human cortex was never intended for representation on an octavo page is evident the moment we attempt to make an accurate picture of it on that scale, for in order to reproduce the real relations of the elements, all save the giant cell bodies must be so reduced in size that their shape is practically lost. Nevertheless, the method of enlarging the cell bodies out of proportion to the enlargement of the thickness of the cortex—as here done—is not the way out of the difficulty, for it obscures a feature of the cortex which is fundamental, and with this picture in mind it would be well-nigh impossible for a student to appreciate, namely, that increasing quantities of white matter between the cell bodies are characteristic both for the higher state of development in the individual and for those species which stand highest in the mammalian series.

Again, in Plate 22 the nucleus of the descending, so-called nasal, root of the fifth cranial nerve is pictured in green—the color used for the *nuclei terminales* of the afferent neurones—although it is correctly described in the text as a motor nucleus, and should therefore be blue. In this connection it may be noted that green and blue are the colors used to distinguish the two sorts of cell-groups shown in this Fig. 1, illustrating the nuclei of the cranial nerves. The colors are poorly chosen, for they are hard to distinguish even in daylight, and under artificial illumination become quite indistinguishable. On this same plate a fiber with a node of Ranvier is shown in such a way—when taken in connection with the explanation—as to leave the impression that these nodes are general characters of the fibers within the central system. This, of course, is not the case.

In Part III., which in the grouping of the plates (27-53) is designated topographic anatomy of the nervous system, we have an important series of illustrations, mainly new, although among them and the four next following plates are some copies of the excellent series of wall charts for which we are indebted to Jacob and Strümpell. Plates 28-30 inclusive, to which attention is particularly called, represent—

somewhat reduced from natural size—a series of sections through the human encephalon, reproducing in color preparations stained by Weigert's hæmatoxylin method. The lithographic work, though somewhat coarse in the printing, is perfectly clear, and the series, which extends from the frontal end of the third to the middle of the fourth ventricle, shows the important structures in this region of the encephalon, a region which it is most difficult for the student to comprehend without aid of this sort.

If one does not have access to Nebelthau's, "Gehirndurchschnitte" or some similar work of the more expensive kind, these plates are certainly worth owning.

In this review the illustrations have been first discussed, since we are dealing with an atlas, in which the text is subordinate to the plates. In the examination of the text, to which we now turn, it will be only proper to keep separate the responsibilities of the author and the translator.

As a rule the translator has produced readable English, but in Parts I. and II. he has often been very careless in his statements of facts and sometimes has missed bodily the meaning of the German terms and constructions. A few selected instances will show what is here meant. For example, in describing the lateral choroid plexuses, toward the bottom of page 9, Part I., the last sentence reads: "Here they (the lateral choroid plexuses) are closely applied to the lateral border of the optic thalamus and passing backward finally enter the descending horn where they become continuous with the pia mater *which enters through the foramen of Monro.*" The italics are our own. The statement is manifestly incorrect. On turning to the German—first edition—it appears that "Unterhornschlitz" is the word which has been rendered *foramen of Monro*.

This sort of thing should not be allowed to pass, for certainly the student of neurology will have trouble enough without mixing mis-translations with his ventricles.

Again, in Part II., middle of p. 25, there is this sentence: "Another portion of the epithelial cells in the neural canal—the neuroblasts—develops into germ cells, which send out first a long, thick process, and later numerous smaller arborizations from which the ganglion cells and nerve fibers are formed."

The erroneous derivation of the germ cells from neuroblasts appears in the German text, but the derivation of ganglion cells and nerve fibers from the cell branches is due to a misreading of the German text, where it is stated that the ganglion cells and nerve fibers are derived from the germ cells—a statement much nearer the truth.

To Part II. belongs the description of Plate 22. We have already said a word concerning some of the figures appearing in this plate; but the description itself calls for remark as it offers several examples of carelessness which should be noted. The following statement is made:

"The motor cranial nerves originate in the blue nuclei; the sensory end in the green nuclei. The cells of the green nuclei form the beginning of the sensory neuron." The trouble is with the last sentence, which at the end should read '*central* sensory neurons,' as it does in the German.

In the part of this same description which relates to Fig. 2 the last portion of the paragraph is particularly choice. It runs as follows: "Each nerve-fiber contains as its principal constituent the axis-cylinder, which is surrounded by a medullary sheath of variable thickness, the white medullary sheath divided into segments by the nodes of Ranvier. The peripheral nerves are provided with an additional layer of extreme tenuity, the sheath of Schwann, *situated within the medullary sheath.*"

The German text merely states that the medullary sheath is divided into segments, meaning the segments formed by the Schmidt-Lautermann clefts and the translator has put in 'by the nodes of Ranvier' on his own responsibility. As the description here applies to the medullated fibers of the central system the general statement is contrary to fact. The next assertion, that the sheath of Schwann—the neurilemma—is situated *within* the medullary sheath, is the outcome of missing entirely the construction of the original which states that the neurilemma is found within the limits of the peripheral nervous system—as contrasted with the central.

Part III., Anatomy and Physiology of the More Important Nervous Pathways, is quite free from this sort of defect which we have just been criticizing; it is a very good summary of the more important pathways in outline and from internal evidence one would say that it was not the work of the same person who had translated Parts I. and II.

If we turn now to the original text of Jakob it appears least satisfactory in those parts which relate to histology, to development and to those portions of the system the comprehension of which depends on a knowledge of the development. The statement concerning the choroid plexus and ventricles are in several cases inaccurate and obscure. For example it is stated on p. 9 of the translation, that the third ventricle has no proper roof—the *velum interpositum* serving that purpose. Oddly enough, on the opposite page (p. 8) there is a cut showing the proper roof of the third ventricle very clearly.



In general, it would be very advantageous if the older nomenclature here used could be replaced by the [BNA] and if the more acceptable form neurone were used in place of neuron. As we are not prepared to discuss the parts relating to pathology and treatment we shall leave that portion of the volume entirely untouched.

If called on for a summary statement concerning the portion of the book we have here been examining, we should say as the curate did of his egg, that parts of it were very good, adding the hope that the text may some day be made as satisfactory as the best of the illustrations.

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*The Human Nature Club.* An Introduction to the Study of Mental Life. By EDWARD THORNDIKE, Ph.D. Longmans, Green & Co. Pp. 235.

It is a significant fact that a number of the books on psychology which have recently been published in this country have been prepared with a view to attracting the popular, rather than strictly scientific, attention. The objections so often urged against attempts to popularize psychology seem to be growing weaker in the face of a strong demand for books of this kind. Certainly, the difficulties of presenting the science in a simple form are being much more successfully met in recent publications than they were in the earlier efforts in this direction.

As a vivid popular presentation of the subject matter of psychology, in untechnical language, and with a rich fund of illustration, Professor Thorndike's book fills very acceptably what must be recognized as an urgent general demand. The dialogue style has been adopted throughout the book. A company of persons organize themselves into an informal club for the observation and discussion of ordinary experiences. Through their own ingenuity and with a very moderate amount of assistance from books and from physicians and others in the neighborhood, they manage to cover with a good deal of completeness the topics appropriate to a text-book on psychology. Their language is very free and easy, seldom rising above purely colloquial terminology, intelligible to every reader. Their illustrations are gathered from their everyday experiences, and from the accounts which their friends gave them of similar common-place occurrences.

Much of the club's psychology will be recognized at once as orthodox from the point of view of Professor James' teachings, to which Professor Thorndike acknowledges his large indebtedness. Contri-

butions drawn from the author's own experiments on animals and on the nature of practice are also largely incorporated. When this general description of the subject matter is given, it is perhaps unnecessary to enter into any details.

Needless to say, there are conclusions reached by the club which invite criticism. But even the most critical reader of these conclusions will doubtless recognize that the work is introductory and not exhaustive, that it aims to inspire the reader to observation and study rather than to give a finished system of psychology.

There is one general question, however, which it is certainly worth while for us to consider in examining this book. What Professor James has called the "biological concept" and has definitely stated to be only a partial mode of psychological discussion has been accepted with such whole-souled assurance by Professor Thorndike that one is forced to ask whether it would not be better to treat consciousness and brain activity with more differentiation in a popular book even at the cost of introducing certain difficulties in terminology or discussion. Is it not true that a great deal of the crude psychology of the day is due to the adoption of certain physiological phrases and apparently simple explanations by those who are utterly incompetent to discuss the real relations between mental life and its physiological conditions? And is there not danger that the trained psychologist will foster this mode of thought by giving in popularized form, and without a statement of their limitations, these physiological explanations which he himself recognizes to be at best incomplete hypotheses and suggestions of what the true relations may be? Take for example one case, perhaps the most extreme case in the book before us, of direct transition from consciousness to its physiological basis. "You're quite right," says one of the members of the club on page 62, "the mind, the brain, is, of course, a tremendously complex affair, and not all of it is at work at once in any single situation." The question is not so much whether the author holds in his own psychology that mind and brain belong together in this way, as it is the question as to whether it is safe to establish in the thoughts of his general readers this easy transition from the one term to the other. For even if the transition is ultimately to be made in this way, certainly the untrained reader should be prepared for the intelligent adoption of this general attitude which is by no means the naïve attitude. If, on the other hand, the psychologist has permitted himself the phrase as a mere tentative mode of expression with the intention of discussing at some later opportunity its serious implications, or

with the intention of modifying it later for some other less easily mastered mode of expression and thought, then he should remember that in the meantime his students may be swelling the number of those who speak or write very unintelligently on psychological problems from the crudest kind of a physiological point of view. The only way to guard against the crudeness which we all deprecate in much of our popular psychology and in much of our present-day pedagogy, is to see to it that every word uttered or written by psychologists shall keep clear the distinction between mental and physiological phenomena.

Professor Thorndike's work is an admirable introduction to observational psychology because of the excellent illustrations which he has sought out and contributed in such large numbers. When it comes to popularizing the explanatory phases of the science and giving it to his audience in untechnical language, he finds, as every one is sure to find, that difficulties multiply. One could wish that this part of the work had been elaborated with more attention to the essential distinctions, even at the cost of introducing some of those technicalities which would, perhaps, have made the style less colloquial, but in the end more educative.

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*Public Worship: A Study in the Psychology of Religion.* JOHN P. HYLAN. (Religion of Science Library.) Chicago, Open Court Publ. Co. 1901. Pp. 94.

The decrease in church attendance, the decline of the influence of the church on the masses, the growing tendency towards Sabbath desecration, the extreme moderation of present-day theological conceptions, are conditions upon which the present volume endeavors to throw some light by a psychological discussion of the nature and meaning of public worship.

In an historical retrospect, the author finds two typical Sabbaths: (1) Recreative, held by the Jews until their captivity, viz., a day of recreation as well as rest. (2) Christian or worship conception, held by later Jews and by Christians, viz., a day for worship as well as rest. A study of the historical conditions that brought about the transition from the 'Recreative' conception to the 'Christian' reveals the psychological states involved in worship. As long as the Jews were prosperous, the 'Recreation' view prevailed, but after the captivity, when misfortunes of all kinds assailed them, a transition to the 'Christian' conception was quickly effected. Believing in Jehovah, they naturally attributed the severity of the conditions of life to His displeasure, and accordingly, in order to win again His favor, they



eagerly betook themselves to worship. In humility of spirit they throng the temples and with receptive minds receive instruction in spiritual and practical life. Thus are open immense possibilities of moral reformation by means of which the race can better adapt itself to its environment; this is the great function of the Sabbath of the 'Christian' type. In brief, while the 'Recreative' Sabbath aims simply to preserve functions already acquired, the 'Christian' conception aims at the establishment of new functions (p. 45). The essential element in producing worship, and the consequent moral reformation (adaptation) is 'adverse environment.' In emphasizing this point an interesting parallel is drawn between organic and mental evolution.

This conclusion the author confirms by his results obtained by the 'questionnaire' method (203 subjects). The answers to such questions as, "State your reasons for going to church" (personal good 173, duty 140, example 77), "Is it the music, the sermon, prayer, or something else that supplies your need in religious worship?" (sermon 52, prayer 41, music 23, fellowship 1), "Does church-going give you a better idea as to how to live?" (leads to kindness to others 30, singing 26, adoration of God 19, impulse toward a better life 6), show that the great function of religious worship is to secure the moral adjustment of the individual to his environment. These results, while extremely interesting and valuable, would be much more conclusive had they been based on a much larger number of answers than the 203 reported.

Starbuck's recent religious studies are also cited to show that conversions occur most frequently at the 'age of greatest physical growth,' when the individual is face to face with the new environment of maturity to which he has to adjust himself, and they thus support the author's general position.

This analysis of public worship enables us to understand present-day conditions. By science and invention man has been able to cope so successfully with his environment that the need of religious worship is not urgent, and so there is a very general tendency to return to the early Jewish conception of a 'Recreative' Sabbath.

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*Methodology and Truth.* J. E. CREIGHTON. *Mind* X. (1). Pp. 45-56.

The scientific account of nature embodies partly the results of observation, but partly a choice as to the way of interpreting these

results. Calling the latter the 'methodological' factor in the outcome, the question arises: how far may such a factor have distorted the image of nature presented to us? Shall we accept the whole scientific product? The writer points to the 'Synthetic Philosophy,' to 'psychology without an ego,' to ethics based on biology and overlooking 'conscious emotion and intelligent will,' as examples of failure resulting from an uncritical complaisance of this kind. Shall we reject the scientific statement altogether? Then, as alternative accounts of reality, we are reduced to offering either a 'thing-in-itself' outside of experience or an 'immediate' within it. These alone are beyond the reach of methodological disturbances, and of these the one is meaningless, the other inarticulate. Science in the meanwhile becomes mere deduction from bare hypotheses.

Now, we may reject *a priori* the view that any science, even in so far as hypothetical, is entirely without foundation in fact, for the reason that a hypothetical judgment, if meaningful, must itself be a statement of some kind of fact. 'All *a* is *b*' or 'if *a* then *b*' may not enlighten us as to the existence of *a*, but the statement has some ground for being made, and that ground is the fact recorded.

In general, a scientific formula requires critical reinterpretation before its meaning becomes clear; in proportion as it is made clear the methodological factor involved ceases to be misleading. The reflection that must guide us in the task of reinterpretation calls attention to 'the necessary abstractions which a science is compelled to make in order to get under way at all.' That is to say, a science, in order to handle a group of facts, first separates them from their context; *e. g.*, 'physical sciences consider the world *as it would be* if it existed out of relation to mind'; psychology as a 'natural science' considers 'the content of consciousness as it would exist if it were independent of any central principle of the intelligence.' 'What philosophy must seek to do is to remove these abstractions, and to evaluate the scientific conclusions from the standpoint of the concrete whole.'

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## HEARING.

*Zur Theorie der Combinationstöne.* FELIX KRUEGER. Philos. Studien, Bd. 17 (2), 1901. Pp. 185-310.

The author compares and criticizes in this valuable paper the observations of himself and others on subjective combination tones.

Much stress is laid on the fact that the phenomena in question are for the most part independent of the existence of overtones and that, consequently, theoretical explanation must not make use of overtones unless their influence is actually proved. The author seems to attribute a little too much importance to his statement that 'Stosstöne' (a term used by Koenig) and 'Subjective Differenzttöne' (used by many other writers) are not essentially different phenomena, but only different names for the same phenomena. This is no new discovery. In the index of Stumpf's *Tonpsychologie*, *e. g.*, the term Stosstöne is not found at all, but only the term Differenzttöne; and the reviewer, whenever he quoted in his publications one of Koenig's observations on Stosstöne, invariably changed this name into Differenzttöne. The author's observations on the relative intensity of the primary and difference tones are especially welcome, since few observers have paid to the relative intensity the attention which it theoretically deserves. Unfortunately, all estimations of the relative intensity of these sensations are still rather vague.

The second half of the paper contains a criticism of most of the more recent theories of hearing, but only of those of German writers, *viz.*, Ohm-Hensen-Helmholtz, Koenig, Voigt, Wundt, Hermann, Ebbinghaus, Meyer, Ewald, Schaefer. All these theories are rightly declared imperfect, except Schaefer's, which is called 'a good success.' Schaefer accepts the theory of resonators in the ear; he explains the subjective combination tones by an accessory theory which is based on the assumption that the inner ear is functionally comparable to a box filled with compressed air; this box has two (or more, corresponding to the number of objective tones) openings which are rhythmically opened and closed to permit the compressed air to flow out. The reviewer has never been able to see herein any analogy to the inner ear; neither has the discussion of this theory by the present author made this analogy appear clearer.

With reference to the reviewer's theory the author makes several remarks which do not seem to be quite just. I will mention here these two only: "Den Einfluss der Phasenverschiebung auf das Ergebnis seiner Curvenzerlegungen hat Meyer nicht untersucht" (p. 285). This very problem is discussed in my paper in *Zeitschrift f. Psychol.*, Bd. 16, pp. 32-33 (Figs. 9 and 10). "Selbst die Helmholtz'sche Theorie der subjectiven Combinationstöne hatte Meyer noch neben der seinigen stehen lassen" (p. 287). This seems to imply that I later declared this theory false. However, that difference and summation tones, though weak in general, are produced in the *tym-*



*panum*, is a mathematical consequence of its asymmetry which I have never denied. But I hold that these tones are not the *strong* difference tones heard so commonly.

It must be regretted that the exceedingly interesting and important investigations published by Emile ter Kuile in Pflüger's Archiv for 1900 are not recorded by the author. Ter Kuile's theory is fundamentally identical with the reviewer's, though different in detail. The author himself does not offer any new suggestions which might lead to progress in the physiological theory of tone perception.

*Zur Theorie der Tonbeziehungen.* RICHARD HOHENEMSER. Zeitschrift für Psych. u. Phys. d. Sinnesorgane, Bd. 26, 1901. Pp. 61-104.

This paper is a good exposition of the theory of Lipps, which tries to explain the psychological effects of tone combinations by assuming that a sensation of tone is *unconsciously* a series of rhythmical sensations, and by referring to the æsthetic effects of *conscious rhythm*. The author briefly rejects (and here every psychologist will agree with him) the musical theories of Helmholtz and Wundt ('overtones') and that of Riemann ('Klangvertretung'). He states that only two explanations of the musical effects are then left: Stumpf's explanation by 'fusion,' and Lipps's by 'unconscious rhythm.' He criticizes Stumpf's theory and shows that it leaves a great many important facts unexplained. Then he defends Lipps's theory against the criticism of Stumpf and applies it to a number of facts to which Stumpf's theory cannot be applied. Anything essentially new, however, is not to be found in the paper.

The reviewer confesses that he sympathizes with Lipps's theory to some extent, but is unable to understand how the musical facts can be *explained* by calling them unconscious rhythms, while admittedly the æsthetic laws of these 'rhythms' are different from those of conscious rhythms. In this respect Stumpf's criticism of this theory is as valid as ever. While Lipps's theory can hardly be regarded as an explanation, its value seems to consist in the fact that it prepares the ground for a real explanation in the future, which must be sought—most probably—in a correlation of the æsthetic effects and the chemical processes (with numerical relations) in the nervous organs. As a preparation for a future explanation Lipps's theory is doubtless more promising than that of Stumpf.

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## MUSIC.

*Contributions to a Psychological Theory of Music.* MAX MEYER.  
University of Missouri Studies, I. (1), 1-80, June, 1901.

A part of this paper was published in the May number (1900) of this REVIEW; the greater part, however, is new. The first chapter contains the fundamental laws of Melody, *i. e.*, a statement of the relationships which are observable in different pairs of successive tones. The second chapter develops what I have called the 'Complete Musical Scale.' It is based on the fact that a succession of tones cannot be called a melody unless it is melodious. Since 'relationship' is only another name for melodiousness, we derive the Complete Scale by a very simple reasoning from the relationships observed in the first chapter. The third chapter contains a structural analysis of a number of well-known melodies, partly with, partly without, a primary tonic. The fourth chapter is entitled Psychological Laws Effective in the Historical Development of Melody. It is here shown that the historical development of music can easily be understood from the psychological theory. Folk song and other early music make use of the tones of the diatonic scale, which is represented in tempered intonation by the following parts of an octave:  $\frac{2}{12}$ ,  $\frac{2}{12}$ ,  $\frac{1}{12}$ ,  $\frac{2}{12}$ ,  $\frac{2}{12}$ ,  $\frac{1}{12}$ ,  $\frac{1}{12}$ . If we assume that early music prefers close relationships to less close relationships, we find that the historical importance of the diatonic scale is founded upon the fact that this scale corresponds (in pure intonation) to *two* series of tones of extremely close relationships; the diatonic scale has—as we learn incidentally—at least two (perhaps more) distinct interpretations, a fact entirely ignored by the theorists, who endeavor to reduce all music to *one* small and arbitrarily selected scale. At the same time we learn why the use of the tone 7 (the pure number 7 without a coefficient) is not found in more ancient music.

In the fifth chapter four different problems are discussed: (1) Some remarks of Charles Wead concerning the use of the number 7 in musical theory and the corresponding pitch in real music, are criticized. The fact is emphasized that this question involves, not only the pure number 7, but any product of 7 and another number. (2) Some writers have asserted that Wagnerian music could not be rendered or theoretically understood except in strictly tempered intonation. It is demonstrated that this is not true, that a sane psychological theory is able to cover Wagnerian music as well as any other kind of music, that from Wagnerian music no argument can be derived in favor of tempered intonation either in theory or practice. (3) The

fact is emphasized that violations of æsthetic laws in real art do not prove that an æsthetic theory is wrong. Many seeming violations, however, particularly in Oriental music, cease to be violations of æsthetic laws as soon as the misinterpretation by the biased European hearer is corrected. (4) It is shown that different parts of the same piece of music are sometimes constructed according to different æsthetic laws. Therefore, in order to understand the structure of a complicated piece, all the parts must be studied singly.

The sixth chapter begins with a discussion of the general æsthetic laws of harmony. In harmony the same relationships are found as in melody, *i. e.*, relationship is effective in simultaneous tones as well as in successive tones. Besides relationship, we observe in harmony *consonance*. The failure to distinguish between these *two* contents of harmony is a serious fault of the musical theories. Consonance is psychologically effective without analysis, but relationships are effective only so far as a sound is heard analyzed. This leads to a brief consideration of the theory of "quality of tone" of a compound sound, which is not satisfactorily treated in the psychological text-books. Stumpf's theory that quality is observable in simple as well as in compound sounds is accepted. Several typical forms of music are then harmonically analyzed: (1) The drone bass of bagpipe music. (2) A mediæval canon. (3) A polyphonic piece by Heinrich Schuetz. The analysis of this piece is especially important because it explains the often-observed falling of pitch in the performance of polyphonic vocal music. (4) A song of Schubert's with its accompaniment. (5) Several measures of the duet in Wagner's *Tristan and Isolde* are analyzed. This analysis demonstrates that no reason exists for assuming that Wagner's music is based on any æsthetic laws different from those of other music. (6) The æsthetic effect of the 'leading chord' is explained by the psychological theory without the aid of any accessory hypothesis.

THE AUTHOR.

#### EXPERIMENTAL.

*Studies from the Psychological Laboratory of the University of Michigan.* Contributed by W. B. PILLSBURY. *Amer. Jour. of Psychology*, 1901, XII. (2).

- I. *The Fluctuations of the Attention in some of their Psychological Relations.* J. W. SLAUGHTER.
- II. *The Effect of certain Stimuli upon the Attention Wave.* R. W. TAYLOR.

Masson's disc was used and the fluctuations were registered upon a moked drum. A normal series showed, in addition to the quickly



alternating periods in which the stimulus was visible and invisible, two longer fluctuations, one from 10 to 15 seconds, and the other from 60 to 80 seconds in length. A dynamograph was introduced to try the effect of an accompanying physical exertion. This made the periods of visibility and non-visibility shorter with a relative increase in the length of the former, and thus had a similar effect to an increase in the intensity of the stimulus or the increase of attention. A finger-plethysmograph was employed to discover what relation might exist between the fluctuations and blood pressure, and it was found that with most subjects they ran parallel with the Traube-Hering waves, while with some the breathing exerted the controlling influence.

With similar apparatus Taylor tried the effect of agreeable and disagreeable distracting stimuli upon the rapidity of fluctuation. With one subject pain from an induction current quickened the rhythm of fluctuation with relatively increased length of the invisible periods, while with another the effect was exactly opposite. For one subject smoking lengthened the waves and also the relative length of the visible periods.

The work is preceded by an interesting historical résumé, and the experiments suggest important physiological conditions of the phenomena. The methods, however, are not suited to the complicated conditions, and the work suffers from the lack of critical analysis of the problem. Subjective fluctuations should have been differentiated from the others, and means should have been employed to indicate involuntary eye movements.

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*Ueber Arbeit und Ruhe.* ERNEST H. LINDLEY. Psychologische Arbeiten, III., Heft 3. Pp. 482-534.

This paper of Professor Lindley is one of the most interesting of the recent products of our psychological laboratories. It owes its value essentially to the skill with which has been used an excellent method developed by Kraepelin and those who have worked with him. To the psychologist not yet acquainted with the work of the Heidelberg alienist the dominant interest of the paper will not be the particular results it sets forth, although they are of unusual weight, but the method itself with the unexpected precision of its revelations.

In continuation of the researches of Amberg, Rivers and Kraepelin, Professor Lindley addressed himself to the task of discovering the influence of periods of rest of various lengths upon mental work which they interrupt. No more than what is absolutely necessary

for a general understanding of the investigation before us shall be said here concerning the method and the often complicated calculations by which the conclusions we are about to report were reached.

The experiments extended over 28 days. Two of them were days of rest. On the other days 60 minutes were spent by three persons in adding columns of printed figures. The number of figures added was taken as the measure of the amount of work done. On the first day the 60<sup>m</sup> were continuous. On the second day there was a rest of 5<sup>m</sup> after the first half hour. On the third day the rest was of 15<sup>m</sup>; on the fourth of 30<sup>m</sup> and on the fifth of an hour. This series of five days was repeated five times. The figures obtained show several variations relatively regular for each subject. They are interpreted as due to the following factors, to each one of which a section of the paper is devoted: 'Uebungsfähigkeit,' 'Uebungsfestigkeit,' 'Ermüdbarkeit,' 'Anregbarkeit' and 'Antrieb.'

It is when considered from the point of view of comparative individual psychology that the most interesting aspect of Professor Lindley's work is seen. Each one of the three subjects yields results strikingly different from those of the two others and yet the regularity and the consistency of the figures are sufficient proof of the reliability of the experiments. Let us consider, for instance, the capacity for improvement (Uebungsfähigkeit), as it appears in the following table:

	1	2	3	4	5
<i>A</i>	2118	2781	2891	2967	3065
<i>B</i>	1619	1939	2366	2649	2812
<i>C</i>	1486	1655	1768	1795	1781

*A*, *B*, and *C* denote the three subjects. 1, 2, 3, 4 and 5, at the head of the columns, indicate the sets of 5 days in their chronological order. The numbers in the columns give the average daily number of figures added during each set of five days. (The average of the second set for *B* is from four days only.) It appears from this table that *C* was initially a great deal slower than *A*. For this initial difference the experimenters knew not how to account. The subjects differ also widely in their improvement-capacity. If the gains are expressed in per cent. of the number of figures added for the first day, the following figures are obtained:

<i>A</i> 10.	0.7	1.1
<i>B</i> 12.2	2.6	1.9
<i>C</i> 3.6	0.8	0.6

The first column is based upon the average of the first ten days; the second upon the average of the second 10 days and the third column upon the average of the last 7 days. *A* began with the largest number of additions. Her gains were also at first absolutely the greatest, but *B* gained from the start proportionally more than *A*. That is, *B* shows the greatest and *C* the smallest gain-capacity of the three.

If one calculates the losses suffered from day to day on account of 'Uebungsverlust'—loss of practice-gain—it is found that the advantage rests again with *B*, inasmuch as he loses only 38% of his practice-gain, while *A* loses 74%. *C* stands between them. In other words, in the case of *A* the greatest absolute gain is followed by the greatest loss, while in *B* we have the greatest capacity for improvement—Uebungsfähigkeit—associated with the greatest gain or retentiveness—Uebungsfestigkeit.

If we now pass to fatigue we find that *C* is susceptible to it in an astonishingly high degree. His fatigue coefficient is 13%, that of *A* 3.8%, and that of *B* 1.8%. These fatigue-differences appear clearly in the effect of the pauses introduced between the two half hours of adding. The following figures give in per cent. of the number of figures added during the first half hour the gains made during the second half hour. The first column is an average of the five days when the two half hours were not interrupted by any rest; the second column the average of the days in which a pause of five minutes came between the two half hours and so on:

$$\begin{array}{l} A + 1.8; + 0.8; + 4.8; + 4.6; + 3.5 \\ B + 4.2; + 1.2; + 2.7; + 3.1; + 2.5 \\ C - 11.3; - 7; - 6.4; - 3.9; - 0.5 \end{array}$$

In the case of *C*, the second half hour is always worse than the first, whatever be the intervening rest, but it is least so when the rest interval is the greatest, *i. e.*, 60<sup>m</sup>. A longer rest would apparently have been of still greater advantage to him. *B* did best during the second half hour when there was no interruption between it and the first, and worst with the smallest rest, 5<sup>m</sup>. In the case of *A*, the most favorable rest period under the circumstances was 15<sup>m</sup>. No one will fail to see the bearing of these differences upon the problem of school fatigue.

It is not to be supposed that the results following upon the rest-interruptions are to be ascribed solely to fatigue. Other factors come into play, notably the tendency to lose what has just been gained by practice, the 'Anregung' and the 'Antrieb.' Rests of 5, of 10, or of



15<sup>m</sup> resulted in a loss for *B*, because they were long enough to destroy the 'Anregung' and to cause a sensible loss of practice, while the compensation due to rest was apparently insignificant, either because there was very little fatigue or because the time allowed was not sufficient to relieve it, or for both reasons.

We cannot speak in detail of the chapters on 'Anregung' and 'Antrieb.' The more general conclusion reached in the latter is that 'Antrieb' is independent both of the capacity for improvement and of fatigue.

In the chapter on 'Personal Differences' attention is drawn to the light thrown by this and similar investigations upon the human personality, and the results obtained by the author are compared, as far as possible, with those of Hoch, Amberg and Rivers.

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#### ASSOCIATION.

*Zur qualitativen Untersuchung der Association.* A. MAYER und J. ORTH. Zeitschrift f. Psychol. u. Physiol., Bd. 26, Heft 1 u. 2, Mai, 1901.

*Experimentelle Untersuchungen über Associationen.* G. CORDES. Philosophische Studien, Bd. XVII., 1 Heft, März, 1901.

Herren Mayer and Orth discuss those associations 'in which the subject reacts to a word spoken to him by a spoken word.' The report of their experiments is disfigured by the constant use of the term 'association' in an inaccurate sense, as applying only to the word pronounced by the subjects in response to the stimulus. The intermediate conscious phenomena are described as 'eingeschaltete Bewusstseinsvorgänge,' whereas these are, of course, themselves associations and, on the other hand, the word, as *pronounced*, is, strictly speaking, a physical, not a psychical phenomenon. The main conclusions of the investigation are the following:

1. Between the verbal stimuli and the verbal responses, in these experiments, there commonly occur intervening conscious phenomena; and such reactions are longer than those in which the verbal response follows without preceding mental phenomena.

2. Feelings and volitional experiences may be members of this psychic series between the experimenter's and the subject's word.

There is certainly nothing new in all this, and the simplicity and roughness of the method, especially of the time-measurement with the observer's stop-watch, hardly justifies the detail of the report. The

results of the experiments certainly do not lead to any new classification of association, and the forms of 'Eintheilung' proposed by the authors are based on no fundamental principle whatever. Their conclusion, that disagreeably toned 'intermediate ideas' lengthen the association more than agreeably toned ideas, will hardly be accepted without confirmation by experiments far more rigorously conducted.

The praiseworthy feature of the work of Mayer and Orth is the stress which they lay on the need of careful introspection on the part of subjects in association experiments, and on the necessity of a careful study of these introspective results. This emphasis on the value of introspective records is the distinguishing merit, also, of the far more significant work of Cordes. In the main, Cordes follows Scripture's method, even making use of the mammoth black box of Scripture's investigation (*Philos. Stud.* VII.). The subject of these experiments sits inside the black box and receives the stimuli, which are mainly visual. The recorded associations are absolutely free, that is, the subject is directed simply to tell in detail what images or feelings or other experiences have followed upon the occurrence of the stimulus. One must read the report of Cordes itself in order to realize the discrimination, caution and skill of the analysis and classification of this material, gained by experiment during several months with eight subjects. The extreme care with which Cordes has studied his associations gives great weight to his two negative conclusions. He insists, in the first place, on the futility of all classifications of association depending on the relation of a word as pronounced to a word as heard. This follows, first, from the artificiality of all these verbal associations, and second, from the marked individuality and the illimitable variety of associative series. In the second place, Cordes asserts in unambiguous terms the impossibility of measuring association times. This conclusion is based on the fact that in all complex and affectively toned associations the verbal expression is inevitably retarded.

This brief notice of the work of Cordes does no justice to his discriminating criticism of Ziehen and Aschaffenburg, nor to his interesting experiments in mediate associations, which he finds as result of experiments with repeated nonsense syllables. (*S.* 71 *seq.*) To the writer of this notice, the value of the work lies, however, far less in its experimental conclusions than in the strictly psychological nature of the method and of the point of view. It surely is significant of the emancipation of psychology from physics that this latest output of the Leipzig laboratory should assert the impossibility of

measuring association times! One hopes that the later work of Cordes will evidence a more cosmopolitan acquaintance with the literature of his subject. He does not allude to any one of the recent investigations and studies, in English, of association.

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## CHILD AND ANIMAL PSYCHOLOGY.

*The Study of Children.* EDWARD THORNDIKE. Teachers College Record, edited by James E. Russell. Columbia University Press, N. Y., May, 1901. Pp. 110.

The general character of this pamphlet is stated in the preface as follows: "The purpose of this number of the *Record* is to give an account of the work done by the department of Psychology in the Teachers College and to present some of the more important data of child study in a form accessible to all students of children and convenient for teachers of special subjects. It will comprise brief statements of the nature and aims of the different courses offered, a more detailed account of the elementary course in child study, reprints of two valuable general articles now practically inaccessible to most students, and sample contributions from the literature of child study to the methods of teaching special subjects."

Three courses in child study are offered, one dealing with the more practical phases of the subject, one with theories of genetic psychology, and one with original researches in genetic and comparative psychology.

The work in the first course is outlined and its character illustrated by samples of questions asked in connection with the critical study of the literature of child study, while the work in the other courses is more briefly outlined.

The study of the contributions of child psychology to special methods is along the lines of reading and literature, writing and drawing, and spelling. Considerable portions of articles by Wissler, Bryan, Hancock, and Rice describing their investigations are quoted with comments and suggestions as to the value of methods used, the accuracy of generalizations made, and their practical application in education.

The child study reprints appended are portions of Johnson's 'Rudimentary Society among Boys,' Schallenberger's 'A Study of Children's Rights, as seen by Themselves,' and all of Galton's 'History of Twins.'



Naturally, in this as in other early attempts at organizing the materials of a new subject of study there is some lack of unity and symmetry, yet every one interested in the teaching or study of child study will find this pamphlet very suggestive and valuable both as an outline and because of the details given.

*Some Criminal Tendencies of Boyhood: A Study in Adolescence.*

EDGAR JAMES SWIFT. Pedagogical Seminary, March, 1901.  
Pp. 27.

This is another of the many *questionnaire* studies published in the Pedagogical Seminary. Like most of the others it is valuable chiefly as a preliminary examination of complex phenomena. Just as a view of a country from a balloon would be of some value to a surveyor, so such 'drag-net *questionnaires*' are suggestive and helpful preliminaries to scientific investigations of new questions.

The object of the *questionnaire* was to determine whether men of unquestioned moral character ever performed or felt impulses to perform acts of a criminal nature, such as have caused boys to be sent to reform schools and men to prison. No tables are given, but it was found that a large proportion of the forty-three teachers, twenty-five college and normal students, and thirty-five business and professional men who answered had felt such tendencies and in many cases had actually engaged in law-breaking and truancy, attempted violence and even murder in fits of anger, robbed orchards, etc., stolen goods and money from relatives, employers and others, set fires and destroyed property in various escapades, and often lied to escape punishment. Considerable space is given to quotations from writers on sociology showing the customs of savage people that are comparable to the criminal impulses and acts of adolescent boys of civilized parents and the general conclusions of the study are indicated by the following quotations:

"The race tendencies of primitive man cannot properly be classified under morality. They are neither moral nor immoral. They are simply stages in evolution in which man finds himself and to the conditions of which all his nature strives to conform."

"Adults read their ideas of morality into children's acts and then catalogue them as right or wrong, while in reality they are often neither. This period of savagery or semi-criminality is normal for all healthy boys. Those whose surroundings are favorable to a life of crime continue in it, finally to end in the reform school, and still later, probably, in the penitentiary, while those of better sur-

roundings pass through it without permanent moral injury, and perhaps, indeed, with a stronger character and a keener insight into human nature."

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*The Child. A Study in the Evolution of Man.* A. F. CHAMBERLAIN. London, W. Scott, N. Y., Scribners (imported), 1900.

Dr. Chamberlain gives us in this work one of the best summaries of child study yet published. Certain chapters of child life are selected for treatment; historical sketches referring to the chief advances achieved in the knowledge of the subject are given, and an eminently fair, critical attitude maintained throughout. The extensive reading of the author is apparent in every chapter. The bibliography is eminently useful; in fact, the whole book gathers together a fund of information and sources valuable in the class-room and readable for all interested in the knowledge of the child.

No criticism is offered on the book, but several points concerning the doctrines treated should be noted. For instance, in reference to the so-called prolongation of the helplessness of infancy, it should be observed that the essence of the problem consists in the growth and increase in plasticity of the nervous system. This increasing plasticity allows greater adaptability and the acquisition of an ever-increasing social heritage. It is increase of plasticity rather than prolongation of *infancy*. Moreover, the progressive medullation of nerve fibers will determine the real limit of this age of plasticity. Thus adolescence really extends to the age of 35 and 40, according to the investigations of Kaiser, Kaes and others. The final justification of the increase in plasticity is, of course, the growth of adaptability.

Chamberlain seems to accept the argument of Fiske's that the prolonged helplessness of the offspring must keep the parents together for longer and longer periods, and that out of this grew sociality, the family, the clan, society. Out of the helplessness of the infant grew the helpfulness of man. As a matter of fact it was due to parental care that plasticity had a chance to develop. Without parental care all variations in the young tending to greater plasticity would increase the helplessness of the young, and hence diminish their chances for survival. Only when such variations as tended to increase the helpfulness of the parents occurred was there a possibility for plastic variations in the young to be sheltered until their growth rendered them useful. The growth in plasticity was the goal, and the helpfulness or

sociality of family, clan, etc., was the means. This is, of course, important from the standpoint of social psychology.

In reference to play it may be stated that Herbert Spencer emphasized the instinctive origin of play ('Principles of Psychology,' Vol. II., chapter on *Æsthetic Sentiments*). The present polemic against the surplus-energy theory will have to be modified somewhat to be even with the facts. The main characteristic of play from the social and biological standpoint is its *initiatory* character; social habits are to be formed which later may be transferred or attached to other aims and ends than pigskins, tops and prizes. Gambling as a play is condemned because of its anti-social habit—waiting for something to turn up. Groos's theory implies that the young are possessed of impulses (broken-down instincts owing to increase of plasticity?) and that play offers an opportunity for the development of these impulses into instincts useful to the adult. Hence the *Vorübung*, *Ausübung* and *Einübung*. In what respect will the adult be better off? Why should not natural selection prefer the full-grown instincts from birth on? To state that play favors the growth of intelligence is to state a fact; it is also the problem of play.

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*Experimental Study of the Mental Processes of the Rat.* II. WILLARD S. SMALL. *American Journal of Psychology*, XII. (2), January, 1901.

In 34 pages Mr. Small presents the results of his study of the behavior of *Mus decumanus* in learning to go directly to the center of a complicated maze. The paper completes his studies of the mental processes of the rat, previous sections of which have been already noted in the REVIEW. Learning a complicated path to a desirable place had been found a useful test of mental traits in the case of chicks and fish and served a good purpose in Mr. Small's hands. He perhaps lessened the value of his observations by leaving the rats to roam about the maze unobserved all night after each trial. It is hard to interpret behavior surely if we see only broken abstracts instead of the continuous whole.

Mr. Small presents evidence of the superior energy and consequent quickness in learning of the males, of the absence of imitation, of the permanence of instincts in spite of long disuse, of the importance of motor and tactile consciousness in the rat's mental economy, of the gradual progress of the animals' learning, and of the lack of reasoning in the common psychological use of the term. His comments on



general animal psychology are interesting and sagacious, though they seem to the reviewer to be concerned too much with verbal rather than real questions. I may be allowed to correct a mistake concerning the 'curves' or graphic representations of the progress of animals in successive trials which I have given in two monographs on animal intelligence. Mr. Small and others credit me with the opinion that these have some significance beyond their face value, as handy graphic representations of the decrease in time taken in successive trials. That is not the case. I made use of them first of all to save space, and secondly, because I found empirically that with cats, dogs, chicks and fish the decrease in time was fairly proportionate to the increase in the tendency to select the appropriate act, and so served as a fairly accurate representation of it. In the case of the monkeys I expressly limited the meaning of the time records.

Mr. Small insists that the rats possessed and were guided by images and feelings of recognition, and puts his descriptions of their learning constantly in terms of images, conscious selection, doubt, etc., with never a word about the impulsive side of animal consciousness. This seems unwise. An animal may feel like going down a certain path, or feel like hesitating, or feel like doing one thing where he could do either of two or more, and still have no images or ideational consciousness whatever. So may human beings. Mr. Small has not, I think, realized the possibilities of learning by the selection of impulses.

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## ÆSTHETICS.

*La valeur sociale de l'art.* G. SOREL. *Revue de Métaphysique et de Morale*, IX., 3, May, 1901. Pp. 251-278.

The problem of explaining the genesis of a work of art in the mind of the artist is, M. Sorel believes, an impossible task. A psychology of genius—a science of the accidental—would be a condition of such a task. But the production of the work of art in so far as a given society is concerned, the determination of the reasons why such a work is so received, seems to him a less formidable undertaking. What then, is art in its relation to society? Is it, as many believe, merely a source of special pleasures? The imperfect state of the psychology of the feelings makes a decisive answer impossible. Art criticism has been too superficial to furnish what might be valuable psychological data. Is the meaning and value of art to be determined by its moral effect?

The moral effect is of very great importance, but it can seldom be determined directly. Time alone can show what the effects of a work of art will be.

The history of art may give some clue. Ancient art differed from modern art in many respects, but the most striking difference, M. Sorel believes, was that ancient art combined the useful and the æsthetic, *e. g.*, the architecture of temples and of tombs, religious rites, etc.

As the arts developed, technique was perfected and finally became all-important; professional skill took the first rank. Art became abstract and sought its justification in the metaphysical theory which made *beauty* the aim of art. But these schools were gradually broken up by a growing individualism which has allowed the dogma of beauty to disappear.

This individualism is charged with merely aiming to please by whatever means; with being regardless of morals or even antagonistic to them. Is art, then, merely a dangerous amusement? The theory that leads art back to play has been developed most completely in England. M. Sorel believes it to be the outcome of economic conditions, in particular the presence of a rich and powerful aristocracy and the presence of superfluous energy. This energy must be expended and art seemed to be its noblest object. In the Renaissance, too, the conception that art was for the pleasure of the aristocracy was widely current.

The play element and the pedagogic element cannot, perhaps, be eliminated, but art can no longer exist for privileged classes. It must become an art of the people, like the Greek art with its sacred dances, choruses of singers, public fêtes, etc. Modern music suggests possibilities in that direction. Moreover, art must be brought into relation to modern ideals. It must find its place in a society which regards work as the most important thing in the world and is becoming more and more intensely absorbed in work. The art of luxury and play must become an art of relaxation—a relaxation which is necessary for intellectual sanity.

The most interesting side of modern art is that which unites use and beauty, which shows the mind of the worker in the work of his hands—not an art for the collection, but an art for use. It should have regard for the means of production rather than the things produced—for the perfection of the whole, and of each of the parts with reference to their place in a useful whole. Such an art, expressing the mind of the worker, will be individualistic. Æsthetics will become more and more intellectual, since art *is* the expression of mind

in work. He who does not understand a given art may understand its æsthetic value by reflection on the interpretation of professionals and by the analogies that exist among all the operations of the inventive mind.

Art should ennoble work and raise manual labor to the level of scientific work.

An obvious criticism on M. Sorel's whole proceeding is his reliance on an interpretation of contemporary sociological and economic conditions for the basis of a scientific theory after rejecting certain psychological interpretations on the ground that they were insufficiently established. His discussions of the economic conditions responsible for the 'play theory' of art do not show clearly in what way superfluous energy is related to art production. His interpretation of Greek art fails (it would seem) to apply to a large part of Greek art. Again, his conclusion as to the growing intellectualistic character of æsthetics seems somewhat hasty, if indeed it is not a begging of the whole question.

His whole article, however, is very interesting; in particular his discussions relating to the ethical bearings of art and the relations of art and work. In the latter he is not far from William Morris's point of view: "An art by the people and for the people, as a joy to the maker and user."

The article contains incidentally criticisms of Taine, Guyau, Brunetière, Prudhon, and Tolstoi.

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*Das Problem des Tragischen.* RICHARD HAMANN. *Zeitschrift für Philosophie und philosophische Kritik*, Bd. 117, Heft 2, and Bd. 118, Heft 1.

This treatment of the problems of the tragic revolves about a distinction between the purely tragic as a subjective value and the objective, æsthetic treatment of the tragic in the tragedy—a distinction which in the history of æsthetic discussion has scarcely been made so marked before, and which has for its object the attainment of a definition which may include the profound modifications made by our modern life in the conception of both.

His investigation of the tragic moment as such, independent of its expression in the drama, leads the author to a position at variance with emotional and ethical theories of Katharsis, as they have found expression in classical criticism. The emotions of fear and pity alone do not suffice to explain the tragical moment. As the later theories of



the tragic have all maintained, in order that these emotions may be tragic they must proceed from the destruction of an ethical person or value. An ethical norm of value, be it ever so shifting, must be presupposed. Hamann finds himself equally at odds, however, with the classical ethical doctrine of the tragic as it finds expression in Lessing, Schiller, and even in a modified form in Lipps. Through all its variations it conceives of the tragic as tragical elevation, and this as brought about by an element of rational reconciliation following upon an ethical dissonance. The two concepts of tragical guilt and poetic justice, while emphasizing the ethical norm or value as a presupposition of the tragic, err in conceiving as a part of the tragic moment an element which would simply destroy the tragic, substitute for it another value. The tragic remains then, ethically, a dissonance resulting from the negation or destruction of an intensified ethical value or norm. "Von Tragik sprechen wir dort wo wir die Zerstörung eines Selbstwerthes, dem wir sonst Existenzberechtigung zuschreiben als ein ungerechtfertigtes Verhängniss, einen Widerspruch mit unserer ethischen Norm, von dem, was sein soll, empfinden. Die Tragödie ist davon nur ein besonders ausgezeichneter Fall."

For Hamann, the conception of reconciliation included in the tragic does nothing more nor less than rob it of its tragical character. It is nothing but a 'displacement' of the norm of value, a robbing of Peter to pay Paul. A value is intensified only to act as a foil which shall increase the value of the ethical norm which displaces it; it may be a vision of an inviolable world-order of the will of God or of mere Fate. The author would probably find here a more complex sentiment—sublimity, tragical elevation—but not pure tragic. The tragic as such remains ethically a dissonance.

But tragedy, as an art, is but a special case of the tragic. And in the dramatically tragic is undoubtedly to be found, in place of dissonance, consonance, katharsis, elevation. Whence this new modifier of the tragic? "Wie kann das was in seiner ethischen Konsequenz abtossend wirkt, æsthetisch werthvoll werden?" The answer to this is wholly in terms of the conditions of the æsthetic psychosis. The reconciliation and its katharsis, in so far as there is one, is, from the ethical point of view, *illusion*, as long as the tragic remains tragic. The consonance is wholly one of function of our sensuous, our emotional and judgmental processes. The ethical dissonance remains in suspension. The technique of the drama is especially favorable to this twofold state of consciousness. The quick procession of pictures, the piling up of intense sensuous, personal and ethical

values, the rhythm of emotions, hope and fear, pity and hatred, the semi-logical satisfaction from the movement of the plot—all produce equally an intensification of the value of the personality involved, and a consonance in the merely intuitive processes. Even ethical values in the presentation become simply elements to enhance the value of the æsthetic whole. Though Hamann nowhere indicates as much, the ideas of tragical guilt, of fate, of poetic justice, which appear so constantly in the classical drama and have often been conceived as essentials of the tragic, would presumably be interpreted by him simply as means among others to produce the æsthetic consonance and necessity of the drama, at a time when they were profound beliefs, and not an essential part of the ethically tragic.

The undoubted basis of this fundamental distinction between the tragic and tragedy is an ultimate distinction between the ethical and the æsthetic. As a provisional differentiation this is important, and the distinction between the tragic and tragedy is, therefore, valuable provisionally. It has also to recommend it, the ease with which much of modern tragedy is explicable under this formula. The reviewer does not feel, however, that Hamann has done justice to the ethical meaning of tragedy, meaning by tragedy the union of the ethically tragic and the æsthetic psychosis. 'Tragical elevation' is itself an ethical sentiment and of ethical significance. The healing of the breach would follow only from a study of the ethical meaning of the æsthetic experience itself. This, of course, was not the writer's problem, and his provisional distinction is enlightening.

WILBUR M. URBAN.

URSINUS COLLEGE.

## PERCEPTION OF TIME.

*Perception of Change and Duration.* G. F. STOUT. *Mind*, IX., 1-7, January, 1900.

This is the presidential address delivered before the Aristotelian Society, November, 1899. It discusses the question as to how far and in what sense it is necessary, when we perceive a temporal process, that representations of prior parts of the time series should be present to consciousness in the perception of succeeding parts. Green and Hodgson are cited in support of the view that in perceiving a time sequence the presentations of prior stages must persist in later stages. This may be called the memory-image theory.

The views of two recent German writers are stated and criticized. Meinong defends the memory-image theory, but on theoretical rather

than on introspective grounds. He maintains, for example, that a melody is not presented until the last note of it is heard. Mr. Stout admits that "we must have *some sort* of apprehension of the terms related in order to apprehend a relation," but denies that these terms must necessarily be presented as distinct memory-images.

Schumann, on the other hand, holds that for psychical elements to form a whole they need not be presented together. The accompanying process which makes such perception possible need not be a memory-image; it is sufficient to assume a physiological or psychical disposition. As Mr. Stout puts it in his *Manual of Psychology*, "The persistent traces of past experience may modify present experience and be modified by it, without reappearance of the content of the past experience in the actual moment of present consciousness. \* \* \* Successive series of a rhythmic character can at their close be apprehended as a whole without mentally reproducing and discriminating in the moment of apprehension the several sequent parts which compose them" (pp. 78-79).

The clue to Mr. Stout's own view seems to be found in the following sentence: "To 'form a single whole' means to act as a whole." This accords with his teaching also in his *Psychology*. It is because a given succession of sounds, for example, stimulates an incipient response on the part of the organism that this series of sounds is apprehended as a whole. This incipient act of the organism requires such a series for the maintenance of its conative unity and continuity, hence this is the point of view from which to interpret the perceptual process. The time unit is always ultimately the activity, as represented in the interest or end. This explains why "a man may be able to estimate lapse of time with a fair amount of accuracy, and yet not have established an accurate relation between his subjective estimate and time as measured by the clock" (p. 387).

H. HEATH BAWDEN.

VASSAR COLLEGE.

#### APHASIA.

*Weiterer Beitrag zur Kenntniss der Beziehungen zwischen Aphasie und Geisteskrankheit.* Privatdocent Dr. KARL HEILBRONNER, Halle a. S. Zeitschrift f. Psychologie, Bd. XXIV., 83.

The present article is a continuation of previously published examinations of an individual case (Wernicke's *Psychiat. Abh.*, Heft. I., and *Zeitschrift f. Psychiatrie*, Bd. LV.).

The patient was a man who, in consequence of an acute mental disturbance, with hallucinations, etc., superficially giving symptoms



of dementia, showed when studied more closely that his defects were referable in large part to the portion of the brain connected with speech. These showed themselves as a motor and sensory aphasia, and in disturbances of the higher intellectual functions which are most closely allied to speech. Thus the apparent dementia was elective, as in many respects the patient in the course of time became as intelligent as he ever had been. From this Heilbronner infers that the pathological process which the psychosis caused, affected most intensively the brain regions in the neighborhood of the speech centers. Though in these regions the damage done was irreparable, thus giving a permanent picture of aphasia, in other parts of the brain recovery had taken place permitting a certain degree of restoration of the intelligence.

FREDERICK PETERSON.

NEW YORK.

## NEUROLOGY.

*A Study of the Neurofibrils in the Ganglion Cells of the Cerebral Cortex.* STEWART PATON. *Journal of Experimental Medicine*, Vol. V., No. I., October 1, 1900. Pp. 21-25. One plate.

The illustrated article of which this abstract is a brief report and summary adds good evidence to the much-disputed question (still unsettled?) as to the existence or not of fibrils within the cell-bodies of cortical neurones. Dr. Paton has no doubt that these fibrils do exist and his illustrations of them make their appearance very clear.

He suggests that the various elements of the nerve-cell body would be classified into four groups better than by the classification common at present, although these would be general rather than strict. These classes are: '(1) cytoplasm; (2) Nissl bodies; (3) fibrils; (4) constituents whose morphological characteristics are not yet definitely known.' This nomenclature would be an improvement especially because it would do away with the very objectionable names in use at present, based on the relative stainability of the structures in question.

It appears certain to the author that the fibrils run directly through the body of the cell; and he thinks that the reticulum connecting these fibrils is outside the cell rather than within the boundary—the latter being relatively a novel view of this largely examined fabric. The 'gemmules' or varicosities (which some still consider artefacts), this observer regards as organs, of some use or other, closely related to the fibrils. (See abstract of article by Stefanowska on page 653 below.)

The fibrils, it appears to Dr. Paton, can be properly stained only by the hematin or the gold stains used by Apáthy or by a method of Bethe's as yet unpublished. A hematin method altered by the author is presented, but this even is admitted to be not entirely satisfactory. Thus it is clear that the staining of the fibrils is a process of great difficulty leading to no very good results, although the author considers that the ordinary lack of freshness of the tissue-material, and especially of human cortical material, is to blame for a great part of this defect in definiteness. The fibrils decompose *very* soon after death, and human material dead less than a day or two is very rarely obtainable. Perhaps to this important consideration is due much of the scepticism regarding these fibrils still preserved by a few experts in neuro-histology. This is a presumption indeed not difficult to accept in view both of the natural probability and of the exact evidence offered by highly competent observers.

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

*A Digest and Criticism of the Data upon which is based the Theory of the Ameboid Movements of the Neurone.* H. H. BAWDEN. *Journal of Comparative Neurology*, X. (2), May, 1900. Pp. 243-264.

The title of this article very fully expresses the latter's nature, except that it fails to indicate that a bibliography containing 115 references to articles and works on this interesting subject may be found at the end of the paper. Taken altogether the article is one of uncommon value to the psychologist, for, in a brief way, it summarizes for him the facts, theories and opinions of this almost uniquely interesting supposition—that the neural units are primarily active, as well as passive, agents in the transmission of portions of the psychophysiological process. But besides this it adds yet another, and a highly competent, opinion as to the respective values of the researches digested, and as to the probable status of the whole matter.

Time is not available for the summarizing of the historical portion of the paper; it is very accessible, but full of the details of a highly technical histology and microscopy, interesting to few save the specialists in these matters. Our brief summary will be confined to the critical portion of the article.

Following Duval and Deyber, Dr. Bawden divides the total presented evidence for the theory into two classes, first, that derived from analogy, and second, direct 'proofs.'

The former class of arguments is based almost wholly on the ameboid movements of the cells of either epithelial, glandular, or cicatricial tissue or on those of the more recently described myo-epithelial cells of the sweat-glands. In criticism the author points out that these movements so far as epithelial and gland cells are concerned take place within the bodies of the cells and are not at all concerned with any alteration of their external contours. Ameboid movement in cicatricial tissue-cells, that is chiefly in case of the leucocytes, no one pretends to deny, but the resemblance between these (the interesting and mysterious white corpuscles of the blood and lymph) and nerve cells is by no means apparent; they do not seem properly comparable in the direction concerned. Again, the waxing and waning of pigment in retinal cells, given by some as analogical evidence, is likewise not at all comparable to the supposed neuronal movements in question, for the granules of pigment merely appear and disappear functionally within the bodies of the retinal cells—the cells' outlines are not changed thereby.

The direct evidence offered by the supporters of the retraction theory relates mainly to the structural and functional status of the gemmules or varicosities seen by a number of observers on the neuronal projections of different kinds. Demoor, Stefanowska and Manouélian furnished Deyber and Duval their chief observational data [see an abstract of a report of work by Stefanowska on this subject, following].

The "facts show," says Dr. Bawden, "that a theory of movements of the protoplasm of the neurones cannot be carried out without constantly interpreting the histological evidence in terms of the histological procedure." This is apparently the root of the whole matter, for it seems that 'histological procedure' produces certain changes in protoplasm sometimes scarcely to be distinguished from natural organs. In fine, then, the critic considers many of the appearances noticed by a number of good histologists to be nothing more than artefacts produced by manipulation; or else that too little allowance has been made for the changes (other than protoplasmic movements in the sense obtaining in this case) which occur in cells under varying physiological conditions. "It is not denied that the neurones may possess an ameboid property. It is simply denied that the evidence which has confidently been put forward in support of the theory is wholly trustworthy as it has been employed. The real problem still remains, which is to demonstrate the existence of this property in living [neural?] tissue. It must be shown in some way which will obviate



the effects of the reagents used, that there is an actual change in the spatial relation of the ultimate ramifications of the nerve cells."

The opinion then derived from a careful study of all available material, which Dr. Bawden wishes to express in this widely summarizing paper of his, is that the theory of ameboid movement of the neurones is unsupported by the facts. And, after all, it does seem probable that the whole hypothesis and the great mass of work and discussion it has occasioned is a rather too striking example of science by the deductive method—that the wish is father to the supposition.

*Sur le mode de formation des varicosités dans les prolongements des cellules nerveuses.* MICHILINE STEFANOWSKA. *Annales de la Société royale des sciences médicales et naturelles de Bruxelles*, Tome IX., fasc. 3, 1900. Pp. 1-18.

This article is a report of an enlightening research done at the Institut Solvay, the details of which the specialist may find in the third part of volume third of the *Travaux de l'Institut Solvay*, under the title, 'Localisation des altérations cérébrales produites par l'éther.' In this present paper, however, the proper product of the two years' work is reported and in a form valuably suggestive to the physiopsychologist. Eight illustrations add to the clearness and conciseness of the article.

The varicosities in question, it is perhaps needless to explain, are those minute nodules or enlargements which, seen so often on the various branches of neurones, have caused so much discussion and conjecture. One of their chief causes of interest to the psychologist lies in their quasi-relation to the now perhaps decadent hypothesis that the ends of the neuronal prolongations spontaneously extend and retract in performing their various functions.

Mlle. Stefanowska's general method of studying these varicosities was simply to etherize white mice to various degrees, examining then their cerebral cortices, properly prepared, under the microscope with high-power lenses. As has been said, she devoted two years to systematically carrying out this, in principle, simple procedure, making it probable that her opinions on the matter are worthy of more than passing notice.

The researcher considers that the changes taking place in the neurones of variously etherized animals may be conveniently divided into four phases, as follows: In the first phase, the dendrites have lost their 'pyriform appendages' (the minute, very short branchlets which give them in some places their characteristic mossy appearance), and have

become quite smooth like the naked collaterals. In the second phase, the dendrites have become made up of short segments alternately transparent and opaque. In the third phase, many dendrites have become apparently sprinkled with a fine powder, or covered with very minute granulations, the axis cylinders and the collaterals as well often partaking in this change. Magnification of from 500 to 600 times is necessary to clearly bring this out in the early formation stage of the varicosities. In the fourth, or final phase, the varicosities are conspicuous on both the cylinder axes and the dendrites, especially in the cortex at the base of the brain, but also in more superficial regions of the cerebrum.

It has been conjectured by some that the formation of these little nodules is due to a mechanical contraction and coalescence of the finer projections of the neurone, but Mlle. Stefanowska, after the examination of thousands of carefully prepared specimens, emphatically concludes that "*the formation of the pearls on the dendrites and on the white fibers is due to a sort of liquefaction of the protoplasm under the influence of the agents which disturb the normal life of the neural cells.*" This is the important opinion of the article—that these varicosities are pathologic formations due to a more or less complete poisoning of the living protoplasm of which the neurones are composed. They are, it is concluded, droplets of liquified protoplasm. This is supported by the well-known fact that these 'pearls' may be produced after the death of the animal as well as before, and by poisoning agents other than ether. Again, the smaller varicosities produced by a slight etherization disappear only several days after they are produced by the action of the anæsthetic—much too long a time to allow the admission that it is a normal process, due to a functional retraction or contraction of the dendrites.

Anæsthesia seems, then, to the writer of the report a distinctly abnormal state, and the varicosities on the neuronal branches seem by no means to be accounted for on any theory of spontaneous neural ameboidism.

*The Central Connections of the Auditory Nerve.* ALDREN TURNER.

Journal of Laryngology, Rhinology and Otology, XV. (3), March, 1900. Pp. 131-133.

We have here an account of experiments performed on the brains of monkeys which seem to add something to the knowledge of a part of cerebral anatomy at present even more obscure than it is important. This study of the conducting paths was carried out by means of the

well-known method of degeneration first largely used by Waller, while the tissues were prepared for the microscopic examination by Marchi's plan. The experiments were of three sorts, first, section of the auditory nerve distal to the auditory ganglion; second, by study of the degenerations following lesion of the accessory auditory ganglion; and third, destruction of the internal geniculate body.

Dr. Turner's own summary will best present the products derived from the experiments: "(1) The cochlear fibers end in the accessory auditory ganglion. (2) In the auditory ganglion the fibers of the corpus trapezoides arise, some passing through the lateral filet of the same side and some crossing to the opposite side. (3) The superior olivary bodies have a close and direct connection with the trapezoid body. (4) Fibers coming from the auditory ganglion pass directly to the internal geniculate body of the opposite side. (5) There is no evidence that fibers pass directly into the temporal lobe without interruption in the internal geniculate body. (6) The complementary corti-fugal tract of the auditory system is the so-called temporo-pontine tract of Bechterew."

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

## NEW BOOKS.

*Experimentell-psychologische Untersuchungen über das Urteil.* K.

MARBE. Leipzig, W. Engelmann. 1901. Pp. iv + 103.

*The Wisdom of Passion.* "SALVARONA." Boston, Mystic River Book Co. 1901. Pp. 248.

*Das Problem des Wirkens und die monistische Weltanschauung, mit besonderer Beziehung auf Lotze.* MSCISLAW WARTENBURG. Leipzig, H. Haacke. 1900. Pp. 256.

*Kants Theorie der Kausalität, mit besonderer Berücksichtigung der Grundprincipien seiner Theorie der Erfahrung.* M. WARTENBURG. Leipzig, H. Haacke. 1899. Pp. viii + 294.

*Public Worship.* JOHN HYLAN. (Religion of Science Series.) Chicago, Open Court Publishing Co. 1901. Pp. 94.

*Beiträge zur Akustik und Musikwissenschaft.* Herausgeg. v. CARL STUMPF. 3. Heft. Leipzig, J. A. Barth. 1901. Pp. 147 + 11.

*Dottrini di Rosmini sull'essenza della materia.* C. GUASTELLA. Scuola Tip. "Boccone del Povero." 1901. Fasc. 1-2. Pp. 20 + 17.



- Proceedings of the Aristotelian Society.* New Series, Vol. I. London, Williams & Norgate. 1901. Pp. 239.
- Introduction to Psychology.* MISS M. W. CALKINS. New York, Macmillans. 1901. Pp. xv + 509. Price, \$2.
- Dictionary of Philosophy and Psychology.* Ed. by J. MARK BALDWIN. In three volumes, I. and II., the text, sold with or without III., the Bibliographies. Vol. I., A to Law. New York and London, Macmillans. 1901. Pp. xxiv + 644. Large 8vo. \$5 (21s), per volume.
- Descartes' Meditations and Selections from the Principles of Philosophy* (and other matters). Trans. by J. VEITCH. Chicago, Open Court Co. 1901. Pp. xxx + 249.
- Report of the (United States) Commissioner of Education, 1899-1900*, Vol. I. Washington, Government Printing Office. 1901. Pp. lxxx + 1280.
- Physik des Seelenlebens.* J. PIKLER. Leipzig, Barth. 1901. Pp. 40. M. 1.20.
- Psychologische Untersuchungen zur Bedeutungslehre.* ED. MARTINAK. Leipzig, Barth. 1901. Pp. vii + 98. M. 3.
- Panideale, Psychologie der sozialen Gefühle.* R. HOLZAPPEL. Leipzig, Barth. 1901. Pp. 233. M. 7.
- Begriffe und Theorien des Modernen Physik.* J. B. STALLO. Trans. from the English, with Introduction by E. MACH. Leipzig, Barth. 1901. Pp. xx + 332. M. 8.50.
- La Psychologie du Rêve.* N. VASCHIDE and H. PIÉRON. Paris, Baillière. 1902. Pp. 95.
- La Mémoire.* J. J. VAN BIERVLIET. Bibl. intern. de Psychol. exper. Paris, Doin. 1902. Pp. 349. Fr. 4.
- Reports of the Cambridge Anthropological Expedition to Torres Straits.* Vol. II., Physiology and Psychology. Part I.: Introduction and Vision. W. H. R. RIVERS. Cambridge, Univ. Press. 1901. Pp. vi + 132. Large 8vo. 9s.

## NOTES.

DR. NICHOLAS MURRAY BUTLER, professor of philosophy and education in Columbia University, has been made acting president of that University, in consequence of the resignation of President Seth Low.

THE following appointments are announced: J. E. Lough, Ph.D. (Harvard), lately of the State Normal School, Oshkosh, Wis., to

the professorship of psychology in the School of Pedagogy, New York University; Frank Nicholas Spindler, as professor of psychology and pedagogy in the State Normal School, Stevens Point, Wis.; A. H. Yoder, of the University of Indiana, to the professorship of pedagogy in the University of Washington; E. B. Holt, M.A. (Columbia), Ph.D. (Harvard), as instructor of psychology in Harvard University, succeeding Dr. Robert MacDougall; W. J. Shaw (M.A., Toronto), formerly at Wesleyan University, as demonstrator in psychology at Princeton University; Dr. Earl Barnes, lately of Stanford University, as special lecturer in pedagogy in the School of Pedagogy, New York University; Dr. W. H. Sheldon, as assistant in philosophy at Columbia University.

DR. J. H. HYSLOP, professor of logic and ethics in Columbia University, has been given leave of absence for a sabbatical year; Dr. A. L. Jones, as lecturer, will take his courses. We regret to hear that Professor Hyslop is ill; he will spend the year in the Adirondacks.

DR. E. E. SCHIEB, of the University of South Carolina, has been appointed professor of philosophy and pedagogy in Tulane University; Dr. T. H. Haines has been appointed assistant professor of philosophy in Ohio State University.

WE note that the Sidgwick memorial is making satisfactory progress. It is decided to establish a Sidgwick Lectureship in moral science, open to women, at Cambridge. The American subscriptions are not as full as seemed reasonable to expect and it is hoped that further amounts will be sent to the undersigned before the close of the year 1901. An international committee has also been appointed to raise a fund for a statue of Auguste Comte to be placed in front of the Sorbonne, Paris. Subscriptions to this fund may also be sent to the undersigned at Princeton, N. J., for the committee.

J. MARK BALDWIN.

WE welcome the first number (July, 1901) of *Archives de Psychologie de la Suisse Romande*, edited by Th. Flournoy and Ed. Claparède, both of the University of Geneva. (Appearing at irregular intervals from the press of Eggimann, Geneva, and Alcan, Paris: Fr. 3.50 per number, Fr. 12 per volume.) We congratulate its editors upon its fine appearance and wish it a long and prosperous career.

Two other new journals are announced which should be of interest to psychologists: *Biometrika*, devoted to the scientific study of bio-

logical problems (already noted in these pages; the publishers are Cambridge University Press, Clay & Sons, London); and *Annalen der Naturphilosophie* (editor Ostwald; collaborators, Mach, Ratzel, J. Loeb, etc.; publisher, Fock, Leipzig), devoted to universal questions of epistemology (*Erkenntnistheorie*) and of scientific method. These journals both illustrate the union of scientific and philosophical enquirers.

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BEGINNING November 1, 1901, all editorial matter for this REVIEW, together with books for notice, should be sent to Professor J. McK. Cattell, Garrison-on-Hudson, New York. Business communications should be addressed as heretofore to Professor H. C. Warren, Princeton, New Jersey.



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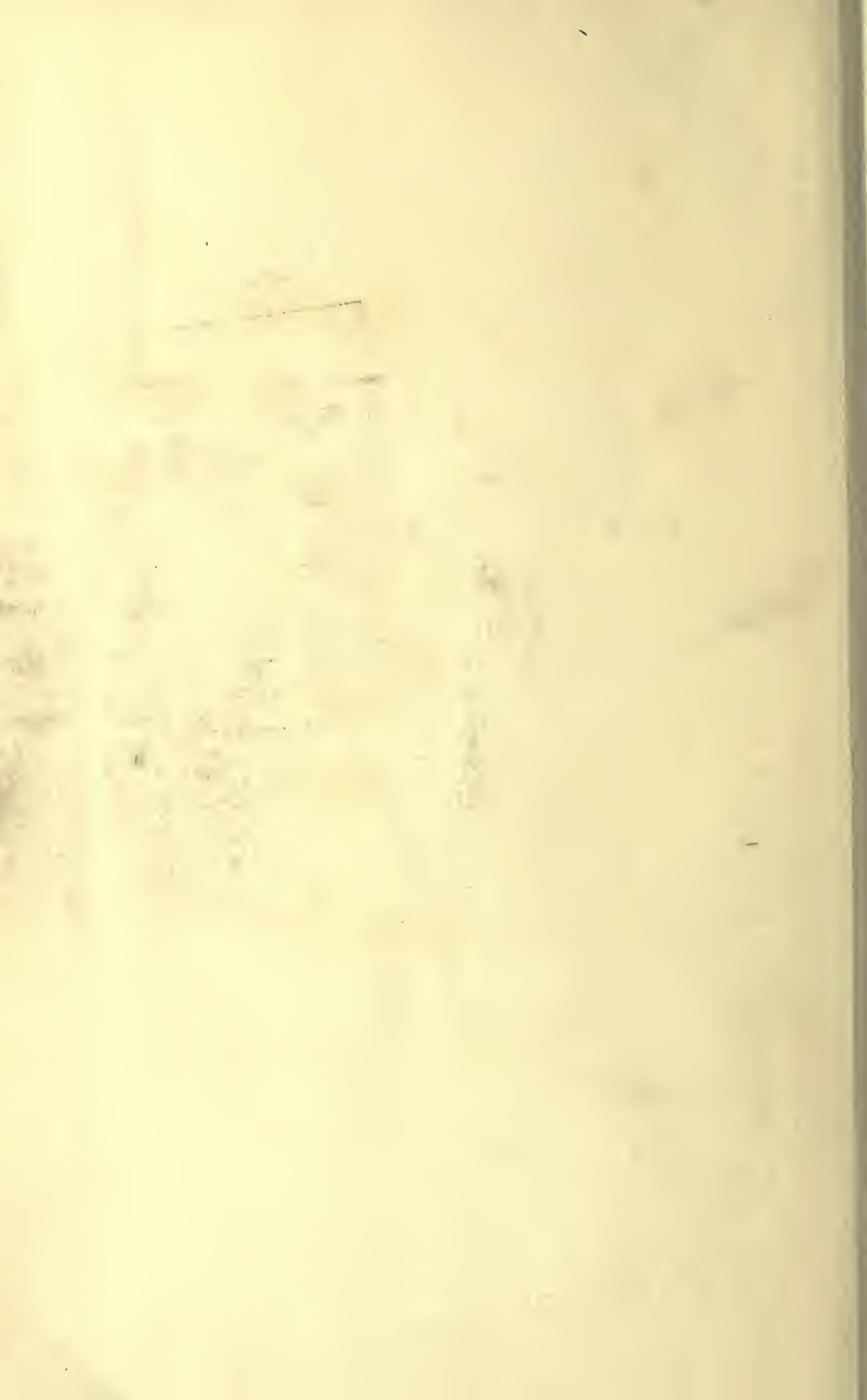












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